EPISTEMOLOGY OF BRIDGING THE THEORY-PRACTICE GAP IN STRATEGIC MANAGEMENT

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ABSTRACT

Knowledge can be created by scholars seeking to advance theory or by practitioners seeking to improve performance and competitive positioning. 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 is memorably captured by Lewin's (1945) maxim, "Nothing is as practical as a good theory." A growing chorus of researchers and practitioners, however, has protested the wide, and growing, theory-practice gap in strategic management. I argue in this paper that bridging the gap is necessary and possible, and that this will require consideration of three core aspects of the gap that have been treated severally but not jointly: strategic management as a social science and an applied science; the sharply different worldviews and modes of gaining and using knowledge for scholars and practitioners; and the fungibility property that permits exchange and cocreation of new knowledge between scholars and managers. Engineering and medicine, like strategic management, are also applied sciences that confront theory-practice gaps. Extracting useful lessons from their experiences and practices, and adding the essential element of strategic management as a social (not physical or biological) science, I developed a model for scholar-manager collaboration that generates empirically-grounded theory advancement and theory-driven performance improvement in a structured sequence of steps. The model is scalable for community-wide implementation, yielding results that can be consolidated into insights that bridge the theory-practice gap in strategic management.

> *"It is a vulgar fallacy to suppose that scientific inquiry cannot be fundamental if it threatens to become useful, or if it arises in response to problems posed by the everyday world." (Simon, 1982: 475)*
>
> *"Problems that are grounded in reality are not reaching researchers with the capacity to undertake relevant research, and practitioners are losing (or have lost) connection to needed help in addressing the real-world phenomena they face, increasingly without a perception on their part that the potential to advance the level of management expertise is even available. As the gap widens, the symbiosis withers, and each side separates from the other." (Drnevich et al., 2020: 48).*

Introduction

A major theory-practice gap exists in strategic management. Extensive literature on the gap (e.g., Bresser and Balkin, 2022; Pfeffer, 2014; Gioia, 2022), also called research-practice gap (Mintzberg, 1991) or relevance gap (Starkey and Madan, 2001), attests to its significance for strategy scholars. Existing studies that have attempted to close the gap include innovative approaches such as engaged scholarship (Van de Ven, 2007), evidence-based management (Rousseau, 2006), and problem-focused, engaged, and pragmatic scholarly discovery (Drnevich et al., 2020). Further, Louis and Bartunek (1992) proposed "Insider/outsider research teams: Collaboration across diverse perspectives"; Rynes et al. (2001) empirically examined the role of academic-practitioner relationships in both generating and disseminating knowledge; Starkey and Madan (2001) suggested "Bridging the relevance gap: Aligning stakeholders in the future of management research"; Burgelman and Grove (2007) combined Andy Grove's experience as Intel CEO with longitudinal field research; Feldman and Orlikowski (2011) described "Theorizing practice and practicing theory"; Sharma et al. (2022) proposed "Cocreating forward: How researchers and managers can address problems together"; and Spencer et al. (2022) suggested "Interweaving scholarship and practice: A pathway to scholarly impact".

And yet recent survey results show that the gap is growing (Choudhury et al., 2021). Mindful that knowledge is fungible (capable of flowing from theory-to-practice as well as practice-to-theory), and that the theory-practice gap has a significant and adverse impact on both theory *and* practice, SMR's Editorial Aims and Scope notes: "The journal takes seriously the input of practitioners to shape strategy scholarship …. The SMR will facilitate integration of strategy research by embracing contributions from multiple disciplinary perspectives and by fostering input from managerial practice to academic research". New knowledge, whether generated in theory or practice, is the common currency that powers advances in both; strategy research rarely reflects this essential duality of theory and practice. And even when cross-fertilization of knowledge across the theory-practice divide is attempted, it is done in small samples, limiting the generalizability and external validity of the findings to the field at large. In addition, disciplinary perspectives outside the social sciences are typically excluded in the strategy literature. As Bettis (1991: 318) cautioned, strategic management needs to loosen "the straitjacket of incremental, footnote-on-footnote approach of a premature normal science" and adopt bolder, innovative approaches to make real progress.

I argue in this paper that it is necessary and possible to bridge the theory-practice gap. Reaching this goal will require systematic attention to three key aspects that prior studies have largely failed to consider. These aspects provide a roadmap for how this paper is structured. First, the historical antecedents of the gap are examined; next, a comprehensive epistemological evaluation of the gap is conducted; and then, pooling together insights from the first two aspects, an integrative model of knowledge coproduction is developed that simultaneously achieves scientific rigor and practical usefulness. The paper concludes by outlining an "action plan" as well as broader implications for future research.

It is useful to begin by asking what events originally triggered the theory-practice gap and why the gap has stubbornly persisted over decades. Institutional antecedents can provide crucial contextual information on the gap's origins and root causes.

A comprehensive epistemological evaluation of the gap would acknowledge that strategic management is at once a *social* science and an *applied* science. As a social science, the two primary actors who possess vital-yet-partial knowledge that must be brought into contact are the scholar and the manager. However, these actors typically hold dramatically different worldviews and different modes of gaining and using knowledge. An essential prerequisite therefore for bridging the gap is to better understand and align these actors' goals and motivations.

And as an applied science, the field of strategic management would do well to recognize that there are other applied sciences, such as engineering and medicine, that also struggle with connecting scholarly research and theory development to practical implications and usefulness. With their significantly longer histories, and aggressive efforts to bridge their theory-practice gaps (including measures such as translational research), engineering and medicine have learned lessons that strategic management can usefully absorb.

The knowledge coproduction model presented here draws upon the yin-yang interdependency of theory and practice, where each is necessary, but neither is sufficient by itself, to gain the needed holistic understanding. (As Immanuel Kant put it, "Theory without practice is empty; practice without theory is blind.") The model is scalable in order to reach capillarily deep into the academic and practitioner communities on one hand, and on the other, systematically consolidate the findings into insights that bridge the theory-practice gap in strategic management.

The Theory-Practice Gap

In 2001, Fellows of the Academy of Management voted Frederick Taylor's *The Principles of Scientific Management* (1911) the most influential management book of the twentieth century. Before he became an author and a pioneer in the formal study of management, Taylor was a practitioner. Trained in mechanical engineering, he observed, theorized, and experimented in his climb through the ranks of middle, senior, and top management. In so doing, he embodied the attributes of theoretician as well as empiricist-experimentalist-practitioner. His theory reflected practice and vice versa. There was no gap between the two. In a similar vein, Leiblein and Reuer (2020: 25) noted that "In earlier years, strategy scholars often had substantial work experience, engaged heavily in consulting, and attended conferences that consultants and business people also frequented".

Over time, however, as studies grew in number, scope, depth, and sophistication (Perrow, 1973), the role of researcher bifurcated from that of the practitioner. This decoupling of academic scholarship from practice gained further momentum with the publication in 1945 of a White House report that helped shape U.S. science and technology policy, a policy that continues to cast a long shadow on current research in all sciences, including social sciences. Because specialists in theory and in practice failed to connect and learn from their counterparts, serious concerns arose, both among managers (e.g., "A renowned CEO doubtless speaks for many when he labels academic publishing a 'vast wasteland' from the point of view of business practitioners", Bennis & O'Toole, 2005) and among researchers (e.g., "The practicing managers we are ostensibly describing and explaining do not read our literature", Gioia, 2022).

Birth of the Gap: The Linear Model

The Biblical phrase “swords to plowshares” refers to converting military weapons and technologies into peaceful applications. This was the rationale behind a letter President Franklin D. Roosevelt wrote to Vannevar Bush, Director of the U.S. Office of Scientific Research and Development, in late 1944. With the end of World War II in sight, the president requested from Bush recommendations on how America might find peacetime uses for its massive war capabilities. Bush responded with a comprehensive report that became a landmark in American scientific history: *Science – The Endless Frontier: A Report to the President on a Program for Postwar Scientific Research* (1945).

In this report Bush argued that funding basic research is in the national interest because science is the “pacemaker” of technological progress. In a section titled “The Importance of Basic Research,” the report defined basic research as “research without thought of practical ends” which leads to general knowledge about nature and its laws, versus applied research that is focused on solving practical problems. Tellingly, he also suggested cordoning off “basic” from “applied” research because of “a perverse law” governing research under which “applied research invariably drives out pure.”

The approach for societal welfare that Bush proposed became known as the “linear model” of innovation, since it postulates that innovation starts with basic research, then adds applied research, development, and ends with production and diffusion (Godin, 2006). Bush equated basic research with science and drew a causal link between science and socioeconomic progress. The linear model codified and institutionalized the separation of “pure”, “fundamental”, or “basic” science from “applied” science, the former referring primarily to theoretical understanding, the latter to practical application. Any connection between the two was viewed as incidental, a spillover benefit of new knowledge flowing *from* theory *to* practice. Thus, the premise of Bush's support for basic science as a driver of economic growth was that fundamental discoveries precede, and subsequently may lead to, successes in the application of that knowledge.

