

Prioritizing Research in Strategic Management: Insights from Practitioners and Academics

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Abstract

This essay presents results from a survey that assesses perceptions of academics and practitioners regarding the importance of, and their satisfaction with, research on contemporary topics in strategic management. The objective of the essay is to provide guidance regarding important areas where respondents believe further research would be fruitful. The responses to survey questions regarding 55 prominent research topics indicate the extent to which existing research programs have under- or over-served the demands of the academic and consultant-practitioner markets. A comparison of responses provided by academics and consultant-practitioners suggest opportunities for current academics to learn from practice. Overall, this essay provides a means to better understand the distinctive contribution of strategic management research and suggests several ways in which future research might focus its effort on questions of interest to practice.

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Introduction

A growing chorus of scholars expresses concern that the strategic management research agenda is drifting from its historical domain and risking its distinctive contribution to business and management science. In recent years, scholars have lamented the growing fragmentation in strategic management research (e.g., Durand, Grant, and Madsen, 2017), have expressed concerns that research questions are increasingly driven by data availability or methodological considerations (e.g., Bettis and Blettner, 2020), and have stated unease with the expanding gap between research and practice (e.g., Bresser and Balkin, forthcoming; Drnevich, Mahoney, and Schendel, 2020). These claims are accompanied by informal observations regarding the loss of standing of strategic management programs within business schools, within management departments, and especially among the world of practice.

Implicit in concerns regarding the direction of the field is the belief that a better or more informed approach exists. Prior work suggests many ways to advance the strategic management research program. Rumelt, Schendel, and Teece (1994) suggest focusing the field on a set of distinctive research questions. Oxley, Rivkin, and Ryall (2016) aim to direct research effort by influencing beliefs and preferences regarding research norms. Durand, Grant, and Madsen (2017) suggest promoting theoretical reviews that build precision, reconcile empirical results, and / or subsume individual theories. Van den Steen (2018) argues that it is important for strategy scholars to explore how certain choices commit, force, or guide other choices. Leiblein, Reuer, and Zenger (2018) suggest highlighting the elemental characteristics that discriminate between strategic and non-strategic decisions.

Several essays in the inaugural issue of the *Strategic Management Review* build on this work and suggest additional ways to highlight current and future contributions of the strategic management

field. For example, Leiblein and Reuer (2020) highlight the importance of encouraging pre-paradigmatic research on problems informed by practice. Drnevich, Mahoney, and Schendel (2020) focus on the importance of engaged scholarship. Teece (2020) underscores the importance of revisiting the fundamental questions in light of changes brought on by the rise of China. Feldman (2020) outlines pathways to reinvigorate corporate strategy research. In sum, this work suggests the need for a “course correction” in our collective research and proposes various opportunities for future strategic management research.

While existing essays envision several different directions the field may take in its quest to develop a rigorous understanding of important and relevant strategic management problems, we are unaware of any data that is available to guide or inform these opinions. As an initial step toward directing this research agenda, we sponsored an informal survey of prominent academics and consultant-practitioners to assess perceptions of contemporary strategic management research. The survey aims to develop insights regarding perceptions of the importance and satisfaction with topical areas that are prevalent in contemporary strategic management research. We purposefully cast a wide net in identifying these research topics by reviewing the domain statements from leading strategic management associations and, when necessary, inviting experts from within specialty interest domains to refine our understanding of these issues. In addition, the survey aims to compare the perceptions held by academics and practitioners in order to develop indications of areas of convergence and divergence along these topical issues. Finally, the survey aims to identify preliminary statements from our survey respondents regarding emerging problems that might help identify future strategic management research opportunities. Given the importance of management practice in directing and informing strategic management field’s research agenda over the past few decades, insights into these themes can serve as not only a reality check on current work but also

can provide initial input as to how the field is both serving practitioners as an important stakeholder group and learning from those actually engaged in strategy work.

We present two sets of primary findings. Our first and primary set of findings compares perceptions regarding the importance of and satisfaction with existing areas of strategic management research across academics and practitioners. While we highlight several areas of agreement and disagreement across these constituencies, two areas of divergence are notable. One, practitioners appear to perceive research in several topical areas such as behavioral strategy, competitive strategy, and entrepreneurial strategy as being of greater importance than academics. Two, practitioners appear to be less satisfied with the extant research in several areas such as corporate scope and the governance of cooperative strategy than academics. Together, these importance and satisfaction findings suggest a way to map existing research topics in the field, identify the extent to which existing research topics have under- or over-served the demands of the academic and consultant-practitioner markets, and suggest current topics that might benefit from additional scholarly attention.

Our second set of findings summarizes comments from respondents on emerging areas of research where the strategic management field may make a substantive contribution. Our discussion of comments on these emerging areas of research is purposefully brief given that our survey results are preliminary and may provide a foundation for a more systematic survey of senior managers. Regardless, the informal comments captured in this set of findings suggest opportunities for exploration in some interesting, emerging areas of study and point out new opportunities for current academics to learn from practice.

Our article proceeds as follows. We begin by discussing the design of our survey. We then report the findings outlined above. We conclude by reporting a set of implications and next steps. Here, we summarize some tentative conclusions that may be inferred from our survey results, develop a call to action that suggests a way to think about the contribution of various types of research published in our journals, and discuss how the *Strategic Management Review* may aid scholars interested in learning from practice and joining this agenda of fostering research on the canonical problems of strategic management as defined by management practice.

Measuring the Importance of and Satisfaction with Strategic Management Research

In the summer and fall of 2019, we administered an exploratory survey to a convenience sample of 473 internationally recognized academics and practitioners. Many of the academic faculty targeted are editorial board members of the *Strategic Management Journal*, *Strategic Management Review*, and *Strategy Science*, and as such are in positions to observe the patterns of submission and acceptance of current work. On the side of the practitioners, we approached senior consultants and partners in leading consultancies. We chose this group as they act as a bridge to practice, and are most versed in and hence can most easily relate to the themes, questions, and theories of academic research. While this choice of respondents may have inherent limitations, given that consultants are most likely to be informed, knowledgeable, and sympathetic to strategy research, it also implies that any differences in perception vis-à-vis the academic faculty would be more meaningful. Ultimately, we targeted the survey to those individuals who can provide insights regarding the likely success of a research initiative. We received a response rate of 26%, with 82 complete responses from academics and 38 from the practitioners. This response rate inspired confidence in our ability to use the survey results to surface issues and inform the design of a more systematic survey that could follow.

The survey was designed to generate both simple quantitative insights as well as to capture broader qualitative observations through open-ended comments. To understand whether and to what extent existing research questions were addressing the needs of academics and consultant-practitioners, a set of quantitative questions were constructed around contemporary topics in strategic management research. Specifically, we developed 55 questions in 11 topical areas of study, drawn from the domain statements of groups at the major academic societies related to strategy, with input from officers in each group to select the five most salient ones. Each question contained a response on a five-point Likert scale, on two outcome dimensions: perceived importance as well as satisfaction with current findings. Table 1 lists all the quantitative questions included in the survey, along with details of the Likert scales utilized. Our subsequent analysis focused on discerning areas of agreement and differences in the perceptions of both importance of and satisfaction with research in the respective topic of inquiry, using both two-tailed t-tests and graphical visualization of the data.

INSERT TABLE 1 AROUND HERE

While the quantitative questions were by their nature reflective of historical and current areas of inquiry, the survey also solicited open-ended responses to capture more forward-looking ideas and sentiments, such as those induced by technological changes, the evolving geo-political climate, and emerging forms of organization. To begin the process of identifying emerging research areas, respondents were given the opportunity to suggest up to four new research areas that they deemed appropriate for the field to address. In addition, survey respondents received the option to communicate any other ideas or thoughts in free form. We utilized these responses to identify emerging topics of importance and to develop a sense of the intensity of interest in these emerging

topical areas. A future survey could more formally assess the importance and satisfaction of such nascent work.

Identifying Possibilities for Impactful Strategic Management Research

At a macro-level, our analysis indicates a remarkable level of convergence between the survey responses provided by academics and consultant-practitioners, in terms of both perceived importance as well as the satisfaction with existing research areas. Of the 110 questions across both outcome variables, only 16 displayed statistically significant differences in ratings, based on two-tailed t-tests at the $p < 0.05$ level.

