J. of the Acad. Mark. Sci. (2016) 44:557–561
DOI 10.1007/s11747-016-0481-x

EDITORIAL

Is "strategy" a dirty word?

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Published online: 8 July 2016
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In her comments upon accepting the 2014 Vijay Mahajan Award for Lifetime Contributions from the American Marketing Association's Marketing Strategy SIG, Dr. Leigh McAlister—echoing concerns that others, too, have raised—suggested that the field has shifted such that methodological sophistication, instead of the importance of the marketing strategy question, has a greater impact on the likelihood of an article being accepted by a top journal. Dr. McAlister implored attendees to pursue substantive questions in a rigorous way and to take pride in being a marketing strategy scholar—not in being solely a marketing methodologist.

Over the last year and a half, and following years of service on the AMA Academic Council and to the AMA/Sheth Foundation Doctoral Consortium (faculty, co-chair, and recruiting host institutions), I find myself frequently reflecting on Dr. McAlister's words. The proportion of student fellows at the Doctoral Consortium who self-identify as "strategy" scholars has steadily declined, from nearly half of fellows 20 years ago (47.2%, 1995 Wharton) to a distinct minority in the most recent years (17.9%, 2015 London Business School). The trend bears out even in the more broad-based data available from the annual *Who Went Where* survey conducted by the American Marketing Association's Doctoral Student Special Interest Group; the proportion of young scholars who self-identify as market strategy scholars has fallen from around 40%, when that survey began in 2003, to just over 20% in 2015 (see Fig. 1). PhD students who have interests in substantive issues that fall squarely within the topical domain of marketing strategy are often advised explicitly by their mentors to position themselves in the market as empirical or analytical modelers, *not* as strategy researchers. Has "strategy" become taboo?

This trend among young faculty parallels a nagging perception among established marketing scholars that the types of articles published in our top journals appear to be bifurcating into "behavioral" or "quant," leaving an increasingly hollowed middle ground for traditional marketing strategy research. The behavioral articles focus on consumer behaviors, draw extensively from theories in psychology and social psychology, and primarily use laboratory (and occasionally field) experiments to generate empirical evidence. Many of these studies are agnostic as to how their findings apply to the marketing strategies of firms.[1] The quant articles feature analyses of large bases of secondary data using sophisticated econometric techniques, drawing on the toolkits and theoretical perspectives of industrial organization and other fields of economics. Although many of these papers are in the context of an important firm decision, too often theory is underdeveloped (used primarily to justify the choice of a particular variable in the model) and the complexity of the analytical requirements causes strict assumptions (correct analytically, but not well fit to the real world) to be imposed. The lack of theory and the context specificity of the analyses combine to cause the empirical findings to have only limited generalizability outside of the focal dataset.

Confronted by this bifurcation, marketing strategy scholars have sought to define the domain of field (e.g., Varadarajan

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[1] I'm pleased to note the commitment of *JAMS* to encouraging consumer scholars to pursue the strategic implications of their work, as articulated in Palmatier (2016).

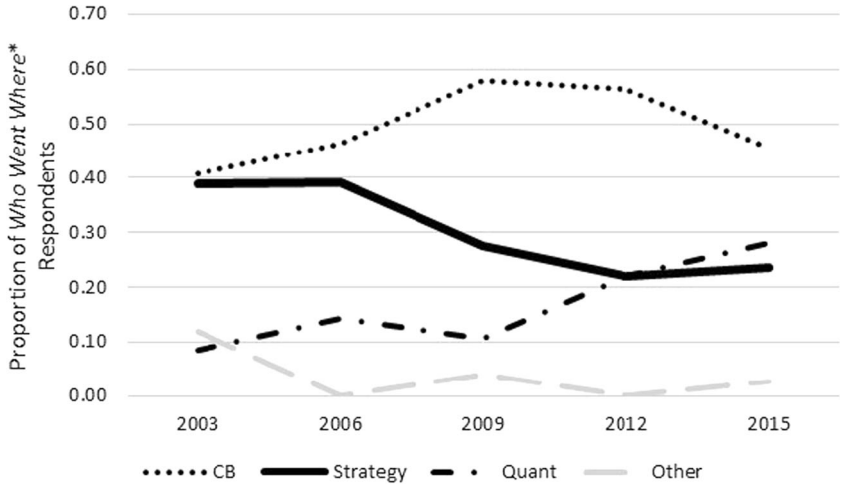

Fig. 1 Proportion of *Who Went Where* respondents who self-identify as consumer behavior (CB), strategy, or quantitative/modeling (Quant). *Results from the annual *Who Went Where* survey are available at: http://docsig.org/who-went-where/

2010) and to encourage rigorous research that has real-world relevance (e.g., Reibstein et al. 2009). However, the proportion of papers in top journals that focus on traditional marketing strategy research topics without pushing methods boundaries—i.e., studies that probe strategic managerial decisions, resource allocations, or strategic constructs—seem to be declining. Even substantive papers that feature competent empirical models can be dismissed for failing to make a methodological contribution (Lehmann et al. 2011).