Rich illustrations abound of the validity of the linear model's theory-to-practice pathway. When the electron was discovered in 1897, it had no known practical use; modern e-commerce revolves around the electron. Theoretical advances in particle physics unleashed the atom's immense power for wartime purposes (the bomb) and peaceful purposes (nuclear power). NASA's publication, "Benefits stemming from space exploration" (https://www.nasa.gov), documents societal benefits produced by human activity in space, including satellite telecommunications, GPS, weather forecasting, solar panels, implantable heart monitors, cancer therapy, and a global search and rescue system. Today, the James Webb space telescope peering into intergalactic space at the edge of the universe, and CERN's Large Hadron Collider probing the nature of subatomic matter at the Big Bang, are driven by an explorer's curiosity, with no immediate commercial applications and unknown future benefits to humankind.

Eminent physical scientists forcefully articulated this lack of concern for practical use, and indeed, boasted of actively distancing research from practice:

- We prided ourselves that the science that we were doing could not, in any conceivable circumstances, have any practical use. The more firmly one could make the claim, the more superior one felt. (Lord C.P. Snow, Cambridge University scientist in physical chemistry, quoted in Stokes, 1997: 31)
- With my first experiments I was sometimes asked by the press what they were good for. And I said with pride, It's good for nothing. I'm doing this purely out of curiosity. (Anton Zeilinger, co-winner of the 2022 Nobel Prize in Physics)

Some eminent social scientists echoed this absolute separation of theory from practice:

- For many years I have begun every class that I have taught with the assertion: I am not now, nor have I ever been, relevant. (James March, at his 1999 Academy of Management Distinguished Scholar Award ceremony)

Science – The Endless Frontier enshrined a fundamental divide between basic and applied research and was influential in widespread adoption of these silos in U.S. federal government policy, favoring the former and marginalizing the latter. But pitting basic research against applied research creates a needless conflict that is taken up by program managers who make decisions on government funding of contracts and grants. These managers require researchers to write applications that conform to the basic-versus-applied labels. Researchers, in turn, organize their activities in ways that optimize the stated objectives of the grants, creating an entire ecosystem geared to reinforce Bush's stance that to increase the quantity of innovative products (the back end), all one needs to do is to increase funding for basic research (the front end).

Ironically, however, the principal example that Bush cited to show that a product of scientific enterprise helped win World War II, namely the radar (Radio Detection and Ranging), became possible not from scientific discovery but from an engineering innovation called multi-cavity magnetron (Narayanamurti and Odumosu, 2016). Thus, although theory has the potential to inform practice, the reverse pathway is also possible. Theoretical understanding doesn't always precede practical application. Innovation isn't always "linear". Layton (1971, 1974) questioned the linear model's one-way directionality and showed that the scientific and technical professions had grown up along parallel paths during the 19th century, when engineers didn't merely apply the knowledge produced by scientists. Rather, technology was its own form of knowledge that sometimes reached advanced stages of development *before* scientific explanations emerged of how the technologies worked. The X-ray and penicillin are transformative – and accidental – discoveries, not the products of basic science. James Watt invented the steam engine before laws of thermodynamics were postulated.

Science does offer powerful insights to technology; more and more technology is science-based. At the same time, a complementary change in recent decades has been that developments in technology became a far more important source of the phenomena science undertook to explain, since many of the structures and processes that basic science explored were unveiled only by advances in technology; more and more science, in other words, is technology-derived. Using Nobel Prize-winning examples (e.g., magnetic resonance imaging, the transistor, the laser), Narayanamurti and Odumosu (2016) studied the "daily micro-processes of research" to show how distinctions between the search for knowledge (theory) and creative problem solving (application) break down when one pays attention to the ways in which pathbreaking research actually happens.

Such two-way flows of knowledge, theory-to-practice and practice-to-theory, enable both to advance (Rosenberg & Birdzell, 1990), *provided* there are open channels of communication and sharing of specialized expertise across permeable theory-practice, basic-applied, science-engineering, understanding-use, discovery-invention, and similar divisions. Studies in strategic management, however, have often failed to grasp that innovations can be theoretical or practical, that powerful ideas can emerge along multiple points in a complex continuum that spans the factory floor, a consulting engagement, an executive suite, or a scholar's desk. Despite the bidirectionality of knowledge flows, "research questions are increasingly driven by data availability or methodological considerations" (Bettis and Blettner, 2020) and "theory is being drawn from parsing texts rather than experience" (Suddaby 2014), that is, scholars often get their research ideas from each other, not from practitioners. And practitioners look for best practices not from academic research but from industry peers, best-selling books, consultants, and practitioner journals such as *Harvard Business Review.* In a provocative essay, Hambrick (2007) pointedly noted researchers' overemphasis on theory, almost entirely forsaking the *practice* of management:

> Management's idolization of theory began, harmlessly enough, as an outgrowth of the field's efforts to demonstrate academic worthiness. In the late 1950s, blue-ribbon Carnegie Foundation and Ford Foundation reports levied withering attacks on business schools for their lack of academic sophistication. As a result, in the 1960s and 1970s all fields of business adopted a new commitment to drawing from basic disciplines (e.g., economics and psychology), to analytic rigor, to the virtues of normal science and, above all, to theory. A scan of the top journals in marketing, accounting, finance, and management for the mid-1970s reveals a pervasive incorporation of theory. Since then, however, the other fields have relaxed their single-mindedness about theory. Confident in their academic standing, other business fields regularly publish – in their top journals, no less – papers that are not particularly theory-based or theory-oriented. Management, however, is stuck. Like insecure adolescents who are deathly afraid of not looking the part, we don't dare let up on our showy devotion to theory" (p. 1347).

The two blue-ribbon reports Hambrick referred to, Gordon and Howell (1959) and Pierson (1959), urged that management research and education should be approached through the three root stems of organizational behavior, economics, and quantitative methods. Tangible evidence for the impact of these reports soon followed, with the proportion of mathematically-framed articles in the *Academy of Management Journal* rising from roughly 25 percent during 1959 – 1966 to nearly 100 percent during 1972 – 1978 (Goodrick, 2002). Marking the reports' 50th anniversary, *The Economist* (2009) observed that although the reports helped business schools become “more respectable” in research terms, they had the fatal flaw of not placing sufficient emphasis on the practical skill of management itself. In effect, by pushing research toward theory featuring “academic sophistication” and “analytic rigor,” with little consideration for the theory’s practical implications or for the possibility that rigorous investigation of real-world problems may lead to theory advancement, as Taylor had done, the reports brought Bush’s linear model to business schools, perpetuating the theory-practice gap.

This gap exacts a steep toll: scholars seldom get their research questions from practice (Suddaby, 2014); managers seldom get their best practices from research (Gioia, 2022). Scholarly research in strategic management is often dismissed as ivory tower ideation, even as practitioners complain of “driving blind” without the support of reliable research. How do managers gain new knowledge and how do scholars gain new knowledge? The next section examines the sources of learning and the complementary linkages that bind scholars and managers in a web of knowledge production and utilization.

Epistemologies of Scholars and Managers

Epistemology is the study of the nature and grounds of knowledge about phenomena: how a person comes to know what he or she knows (Mitroff and Mason, 1982). The Oxford English Dictionary defines knowledge as “the apprehension of fact or truth with the mind; clear and certain perception of fact or truth; the state or condition of knowing fact or truth.” Apprehension of facts or truths can be achieved through the epistemologic paths of critical thought, observation, or experiment, each path involving distinct human reasoning processes (Churchman, 1971).

Scholars combine observation with critical thought to produce new knowledge through abstraction, aggregation, and generalization of particular cases. Practitioners, too, are “creators of knowledge" (Pfeffer and Sutton, 1999), combining the paths of observation and experiment: "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: 5-6).[1]

Knowledge is fungible, through whichever epistemologic path it is gained; it cannot be compartmentalized into a theory silo and a practice silo. Good ideas (new knowledge) born during the theorizing process can be immediately used to improve practice; insights generated as practitioners strive to improve performance outcomes (sometimes called incremental or radical innovation) can likewise be used to strengthen theory. Kaplan underscored the fungibility of knowledge by saying "Theory is *of* practice" (1964: 296, emphasis in original). Theory can emerge only from the phenomena it purports to describe, explain, and understand. In the literatures on knowledge, knowability, and limits of knowledge, the Genetic Argument refers to the paradox that "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" (Williams, 2001: 176). Practitioners must have in mind a theory (implicit or explicit) that guides their decision making; and theorists must have some empirical context for theorizing. A gap that locks the two in separate vaults denies the complementarity and inextricably intertwined nature of theory and practice.

Management as a *Social* Science

There is a fundamental "unity of science" that cuts across all disciplines, because "all sciences, whatever their subject matter, are methodologically of one species" (Kaplan, 1964: 31). But as a social science, management has epistemologic differences that separate it from the physical and biological sciences, in that the nature of the subject-matter under study is different. Organizations, and the people who comprise them, are not immutable physical objects like atoms or cells that have unchanging properties (Drnevich et al., 2020). Rather, “the data for behavioral science are not sheer movements but actions – that is, acts performed in a perspective which gives them meaning or purpose,” so we must “distinguish between the meaning of the act to the actor and its meaning to us as scientists, taking the action as subject-matter” (Kaplan, 1964: 32). Strategy scholars, in other words, are involved in a double process of interpretation, having two different things to understand: from the manager’s perspective and from the researcher’s perspective. The theory-practice gap cannot be bridged absent an acceptance of the uniqueness and importance of practitioners’ and researchers’ perspectives.

