The State of the Union in the Field of Strategic Management: Great Theories. Imperative Problems.

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Abstract:

The field of strategic management has had great success over the past fifty years. We have developed bodies of theory that are compelling and important. However, these theories are not unified by a common conceptual architecture. As a result, further progress 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.

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What is the present state of the union of the field of strategic management? In this essay, I argue that the field has evolved over the past half century into a mature and important discipline in its own right. A hallmark of this success is the emergence of bodies of theory that are uniquely strategic, robust logically, and well-tested empirically. At the same time, a critical weakness in the field is the absence of a unifying, accurate framework of underlying constructs that describe the purposes of different types of organizations, including firms. We have built our theory primarily on abstract constructs that reflect a 19th century Benthamite view of organizations as austere entities rather than as a sprawling variety of forms that are variously formal, informal, complex, simple, temporary, permanent, hierarchical, modularized, market-matching, public, private, automated, unautomated, and so on. The absence of a robust framework of core constructs impedes the integration of ideas from strategic management with those from other disciplines. It also makes our field brittlely resistant to phenomenological developments that challenge foundational constructs. These developments, which include climate change and the pandemic, are of stunning importance. As a result, the field is under pressure as phenomenological changes compel coordinated strategic responses across institutions of many types. We don't have enough to say. We aren't helping as much as we might. It is imperative that we interrogate our foundations to strengthen our core constructs and integrate our theory. Only after we achieve greater synthesis can we support public-policy and other types of recommendations for allocating resources to address the pandemic, climate change, and other urgent problems that demand a strategic response from organizations of all types.

Theoretical Accomplishments and Challenges

Since its founding more than a half-century ago, the field of strategic management has flourished. This success has been driven in part by the development of critically important bodies of theory that have delivered insights for managers in organizations of all types, including for-profit corporations, non-profit organizations, and governmental agencies. Prominent bodies of theory include transaction-cost economics, organizational economics, capabilities-based views, the resource-based view, the behavioral theory of the firm, agency theory, evolutionary theory, mental models, value-based strategy, network analysis, eco-systems theory, non-market strategy, real options, innovation studies, bounded rationality, congruence theory, path dependency, human capital, stakeholder theory, and corporate social responsibility, among others. Empirical verification, extension, and development of each of these bodies – often using multiple methods, such as complementary qualitative and quantitative analysis – led to improvement in the theory. As a result, the epistemology of the field is reliable for researchers. The relevance and character of our core theories are not normally questioned in our publication processes, for example. By and large, we understand each other when we evoke these theories.

I would like to suggest, however, that there are some basic problems with our theories that reflect the circumstances of their histories. At its founding, the field of strategic management emerged primarily from the field of administrative studies, which was a professional area of scholarly engagement. Contributors to this field had backgrounds in operations research, military economics, industrial economics, business history, and business practice. At the outset, the ultimate goal of strategy was to establish a purpose that would guide members of an organization in coordinating effort, allocating resources, and engaging with one another and with external constituents to create value for society.

What marked the transition from administrative studies to strategic management was the integration of ideas with those from industrial-organization economics, finance, sociology, and social psychology. As it happened, the transition occurred contemporaneously with developments in Finance surrounding Milton Friedman's 1970 essay in *The New York Times* about the purpose of business as maximizing shareholder value. Qualitative and administrative conceptualizations of social value gave way to shareholder value creation – often through appreciation of retained earnings as represented by increasing profitability – as the orienting goal of strategic management. As economists sought to employ the insights that emerged from our antecedent fields to open the "black box" of the firm, the emergent theories of our field were founded on assumptions that reflected the traditions of neo-classical economics. Many of these assumptions – including perfect information, competitive markets, the normative legitimacy of Pareto optimality, etc. -- have since been challenged by scholars of strategic management, but their legacy continues to influence the ways in which we think about the relationships between organizations, markets, and competition. A central challenge has been difficulty in integrating insights, such as from the Carnegie School, through frameworks that accommodate variation in the extent to which organizations conform to various assumptions about the unification of action, and the rationality of unified organizational action. As a result, our bodies of theories generally point to principles and relationships that are robust, but we are up against the boundaries on the basic constructs of our field. Because we did not develop our bodies of theory from a framework of internally coherent first principles, but rather evolved from disciplines with conflicting theoretical premises and assumptions, our field of strategic management has constructed an architecture of ideas on an inherently shaky foundation.

