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Factors affecting the study of important marketing issues: Implications and recommendations

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ABSTRACT

Academic research in marketing has produced many useful insights and tools over the years. Lately, however, an increasing number of scholars have voiced concern that much of the current academic research is not particularly useful. We explore reasons why this may be the case, and offer a set of recommendations to address them. We first identify three general factors that must be present in order for academics to develop important marketing insights: (i) *awareness* of important marketing issues, (ii) *ability* to address these issues, and (iii) *motivation* to address them. Using this as a framework, we identify several variables that may be reducing the likelihood of our studying important marketing issues. These variables suggest actions that we may take to increase the likelihood of our producing important marketing insights and tools.

Over the last several decades, academic research in marketing has generated a number of important theories, frameworks, models and decision-making tools. There are many examples of such work: Howard and Sheth (1969) build a theory of buyer behavior; Parasuraman, Zeithaml, and Berry (1985) develop a theory of service quality; Tuli, Kohli, and Bharadwaj (2007) propose a new view of customer solutions; Srivastava, Shervani, and Fahey (1998) develop a framework that links market-based asset management to shareholder value; Guadagni and Little (1983) develop a model for predicting effects of marketing actions on brand choices; Green and Srinivasan (1990) discuss conjoint analyses for designing products; and Rust, Lemon, and Zeithaml (2004) develop a customer-equity based approach for comparing marketing investments.

In recent years, however, an increasing number of scholars has voiced concern that the usefulness of research published in academic journals has diminished substantially. For example, Reibstein, Day, and Wind (2009) suggest marketing academia may be losing its way. Similar concerns are raised by other scholars including Lehmann, McAlister, and Staelin (2011), Coulter (2016), Moorman (2016) and Steenkamp (2017). Prompted by these concerns, editors of major journals have made calls for more relevant research (e.g., Inman, Campbell, Kirmani, & Price, 2018; Moorman, van Heerde, Page Moreau, & Palmatier, 2019). Similarly, Responsible Research in Business and Management (RRBM), a relatively new organization formed by prominent academics, notes that current research is often disconnected with real-world challenges. These developments suggest that while some articles published in recent years address important issues, their proportion relative to the total number of articles published has declined. Similar observations have been made outside of the marketing discipline (e.g., Bartunek, Rynes, & Duane Ireland, 2006; Gunther McGrath, 2007; Rynes, Bartunek, & Daft, 2001; Shapiro, Kirkman, & Courtney, 2007).

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Why might this be the case and what can be done about it? RRBM's position paper observes that the reasons for this decline lie in the research ecosystem that focuses on scholarly impact more than societal impact. The position paper highlights the need to change the incentives and culture around publications.[2] The paper lists a number of high-level suggestions such as (i) elite journals should encourage and publish problem-centered research, (ii) scholarly associations should reinforce professional commitment among members, (iii) university administrators should develop a vision and strategy to encourage faculty to work on research that would make a positive difference in society, (iv) accrediting agencies should convene deans and academic leaders to discuss responsible research, and so on.

We complement the above-noted work by approaching the problem from the perspective of scholars working individually or in teams, making choices at the "workbench" level about what research problems to study. We develop a simple but comprehensive framework to identify a broad set of reasons why scholars may choose to study less important research questions/issues – the "lost before translation" problem (Shapiro et al., 2007) – and suggest ways to deal with them at a more specific, granular level.

Briefly, we argue that scholars are likely to address important marketing issues if three conditions are satisfied: First, scholars are *aware* of (at least some) important marketing issues. Second, scholars have the conceptual and methodological *ability* to address those marketing issues along with the ability to make judicious tradeoffs between research importance and methodological rigor. Third, scholars have the intrinsic and extrinsic *motivation* to address important issues.[3] The framework is a variation of the ability-motivation-opportunity (AMO) framework employed by previous research in several disciplines (e.g., Appelbaum, Bailey, Berg, & Kalleberg, 2000; Boxall & Macky, 2009; MacInnis, Moorman, & Jaworski, 1991). Our framework incorporates "awareness" rather than "opportunity" because the former likely is a more critical antecedent in our context (see Fig. 1).[4]

This framework helps pull together a number of individual and institutional variables that may explain the decline in the "market share" of articles dealing with important marketing questions. The framework provides an organized grouping of the antecedent variables, and has the potential to help identify additional antecedents in future research. Importantly, the antecedents suggested by the framework directly point to practical, fine-grained actions that may be taken by researchers, promotion & tenure committees and administrators to increase the likelihood of scholars studying important marketing issues.[5]

We consider in turn each of the three factors comprising the framework—awareness, ability, motivation—and develop a number of ideas for enhancing each of the three factors. Where pertinent, we note ideas offered by other scholars along with ours in order to increase the formers' accessibility to readers. Our recommendations are summarized in Table 1. We start our discussion, however, by suggesting that the use of the term "relevance" to refer to research that is important for the field may actually be contributing to the decline of impactful papers.