To minimize the risk of over-interpretation of our results, we report responses by academics and practitioners to our survey via response histograms and plots. Figures 1 and 2 display histograms of the survey responses to the subset of the 110 questions in our survey for which there was the highest convergence between academics and practitioners regarding the perceived importance and perceived satisfaction with research in a given topical area, respectively. In both figures, the “topic area” rows indicate the broad area from which a question was assigned. The “question theme” rows indicate the corresponding survey questions. The distribution of responses to a given question is reported separately for academics and for practitioners, but in pairs as labeled next to the bars. Distributions are provided in terms of percentage of respondents selecting each of the five possible Likert responses (e.g., not at all important to extremely important). The arithmetic mean of the response to the question for the academic and practitioner subsets is indicated in the middle of each bar.

Our analysis of the survey responses yielded several thought-provoking results. Figure 1 reports results for the 12 (out of 55) questions where there was a high degree of *agreement* across

academics and practitioners regarding the *importance* of a research question. These 12 questions were identified by a t-test of the difference in means between academic and practitioner responses (cutoff was $p > 0.9$). Figure 1 illustrates there are numerous research questions which are deemed nearly equally important by both scholars and consultants, spanning several topical areas. Many of these topical areas are canonical to the strategic management field, relating to competitive, corporate, and cooperative strategy.

Figure 2 reports results for the 8 (out of 55) questions where there was a high degree of *agreement* across academics and practitioners regarding *satisfaction* with the quality and quantity of existing research. As Figure 2 demonstrates, there are some questions with perfect or near-perfect agreement in terms of the perceived satisfaction by both types of respondents, though these are in different topical areas. At the same time, Figure 2 also indicates that even when academics and practitioners are equally satisfied with existing research in a given area, the absolute level of satisfaction with our current knowledge base is relatively low, remaining below 3 on average. Taken together, these results suggest that the scholarly field can take heart in the relevance of much of its work addressing canonical research questions, but that there is ample scope to expand its efforts to have more impact on practice.

INSERT FIGURES 1 AND 2 ABOUT HERE

An examination of the perceptions of satisfaction with areas of research that *both* academics and practitioners deem *important* provides further direction regarding areas where additional research is likely to be impactful. Figure 3 reports results for research questions that both academics and practitioners indicate are highly important. Fundamentally, a mere six questions are deemed very or extremely important (greater or equal to 4 in the ratings) by both groups. Notably, all six

questions viewed as important by both academics and practitioners fall into the competitive and corporate strategy topical areas. In only two of these six research questions are there significant differences in perception of satisfaction (around vertical integration decisions, as well as corporate scope), but in five of these six questions, scholars appear to be more satisfied than practitioners. Overall, satisfaction with the current insights provided by this work is consistently modest (below a rating of 4). These responses may suggest some complacency amongst academics for research in mature topical areas, even as there is clearly greater potential for impact on managerial practice.

INSERT FIGURE 3 ABOUT HERE

The findings indicate that while there is a strong agreement on many important topics of study, there is also scope to enhance the impact of the field's work. As Figures 4 and 5 illustrate, there are critical and significant differences in perception amongst academics and practitioners in several research questions spanning topical areas. These charts represent the analog of Figures 1 and 2, but focus on those questions which showed the greatest divergence between researchers and consultant-practitioners (as defined by a t-test difference in means of $p < 0.05$). For these questions, what is striking is that practitioners consistently rate both the importance of classic research questions higher (Figure 4), and satisfaction with the knowledge and findings provided by current research lower (Figure 5), than academics, reinforcing the earlier insights. Specifically, there are a number of areas where a gap exists between the perceptions of academics and consultants. These gaps are particularly salient in the areas of competitive, cooperative, and corporate strategy, especially in terms of satisfaction. While these gaps may be a result of research that is "lost in translation", the fact that the gaps are largest in mature areas of research where terms are more standard works against this hypothesis. Moreover, by deliberately surveying consultants, we would

expect that they would be better versed with the terminology, methods, and findings of academic research in their role as a bridge to other corporate executives.

INSERT FIGURES 4 AND 5 ABOUT HERE

In order to more clearly depict target rich areas for impactful strategic management research we plot the perceived importance of and satisfaction with research findings on all the topical areas in our survey. The scatter plots depicted in Figures 6 and 7 graph the importance vs. satisfaction scores for each of the 55 research topics, by respondent type, following a representation similar to that used in the “jobs to be done” innovation literature (e.g., Ulwick, 2018).¹ These figures further separate the landscape of existing research topics into three sections: the portion of the figure left of the solid line indicates instances where respondents reported satisfaction scores that are greater than the corresponding importance scores. The area between the solid and dashed lines indicates where the perceived levels of satisfaction with our current knowledge is moderately below the perceived importance of the topic. The area to the right of the dashed line indicates research questions that our respondents indicate are important but where the existing knowledge is less satisfactory. The intuition is to observe the size of the gaps between importance and satisfaction for each question, with the former being much greater than the latter indicating a topic is underserved, and the opposite being considered overserved. While our data indicates that no topical areas overserved, several fundamental topics are underserved from the perspective of practitioners, even as academics view them as being sufficiently studied. These include questions

¹ We note that the graphs reported in the product innovation literature often utilize a different set of axes (e.g., Ulwick, 2018). Whereas Ulwick calculates relative importance and satisfaction scores for new product development efforts, our figures are based on the raw importance and satisfaction scores from each of the question themes in our survey.

around corporate strategy, the importance of resources, competitive advantage, cooperative strategy formation, and dynamic capabilities.

INSERT FIGURES 6 AND 7 ABOUT HERE

A deeper examination of the divergence in perceptions of the respondents reveals further insights that reinforce the above findings. Figures 8 and 9 depict the questions which respectively have the largest and smallest rating differences between importance and satisfaction, and whether it was academics or practitioners who on average perceived the gap. The variation in this analysis is that, instead of simply comparing the arithmetic means of importance and satisfaction for each question (by respondent type), we compared the percentage of responses to that question which were rated 4 or higher (for each group). This represents the respondents who marked their importance score as either very or extremely important, and those who rated their satisfaction as somewhat or extremely satisfied.

What is noteworthy is that the percentage scores for satisfaction lag those for importance for all the questions. It is the practitioners who generally as a group are the least highly satisfied relative to the high importance they attribute to questions, dominating the list of the largest differences (Figure 8). Notably, this occurs for questions relating to the competitive, cooperative, and corporate strategy areas. Consultants and practitioners are looking for more insight in these important topical areas. On the other end of the spectrum, academics seem to be the most highly satisfied relative to questions of high importance, dominating the list of the smallest differences (Figure 9). Here too, questions related to cooperative and corporate strategy areas are visible, though not as prominently.

Collectively, these visualizations underscore our observations on both the potential of further studying canonical questions for impact on practice, even as academics appear to exhibit more satisfaction with existing research and knowledge in these domains. The findings from our survey, while tentative, suggest that the largest opportunities for strategic management research to impact practice exist when addressing questions related to: (1) the definition and measurement of competitive advantage, (2) associations between investment, resources, and competitive advantage, (3) associations between specific corporate governance modes (e.g., alliances, acquisitions, internal development, divestitures) and performance, (4) the differences in strategy formulation and implementation across new ventures and established firms, and (5) the skills required to perform strategy work as well as how these skills are acquired or developed.

INSERT FIGURES 8 AND 9 ABOUT HERE

Beyond the quantitative data, in evaluating the responses to the open-ended questions, we observed that 54 respondents identified over 200 different topics of interest for future exploration. Table 2 lists a selection of topics that were submitted, which we interpreted as a sample of emerging areas the field might productively explore. We categorized the questions: those directly addressing recent phenomena and contexts, and those related to more canonical topics, even if inspired by emerging phenomena.

We gleaned two insights from this exercise regarding areas that appear to be worthwhile to explore in future research. One set of questions concerns the rising prominence of phenomena such as the influence of China, the implications of digitization and enhanced data access, or the emergence of new organizational forms for strategic management research. The second set is related to our own understanding of academia-practice gaps concerning canonical questions such as the role of

interdependence across multiple decisions, the role of traditional questions regarding corporate strategy, and the importance of the question of competitive advantage.