In linguistics, the terms phon*etic* and phon*emic* distinguish sound structures as analyzed by a linguist and a native speaker, respectively (Morey & Luthans, 1984). “Emic” denotes a research orientation centered on the native’s, or insider’s, view of reality. “Etic” denotes a research orientation centered on outside researchers, who have their own categories by which the subject’s world is organized. The emic view posits that “the subject, not the researcher, is the best judge of the adequacy of the research”; the etic view posits that “the researcher is the best judge of the adequacy of the research; the subject’s opinion may be interesting, but it is not really relevant” (1984: 29-30). Kaplan (1964: 310) cites the Jain parable of ancient India, featuring six blind men and an elephant, to demonstrate how observers of the same object may come away with entirely different impressions.[2]

A scholar and a manager, likewise, each sees a valid and useful – albeit partial – view of the phenomenon under study. As an insider, the manager has an emic, or subjective, perspective; as an outsider, the scholar has an etic, or objective, perspective. Managers possess a deep reservoir of localized knowledge gained through firsthand experience. Scholars possess expertise in the philosophy of science, methodology of research, and the theories that are relevant to the manager's industry and company. Managers typically do not have the luxury of pausing to conduct qualitative, cross-sectional, or longitudinal studies, relying instead on their practical, on-the-ground experience; scholars may not be privy to the demands of the day-to-day decisions that the manager must make. Separately, neither is able to appreciate the whole elephant. Jointly, their perspectives complement each other to provide the most complete, accurate, and useful account that can help both.[3] Morey and Luthans (1984) clustered these terms to propose two research approaches, objective/nomothetic/quantitative/outsider and subjective/idiographic/qualitative/insider. Past scholarly work in management has consistently shown that the former approach dominates the latter (Eisenhardt 1989), contributing to relative neglect of the manager's perspective and to perpetuation of the theory-practice gap.

Beyond the Linear Model: Pasteur's Quadrant

Stokes (1997) reconceptualized the linear model's basic-applied research dichotomy by adding a second dimension: considerations of use. Previously, the question of whether research is inspired by a quest for fundamental understanding was answered in a binary fashion, yes for basic research and no for applied research. However, the reality of theory-to-practice and practice-to-theory knowledge flows make this an oversimplification of the actual process of knowledge generation. Stokes's quadrant model comprises a 2 x 2 matrix that also weighs whether research is inspired by considerations of use (yes or no), such that four cells emerge.

Quadrant A: Quest for fundamental understanding? Yes. Considerations of use? No. Exemplar: Niels Bohr. Bohr's search for a model atomic structure was a pure voyage of discovery, basic research guided solely by the quest for understanding without thought of practical use.

Quadrant B: Quest for fundamental understanding? No. Considerations of use? Yes. Exemplar: Thomas Edison. Edison's research was guided solely by applied goals without seeking a more general understanding of the phenomena of a scientific field. A brilliant inventor, Edison kept his Menlo Park coworkers from pursuing the deeper scientific implications of what they were discovering and focused on electric lighting and other commercially profitable products.

Quadrant C: Quest for fundamental understanding? Yes. Considerations of use? Yes. Exemplar: Louis Pasteur. Pasteur's basic research in microbiology sought to extend the frontiers of understanding but was also inspired by considerations of use.[4] This quadrant is entirely outside the conceptual framework of the Bush report.

Quadrant D: Quest for fundamental understanding? No. Considerations of use? No. Exemplar: German notion of *Wissenschaft*, or exploring particular phenomena without aiming for general explanatory objectives or any applied use to which the results will be put.[5]

Thus, 52 years after the publication of Vannevar Bush's report, Stokes laid out a more realistic view of research, its motivations, and its fruits. However, Stokes's focus on researchers' motivations, rather than on new idea generation (i.e., the fuller picture of theoretical advances and their practical implications) limited the ability of his model to meaningfully go beyond the linear model. Most significantly, his quadrant model preserves the language of basic and applied research. And yet research is a complex, nonlinear process. To go beyond the linear model (basic versus applied), Narayanamurti and Odumosu (2016) proposed the terms "discovery" and "invention", where discovery is the "creation of new knowledge and facts about the world" and invention is the "accumulation and creation of knowledge that results in a new tool, device or process that accomplishes a specific purpose" (pp. 31-32).

Thompson (1956) envisioned that "An administrative science will be an applied science, standing approximately in relation to the basic social sciences as engineering stands with respect to the physical sciences, or as medicine to the biological" (p. 103). In the words of C.H. Llewellyn Smith, former Director-General of the European Organisation for Nuclear Research (CERN), science roughly equals knowledge and technology roughly equals the means by which knowledge is applied. Engineering and medicine are the technologies for applying scientific knowledge gained from fundamental academic disciplines such as physics, chemistry, and biology. Certainly, technology can learn from, and inform, science, as discussed above. But engineering and medicine are still concerned with the application of knowledge to solve practical problems, not with uncovering some of nature's best-kept secrets about basic truths that govern static and universal phenomena. Similarly, management is the technology that adds value by generating and answering practically relevant questions about real-world phenomena in the dynamic environment in which organizations operate.

Strategic management is not alone in confronting challenges of the theory-practice gap. The problem occurs in other "applied" fields as well. We examine next how other fields structure synergistic interactions between scholars and practitioners.

Strategic Management as an *Applied* Science: Lessons from Engineering and Medicine

Thompson (1956) observed that "Achievements in the physical and biological sciences, and in their sister applied sciences (engineering and medicine), have demonstrated most convincingly the practical value of theory …. Basic discoveries in the biological and physical sciences are incorporated at applied levels with impressive speed. Effective channels have been built for funneling new knowledge into medicine and engineering. By contrast, administration is relatively isolated from the basic social sciences” (p. 110).

Engineering and medicine have much longer histories than management. The two reports on business education discussed above, Gordon and Howell (1959) and Pierson (1959), were published after the Flexner (1910) report on medical education and the Grinter (1955) report on engineering education. Medical education draws upon the basic sciences of biology, physics, and chemistry and on the underlying sciences of anatomy, physiology, pathology, and pharmacology; engineering education draws upon the basic sciences of mathematics, physics, and chemistry and on the underlying sciences of fluid mechanics, thermodynamics, electrical theory, and properties of materials. These sciences have centuries-long histories. In contrast, business education draws upon the basic sciences of mathematics and behavioral-social sciences and on the underlying sciences of organizational behavior, quantitative methods, and economics. (Mintzberg [2022] offers an interesting variation on this schema.) Of organizational behavior, Gordon and Howell conceded that “Research on organizational problems is still in its infancy” (1959: 382). Quantitative methods, likewise, had only a brief track record, having evolved through military applications during World War II. Economics was the only relatively mature field among the underlying sciences for business education. Still, historical differences notwithstanding, younger sciences can observe and learn from how more mature sciences continue to struggle with epistemologic problems that are common to all sciences.

Engineering

In their book *Cycles of Innovation and Discovery: Rethinking the Endless Frontier*, Narayanamurti and Odumosu (2016) analyzed two very different models of research: Bush’s linear model discussed above and a model practiced at Bell Labs in Murray Hill, New Jersey. The latter model has produced multiple Nobel Prizes in science and has also created pathbreaking practical applications. In other words, "theory is successfully productized" and "rigorous science is taken to market" in the Bell Labs model. Why the difference between the two models and what might management learn from engineering?

A major challenge facing the communication industry in 1947 was to build an amplifier that would allow signals to be sent clearly over long distances. At Bell Labs, William Shockley and his colleagues in the solid-state research lab showed how a small bit of germanium was able to amplify an electrical signal, something that had until then required the use of a vacuum tube: the transistor had been invented. This transformative achievement, Narayanamurti and Odumosu argued, became possible because of a combination of (1) an interdisciplinary team (2) that is capable of moving between theory and practice, between discovery and invention, between building devices and testing them, and (3) an institutional culture supportive of such work. Unfortunately, such porous boundaries between research and application remain an exception, not the rule, as these authors illustrate with two current examples: creating solutions to future energy requirements and BRAIN (brain research through advancing innovative neurotechnologies). In both cases, a stovepipe mentality and bureaucratic fear of crossing boundaries between pure and applied science has segregated research efforts and prevented an integrative, holistic model of research where the practical effects of theoretical findings have been left untested.

Nonetheless, in science and engineering research, "deeply grounded in theory" and "significant practical benefits" are not always mutually exclusive. Engineers can be engaged in activities that are "self-evidently science" and scientists can be engaged in activities that are clearly engineering. Jack Kilby, electrical engineer, won the 2000 Nobel Prize in Physics for his invention of the integrated circuit. Charles Kao, electronics engineer, won the 2009 Nobel Prize in Physics for his work on optical fiber communication. Similarly, scientific breakthroughs in materials science have led to new materials with enhanced properties used in engineering applications in aerospace, construction, electronics, and manufacturing; breakthroughs in quantum mechanics have revolutionized electronics by enabling the development of smaller, faster, more efficient memory chips and sensors; and breakthroughs in renewable energy, energy storage, and environmental sciences have contributed to the development of cleaner, more sustainable energy sources such as solar panels, wind turbines, battery technologies, and energy-efficient systems for buildings and transportation.