1. Importance versus relevance of a research issue

Scholars often highlight the need for greater relevance in research. For example, a frequent debate in the discipline revolves around whether there is a trade-off between research rigor and relevance (e.g., Kumar, 2017; Rust, 2018). The term "relevance" in such discourses is used to imply the importance (or significance) of a research issue/question addressed in a paper. However, the term "relevance" has a much narrower meaning. The Oxford Dictionary defines relevance as "the quality or state of being closely *connected* or appropriate" to something (emphasis added). Being *connected* to something, however, is very different from being *important*. "Importance" is "the state or fact of being of great *significance or value*" to something, per the Oxford Dictionary (emphasis added).

While all marketing issues/questions are by definition connected to marketing phenomena (and therefore *relevant* to marketing), answers to some marketing issues are of greater significance or value (and therefore more *important)* than others. For example, consider two issues: (1) do consumers find four-color brochures more appealing than five-color brochures, and (2) under what conditions do online channels serve as substitutes versus complements to offline channels? Both issues are about marketing and therefore of *relevance* to marketing. However, the first issue is less *important* than the second issue because answers to the latter issue are likely to change the behaviors of senior practitioners in a more significant way than answers to the first issue. In sum, not all issues relevant to marketing are important (but not vice versa).

There are multiple reasons why marketing scholars may choose to address research questions/issues that are relevant but not particularly important. First, it is possible that marketing scholars evaluate research questions/issues through the lens of relevance rather than importance given the widespread use of the term "relevance" in marketing discourses (e.g., Kumar, 2017; Rust, 2018). Second, scholars may choose to study relatively less important questions/issues because it is easier to isolate effects for such issues with the level of methodological rigor expected by major journals, and the reward system in schools encourages research quantity over research importance. Issues of greater importance tend to be more ambiguous and hence more difficult to study with comparable rigor (Roberts et al., 2014a). A central point of this paper is that while it is essential for academic papers to be rigorous, a narrow or heuristic interpretation of its meaning inhibits the development of research on important questions/issues. In a later

[2] https://rrbm.network/position-paper/

[3] In addition to having awareness, ability and motivation, scholars must also believe that others (e.g., editors, reviewers) believe a given topic is important.

[4] We note that opportunities presented by financial resources and connections can play an important role, and these may be distributed unevenly across schools. These resource issues, however, are not the focus of this paper.

[5] Our framework focuses on factors which impact the likelihood of scholars studying important marketing issues. It addresses the "lost before translation" problem (Shapiro et al., 2007). It does not address the "lost in translation" problem – that of poor communications of research findings/conclusions to target stakeholders.

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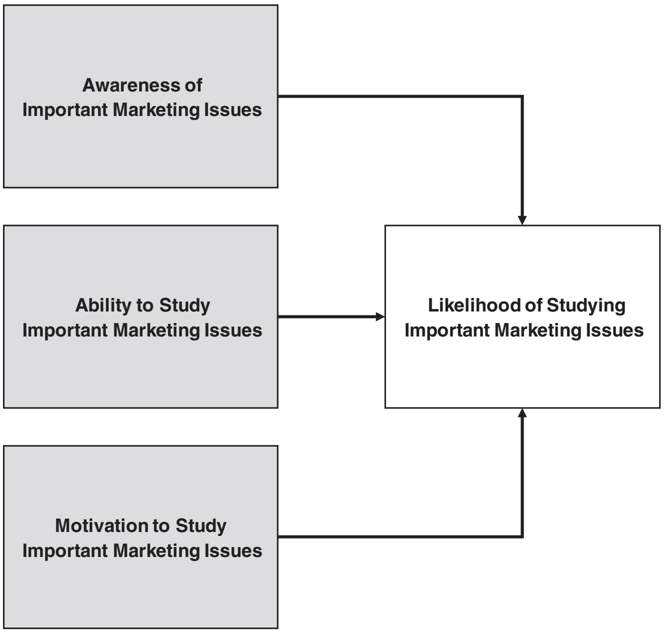

Fig. 1. Factors influencing the likelihood of studying important marketing issues.

section, we suggest a broader and more appropriate construal of rigor, and recommend balancing it with the importance of an issue investigated in a research project.