We argue that for the field to advance and enhance its impact on practice, the new phenomena that were identified need to be examined not merely for their own sake, but to help answer the canonical questions of the field. Building such bridges can help foster cumulative knowledge by tying research to the field's historical core. It can also curb fragmentation and promote distinctive contributions of the strategic management field. Two examples from our survey stood out as suggesting specific ways in which the field might support this kind of work. One scholar observed that "AI, machine learning, blockchain, etc. are emerging as a disruptive set of digital technologies that may have profound implications for the future of work and income inequality. ...what do we have to say as strategy scholars to such big questions?" Another suggested "[r]evisiting the classic strategy question of "how does management matter to organizational outcomes" in the light of technological advances that potentially change the role of manager."

INSERT TABLE 2 ABOUT HERE

To complete our reporting of the findings, it seems worthwhile to highlight some of the comments we received in response to the final open-ended question soliciting remaining ideas or reactions to the state of the field. The received replies relate to the need for, and the challenge of, our purpose here: to bridge the gap between theory and practice, in the pursuit of research that is both rigorous and relevant. In support of our mission, one respondent said "Thank you for initiating and continuing this important work", and another lamented that "there was great work that has gone fallow." In terms of the challenge at hand, one academic noted that "most of the topical areas suffer from poor theory, so that even when there are copious empirical results, you cannot get to the key

questions from the results.” As a potential solution, another faculty member suggested that the field “[c]onsider a different knowledge production process (e.g., review, methods, selection of questions)”. We hope our data reveal at least a tentative path toward identifying areas of impactful strategic management research as well as an ordering of research topics that might be targeted while engaging in relevant, responsible, and impactful strategic management research. We do not intend to derive a comprehensive research agenda from these findings or draw hard and fast conclusions on specific research priorities. We view the survey and our findings as an initial effort that flags areas of research that will benefit practice and serve these stakeholders of the field while bringing the voice of practice into the research process.

Implications and Next Steps

It is frequently observed at conferences, in the press, and in other outlets that there is a gap, and perhaps a growing gap, between management practice and its needs on the one hand versus what is studied and taught at business schools on the other hand. For instance, the organization Responsible Research in Business and Management (RRBM) concludes that business research is failing to live up to its potential in influencing best practices and policies, and this group is calling for large-scale shifts in criteria and incentives to bring about fundamental changes in journals and business schools (see also Drnevich, Mahoney, and Schendel (2000) and Bresser and Balkin (forthcoming)).² Yet we are not aware of evidence specific to the strategic management field regarding any potential differences in perceptions between academics and practitioners regarding the importance of strategic management research or satisfaction levels with the field’s output to date. We therefore sought to carry out an informal assessment to foster conversations on the

² <https://www.rrbm.network/position-paper/>

scholarship-practice nexus and discover ways that the field might better learn from practice and ultimately better serve practice. We hope that this initial effort might prompt additional and fruitful research and initiatives that could inform the field's research agenda and advance the *Strategic Management Review*'s mission.

Several key findings stand out from our informal survey of senior academics and practitioners. To begin with, in view of the large battery of questions we put to our respondents, the degree to which there is convergence is fairly remarkable. Practitioners and academics agree on the degree to which certain topics are important, and their agreement spans a broad swath of questions within the field's interest areas.

At the same time, the often-modest satisfaction scores and existence of several critical differences in perceptions across academics and practitioners also point out opportunities for advances. In particular, there is less satisfaction among practitioners (for the areas judged to be important) for the topics that represent some of the canonical problems of strategic management, including those related to competitive strategy and corporate strategy. This suggests that there is still important research to be done to inform practice in what can be regarded as the historical core of the field. It is also noteworthy that it appears academics are more satisfied with the field's accomplishments in these areas than practitioners. This pattern might reflect some complacency, perceptions of maturity, or prizing the search for novelty and topics more distant from these core questions and topics.

While we do not believe it is appropriate to draw firm conclusions on the sources of such differences across academics and practitioners from this survey, others' observations on the field's development may play a role, and it is also interesting to speculate on contributing factors. To

begin with, scholars have often voiced concerns with growing fragmentation in the field and what this potentially means for the field's future (e.g., Oxley *et al.*, 2010). For instance, Durand, Grant, and Madsen (2017) suggest that fragmentation can be seen in the development of specialized sub-groups in strategy scholarship, in the growth of phenomena and analytical issues comprising the field's research, and in the theoretical perspectives employed. They suggest fostering literature reviews as one means of addressing fragmentation in the field, and the *Strategic Management Review* will publish such reviews that are also forward looking. Consistent with the expansion of, and drift in, the field's domain, Leiblein and Reuer's (2020) topic modeling results highlight entropy and shifts in central topics in strategic management research over thirty-five years. They also report that 65 percent of author-supplied keywords and 41 percent of subject terms in *Strategic Management Journal* appear only once over four decades (1980-2015), which might reflect fragmentation as well as authors' search for differentiation of their articles.

The dynamics between the needs of practice and scholarship can also be seen in the development of complex concepts and terms in strategic management, which makes it difficult to develop and use measures and constructs in a consistent way. Lieberman (2021), for instance, revisits one of the core concepts of strategic management – competitive advantage – and notes that it has been defined and operationalized in a number of different ways. He provides several concrete scenarios in which different hypothetical firms may or may not enjoy a “competitive advantage” based on the definition or measure used, thereby underscoring the importance of different definitions that are often not appreciated. It is also interesting and important that the perhaps most theoretically compelling definition is use today – greater value creation compared to direct rivals (i.e., willingness to pay less cost) – suffers from an inability to be aggregated across products and segments (e.g., Postrel, 2018). The dynamic is that to better inform practice, research now needs

to “catch up” with a term introduced by the field early on to address practical problems concerning the sources of superior relative performance.

As a second example of an essay designed to provide more clarity on key concepts popular in strategy, Collis and Anand (2021) observe that there are different types of “dynamic capabilities” that fall under that label. They distinguish dynamic capabilities that “move the production frontier outwards” and dynamic capabilities that represent a reconfiguration of activities aimed at shifting to “a new strategic position.” They also note some limitations of the dynamic capability concept, including the idea that these capabilities can be substitutable and are themselves strategic choices (see also, Pisano, 2017). Given that many of our survey respondents are senior consultants, it might be that they are more likely to understand and appreciate the nuanced distinctions between such concepts used in strategic management. It would therefore be interesting and valuable to obtain additional information from executives and other managers in different industrial contexts about the importance and value of strategic management research. For example, it would be interesting to see how our findings might change if we surveyed practitioners from mid-tier consultancies or practitioners from middle market companies or those based in emerging economies.

Leiblein and Reuer (2020) demarcate several distinct generations of strategic management scholarship that might have contributed in part to the disconnect between research and practice. Following the birth of the field as we know it in the late 1970s, efforts focused on crystallizing the canonical problems of strategic management, as exemplified by the Pittsburgh conference in 1977 (Schendel and Hofer, 1979) and especially by the identification of the fundamental issues in strategy in the mid-1990s (Rumelt, Schendel, and Teece, 1994). The four questions featured by Rumelt *et al.* – how do firms behave? why are firms different? what is the function of or value

added by the headquarters in a diversified firm? and what determines success or failure in international competition? – catalyzed as well as directed research in the field.

While these fundamental issues also served to underscore and promote the distinctiveness of strategic management research, they were not intended to be static. In fact, one of the book's authors has called for them to be revisited as a matter of theory as well as policy in light of China's rise and policies, which challenge the Washington consensus that is implicit in some strategy research (Teece, 2020). Debate has emerged as to the implications for strategy research and pathways forward for theory development (Buckley, 2021; Teece, 2021). This exchange raises broader questions regarding the boundaries of the field and value of the fundamental issues in guiding future research: Do they need to be reinforced today? Is this even possible or desirable given changes in practice and strategy scholarship? What fundamental issues need adding, deleting, or modifying? How can new scholars better link into the field's historical core?