These problems impede the development of the field in several specific ways. First, when we contribute to lines of theory in strategic management (i.e., those in the list above that begins with transaction-cost economics), we almost inevitably get tied up in the review process by questions about the veracity of the core constructs and central measures. We are asked questions such as: OK, you are writing through the lens of, say, the resource-based view, but what are these resources about which you are writing, really? Are they truly resources, or are they human capital (for example), and if they are human capital, aren't they subject to human agency and bounded rationality? These are only examples, and I could go on. My point is to illustrate that, because we have not achieved a synthetic framework on the core constructs and central measures that gird and connect all our bodies of theory, we often get stuck in circular arguments with reviewers that are often resolved through the addition at the ends of papers of paragraphs calling for further research to achieve a resolution.

Second, when we seek to address important questions that evoke issues at the boundaries between our bodies of theory, we are almost always set back by a failure of the integration. One of the most challenging facets of this type of problem arises from our very success: It is often viewed as offsides to cite foundational articles without citing their influential contemporary successors. All the work required to unravel the line – with all its complications and acknowledgements of problems of construct integrity – stands like a brick wall in the way of making scientific progress on improving the foundations. Consider, for example, the fate of a scholar who submits to one of our journals a paper on the perceptual biases of principals and agents in a principal-agent negotiation. This scholar would typically be pushed by reviewers – typically drawn from each of the relevant disciplines – to do justice in the paper to the entirety of the insights emerging from the various literatures on perceptions, biases, and agency. And then, perhaps at the second round of review, questions might typically arise about the initial conditions that gave rise to the authority and hierarchy of relationships between the actors, and the factors that gave rise to the transaction and situation, and so on. The broad point that I hope to convey is that, because we do not have a unifying architecture of foundational constructs and basic theoretical relationships between those constructs, we have built bodies of theory that are at once evolved and increasingly specialized, which makes their integration at the contemporary margin virtually impossible.

Third, these theoretical challenges in our field also mean that the large, phenomenological problems that stare us in the eye – i.e., problems that we sometimes have described as "the most important strategic challenges of our time" and/or "grand challenges" – are largely beyond the reach of our current bodies of theory at scale. Think of the pandemic, for example. None of our theories explain why and how pharmaceutical companies should commit resources to the development of life-saving vaccines from which they may not profit in either the short or long run, despite their having done so. Similarly, our theories offer no insights on how private organizations should invest to remediate climate impact. Now, to be sure, these problems also stare organizations in the eye in the sense that they have become so pervasive that they are reduced to practice across economic and organizational life. Many scholars have deployed our bodies of theory to study specific accomplishments, within the boundaries of the theory, of innovation in climate products, immigration entrepreneurship, and dissemination of healthcare across international borders, for example. But we have yet to find a way to contribute insights for policy at a scale that is commensurate with the phenomenon and that is also supported by our theory. (The urgency of the pandemic and of climate change, for example, has led some scholars to write books on these topics that are not bound conceptually by the demands of our bodies of theory.) The absence of a unifying architecture to support the integration of our bodies of theory is getting in the way of our addressing issues the most important strategy problems in the world around us. One way forward would be to develop a rich intellectual history of the purpose of the firm that integrates insights and influences from law, public policy, administrative studies, economics, political science, and even risk-based analysis on the evolving purpose of the corporation in society. Such a history could serve as a basis for identifying theoretical developments that have become taken-for-granted without full interrogation. By reflecting comprehensively on their implications, we would be in a position to refine our core constructs for a more coherent, humanistic, and flexible theory of the firm.

Finally, we are beginning to see studies by prominent scholars pointing to the opportunities for developing a more robust architecture of constructs and relationships that can unify the bodies of theory that have developed in the field of strategic management. Scholars of corporate social responsibility have argued that the legitimacy of the corporation is at stake as corporations fail to fulfill their social mandates. Gerry George's many editorials in the *Academy of Management Journal* call for this effort. In some instances, the call is for conceptual clarity; in others, it is for greater precision.

What is clear is that there are many obvious inconsistencies that permeate our field: As Barney (2018) points out, shareholder value optimization is not consistent with the resource-based view. The foundation of the resource-based view is as the dual to the product-market optimization that we inherit as a field from our cognate discipline of Industrial Organization economics (Wernerfelt 1984). Barney's (2018) claim thus calls into question the robustness of the entirety of the theoretical architecture that pervaded the field in the 1970s and 1980s. Others note that markets usually do not function in the ways stipulated by industrial economics. Indeed, in many situations, markets reflect the social systems in which they are embedded. Capital does not move openly across international boundaries. Rational choice by enlightened, self-interested actors has given way in various domains to irrationality (not to mention that both enlightenment, self-interest, and action have not been fully constructed).