Conversely, the National Academy of Engineering's Charles Stack Draper Prize for Engineering is recognized as the preeminent award for engineering achievement, equivalent to the Nobel, honoring "an engineer whose accomplishment has significantly impacted society by improving the quality of life." Among the many scientists who have received engineering's highest honor is T. Peter Brody, a physicist, who won the 2012 Draper Prize for his work on liquid crystal displays.

The linear model at best captures a partial view of how new ideas are born, how innovation occurs, and how "knowledge grows through a richly interwoven system of scientific and technological research in which there is no clear hierarchy of importance and no straightforward linear trajectory" (Narayanamurti, Odumosu, & Vinsel, 2013: 31). McKelvey (2006) used the "knowledge food chain" metaphor to illustrate this point: "In earthquake country, the engineering food chain looks like this: physics, earthquake science, engineering, city building code departments, builders, buyers" (p. 822). Conversely, new demands from buyers (e.g., higher earthquake resistance; lead pipe or asbestos elimination; energy efficiency) may force builders to respond, leading to building code revisions, engineering standards adaptation, and new research in the sciences. There is no fixed starting or ending point for knowledge creation (research) or knowledge application (practical use).

Implications for management from the experiences of the more mature applied field of engineering include the following: Institutional barriers perpetuate the theory-practice gap. Institutions can deliberately choose to create the conditions that remove barriers, increase the porosity of boundaries between research and application, and recognize the fungibility of knowledge. Just as more and more technology is science-based and more and more science is technology-derived, the field of management can change its institutional practices whereby practice is increasingly based on rigorous, relevant theory and research is increasingly derived from practical problems. Some means for achieving these objectives are discussed in the next section.

Medicine

Scientific discoveries in biology, genetics, and pharmacology are used to understand disease mechanisms, identify potential drug targets, and design drug molecules with specific properties. These findings are then translated into the development of pharmaceuticals and therapies. Scientific discoveries in physics and signal processing can lead to advances in imaging technologies (e.g., MRI, CT scans, PET scans) that allow for non-invasive visualization and detection of diseases, assisting in diagnosis and treatment planning. Scientific discoveries in biomechanics and biomaterials contribute to the development of advanced prosthetics, implants, and medical devices, which enhance patient mobility, replace or augment lost bodily functions, and improve overall quality of life. McKelvey's (2006) knowledge food chain in medicine is:

“Biology, medical research, medical schools, PhDs/MDs, 4^{th}- to 1^{st}-level hospitals, GPs, patients.” He is careful to point out that “Food chains can be read from either direction. Thus, in life science, the discovery of DNA eventually leads to new molecules in drugs that cure patients. The increasing prevalence of Alzheimer’s disease in patients leads to stem cell research” (pp. 822-823).

However, taking a promising drug molecule or other medical idea from a science lab to its intended destination (the patient or, more broadly, the health system) is typically a long and cumbersome process, the equivalent of a theory-practice gap in management studies. To expedite the process, the medical profession has developed “translational research,” a term that has at least two meanings (Woolf, 2008). The first is the “bench-to-bedside” enterprise of harnessing knowledge from basic sciences to produce new drugs, devices, and treatment options for patients; this is the interface between basic science and clinical[6] medicine, the end point being a new treatment that can be used clinically and commercialized (“brought to market”). More formally, in this version translational research refers to “effective translation of the new knowledge, mechanisms, and techniques generated by advances in basic science research into new approaches for prevention, diagnosis, and treatment of disease essential for improving health” (Fontanarosa and DiAngelis, 2002).

Translational research has another, more expansive meaning, one in which discovery and production of a new molecule, the endpoint for bench-to-bedside, is only the starting point. This version is especially relevant for health services researchers and public health investigators whose studies focus on health care and health as the primary outcome. Here, translational research refers to translating research into practice, that is, ensuring that new treatments and research knowledge actually reach the patients or populations for whom they are intended and are implemented correctly.

The National Center for Advancing Translational Sciences (NCATS) defines the goal of translational research as “to translate [move] basic science discoveries more quickly and efficiently into practice” and outlines a “translational spectrum” comprising five stages of translation, as shown in Figure 1.

Put Figure 1 about here

Two observations are in order. First, the entire process, from the discovery phase of basic science research (T0) to translation to societal application and impact evaluation (T4), rests on intense knowledge sharing and feedback loops across willing partners, as indicated by bi-directional arrows throughout Figure 1. The specialized knowledge produced in biological research facilities at stage T0 (microscopy, proteomics, histology, flow cytometry) is processed, in order, in clinical research facilities (T1, T2), in commercial and clinical partnerships (T3), leading finally to rollout to communities (T4), where Implementation Science and Health Economics are used to assess population-level outcomes and impact on policy. Thus, the overall objective of the five-stage process is to achieve optimal fit between a new idea (drug molecule, device, etc.) to the needs of the particular target population for which it is intended.

Second, the goals, settings, study designs, investigators, and challenges (“roadblocks”) differ for bench-to-bedside and the broader version. The former involves “the transfer of new understandings of disease mechanisms gained in the laboratory into the development of new methods for diagnosis, therapy, and prevention and their first testing in humans;” the latter involves “the translation of results from clinical studies into everyday clinical practice and health decision making” (Sung, Crowley, & Genel, 2003: 1281).

Bench-to-bedside research requires knowledge of molecular biology, genetics, and other basic sciences; appropriately trained clinical scientists working in laboratories equipped with cutting-edge technology; and a supportive infrastructure within the institution. This version struggles with biological and technological mysteries, trial recruitment, and regulatory concerns.

The “laboratory” for the broader version of translation research is the community and ambulatory care settings, where population-based interventions and practice-based research networks bring the results of bench-to-bedside to the public. This version requires different research skills: knowledge of the implementation science of fielding and evaluating interventions in real-world settings and of the disciplines that inform the design of those interventions, such as clinical epidemiology and evidence synthesis, public policy, financing, informatics, and mixed methods/qualitative research. This version struggles with infrastructure and resource constraints, human behavior and organizational inertia, and the messiness of proving the effectiveness of “moving targets” under conditions that investigators cannot fully control.

Implications for management from the experiences of the more mature applied field of medicine include the following: Management can and must make bridging the theory-practice gap a priority and create mechanisms to achieve this goal. Medicine’s laudable efforts in translational research can offer a blueprint for management. Specific action items for executing such a program in management are outlined later in the paper. A caveat applies, however. Given the epistemology of management, “the gap is not simply the product of a knowledge transfer problem (i.e., converting or translating knowledge from theory to practice …. But rather, is a much more fundamental knowledge production problem (i.e., a problem in how we create knowledge and what knowledge we create)" (Drnevich et al., 2020: 42). Bearing in mind the history of the theory-practice gap, lessons from other applied fields, and the importance of emic/etic perspectives, the next section proposes a model of scholar-manager partnerships.

Redirecting the Focus of Management Studies: Co-Production of Knowledge in Scholar-Manager Collaboration

The subject-matter of social science involves people, not physical or biological objects with unchanging properties like atoms or cells, as noted above. Bridging the theory-practice gap therefore involves bringing key actors who possess vital knowledge into contact in a carefully structured progression of interactive steps as shown in Figure 2. Each step represents specific action items for scholars and managers and will be discussed in turn.

Put Figure 2 about here

T-1: Communication between boundary-spanners. Effective communication is a prerequisite for successful collaboration. Prior studies have often ignored this crucial lesson. Because the parties involved likely come from very different "worldviews" (Biscaro and Comacchio, 2018), before commencing joint work, it is essential that the groundwork be laid for ensuring shared understanding. This involves aligning the scholar's and manager's ontological and epistemological assumptions, language and metaphors, and end goals.

Gioia (2022) emphasized why scholars must learn that for managers, multiple, socially constructed ontological realities may exist; managers make decisions based on these realities. Managers, in turn, must learn that for scholars, the nature of reality may contain elements that are fixed, stable, observable, and measurable; scholars gather and analyze these data.

Epistemologically, scholars must learn that valid ways of gaining useful knowledge may include understanding the meaning of the process or experience (emic); managers must learn that valid ways of gaining useful knowledge can include scientific research that is objective and quantifiable (etic).

Language also plays a crucial role in shaping worldview (Carroll, 1956) and in creating shared understanding and integrating knowledge across boundaries (Carlile, 2004; Whittle, Vaara, & Maitlis, 2023). For scholars and managers communicating across deep knowledge differences, close attention to each other's language becomes important. Just as multinational corporations invest in cross-cultural training programs to enhance their prospects in foreign countries (Black and Mendenhall, 1990), facilitating effective communication is a critical initial step in ensuring that scholars and managers talk to each other, not past each other.