Leiblein and Reuer (2020) note that more recent generations of strategy scholarship have emphasized theoretical developments, disciplinary precision, and empirical advancements (the so-called identification revolution) over topics and management practice *per se*. Drnevich, Mahoney, and Schendel (2020) argue that ground-breaking insights in strategic management emanated from a pragmatic, engaged, and problem-focused process that is not emphasized in current doctoral education or current research. Thus, an open question is whether the increasing focus on disciplinary theory and/or empirical tools have crowded out or impeded learning from practice as an unintended consequence.

As we consider potential contributions to knowledge in the future, it might be useful to consider a typology that asks three questions: (1) Is this good strategy research? (2) Is this good social

science? (3) Is there good identification? Figure 10 summarizes a depiction of the research landscape that allows us to ask a set of questions about individual studies and current scholarship being carried out in the field. Without a doubt, region 1 represents valuable research as it is theoretically-grounded work that draws upon economics, sociology, etc. and seeks to identify causal relationships. But how about region 2 or region 3? Would some scholars new to the field argue that one or both of these regions are null sets today? In fact, these regions represent some of the earliest and most influential work that propelled the field forward, and this work was often non-empirical or based on a limited number of illustrations or the work experiences of authors (e.g., Penrose, 1959; Chandler, 1962; Cyert and March, 1963; Ansoff, 1965; Williamson, 1975). Is it possible to do “good strategy research” today without a strong pre-existing theoretical foundation? Without an identification strategy? We raise these questions for reflection. We also appreciate that the taste preferences, sensibilities, and research styles of individual scholars and editors will differ, and we believe this intellectual diversity is a strength of the field. Our hope is also that research in regions 2 and 3 continues to be valued, and indeed this is an important reason why this journal exists.

INSERT FIGURE 10 ABOUT HERE

We also need to ask the opposite question, one posed by Durand, Grant, and Madsen (2017: 4): is *everything* “strategy”? That is, research falling into regions 5, 6, or 7 can certainly contribute to knowledge, but are we actually clear about whether this work is *not* “good strategy”? As an alternative to the fundamental issues as an organizing device for the field to answer this question, Leiblein, Reuer, and Zenger (2018) identify unique characteristics of strategic decisions – interdependence of contemporaneous decisions, inter-actor interdependence (e.g., competitors), and inter-temporal interdependence – to distinguish strategic management research from

scholarship in finance, organizational behavior, and other functional areas in the business school. We encourage authors and editors to consider how individual research studies and the evolving strategy literature might better leverage and enhance the distinctiveness of strategic management. This too is an important reason why this journal exists. As scholars take on novel topics in novel domains, we see opportunities to tie back to the field's historical core and the canonical problems of strategic management, a theme brought home by our survey results, particularly from practitioners. This can help in building cumulative as well as distinctive strategic management knowledge as informed by management practice.

So how might the *SMR* foster learning from practice? We have instituted a number of distinctive features into our organization and processes to begin to do so. At our conferences, we foster pre-paradigmatic work that facilitates dialogue and debate as well as relies on input from practice. For instance, at one of our launch conferences, convened at UC Berkeley, David Teece provided a keynote speech that discussed how strategic management can and should provide input to public policy debates on competition policy, given the systemic perspective of strategy research that can approach changes in technology and geopolitics. At a launch conference at Columbia Business School on corporate renewal, James Gorman, the CEO of Morgan Stanley, provided his ideas on strategic management and organizational renewal, followed by a panel of private equity consultants. A conference on cooperation in Palermo, Sicily involved practitioners that included representatives of regulatory authorities in Europe. We welcome commentaries from practitioners who can provide perspectives from practice that offer interesting inputs to the strategic management research enterprise, and the *SMR* has also put in place a Business Practice Advisory Board for this purpose. While the *SMR* publishes essays rather than theory-testing empirical studies, both by design as well as to complement existing strategy outlets, we also welcome

examples, illustrations, and data presentations that would foster the development of pragmatic, problem-focused research.

Our initial, informal assessment of academics' and practitioners' perceptions of strategic management relied on the field's interest areas to generate critiques as well as input for scholarship. This foundation for our survey therefore naturally reflects the current literature and organization of research, and is therefore backward looking to some extent. In view of this limitation, we see valuable opportunities to learn from practice using more open-ended research approaches and techniques to identify cutting-edge areas where scholarship is needed to serve practice. This might help prioritize certain specific topics related to shifts in technology and strategy (e.g., AI, gig economy, acquirhiring, among many others), could help in understanding the roles of strategy research for the challenges facing society (e.g., public policy, future of capitalism, etc.), and could also identify implications for strategy careers and roles (e.g., consulting, chief strategy officers, CFOs engaged in strategy work, etc.). Our initial survey helps to take stock of the field and gauge perceptions of academics and practitioners, and we intend to promote more systematic survey work and other efforts to foster learning from practice. In these initiatives and in the essays curated by the *SMR*, we intend to promote integration of strategic management research and to encourage research closely connected with the field's canonical problems as defined by management practice.

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Table 1

Quantitative Survey Questions and Likert Scales

TOPIC AREA	SURVEY QUESTIONS <i>How important is it that you are able to...</i> <i>How satisfied are you with the quantity and quality of research that helps you to...</i>
Competitive Strategy	Identify performance metrics, tools and processes to define and analyze possible sources of competitive advantage?
	Identify the role that firm resources and capabilities play in creating and sustaining competitive advantage?
	Identify the role that firm market positions play in creating and sustaining competitive advantage?
	Identify the role that dynamic capabilities - the firm's ability to adapt its resources and capabilities - play in creating and sustaining competitive advantage?
	Identify how groups and clusters of firms emerge and affect industry structure and the conduct and performance of firms?
Corporate Strategy	Identify how changes in corporate scope (e.g., additional product and service lines) affect firm performance?
	Identify how different corporate investment decisions - such as acquisitions, alliances, divestitures, and internal development - affect firm performance?
	Identify how vertical integration decisions - such as choices to make, buy or borrow key elements of the firm's value chain - affect firm performance?
	Identify how corporate resources - such as a diverse management team or proprietary process that might be used in multiple product categories - influence the effect that corporate scope has on firm performance?
	Identify how corporate organizational structures - such as those organized by product lines, geographies, or customer segments - affect corporate strategy decisions and firm performance?
Global Strategy	Identify what roles cultural, administrative, geographic and economic factors play when firms decide to enter or exit markets in different countries and regions?
	Identify how firms manage the development and implementation of strategies that are tailored to different countries and regions?
	Identify how firms coordinate and reconfigure their multinational activities in response to changing global market conditions?
	Identify the roles global political leadership, policies and regulation play in defining feasible global strategy development and implementation opportunities?
	Identify how firms pick and prioritize global markets based on their objectives (e.g., those with high-profit and growth opportunities; those that provide access to valuable knowledge or other resources; those that are less likely to generate aggressive competitor responses)?

Cooperative Strategy	Identify why and when firms choose cooperative strategies, implemented through alliances and joint ventures, rather than competitive strategies implemented through organic growth, mergers and acquisitions?
	Identify how firm resources and capabilities influence decisions to adopt cooperative strategies and the impact such strategies have on firm performance?
	Identify how the industry and competitor environment influences decisions to adopt cooperative strategies and the impact such strategies have on firm performance?
	Identify how firms govern cooperative strategies and how these governance practices affect the firm's and their partners' performance?
	Identify how firms successfully develop and implement multiparty, rather than single-partner, cooperative strategies?
Stakeholder Strategy	Identify the most effective approaches to stakeholder management and how firm- and stakeholder-specific attributes affect these approaches?
	Identify how stakeholder engagement impacts the effectiveness of competitive, corporate, growth, and innovation strategies?
	Identify the circumstances under which a stakeholder-oriented firm might outperform shareholder-oriented firms on economic value creation and financial performance metrics?
	Identify when and how management teams successfully engage firm stakeholders when changing their strategies, operations and culture in order to pursue economic, social and environmental sustainability goals?
	Identify how individuals and interest groups influence firms' stakeholder orientation and engagement strategies, and thus influence the firm's economic, social and environmental performance?
Leadership and Governance	Identify the relation between senior leadership styles and approaches (e.g., authoritarian, collaborative, innovative, visionary) and a firm's strategy choices and performance?
	Identify how different governance mechanisms (e.g., ownership structure, debt structure, board composition and leadership, incentive pay) affect firm strategy choices and their performance in the market?
	Identify how firm-leader values and objectives impact the ethical and socially-responsible actions that firms take and do not take?
	Identify how global firms align their governance processes with the multitude of specific governance standards and expectations across the globe?
	Identify how the senior management team's personal and professional relationships influence the firm's strategy choices, organizational structures, and other crucial decisions they make?
Knowledge and Innovation Strategy	Identify what role proprietary knowledge (and access to proprietary knowledge) plays in driving innovation and potential competitive advantage?
	Identify how the firm's organizational learning process is developed and managed, and how the information generated from this process is incorporated into the firm's knowledge base?
	Identify how the firm uses individual and group learning to drive innovation?
	Identify what processes the firm uses to transmit and share knowledge within and among organizations?
	Identify what are the primary issues preventing organizations from more effectively managing firm knowledge, learning and innovation, and what actions firms can take to address these issues?