In sum, we need a unifying framework of constructs, core relationships, and understanding of organizational purpose. Specifically, we need a shared understanding of where the risks come from that justify the limitations on liability that the firm's investors enjoy. We need a better theoretical connection between micro-organizational understandings of individual contributions to corporate achievement, and of the firm's obligations that arise from those contributions. We need a less austere and more humanistic set of constructs regarding shared purpose and collectively valuable corporate achievements. And we need the whole lot to be coherent and integrated. Strategy scholars are in a unique position to accomplish this development of theory because, in contrast to economists, we tend to agree that we need a unified theory of the firm. In other words, this weakness opens the door for progress.

The need for this work is also reflected in the way that many of us have taught in our MBA classrooms, i.e., using the case method. Cases are in and of themselves valuable vehicles for conveying insights, but which insights? What is the theory of the case? Here, we tend to draw most on the administrative traditions that gave rise to our field more than 50 years ago. Of course, in many MBA classrooms, heroic teachers strive to use cases in ways that are nuanced and aligned with validated approaches to learning, but that is not always the case, and it certainly was not the prevailing case when our field was in our infancy. I draw on my own experience first as an MBA student back in the early years of the field's development, and then as a young professor learning to teach by the case method: Often what we do is use cases to lure our students into taking positions that are doomed; we then reveal what occurred, thus humiliating our students into the recognition that their instincts were wrong; and then we promote an idea or analytical tool that can support them in avoiding the trap in the future. But what are we really teaching? I think that, in too many cases, we have taught our students how to be instrumental absolutists, and how to manipulate those around them into adopting an idea or analysis. It's time to change this too.

Urgency of the strategically important phenomenological problems around us

I would like to speak plainly as a scholar, educator, and citizen about what I see as the strategically important phenomenological problems around us. In so doing, I am unsettled because my scholarly expertise does not extend to all the issues that I raise. At the same time, it is this lack of expertise that I would ask us all to address.

There is unconscionable inequality of opportunity in the world, due at least in part to the systems to which business schools have contributed (Amis et al, 2018). The job insecurity that has given rise to populism is a facet of this inequality. Yet another is the privatization of prisons, for example. We need to rethink how our economic systems are functioning to create value. In short, prevailing institutions must be evaluated for their alignment with the purpose and principles that provide legitimacy to the system in which these institutions are embedded.

The emergence of nationalistic, authoritarian governments in this decade of the 21st century raises basic questions about the framework of international institutions. The social psychology that has given rise to authoritarianism in many of our institutions must be understood fully. What are the consequences of restrictions on international trade not only for economic exchange but also for freedom of immigration and for global security? What are the relationships between economic and non-economic exchange? Immigration and refugee crises create profits for large numbers of economic actors (McGahan, 2019). What are the obligations of institutions that acquired resources and achieved scale during the post-World War II era of globalization? Scholars of strategy are uniquely qualified to address these questions.

The pandemic has exposed both the tremendous capacity of private corporations to respond to international emergencies with innovative new products such as vaccines. At the same time, it has demonstrated how access to these life-saving medicines is shaped by organizational supports and barriers at the level of distribution, supply chains, governments, and in broad social systems. The wealthy are first in line for vaccines and for treatment and have greater capacities to social distance and work from home. Essential workers run core systems at the risk of exposure and often without job security. The poor, the elderly, and the vulnerable suffer more frequently and more deeply. Fundamental structural inequity is built into institutions but is not addressed by our theory. That must change.

The pandemic is not the only urgent existential threat that our theory does not address adequately. The climate crisis is upon us with no successful strategy deployed at scale. The accomplishments of the Paris Accord are eroding. Organizations, including corporations, governments, and other actors, are responding to the climate crisis, but projections of increases in ambient temperatures demonstrate that progress is not occurring quickly enough to avert widespread humanitarian and economic devastation.

The simple math of sustainable consumption will require that humanity does more with less. For many years, we have known that human consumption and the accompanying environmental degradation are not tenable indefinitely. Without effective intervention, the reductions in consumption that will be imposed upon us by the physical limitations of what the planet can support will affect everyone, intensifying inequality. Effective strategies for redirecting resources must be developed.