Effective communication requires scholars and managers to be transparent about their desired end product; although both broadly wish to understand what makes organizations effective and efficient, their interest in working together is driven by somewhat different final outcomes, with scholars seeking to extend theory and managers seeking to strengthen competitive position and improve performance metrics. These goals should be shared and the benefits of collaboration *to the other side* should be outlined in a "value proposition." Scholars may initially need to sway managers with a clear, compelling statement of the anticipated benefits of collaborating on coproduction of knowledge. In this, as Nelson (2016) noted, knowledge sharing can simultaneously advance and challenge both academic and commercial interests; managing these sharing/secrecy tensions around scientific knowledge disclosure is therefore an important concern.

T0: Problem identification. Following the foundational, context-setting step of facilitating effective communication between the principal actors (T-1), the model's next step is for the scholar and manager to jointly identify and prioritize (rank-order) problems to work on. Haveman et al. (2020) challenged scholars to consider "What *substantively important questions* are being ignored by management research". For bridging the theory-practice gap, substantively important questions are the problems that manager consider to be intractable, strategically important, and in need of in-depth investigation. This is consistent with Simon's (1982: 475) observation that "The real world, in fact, is perhaps the most fertile of all sources of good research questions" and with Hamel and Birkinshaw's (2023) observation that new knowledge in management research should meet the criteria of novelty, salience, and usability *for the practitioner*.

Such substantive, managerially important questions can sometimes also involve theoretical anomalies. As Poole and Van de Ven (1989) noted, little attention has been paid to the tensions, inconsistencies, and contradictions among explanations of the same phenomenon, such that current management literature contains dilemmas and paradoxes that need resolution. Pursuing, rather than dismissing, these apparent anomalies can offer important opportunities to develop deeper knowledge that at once creates practically useful solutions and more encompassing theories.

T1: Ideation. The next step in knowledge coproduction is for the scholar to evaluate whether and to what extent existing theory helps in understanding, explaining, and predicting the phenomena under investigation. If there is a good fit, the research becomes a replication and validation story, where current practice may be accurately explained by extant literature. In cases where the phenomena are beyond the scope of extant literature, or where practice is inconsistent with received theory, genuine opportunities lie for probing the disconnect: How does current theory need to be revised or expanded? What new theoretical explanations might fit the observed reality and what testable hypotheses might be derived from these new explanations? As Barney (2005) wrote, "I did not know it then, but my consulting experience had actually led me to the question that was to organize my intellectual life for the next twenty years" (p. 290). Barney began to look for "theory opportunities," or "any actual business phenomenon that is apparently inconsistent with received theory" (p. 298).

Ideation's important role is well captured by Ployhart and Bartunek (2019: 496), who inverted Lewin's maxim ("Nothing is as practical as a good theory") to suggest that there is nothing so *theoretical* as good *practice,* and added: "So here's the surprising conclusion: If we as scholars start by focusing on contemporary organizational phenomena, then our theory more likely will be novel and insightful (two characteristics that are frequently used to gauge 'contribution'). Our work may not fit as neatly into already established academic categories as we are used to; phenomena don't know or care about academic silos, programs of research, disciplinary assumptions, or convention. Therefore, understanding phenomena may require us to develop new approaches, new cross-disciplinary insights, and so on in ways that have not been considered. And, thus, relaxing the steel grip of past theory to more fully embrace contemporary phenomena can lead to more innovative theoretical developments that are also actionable."

T2: Operationalization. T2 extends T1's theoretical conclusions to the realm of practice. Novel and actionable theoretical ideas are molded into a concrete action plan by the scholar-manager team, which identifies specific changes to current practice that would permit an empirical test of the merits of the conclusions drawn from the previous step.

Developing practical recommendations from new theory involves the merging of corporate and academic mindsets and an intense exchange of theoretical and practical knowledge. Much knowledge is, however, tacit and sticky (Szulanski, 1996), making its transfer problematic even when there is a will to do so. This is among the potential barriers to bridging the theory-practice gap, and it further reinforces the importance of facilitating effective communication as outlined in step T-1 above.

A good example of operationalization is Tushman and O'Reilly's (2007: 771) work with IBM managers over five years. "Anchored in joint respect for research as well as relevance", this project produced virtuous cycles whereby "knowing affects doing and doing, in turn, affects knowing." IBM was fully aware that these scholars had distinct research agendas in innovation, leadership, culture, and design. By applying research to IBM's strategic issues, practice at IBM was favorably impacted and theoretical understanding was deepened in key strategy areas such as ambidextrous design, dynamic capabilities, and senior teams. Importantly, the level of trust built over the years resulted in a unique social network data set and in access to the full set of IBM's attempts to execute cross-line-of-business innovation.

T3: Proof of Concept. In T3, recommendations for practice jointly developed by scholars and managers in T2 are implemented. However, there is typically a vast number of variables and unknowns involved, which make the outcomes uncertain. Before a full roll-out, it is customary in fields such as drug development, software development, film making, and engineering, to launch a limited (also known as pilot or beta) test of the action plan.

Called proof of concept (POC), this is a demonstration in principle, with the aim of verifying that an idea is feasible, has practical potential, and its underlying assumptions are valid. POC also helps to identify potential challenges and unanticipated limitations and thus to gather feedback to refine the idea as appropriate. As Reynolds (1971) noted, such studies in controlled settings allow certain types of measurements to be made that could not have been made in natural settings and allow researchers to isolate certain processes that are confounded with other processes in natural settings.

In 1878, for example, Louis Pasteur accidentally exposed chickens to an attenuated culture of cholera. Later, when the chickens were exposed to cholera culture, the effect was the opposite of what he expected: they did not die but survived, revealing to him the theoretical insight of the immune effect. Then in a public display of POC, Pasteur ran a controlled experiment on two groups of sheep, a control group that did not receive weakened-virus shots and another group that did. After being exposed to the actual virus, sheep in the former group died and those in the latter group remained healthy. Germ theory was born and the idea of vaccines entered the medical profession and the public's consciousness. By jointly developing and implementing action items that merge the strengths of both theory and practice in controlled settings, management studies can similarly take major strides forward.

T4: Evaluation. Anteby (2013), Hudson and Okhuysen (2014), and Behfar and Okhuysen (2018) have argued persuasively that in evaluating knowledge claims about "how we know" and "what we know", it is important to bring the researcher, as an active reasoner, into focus as a central player. In T4, the scholar and manager jointly evaluate whether POC trial outcomes meet expectations, exceed expectations, or fall short. Meeting or exceeding expectations would support the notion that such collaborations can effectively select pressing real-world problems, ideate and operationalize solutions, and test those in controlled settings to achieve real progress that neither could have done alone; theory and practice both advance. The next step (T5) discusses scaling up, where the results of many such studies, each addressing specific aspects of a larger phenomenon, are integrated into broader conclusions (Cronin et al., 2022).

If, however, evaluation of T3 results fails to meet expectations, opportunities arise for fresh learning. Feedback loops, as represented by dotted arrows in Figure 2, point the way to reexamining the theoretical explanations (T1), practical recommendations jointly derived from those explanations (T2), and the POC trials implementing the recommendations in controlled settings (T3). This evaluation may lead to correcting one or more of these earlier steps, itself an advance in our understanding of organizations.

Step T4 has the important goal of deeper understanding that strengthens practice with the benefit of theoretical insight and, concurrently, reshaping theory to more accurately and usefully reflect real-world phenomena. Theory and practice both emerge stronger, and more closely tied, through this process.

T5: Scale Up. In translational research in medicine, new molecules are “scaled” from bench to bedside to community (Figure 1, steps T0, T2, T4, respectively). In organization studies, “satisfactory outcomes” in T4 are scalable in at least three ways. One is to expand from successful POC trials in controlled settings to full operations within a company. Another is for the scholar, with encouraging results in hand, to collaborate with larger groups of managers in different organizations facing similar real-world problems, to implement the action items and evaluate the results in broader studies. Such enlargement of scope is possible while maintaining standards of rigor and validity (Eisenhardt, 1989; Yin, 1984). A third scaling strategy was proposed by Lakatos (1970), who argued that knowledge growth emerges from progressive research programs that involve multiple theory components, each of which is focused on a specific aspect of a larger phenomenon.

Scaling up is critically important because, as Shapiro et al. (2007: 263) explained, “The solution must involve *all of us*”, that is, we need *community-wide* adoption of knowledge coproduction in management. Such democratization of research, where large numbers of scholars collaborate with equally large numbers of managers in the quest for novel solutions to complex real-world management challenges, can galvanize a virtuous spiral of stronger management theory and organizational performance at an unprecedented scale. The question arises, however, as to whether the proliferation of studies may also create the problem of integrating newly-gained insights coherently in ways that move the field forward. Researchers have proposed two approaches to address the potential for “overabundance” of theories.

Leavitt et al. (2010) defined theory “pruning” 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, a greater volume of practically relevant research projects in a broad variety of contexts, covering diverse strategy topics, can be funneled into a narrower set of generalizable knowledge streams.

Similarly, Cronin et al. (2021) noted that discussion typically tends to focus on improving “unit theory”, which frames empirical work on specific aspects of a phenomenon, rather than “programmatic theory”, which orients scholars toward what the unit theories collectively support as settled science. Arguing that programmatic theory must drive the research process, these authors proposed a model for how verified unit theories collectively make up programmatic theory.