Behavioral Strategy	Identify how bounded rationality (e.g., data and data processing limitations) influences strategy formulation, implementation and firm performance?
	Identify how individual and team-based cognitive biases affect strategic decision making and what actions firms can take to manage these biases?
	Identify how environmental and firm-specific risks and uncertainties affect individual and team-based cognitive biases? What specific actions can firms take to manage these uncertainties and biases?
	Identify how human emotions, personal motivations, and pathologies influence strategic leaders and their decision-making processes?
	Identify how the social interactions and social psychology of different groups influence their decision-making processes, biases and outcomes?
Entrepreneurial Strategy	Identify what role strategy and strategy formulation play in a new venture firm relative to an established firm? Understand what might explain any differences between these two contexts?
	Identify how entrepreneurial ventures develop new ecosystems and/or leverage existing ecosystems (e.g., suppliers, customers, regulators, competitors, investors) to create and capture new value?
	Identify how value-creating innovation processes differ across entrepreneurial and established firms?
	Identify how firm risk- and uncertainty-management processes differ across entrepreneurial and established firms?
	Identify how stakeholder roles (e.g., investors, launch customers) differ across entrepreneurial and established firms?
Human Capital Strategy	Identify the mechanisms by which human capital may allow a firm to generate competitive advantage?
	Identify how firms manage the "value appropriation" challenges presented by "star" employees with high bargaining power?
	Identify how competitive interactions within labor markets (e.g., talent poaching) differ from competitive interactions in other strategic factor markets (e.g., financial capital, technology markets)?
	Identify which factors best explain differences in human capital productivity both within and between industries?
	Identify the mechanisms by which human capital interacts with other forms of intellectual capital (e.g., social capital, organizational capital) to better choose and pursue new value-creation opportunities?
Practice and Process of Strategic Management	Identify the common elements of "prototypical" strategy development and implementation processes? For instance, who is involved and what do they do?
	Identify the impact strategizing activities (e.g., planning processes, research, deep conversations) have on firm performance in the near- and long-term?
	Identify the skills which are required to perform insightful strategy development and implementation work and how these skills are developed?
	Identify the strategy tools and frameworks practitioners use most often, and how helpful they are in strategy formulation and implementation processes?
	Identify how research may inform managerial practice, and how insights from practitioners may help scholars develop more relevant theories?

RESPONSE LIKERT SCALES

QUESTIONS RELATED TO IMPORTANCE

5=Extremely important, 4=very important, 3=moderately important, 2=slightly important, 1=not important at all

QUESTIONS RELATED TO SATISFACTION

5=Extremely satisfied, 4=somewhat satisfied, 3=neither satisfied nor dissatisfied, 2=somewhat dissatisfied, 1=extremely dissatisfied

Table 2

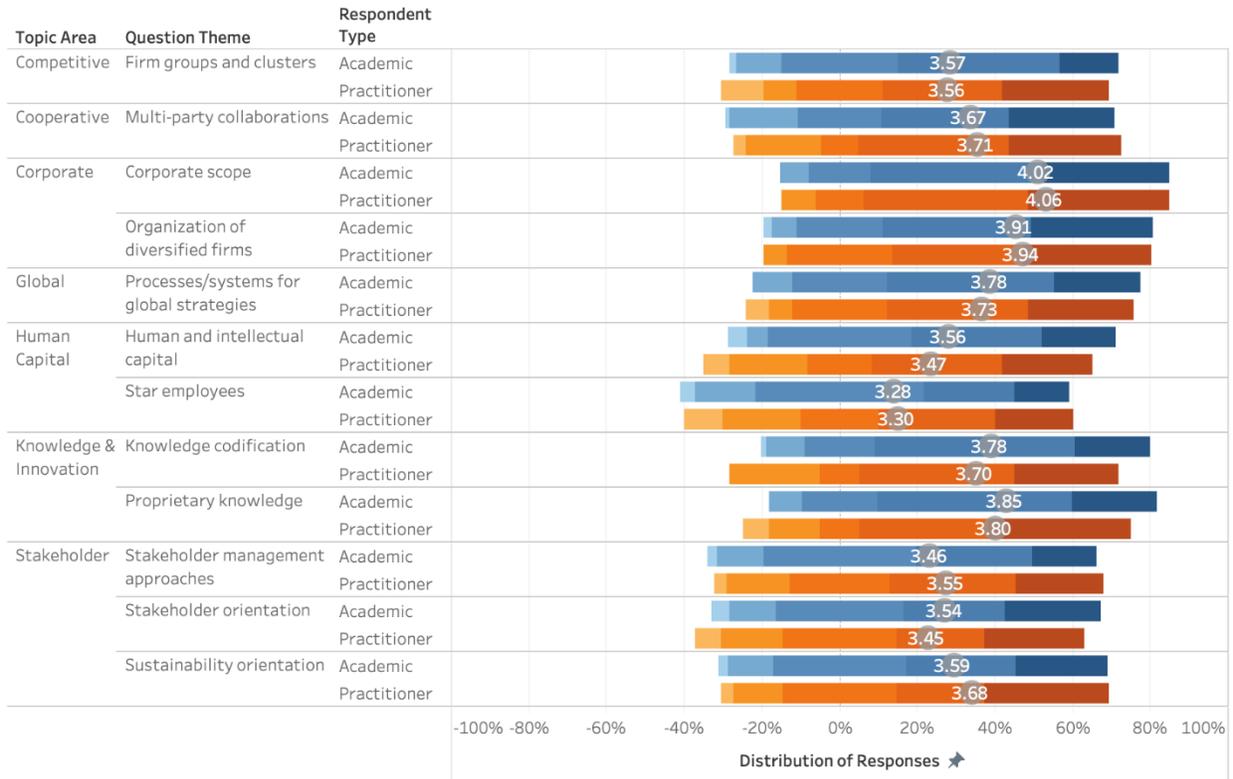
Open-Ended Survey Comments: Forward-looking Questions

What additional questions do you deem appropriate for the field to address?

QUESTION/ TOPIC FOCUS	QUESTION/TOPIC
Emerging Phenomena	Are firms that were owned by private equity firms “better off” than firms that were never acquired by PE firms?
	What will be the Chinese influence on financial markets?
	In the increasingly digital world, platforms are increasingly important. How should (or how do) platforms emerge for value creation? How does a firm capture value in light of formation considerations?
	What is the role of data in strategy? What part of the data value chain is most important to own?
	How should firms deal with data gathered on individual customers and privacy; should there be more attention to ethics in data gathering and use?
	How does the "gig economy" change industry competitive dynamics?
	How can firms be more inclusive? Why would they want to be?
	What are new metrics, strategy frameworks, and management practices that help companies engage in social transformation during the climate crisis?
	How about newer research topics such as the environment, or newer forms of organizing such as crowdsourcing?
	What alternative modes of organizing can solve grand challenges of today's society?
Canonical Questions	Should profitability and sustained competitive advantage remain at the core of our field? Is it sufficient?
	What is the role of interdependence of multiple tactical decisions in strategizing?
	How do (or should) multi-business firms deploy their resources so that they perform better than a collection of comparable single-business firms?
	How would an unrelated diversification strategy create value within post-industrial economies (or are value-creating conglomerates an artifact primarily of newly-industrializing economies)?
	How to link corporate development and results so we avoid four more decades of pointless merger integration research? Seriously - no accumulated results to date nor any real theory.
	How do firm respond to political or institutional disruption?
	How are markets created?
	How do executives critique their mental models (and their effect on strategic decisions)?
	What is the role of the chief strategy officer?
	How can we make our theories and frameworks more predictive (rather than reactive)?