Moreover, we lack a common understanding of how "importance" may be operationalized. Drawing on Zaltman (1983) and Jaworski (2011), we suggest that a research issue's importance may be operationalized by three inter-related criteria:

i. *Number* of stakeholders that are likely to change their behavior because of the answer to the issue. We define marketing stakeholders in a broad way and include managers as well as public policy makers, consumers, academics, consultants, law enforcement and societal groups. An issue that affects a greater number of stakeholders (in an affected subgroup) is arguably more important than one that affects just a few.[6]
ii. *Status*[7] of the target stakeholders (within a given subgroup) who are likely to change their behaviors. Changes in the behavior of higher status individuals have greater impact, and are arguably more important. For example, an issue with implications for changes in the behavior of CMOs is likely to involve bigger budgets and lead to more extensive organizational changes than an issue with implications for assistant brand managers, and hence may be considered to be more important. The same applies to issues with implications for high-level public policy makers, key opinion leaders or social media influencers or consumer activist groups.
iii. *Magnitude* of expected change in stakeholder behaviors. Research on an issue that could lead to major changes in a manager's strategies, tactics, investment patterns, and the overall nature of work is arguably more important than research on another issue likely to lead to smaller behavioral changes of the manager. The same applies to research on issues that may lead to major changes in behaviors of consumers at points-of-purchase.

Importantly, the above-noted inter-related variables—*number, status and magnitude*—help assess the importance of a *specific* research issue or question rather than that of a broad research topic. For example, while the broad topic of digital marketing is an important one, some issues/questions belonging to this topic (e.g., location of a banner ad on a web page) are less important than other issues (e.g., developing an ecosystem for launching new products).

In many cases, answers to research issues may immediately suggest specific actions for the marketing practitioners impacted by the issues. In such cases, assessing the importance of the research issues is relatively straightforward. In other cases—for example when a research project develops a new scale/measure or a new estimator—the actions these stakeholders might take based on the research may be less clear. We suggest that in such cases the research issue's importance be assessed using a "line of sight"

[6] We make an explicit distinction between academic stakeholders and non-academic stakeholders. While academic stakeholders are important to consider, this paper primarily focuses on non-academic stakeholders.
[7] Where applicable.

Table 1
Recommended practices for increasing the likelihood of studying important marketing issues.