Outcomes of scaled-up implementation in broader studies (T5) can be satisfactory or unsatisfactory. Unsatisfactory outcomes provide opportunities for deeper exploration of the variance from expectations. Feedback loops, as represented by dotted arrows in Figure 2, can suggest where changes might be needed. Satisfactory outcomes represent improved organizational performance for managers and, for scholars, offer publishable results that combine the strengths of theory and practice.

In a survey, Shapiro et al. (2007) explored whether the field of management has a *knowledge transfer* problem (that may be solved by more effective translation of research into publications, frameworks, and tools that managers can use in their work), or a *knowledge production* problem (that may be solved by more collaborative joint research efforts between scholars and practicing managers). Solutions to the former problem, called lost *in* translation, might focus on changes to editorial policies at top journals, development of new practitioner-oriented journals, and more formal recognition and rewards for publications with a substantial impact on practice. For a knowledge production problem, any chance for impact on practice is lost *before* translation, so solutions might focus on ways to foster more researcher-practitioner collaboration as research programs are developed and carried out. Any solutions, survey results showed, must face some hard facts: academics and managers do live in different worlds; the gap between research and practice is wide; and solutions must address both lost before translation and lost in translation problems. Figure 2 in this paper addresses lost *in* translation problems by facilitating effective communication (Step T-1) and ensuring that research results are translated into practice (T3, T5). Figure 2 also addresses lost *before* translation problems by engaging both sides in problem identification (T0), development of recommendations for practice (T2), evaluation of results (T4), and scaling up (T5).

The premise underpinning Figure 2 is straightforward: Solving chronic problems of highly complex social systems – organizations – is beyond the capability of current knowledge in management theory; it is also beyond the capability of current knowledge in management practice. So long as the two knowledge pools remain distant and uncoordinated, the complementarity of theory and practice will remain unrealized. Closure of the theory-practice gap via creation of valid, useful, generalizable insights beneficial to both scholars and practitioners is an attainable goal; a systematic path toward this goal was presented in Figure 2. The next section examines the practical implications of executing this research program.

An Action Plan[7]

The concrete actions resulting from Figure 2 differ substantially from current practices, not only for the individual scholar and manager but for the entire knowledge ecosystems to which each belongs. Stakeholders on the "theory" side include, in addition to scholars, their academic institutions (management departments, business schools, universities), professional bodies (e.g., AOM, SMS, AACSB), and scholarly journals (the so-called gatekeepers of research publications). Stakeholders on the "practice" side include, in addition to managers, their companies, industry associations, trade and practitioner journals, and so on. Successfully executing efforts to bridge the theory-practice gap will require buy-in and participation from all stakeholders.

Focusing first on specific changes needed on an individual level, more scholar-manager interactions will need to occur, with the scholar seeking to advance theory (while learning some key aspects of practice) and the manager eager to improve organizational performance (while learning some key aspects of theory). The traditional outsider/insider roles of scholars and managers are intermingled, with scholars entering the emic space and managers entering the etic space, and with both going deeper than description (the “what”) and into the “how” and “why” in order to create new, useful knowledge. For scholars trained to work on solo- or joint-authored papers with other scholars (etic only), and for managers conditioned to dismissing discussions involving theory and academic research (emic only), such collaboration will necessitate acquisition of new skillsets. Although expertise in one’s content area remains a necessary condition, it will no longer be sufficient. Both must be able to “speak” the other’s language and collaborate effectively in the coproduction of knowledge.

Such need for new skillsets in high-growth areas leads predictably to the creation of new positions and job titles. In medicine, for example, pharmaceutical companies, hospitals, and universities are aggressively training and recruiting for translational research (Carpenter, 2007). GSK, a major drug company, recently advertised for the position of “Associate Director of Translational Research Academic Partnerships”.[8] (A comparable opportunity for business schools would be to create positions of Translational Research Corporate Partnerships.) The Mayo Clinic offers a popular master’s program in translational research, aimed mostly at MDs (practitioners). On the research side, Gary Koretzky, associate director of University of Pennsylvania’s MD-PhD program, has emphasized translational research training for PhD scientists: “Institutions have started to recognize that if you give them [basic science PhDs] the vocabulary of medicine and a sense of how physicians think about problems that they encounter with patients, they’ll find it easier to do research that is both scientifically rigorous and relevant to disease processes and patient care.”

More generally, recognizing the urgency (and benefits) of speeding up the implementation of drug molecules and new therapies from theory to practice, medicine has moved quickly in the 21st century to create mechanisms and incentives that prioritize translational research. The

National Institutes of Health (NIH) has formed centers of translational research at its institutes and in 2006 launched the Clinical and Translational Science Award (CTSA). Dozens of CTSA-funded academic centers have been established. Universities are transforming themselves to compete for CTSA grants. (Comparable opportunity for Management: establish Management Knowledge Coproduction Award, to recognize achievements in each of the individual steps of Figure 2.) As a dedicated field of study in the medical context, PhD and graduate certificate programs in translational research have been created at Duke University, Emory University, George Washington University, among others. Foundations, disease-related organizations, and individual hospitals and health systems have also established translational research programs. Since 2009, hundreds of specialized journals devoted to the topic have been launched, including *Translational Medicine, American Journal of Translational Research, Clinical and Translational Science,* and *Annals of Translational Medicine.* After NCATS was established in 2006, Australia followed with the Translational Research Institute and Europe with the European Society for Translational Medicine.

The above burst of gap-bridging activity in medicine is not evident in business, in part because academic “elites”, such as deans and journal editors, are focused on the wrong metrics (Pfeffer, 1993, 2014). Urgent institutional mobilization is possible, but only if leaders, both on the theory side and the practice side, designate this as a priority, create mechanisms, and provide incentives and resources to make it a reality.[9]

For example, companies and industry groups need to understand and acknowledge their indispensable-yet-partial role in the knowledge food chain. With this understanding, they need to consider how allocating resources (such as new positions, dedicated personnel, data access) to collaborative projects with scholars can be investments with potentially high ROI in the form of actionable new knowledge, improved performance, and stronger competitive position.

In academia, new consensus is needed in the mission statements of professional bodies, universities, business schools, and management departments. AOM, SMS, and AACSB, for example, will need to articulate a new vision of the role of theory and the theory-practice relationship. Similarly, P&T standards that currently emphasize publications in "A" journals may need to be revised to include a minimum requirement of goal-oriented work between faculty and practitioners. As Pfeffer (1993: 613) noted, consensus is ultimately forged by the "elites," the leaders and gatekeepers of academic institutions. The elites in management scholarship may need to rethink the definition of "scholarly output" and what constitutes "outstanding research" in light of Pfeffer's (2014: 466) plea that the solutions to these problems "require some fundamental changes in how we evaluate and review colleagues and practice science." Bold new ideas for enhancing strategic management knowledge by changing the governance structure of academic journals have recently been proposed by Bresser and Balkin (2022).

Additional proposals consistent with the rationale of Figure 2 are available to facilitate the needed changes. Tushman and O'Reilly (2007: 771-772) suggested that if we wish to make operating in Pasteur's quadrant (blending theory and practice) an important mandate for business schools, then our doctoral programs must encourage students to identify research questions anchored in managerially important organizational phenomena. And since these phenomena are inherently cross-disciplinary, theory and methods from multiple disciplines should drive our students' training. This would require a matrix-like design, where a PhD or DBA student would choose a research question on the basis of a phenomenon (the horizontal axis of the doctoral program design) and then select several disciplines to inform this research question (the vertical axis of the design). Helped by senior faculty advisors, students could leverage executive education settings to get closer to their phenomenon of interest and test their ideas by interacting with managers knowledgeable about the world of practice. Such dissertations rooted in broad, substantive problems (e.g., interdependent innovation) can provide bases upon which emerging scholars can build research streams. This change in mindset would impact junior faculty, too. Their promotions are currently based on their ability to do first-class research, as judged by disciplinary rigor (Bohr's quadrant). Too often, however, such research is so discipline-based that it lacks external validity. Encouraging junior faculty to engage with practitioners while the practitioners are on campus – not as consultants but as researchers interested in more deeply understanding organizational phenomena – can help faculty discover gaps between phenomena as they exist in practice and the current state of academic knowledge. This is a win-win: the practitioner gets research-based insights; junior faculty get closer to the reality of the phenomena they are studying. The theory-practice gap shrinks.

Shapiro et al. (2007: 262), likewise, developed two recommendations that merit wider attention and adoption. One is sponsoring sabbaticals for academics in business practice as either "translators" of research results or as researchers on a set of practitioner-oriented research issues. The other is to encourage more practitioner sabbaticals as executives-in-residence at business schools or as fellows at research institutes in which they help shape and participate in research programs. Both ideas would promote more boundary-spanning behavior that acknowledges the *applied* nature of management.

Future Research Directions: Raising the Level of Knowledge Connectivity

Management is not an island; it resides, rather, in a knowledge network that includes management practice, other sub-fields of business, and non-business fields (which include, but are not limited to, social sciences). Each of these areas possesses its own vast literature and potential to contribute valuable insights. Unlocking the specialized expertise of other areas can leverage existing management scholarship, broadening and deepening it and opening pathways for new knowledge creation. In short, future research should extend the core theme of this paper: connecting knowledge pools for mutual benefit. Scholar-manager collaboration (Figure 2), in this sense, is only the first baby step in realizing the full potential of management knowledge development.