Figure 1

**Histogram of Responses:
Agreement in Research Topic Importance Between Academics and Practitioners**



LEGEND

Shades represent distribution of responses, in terms of percentage of respondents selecting 1 to 5 in each of the questions

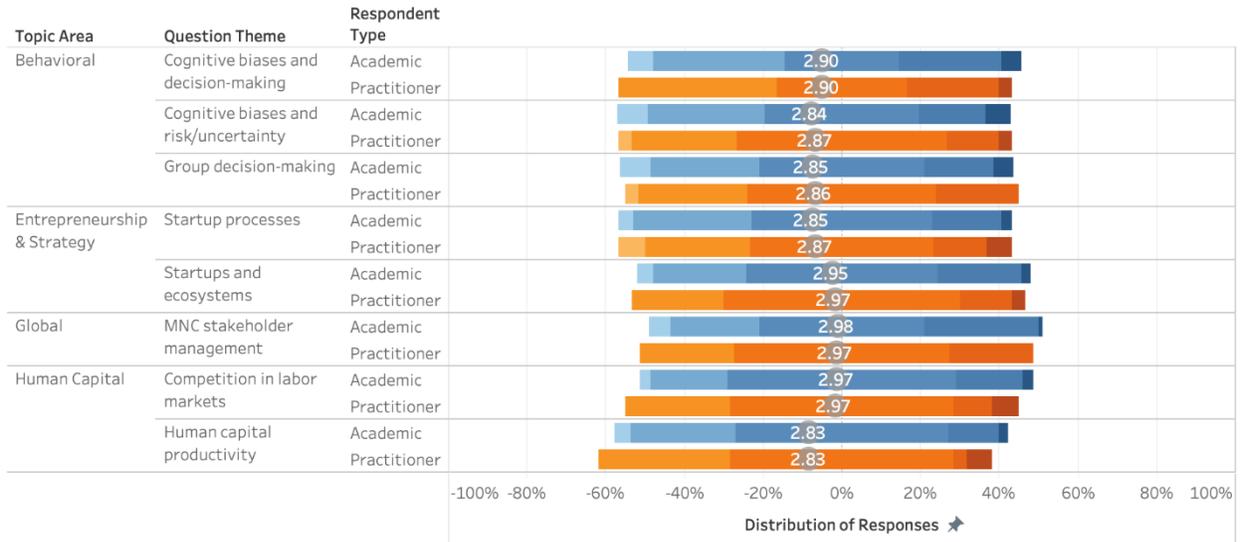
Mean is indicated for each question

Each bar represents 100% of responses, but is visually displaced based on the mean

Results shown for questions where t-test difference between academics' and practitioners' ratings was $p > 0.7$

Figure 2

**Histogram of Responses:
Agreement in Research Satisfaction by Topics Between Academics and Practitioners**



LEGEND

Shades represent distribution of responses, in terms of percentage of respondents selecting 1 to 5 in each of the questions

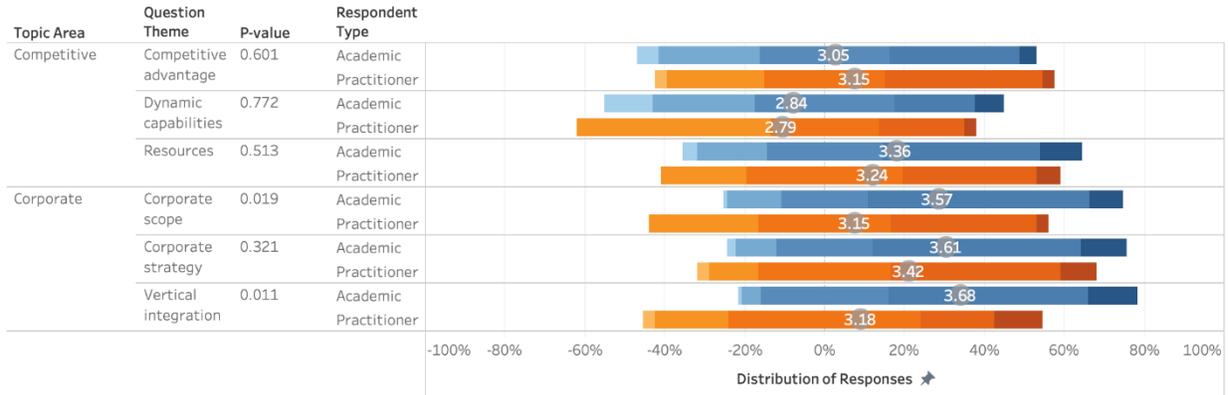
Mean is indicated for each question

Each bar represents 100% of responses, but is visually displaced based on the mean

Results shown for questions where t-test difference between academics' and practitioners' ratings was $p > 0.9$

Figure 3

**Histogram of Responses:
Research Satisfaction in Topics Deemed Very/Extremely Important by Both Academics
and Practitioners**



LEGEND

Shades represent distribution of responses, in terms of percentage of respondents selecting 1 to 5 in each of the questions

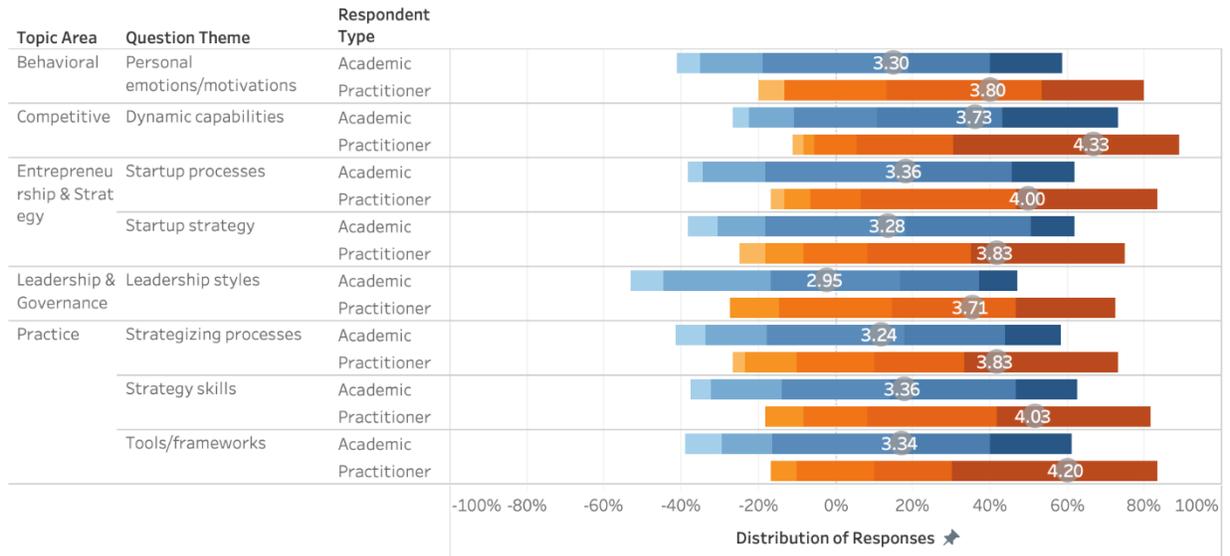
Mean is indicated for each question

Each bar represents 100% of responses, but is visually displaced based on the mean

Results shown for questions where both academics' and practitioners' ratings were ≥ 4