Factor	Current practices/status	Recommended practices
Importance vs. *Relevance of a research issue*	Marketing scholars (including reviewers) tend to evaluate research questions/issues through the lens of relevance rather than importance. This overestimates the importance of issues examined in a paper.	1. Evaluate research projects based on the "importance" of the issue investigated and not its (mere) "relevance." The importance of an issue can be operationalized by the number of marketing stakeholders that are likely to change their behaviors, the status of the target stakeholders, and the magnitude of expected change in stakeholder behaviors.
Awareness of important marketing issues	Scholars tend to select research problems without adequate awareness of issues of importance to marketing stakeholders. This can lead to their working on "phantom problems" – research problems that are less important and even unimportant to marketing stakeholders.	2. Institute and encourage mechanisms for increasing direct interactions between scholars and marketing stakeholders (e.g., such as consulting, internships in commercial and non--commercial organizations, executive education, sponsored research and community-engaged research). 3. Rebalance curricula of doctoral programs, giving more weight to seminars focusing on substantive marketing issues.
Ability to study important marketing issues	Researchers often refrain from working on important research issues/questions because they think they do not have the ability to do the research well, or that knowledge gatekeepers (editors, reviewers) do not have the ability to evaluate the research appropriately. These abilities include (i) conceptual ability, (ii) methodological ability, and (iii) the ability to make judicious trade-offs among research design elements.	4. Develop conceptual and theory construction abilities of doctoral students and faculty via doctoral seminars, research workshops, and developmental sessions at conferences. 5. Evaluate the conceptual rigor of papers based not on whether they incorporate a currently fashionable or prominent theory from another discipline, but rather based on the intrinsic soundness of the conceptual work. 6. Assess the probability of a study's conclusions being false considering the study's aim (testing for presence of effects versus estimating effect sizes). An insistence on using state-of-the-art methods in all papers regardless of their aims is counterproductive. 7. Make judicious tradeoffs between the importance of the issues examined in a studies and the probability of the study's conclusions being false.
Motivation to study important marketing issues	Marketing researchers self-identify themselves as "behavioral," "quantitative" and "strategy." Doctoral students are admitted and faculty members are hired with little consideration given to whether they have an intrinsic interest in Marketing. These practices lead to limited intrinsic motivation of marketing scholars to study important marketing issues. Administrators and promotion & tenure committees tend to consider the number of papers published in top journals as a heuristic for research contribution. This limits scholars' extrinsic motivation to study important marketing issues that may take more time and result in a fewer number of publications.	8. Move away from identifying ourselves as "behavioral," "quantitative" and "strategy" and instead develop self-identities around substantive topics that are uniquely or largely marketing. 9. Screen for intrinsic interest in marketing of faculty (and doctoral applicants), especially when hiring from non-marketing doctoral programs. This should not be based on what candidates say or the journals in which they publish, but rather on the actual topics and issues on which they work. 10. Consider the three components of "Issue Importance" developed in this paper to evaluate the importance of a faculty member's work rather than just the number of publications or the stature of the journals in which it is published. 11. Limit the number of papers considered for promotion and tenure decisions to 1–2 at each promotion stage (while allowing for exceptions for clearly demonstrable programmatic research that cumulatively makes a strong contribution). 12. Develop a portfolio of research projects, such that it includes at least one project that addresses an issue of high importance.

to the action implications likely to be developed by academics in subsequent research using the new scale or estimator. This also applies, for example, to review papers and meta-analyses, which can reveal useful insights and encourage other academics to subsequently study issues with strong actionable implications for marketing practitioners. In absence of such visibility to potential future use, it is difficult to make the case that a research issue is important.[8] As Lilien (2016, p. 553) notes, "Practitioners are the ultimate audience for B2B academic research. To my mind, academic research that has neither direct nor indirect implication for practice in some way is hard to justify."

2. Awareness of important marketing issues

It is self-evident that scholars can address important issues concerning marketing phenomena only if they are aware of such issues. In absence of such awareness, scholars risk working on issues that may be less important and perhaps unimportant to marketing stakeholders (issues that a colleague terms "phantom problems").

[8] We recognize that it was difficult to imagine practical uses of many results/findings in the natural sciences at the time of their discovery. However, in social sciences such as marketing we are unaware of any such examples, and believe that few, if any, are likely to exist. We analyzed the share of winners of the Paul E. Green (annual) Award between 1996 and 2015 who subsequently also won the William F. O'Dell Award (for long-term impact). Out of a total of 21 Green Award winners, 8 also won the O'Dell Award. This indicates that a non-trivial number of articles considered to be important at the time of publication were later judged as having had a long-term impact.

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2.1. Direct interactions with marketing stakeholders

Scholars identify research issues/questions in a variety of ways. These include (1) wondering about the impact of technological and social changes on marketing phenomena, (2) consulting sources that document research priorities and directions (e.g., MSI Research Priorities), (3) reflecting on puzzles, paradoxes or issues they have personally encountered as marketing stakeholders, (4) obtaining datasets and identifying research questions they can address,[9] (5) learning practical problems or dilemmas faced by marketing stakeholders during conversations at their workplaces, seminars and conferences (Lehmann, 2014).

While these (and other) ways of identifying research issues are potentially useful, we suggest that scholars are more likely to become aware of marketing stakeholders' truly important problems through *direct interactions* with them. Direct interactions with marketing stakeholders—whether they are managers, consumers, or policy makers—enable a richer understanding of their problems, the context of the problems, and the form of research findings that are likely to be instrumental in changing the behaviors of those stakeholders (Rynes et al., 2001). This is consistent with Roberts et al. (2014a) who find that questions/issues studied in many articles that go on to have academic and practice impact have their origins in consulting assignments (see also the importance of intermediaries as marketing stakeholders discussed by Lilien, 2011). Many of the early works in marketing science were based on practical problems emerging from such interactions (Winer, 2014). This suggests several action recommendations for researchers and school administrators.