Why has this divide emerged and how does it impact the marketing strategy scholar?

A partial cause of the evolving shape of the field is the increased specialization of the methodological training in PhD programs and the concomitant increasing methodological standards in the review processes of top journals. Conversations with marketing PhD program coordinators clearly confirm that, over the last 15 years, the balance of seminar coursework has tilted sharply in the direction of methods and away from marketing theory or domain content. After taking a small number of seminars with their entire cohort, students identify their "track" early and pursue a highly-specialized sequence of advanced methods courses to build their toolkit, either as a behavioral scholar or as a quantitative marketing scholar. In turn, as review teams are increasingly populated by these scholars whose training emphasized methodology, methods concerns take on a prominence in the review process. Scholars whose methodological skills match those of the reviewers are more likely to navigate the review process successfully.

For scholars who investigate substantive marketing strategy topics, but who were trained primarily in survey research methods, the divide is particularly challenging. Part of the challenge is in the empirical analysis of data, but the most critical challenge raised by the divide might revolve around the identification of research questions.

Empirical challenges

Much of the recent struggle mainstream marketing strategy research has faced in achieving top-tier journal space is due to failures by scholars to adapt in the face of evolving methodologies that increasingly enable the scholar to rule out competing explanations for results (Wittink 2004). For example, although the field recognizes limits in our ability to control for all sources of endogeneity (omitted variables, simultaneity, self-selection, etc.; Shugan 2004), the problems caused by endogeneity are now better understood and, in turn, the standards to which scholars are held to rule out or control for endogeneity are higher now than a decade ago. In a similar way, as our field's understanding of what it takes to establish causality continues to evolve, our standards on that front increase as well. For example, of great lament to many strategy scholars who were trained in survey methodology, the days are long gone of collecting data via a single cross-sectional survey, analyzing the results via SEM, and receiving a positive response from the editorial team of a strong journal. Many survey-based studies can be appropriately criticized for common source concerns, inability to empirically demonstrate causality, nested data concerns, and haphazardly constructed models that fit the data decently but do not demonstrate their superiority over equally plausible alternative models (see Rindfleisch et al. 2008 for an excellent discussion of these issues and potential paths forward for the survey researcher).

Challenges in the identification of research questions

Many questions of potential interest to the marketing strategy scholar involve the attitudes and beliefs of practicing managers and real consumers. Some of these internal states can be tapped by experiments or inferred from behavioral artifacts that reside in secondary data, but many can only be measured via primary data collection (e.g., surveys or conjoint). Thus,

some fear that the behavioral–quant divide circumscribes research into an entire category of marketing strategy questions.

Even indirectly, a methods focus can alter the research questions asked. When the data required for the use of a sophisticated analytical technique are simply not available, researchers are forced either to change to a different research question or to accept the limitations of their chosen approach. More problematic is when our work is guided more by the cool datasets we can find or the advanced methodological techniques we can employ; substance becomes a strawman or an afterthought. Further, while rigor is always of top importance, the review process can turn into a methods gauntlet in which substantive contribution is squeezed out in efforts to fully address every diverse methods concern (usually accurate, but often incremental or tangential) raised by a methods-focused review team (Reibstein et al. 2009).

For studies that are driven by data availability or by data being amenable to a specific method, theory is at risk of becoming an afterthought, with either no explicit theory-grounded framework being developed or "theory" being the restatement of commonly held beliefs (supported by a seminal cite or two). In turn, because the selection of variables is driven by the dataset, rather than by theory, the findings are hard to reconcile with what we already know; if a study does not have antecedents or controls in common with extant studies, the results are hard to compare. I believe that explicit theory provides the essential framework by which a scholar can link their findings from an individual study back to larger scholarly discussions. Without a foundation in theory, studies cannot be directly contrasted, and they pile up in an ad hoc manner instead of advancing our field's knowledge in a systematic way.