Figure 4

**Histogram of Responses:
Disagreement in Research Topic Importance Between Academics and Practitioners**



LEGEND

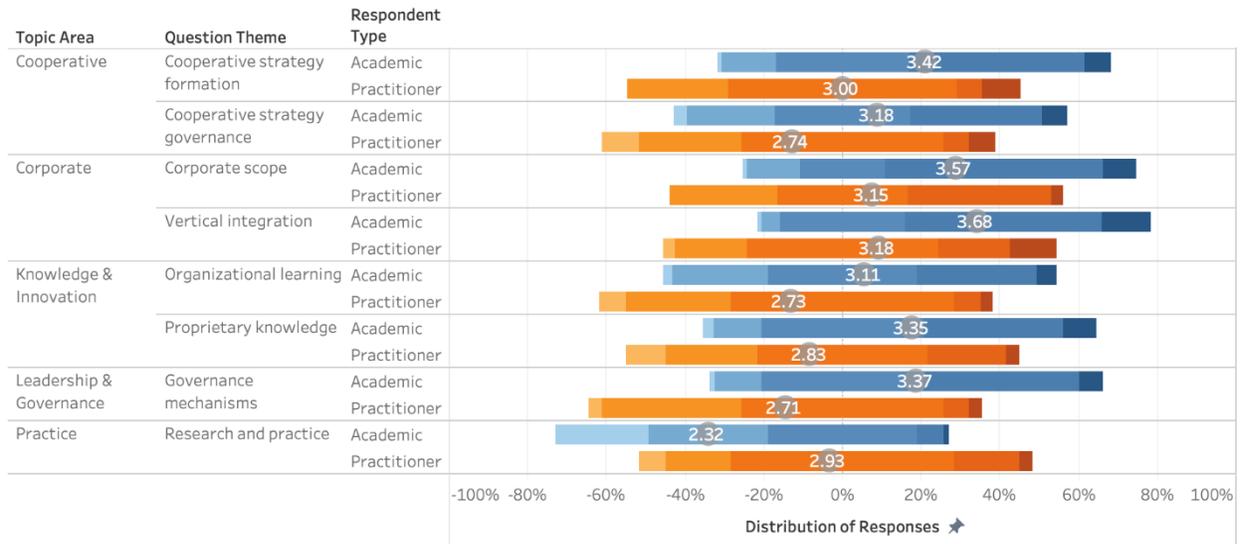
Shades represent distribution of responses, in terms of percentage of respondents selecting 1 to 5 in each of the questions

Mean is indicated for each question

Each bar represents 100% of responses, but is visually displaced based on the mean

Results shown for questions where t-test difference between academics' and practitioners' ratings was $p < 0.05$

Figure 5
Histogram of Responses:
Disagreement in Research Satisfaction by Topics Between Academics and Practitioners



LEGEND

Shades represent distribution of responses, in terms of percentage of respondents selecting 1 to 5 in each of the questions

Mean is indicated for each question

Each bar represents 100% of responses, but is visually displaced based on the mean

Results shown for questions where t-test difference between academics' and practitioners' ratings was $p < 0.05$

Figure 6

Appropriate Servicing: Satisfaction vs. Importance of Research Topics for Academics

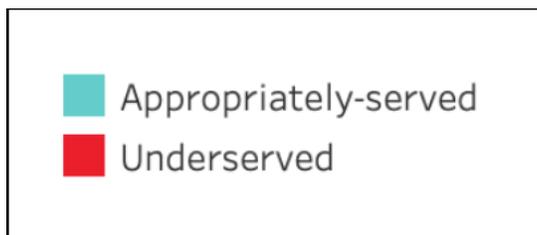
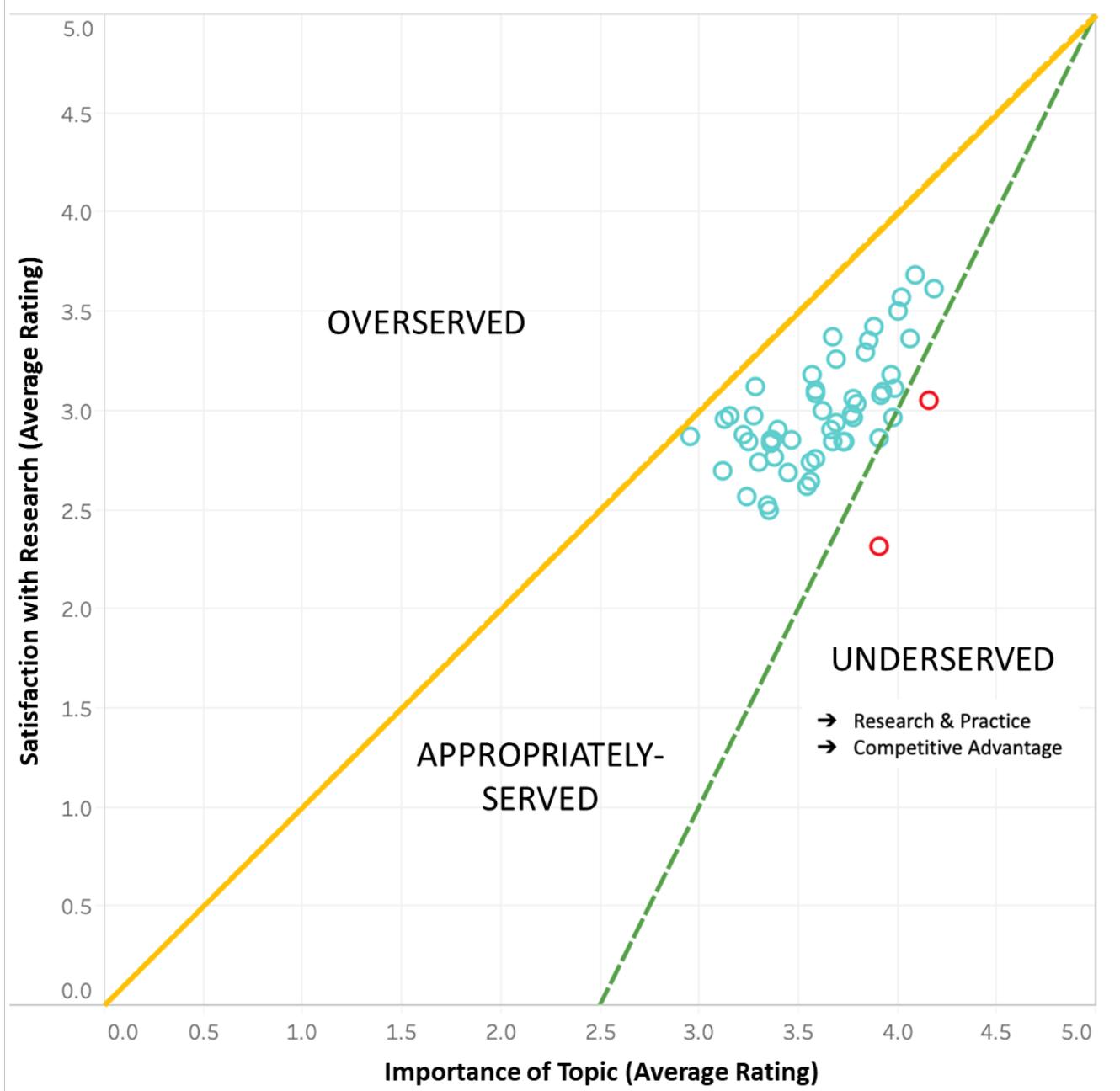


Figure 7

Appropriate Servicing: Satisfaction vs. Importance of Research Topics for Practitioners