To be sure, micro-level 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 the *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. But it leaves untapped many other fields of knowledge that can potentially transform management by making it theoretically better grounded and practically more relevant.

The same fungibility of knowledge that Lewin (1945) wrote about, and that holds across the theory and practice divide of management, also holds across management and a number of other disciplines. But because as a field management has been largely inward-focused, it has forgone opportunities for synergistically leveraging the deep specialized expertise available in other disciplines.

A profoundly intriguing question is: What might have happened if Gordon and Howell (1959) had not urged in their influential report that management research and education be approached through the three root stems of organizational behavior, economics, and quantitative methods? As the expression “When you define a picture, everything else becomes the background" suggests, focusing attention on these three stems, each of which is no doubt individually useful, relegates to the background key issues that impact the *practice* of management.

Consider, for example, other sub-fields of business (e.g., R&D, production, finance, marketing, distribution). In a seminal article, Prahalad and Hamel (1990) noted that a company's core competence lies not in excelling in any single activity in the value chain, but rather in the *bundling* of knowledge and skills housed in various parts of the company: diverse groups of employees within the company coordinating their efforts in a customer-facing way to gain a sustainable competitive advantage. Although we may segment 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 strategic importance of this interrelatedness of all sub-fields of business was vividly driven home by the post-pandemic meltdown of global supply chains, when many corporate executives urgently sought permanent alternatives to China, along with entirely rethinking their sourcing, manufacturing, logistics, sales, and distribution practices. Proactively (rather than reactively) joining forces with other sub-fields of business can broaden management's reach into cognate areas of knowledge.

Going beyond sub-fields of business, Mason Haire (1964) asked a simple question that still awaits an answer: "Why have the social sciences contributed so little to the practice of management?" 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 economics, psychology, and sociology; but as a social science, what important opportunities might management be missing by ignoring the other areas listed above? To take one example, history 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. The same might be said of other knowledge areas in social sciences, whose impact on management is poorly understood and largely ignored.

The power of this idea can be illustrated with the launching in 1929 of the field-shaping journal *Annales d’histoire economique et sociale.* French history scholars Marc Bloch and Lucien Febvre sought to transform the study of history by opening it up to the concepts and developments in other branches of the human sciences, including economics, geography, sociology, politics, and anthropology. Although history had a strong tradition of rigorous research before *Annales,* a broader understanding of the complex range of factors that contribute to historical change could, and did, reshape and deepen the worldwide study of history.

Exciting prospects for expanding the horizons of future research in management are not limited to the social sciences. With the onset of the Fourth Industrial Revolution (Industry 4.0), four types of disruptive technologies are currently dominating discussions in global business: connectivity and computational power (e.g., cloud, blockchain, sensors, IoT); analytics and intelligence (e.g., AI, ML); human-machine interaction (e.g., VR, AR, advanced robotics, autonomous vehicles); and advanced engineering (e.g., nanoparticles, 3D printing, renewable energy). Every aspect of management in all types of industries – manufacturing, healthcare, retail, military, offices, construction, transportation, entertainment, smart cities, and more – is profoundly impacted by Industry 4.0, which gives management scholars opportunities to develop research approaches with the complexity and rigor that match the complexity and variety of the real-world problems being addressed.

Dealing with the integrated nature of organizations as complex social constructions within traditional disciplinary boundaries is increasingly infeasible; multidisciplinary, interdisciplinary, and transdisciplinary research is needed. As Choi and Pak (2006:353) noted, multidisciplinarity draws on knowledge from different disciplines but stays within their boundaries; interdisciplinarity analyzes, synthesizes, and harmonizes links between disciplines into a coordinated and coherent whole; and transdisciplinarity integrates the natural, social, and health sciences and transcends their traditional boundaries. There is an increasing need for merged expertise that goes beyond the interdisciplinary intersection of fields and leads to the emergence of new disciplines.

One example is the new area of tissue engineering, which combines developmental biology with engineering and materials sciences to replace or improve tissue, organs, and other biological functions (Sharp and Langer, 2011). "This is not a typical interdisciplinary situation where a cell type can be given over to an engineer or an engineer can guess what kind of scaffold will work in a biological system. Rather, there must be multidisciplinary collaboration *from the start,* with all participants having common reference points and language" (2011: 527). The challenge, and opportunity, for cutting-edge management scholarship is to learn from, and inform, specialists in nascent, high-growth areas of Industry 4.0 to jointly develop solutions to novel problems.

Conclusions

The primary focus of this paper has been to extend prior research by better understanding the epistemology of strategic management as a social science and an applied science, and thereby to develop a model of knowledge coproduction that joins the emic and etic perspectives to lead systematically to theory-driven, performance-enhancing solutions (new knowledge). Leiblein and Reuer (2020: 16) noted that, "At the field level, we believe that there are multiple pathways to achieving theoretically rigorous and managerially relevant research". Figure 2 offers one promising, actionable pathway. Specific actions, different from current practices, were outlined. Broad adoption of this model has the potential to transform the theory-practice gap into a theory-practice loop, with each scholar-manager collaboration yielding better theory and improved performance.

Chaudhuri et al. (2021: 14) proposed a set of questions for judging potential contributions of research to knowledge, among them: (1) Is this good strategy research? and (2) Is this good social science? We might add a third question to this useful list, (3) Is this good applied science? Bringing these elements together would validate SMR's mission and, more broadly, serve the needs of the academic and consultant-practitioner communities.

This would represent significant progress, but we need not stop there. In considering the question of whose interests academic research should serve, Davis (2015: 186) argued that scholars need to think beyond the academic community, beyond even the practitioner community: "our obligation is to society more broadly". To put this in its proper context, organizations are the dominant form of social structure. Better management of organizations, informed by relevant research, can raise the effectiveness and efficiency of numerous types of organizations (corporations, government, non-profit, religious, social, political, cultural, labor unions, charitable, educational, and more). McGahan (2022: 25) further extended this thought by urging application of 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 immigration, 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". With such high stakes, it is imperative that Hamel and Birkinshaw's (2023) lament that "In comparison to other fields such as medicine or engineering, the impact of management research is disappointing – most of it is neither practical nor profound" be addressed, quickly and effectively. An essential first step in deepening the societal footprint of strategy scholarship is to bridge the wide gap between theory and practice in strategic management.

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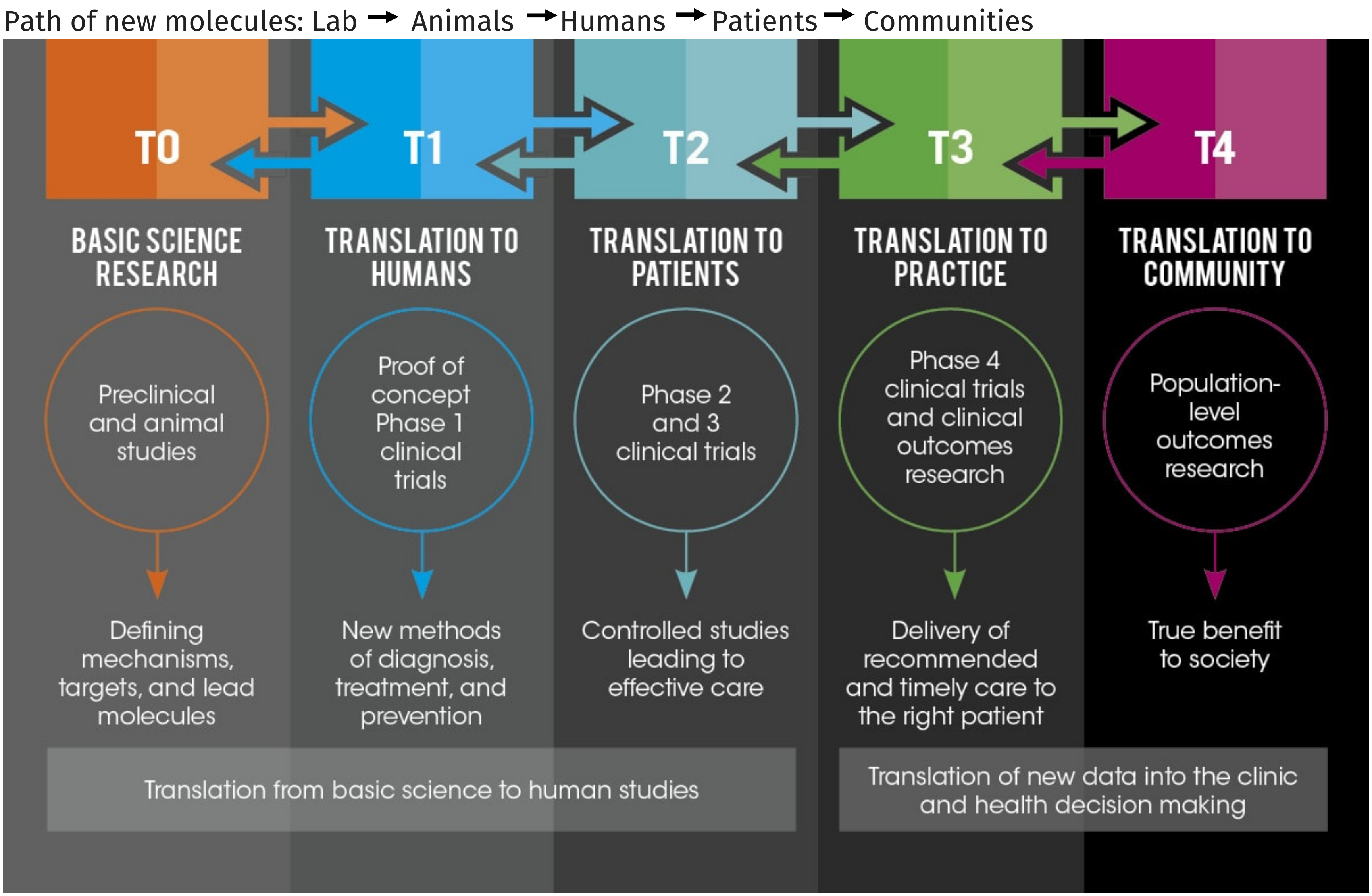
Footnotes