Where to from here?

What is the path forward? Our field must continue to strive to identify marketing strategy questions for which answers shed light on critical issues facing firms and customers. And, clearly, if the hallmark of our field is the asking of important substantive questions, not every paper should be required to use large secondary datasets, make a methods contribution to empirical or analytical modeling, and appeal to an economist; else, the domain of feasible questions would be severely constrained. But it is also apparent that scholars who investigate traditional marketing strategy topics need to provide a stronger empirical foundation for their conclusions. Further, to avoid making a one-off, descriptive contribution, well-done empirical scholarship needs stronger linkages to underlying theory. In short, intelligent balance is needed.

Let us drive two stakes into the ground.

- First, substantive marketing strategy questions are and should continue to be the hallmark of those who identify as marketing strategy scholars (Reibstein et al. 2009; see Varadarajan 2010 for insights into the topical domain of marketing strategy). Basic science is important and should not be ignored, but, as a field, our collective work should advance knowledge and the practice of strategic marketing.
- Second, equally important is rigor—both conceptual rigor and methodological rigor; without rigor, we can have little confidence in empirical findings, no matter how interesting or elegant the question.

From these starting points, I offer the following recommendations to methods specialists:

- *Survey research specialists*: strengthen your design and analysis to reduce alternative explanations for results; limit the use of surveys to questions that are appropriate, given the inherent strengths and weaknesses of the method; use multiple methods to address issues (e.g., causality) for which surveys are ill-suited.
- *Quantitative research specialists*: recognize that, although survey research has certain weaknesses that an econometric analysis of secondary data does not, surveys can (1) address questions that secondary data analyses cannot and (2) provide further insights into phenomena documented via secondary data analysis. Use surveys accordingly in your own work; when reviewing manuscripts for top-tier journals, hold to high standards, but do not dismiss survey-based research out-of-hand.
- *Behavioral research specialists*: recognize that research based on surveys or secondary data may not have the ability to demonstrate causality like an experiment, but those methods can provide insights into external generalizability. Follow the guidance in Hamilton (2016); consumer-based marketing strategy scholarship has an important role in the discipline.

To all three groups, I suggest that we need to instill in our PhD students the value of dialogue across research groups. One thing I appreciate deeply about the AMA/Sheth Foundation Doctoral Consortium is that it brings together scholars from across all topical and methodological areas. But one weekend of happy togetherness cannot overcome an ongoing routine of siloed thinking and training. Perhaps we need to better model open-minded scholarship for our students in daily interactions. On a broader scale, as a community of scholars, two near-term steps that we could take are to (1) bring together thought leaders from the different groups into special sessions at conferences to discuss the schisms that divide the discipline of marketing, to articulate the consequences and explore potential solutions, and (2) explore opportunities to bring back into our PhD programs a bit more focus on the substantive (relevant problems) and conceptual (theory development) domains.

I want to conclude with more general recommendations. If the substantive marketing strategy question is our hallmark, what should be the standards of evidence required by the discipline? I believe three categories of evidence combine to ensure rigor and relevance. In fact, combining these three approaches in the study of a single phenomenon provides stronger collective evidence than could be provided by any single approach.

First, I believe that we need evidence that the phenomenon of interest (e.g., a construct, a relationship between variables) actually exists. Do firms, markets, or consumers actually behave in ways that suggest that the scholar's premise has merit? Although surveys can provide this evidence, and qualitative methods can be used to probe a poorly understood phenomenon, here is a very appropriate place for secondary data. Secondary data are artifacts of actual human decisions and behavior that cannot be explained away by any recall or response biases that may drive survey responses (Houston 2004). By investigating secondary data (whether publicly available data or that collected from firms' archival records), initial evidence of customer and firm behaviors that are relevant to these kinds of questions can be gained. These data can be investigated in fairly simple ways to provide descriptive evidence that the phenomenon appears to exist in the real world, providing assurance that further work has merit, and setting the stage for other types of evidence. Alternatively, more sophisticated analyses of secondary data can go on to more specifically address substantive research questions.

Second, we need evidence of the *mechanisms* that explain any relationships that are observed empirically. Only when we can explain the mechanisms by which the influence in an empirical relationship is exerted can we be confident that the findings are not completely unique to the current conditions or the dataset. Further, the precision of theoretical implications and the degree to which actionable, specific insights for managers are generated are driven by precise evidence of mechanisms, reducing the likelihood that results are due to some unmeasured variable. In this case, the enhanced rigor of providing causal evidence via experiments actually increases the relevance for managers (see Jaworski 2011 for guidance for conducting and reporting research results to be useful to practice).