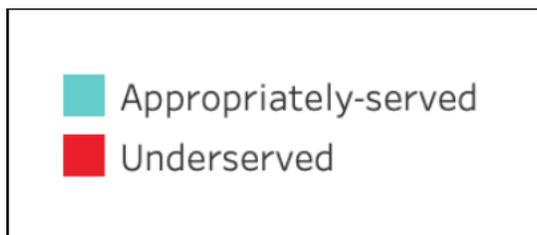
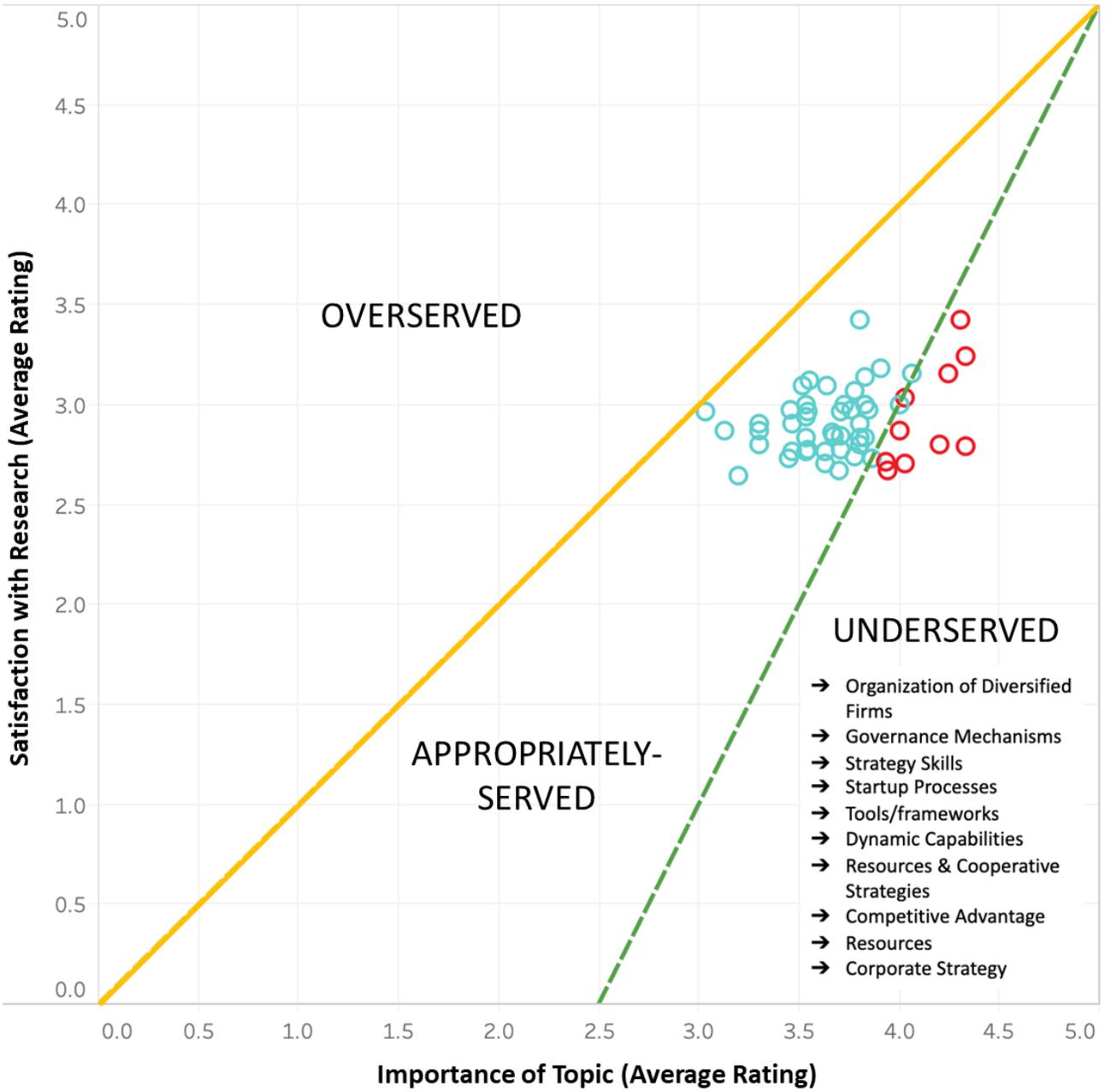


Figure 8
Research Topics with Largest Gaps Between Satisfaction and Importance
(Difference $\geq 50\%$)

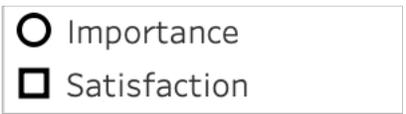
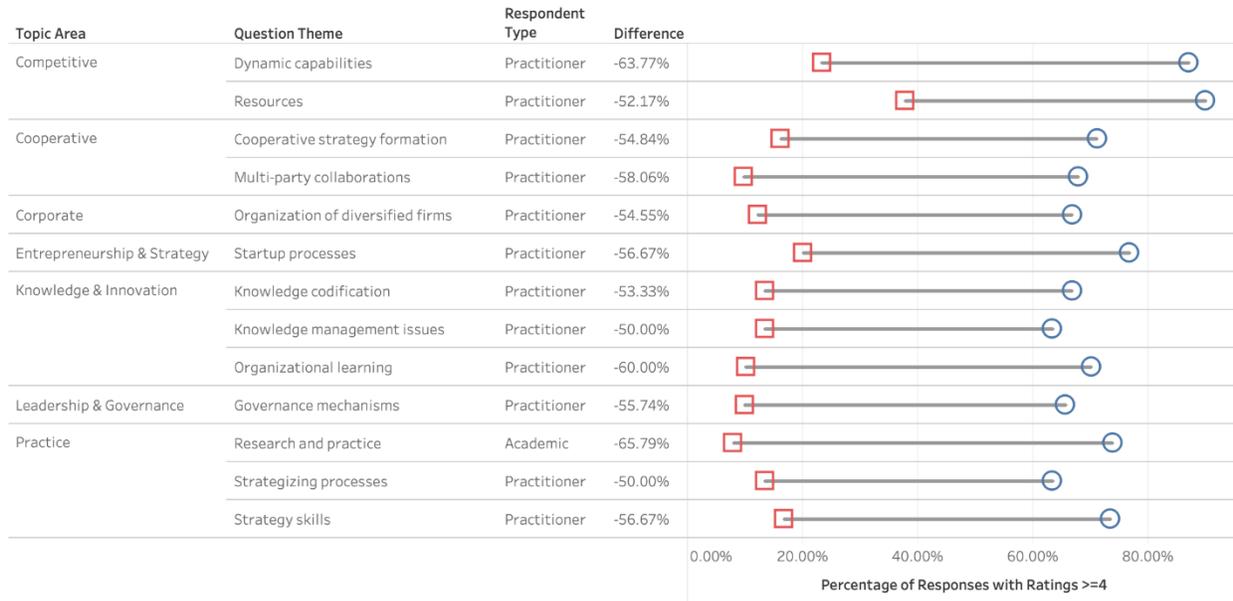


Figure 9
Research Topics with Smallest Gaps Between Satisfaction and Importance
(Difference $\leq 20\%$)

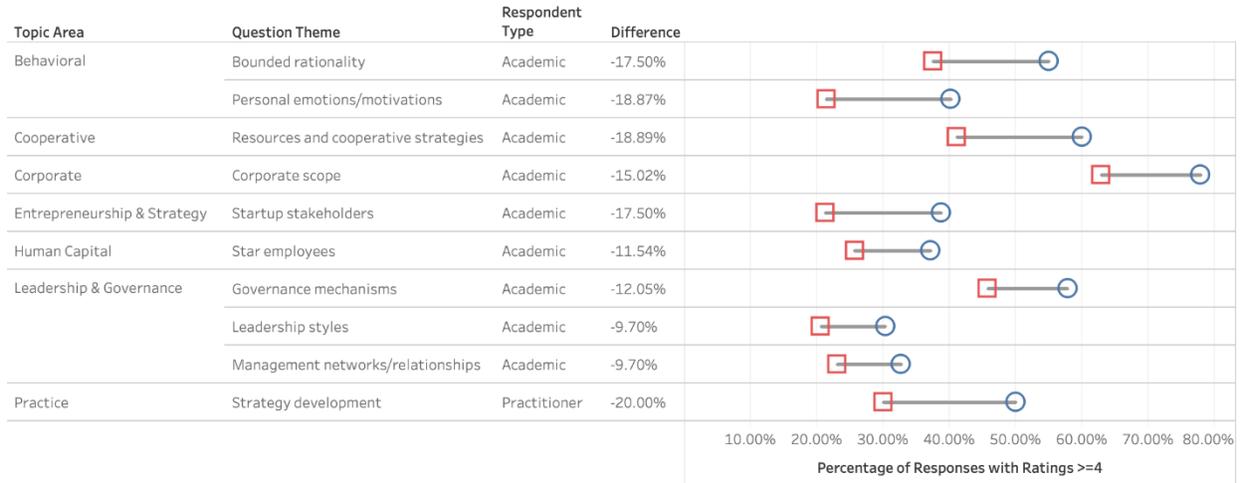


Figure 10
A Taxonomy of Research Studies