1. In his "Plea for Lawyer-Schools," Frank (1947) urged that training in legal theory be paired with practical exposure: "What would we say of a medical school where students were taught surgery solely from the printed page? No one would teach the art of playing golf by having the teacher talk about golf to the prospective player and having the latter read a book relating to the subject. The same holds for toe-dancing, swimming, automobile driving, hair cutting, or cooking wild ducks. Is legal practice more simple?" This logic of experiential learning can be extended to other applied fields such as business.
2. A group of blind men heard that a strange animal, called an elephant, had been brought to the town, but none of them were aware of its shape or form. Out of curiosity, they said "We must inspect it and know it by touch, of which we are capable." So, they sought it out, and when they found it they groped about it. The first person, whose hand landed on the trunk, said, "This being is like a thick snake." For another whose hand reached its ear, it seemed like a kind of fan. Another person, whose hand was upon its leg, said the elephant is a pillar like a tree trunk. The blind man who placed his hand upon its side said the elephant "is a wall." Another who felt its tail, described it as a rope. The last felt its tusk, stating that the elephant is that which is hard, smooth, and like a spear.
3. As knowledge theorist Chris Argyris (1991) might put it, both scholars and managers need to rise above single-loop learning (repeated attempts at the same issue, with no variation of method and no questioning of the goal) to double-loop learning, which entails the modification of goals or decision-making rules in light of experience. Rather than rigidly adhering to their respective perspective (etic or emic), double-loop learning broadens the perspective by recognizing that the way a problem is defined and solved can itself be a source of the problem, allowing for incorporation – in addition to their own knowledge – of new knowledge from the other side's perspective.
4. Stokes (1997: 79-80) provided additional examples of work by investigators who were directly influenced both by the quest for general knowledge and by considerations of use: Keynes wanted to understand *and to improve* the workings of modern economics; Manhattan Project physicists wanted

to understand *and to harness* nuclear fission; Langmuir wanted to understand *and to exploit* the surface physics of electronic components.

5. Quadrant D is not empty. Stokes (1997) cited as an example birdwatchers who track markings and the incidence of species, as reported in *Peterson's Guide to the Birds of North America.* Studies in this quadrant can be important precursors of research in Edison's quadrant or even, as happened with Charles Darwin's classic, *The Origin of Species,* in Bohr's quadrant.
6. "Clinical" refers to something involving or relating to the direct medical treatment or testing of patients.
7. I thank an anonymous reviewer for this suggestion.
8. Posted online on 22 June 2022, responsibilities of this position include: establishing strong relationships with the development teams to understand their research strategy and identify scientific gaps that academic collaboration can address; assist in identifying potential collaborative partners by developing a solid understanding of the research activity (through publications, congresses, and stakeholder insight) within translational and technology areas of importance to GSK Oncology and at key academic institutions.
9. Thoughtful executives and serious scholars might look for lessons in urgent institutional mobilization to prior success stories such as the Manhattan Project's race with Nazi Germany to build a functioning atom bomb (1942-1945); President Kennedy's moon shot initiative (1961-1969) that led from an aspirational challenge to a successful moon landing; and the unprecedented speed of testing, approval, production, distribution, and administration of mRNA vaccine (2020-2021) that saved millions of lives. In each case, overcoming seemingly insurmountable hurdles, theoretical knowledge was translated into practical results under tight deadlines.

Figure 1. Translational Research in Medicine: Basic Science to Practice



ource: University of Arkansas for Medical Sciences, Translational Research Institute, 2022. Reproduced with permission.

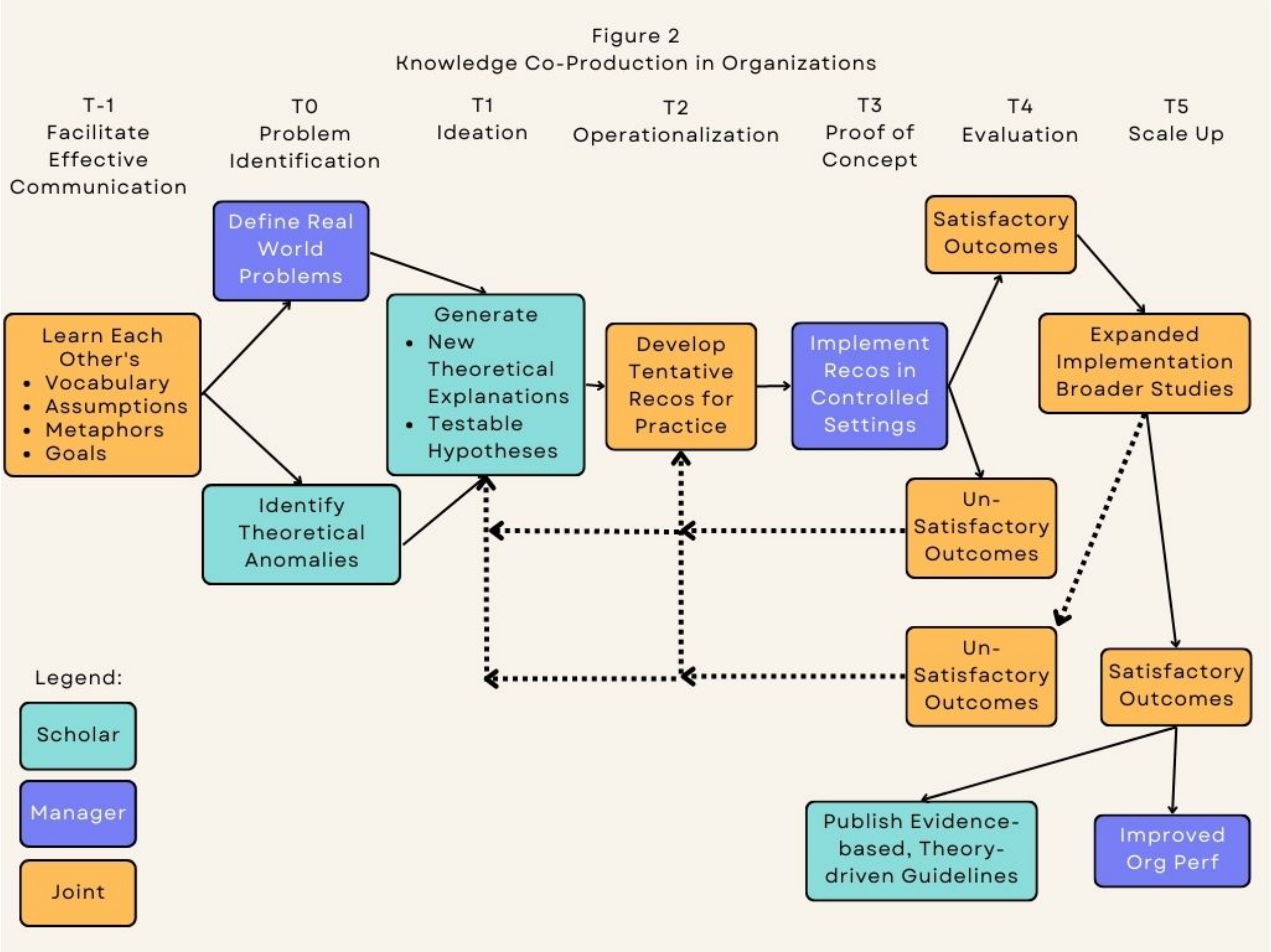


Figure 2
Knowledge Co-Production in Organizations
T-1
Facilitate Effective Communication
T0
Problem Identification
T1
Ideation
T2
Operationalization
T3
Proof of Concept
T4
Evaluation
T5
Scale Up
Define Real World Problems
Learn Each Other's
• Vocabulary
• Assumptions
• Metaphors
• Goals
Identify Theoretical Anomalies
Generate
• New Theoretical Explanations
• Testable Hypotheses
Develop Tentative Recos for Practice
Implement Recos in Controlled Settings
Satisfactory Outcomes
Un-Satisfactory Outcomes
Expanded Implementation Broader Studies
Un-Satisfactory Outcomes
Satisfactory Outcomes
Publish Evidence-based, Theory-driven Guidelines
Improved Org Perf
Legend:
Scholar
Manager
Joint