Because surveys cannot demonstrate causality unless a tight, longitudinal design is implemented, and lagged secondary data establishes sequence but does not shed light on attitudes or beliefs, for this second type of evidence, experiments—in the laboratory and/or in the field—can play a big role. Strategy scholars have under-employed experiments over the years, perhaps chastened by concerns that the college students that populate most university subject pools are not like "real consumers," and certainly not like "real managers." However, for questions revolving around managerial decisions, careful selection of samples (e.g., EMBA students, alumni panels, Amazon Mechanical Turk) and thoughtful design can ameliorate many of these concerns. Further, when examining customer responses for consumer-based marketing strategy questions, Hamilton (2016) presents useful guidance. When done well, results from experiments provide evidence of how and/or why the phenomenon works, such as, for example, *why* a change in product subtitling might result in a change in sales (Brasel and Gips 2014).

Third, because most strategy phenomena are embedded in a complex world, a thoughtful scholar provides evidence of a focal phenomenon's nomological network. A rich, theoretical framing insures that the study is grounded, versus ad hoc, and can be related back to a broader body of knowledge. Note that embedding a construct into a nomological network is, by nature, a theory-laden endeavor. Relevant relationships, as specified by theory, need to be included and operationalized appropriately. The nexus of theory and practice can be explored to identify intervening variables (e.g., moderators or mediators) that can be explored to see if the phenomenon of interest can be altered by managerial decisions or factors under the manager's, partner's, or customer's control.

Providing nomological evidence is perhaps the best role for surveys. In fact, if other research approaches provide the evidence that surveys are poorly suited to provide (e.g., secondary data provides substantive evidence and experiments provide causal evidence), surveys are particularly well-suited for testing nomological networks (via structural equations models).[2] Scholars who use surveys have, over time, improved their approaches to confront some of the common criticisms of survey research with improved design, such as reducing common source concern by collecting dependent variables from different sources (e.g., Podsakoff et al. 2003) or by providing suggestive evidence of causality via longitudinal design (e.g., Rindfleisch et al. 2008). Concerns can also be addressed through analysis strategies, such as using methods that parcel out variance due to interdependencies (e.g., hierarchical linear modeling), including a common method factor in models, or empirically comparing the fit of a model to theoretically reasonable rival models.

Does every strategy article need to provide all three types of evidence: secondary, experimental, and survey? If introducing a new phenomenon to the field, this compilation of evidence reduces alternative explanations and provides a rich foundation for follow-up research. However, conducting and reporting all three in a single paper can be a challenge and is probably not needed for narrower questions that target a more incremental contribution. In the end, though, if a scholar's approach leaves too many questions unanswered from the substantive, theoretical, or methodological domain (Brinberg

[2] Other data sources can be useful for nomological evidence, as well; e.g., secondary data proxies can be found and validated for many marketing strategy constructs (Houston 2004).

and McGrath 1985), their paper will, despite an important research question, either (1) fail to survive the review process at a good journal or (2) get published on the strength of its research question but have little long-term impact.

In closing

"Strategy" is not a dirty word. Strategy scholars have been rightly (if often too harshly) criticized from two sides. Some of us continue to ask interesting questions but have failed to keep up with methodological advances in the field; our evidence is not strong enough to support our claims. Others of us have focused so heavily on methodological rigor that our work is irrelevant and atheoretical (Lehmann et al. 2011). The path forward is to avoid both ditches.

My hope is that this editorial will encourage dialogue among scholars in our discipline. If we are addressing important, substantive questions, we are stronger together—providing corroborating and complementary evidence of a phenomenon from different vantage points rather than as increasingly disparate groups. I hope it also challenges us as reviewers, Area Editors, and Editors-in-Chief to reconsider how we evaluate marketing strategy manuscripts. Finally, as individual scholars, we can strive to provide strong, multi-faceted evidence regarding important substantive problems rather than slicing narrower and narrower slices of contribution using increasingly sophisticated specialized methods. Together, we can avoid the kind of marginalization toward which some argue marketing scholarship may be headed (Reibstein et al. 2009).

Acknowledgments The author thanks colleagues Bob Leone, Texas Christian University, Rebecca Slotegraaf, Indiana University, and Manjit Yadav, Texas A&M University, for their challenging comments on early drafts of this article.

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