The promise of artificial intelligence, machine learning, 5G, quantum computing, and advanced analytics is extraordinary, but there are significant risks. These include (but are not limited to) loss of privacy, increased inequality, abusive of power, centralization of wealth, amplification of biases, and promotion of the systems that gave rise to the problems listed earlier. Understanding both the risks and opportunities in new technologies requires our focused attention as scholars of strategic management.

The most important strategic problems of our time are supra- and inter-organizational, but the solutions require coordinated organizational responses. The pandemic, climate change, inequality, authoritarianism, persistent poverty, and unsustainable consumption (to name only a short list of these challenges) demand our attention immediately. We don't have much time. We urgently need to address the foundational problems in our theory so that we can respond at scale.

Promising directions

Several important potential directions for building new foundations immediately come to mind as important opportunities. The first reflects the historical origins of the corporation based on common-law traditions and informal arrangements. By examining these origins and the subsequent development of critical legal arrangements such as the juristic person rule and the limitation on investor liability, scholars can develop a nuanced and richer understanding of how firms were intended to function – at least in many Western countries – as ameliorating of risk. These specific arrangements were designed both to enable risk-taking through the pooling of financial capital and the conveyance of responsibility for failure away from investors, managers, and employees. Those steps and their consequences must be reassessed and interrogated fully as an important part of the project of rebuilding theory.

Second, scholars of strategic management face an opportunity to build new constructs that take seriously the idea that people are variously irrational, emotional, inspired, interconnected, and otherwise deeply interdependent. Our theories tend to privilege the idea that organizations can be designed to generate specific outcomes on the assumption that people are rational. But the essence of the organization is to enable complementarities. We need to understand those better. In particular, we need more fine-grained conceptualizations of what constitutes complementarities, and of where such complementarities come from given that the people behind them are not always – or even primarily – rational.

Third, we must address the ways in which firms interact with other institutions in a broader range of contexts than markets, and on terms that cannot be fully captured as competition or cooperation. Some progress has been made on this front, but much more is required to develop insights that link an understanding of macro-social institutional dynamics with the nature, responsibilities, and performance of organizations that are embedded in the dynamics.

Fourth, the theory in our field must deal in nuanced ways with variation in the ways in which societies authorize stakeholder claims on organizations, and on how those claims evolve over time. Our understanding of stakeholders as unitary actors must evolve. Our conceptualization of value, and of how it is measured and assessed, must evolve.

Fifth, theory in the field of Strategic Management must evolve to consider a wide range of ways in which organizations are performative. The standing emphasis on returns on invested capital as the primary dependent variable in our studies has legitimized the idea that the primary effect of organizational action is in generating these returns. What is becoming abundantly clear, however, is that the effects of organizational action on social outcomes are much more varied and nuanced, and include everything from the generation of priceless life-saving vaccines and devastating environmental impact. A new framework is needed to capture the range of ways that organizations deliver effects on society.

Finally, the field would benefit from a shift in the orientation of our theory to reconsider the purpose of the organization in society, and the purpose of the practice of organizational strategy. We have, in our early foundations, a conceptualization of strategy as advancing societal prosperity beyond what can be accomplished by individuals working independently of one another. The purpose of the limited-liability corporation was to enable risk-taking by individuals working together to accomplish something valuable for the communities in which that organization takes place that could not be otherwise accomplished. Much more work is needed to assess the implications of this conceptualization of strategy as advancing the accomplishments of organizations – and especially corporations – in promoting the public interest.

A strategy for the field of strategy

We must build from our successes while, simultaneously, acknowledging that our field suffers from the absence of a strong, secure, architecturally robust foundation of constructs and core relationships. We face opportunities to engage with our students and colleagues in new and innovative ways to build new theory, new empirical insights, practical guidance for managers, and innovative public policies. It is time to think more clearly about inter-organizational action to address the most important problems around us. This will require thinking more deeply about the reasons why organizations exist along with their rights and obligations. Relationships between governments and corporations, and the nuanced ways in which inter-organizational collaboration between them occurs outside markets, are central topics that have not been sufficiently explored. We need more research on the purpose of the organization in society, and of the implications of that purpose for public policy and managerial action. Core constructs, such as the emergence of organizational identity as distinct from that of founders, must be integrated with our understanding of the bases of economic value creation. Our theoretical foundations must explain how people work together strategically to accomplish collective action in and across organizations to create value in the face of our most important problems. It is our responsibility as educators to drawn on our disciplinary insights, methodological skills, pragmatism, and phenomenological insights to lay the groundwork for potential solutions to grand challenges.

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