Doctoral program applicants as well as faculty applicants who have previously interacted with certain marketing stakeholders (e.g., marketing managers, public policy officials), or were marketing stakeholders themselves, are more likely to be aware of important marketing issues that could benefit from research. Thus, a straightforward (but perhaps not non-controversial) recommendation is for faculty and school administrators to give greater weight to such experience in selecting students into doctoral programs and in hiring faculty.

In addition, individual faculty and school administrators may develop formal and informal mechanisms for fostering direct interactions between researchers and marketing stakeholders. For example, many faculty researchers undertake periodic internships (or externships) in business organizations (such as the Association of National Advertisers that has a formal internship program), public policy bodies and non-governmental organizations. This is consistent with Lilien (2016, p. 552) who advocates that B2B researchers undertake internships in business organizations. At the same time, faculty may be concerned that internships are time consuming. A solution for this is shorter periods of immersion of a few weeks, which can produce a lot of the benefits of longer ones at considerably lower cost.

Similarly, many faculty researchers undertake some level of consulting and executive education activities in part to better understand current issues of concern to executives. This recommendation is consistent with Roberts et al. (2014a) who indicate that symbiosis with consulting is a characteristics of articles achieving high academic and practice impact. It also mirrors the recommendation of Raju (2005) that business school faculty could benefit spending time with "patients" similar to what academics in medical schools commonly do. Stremersch and Winer (2018) likewise recommend that schools encourage senior faculty to engage with practitioners through consulting.

Faculty researchers may initiate these activities on their own, and/or school administrators may encourage and facilitate these activities along with material support. For example, a top-tier business school in the US requires all of its faculty members to do a minimum level of executive education each year. Another top-tier school at one point considered providing summer support to faculty members only if they could get a business or another organization to commit to pay for a part of their research (this idea was found wanting on other dimensions, and eventually dropped).

2.2. Community-engaged research

One way of generating particularly rich interactions with marketing managers, consumers, law enforcement officials and policy makers is community-engaged research. Researchers commonly view themselves as doing research *about* individuals and/or organizations engaged in marketing. The alternative approach of community-engaged research refers to doing research *with* individuals and/or organizations that are the focus of study. For example, Zaltman et al. (1982) discuss the "theories-in-use" approach that involves engaging with marketing stakeholders to elicit their knowledge about marketing phenomena and develop formal theory. Such an approach enables researchers to become more aware of marketing stakeholders' problems and at the same time develop more influential research, as evidenced by several award-winning articles using the approach (e.g., Challagalla, Murtha, & Jaworski, 2014; Parasuraman et al., 1985; Tuli et al., 2007). It also addresses the "translation problem" of management research by making academics and practitioners appreciate and understand each other's worlds better (Shapiro et al., 2007). For a more detailed discussion of community-engaged research see Van den Ven (2007), and for a detailed discussion of the theories-in-use approach see Zeithaml et al. (2020).

We recognize that co-producing new knowledge with community members takes more time. This may inhibit faculty scholars concerned about demonstrating productivity (when defined narrowly as the number of published articles) for their annual evaluations, promotions and/or tenure. Addressing this concern calls for promotion & tenure committees and school administrators to give greater weight to research importance/impact (versus number of articles in top-tier academic journals - see Shapiro et al., 2007). As we suggest later, this may be encouraged by limiting the number of research pieces that may be considered for

[9] We should point out that this approach increases the odds of scholars becoming victims of the "streetlight effect" – looking for car keys under a streetlight rather than the dark parking lot where they were lost.

promotion/tenure to one or two. However, as discussed later, even if this were to not happen, researchers, including junior faculty, can readily accommodate a community-engaged project in their research portfolios, and benefit from it personally.

2.3. Doctoral curricula

In recent years, doctoral curricula have been including an increasing number of methods courses and seminars, as well as courses in the so-called foundational disciplines especially psychology and economics. This has led to a reduction in the exposure of doctoral students to research that is prominently focused on marketing phenomena. Consistent with calls by several scholars (e.g., Houston, 2016; Lehmann et al., 2011; Stremersch & Winer, 2018; Yadav, 2010), we recommend rebalancing the curricula with more courses/seminars dealing with substantive marketing theories, findings and methods in order to increase doctoral students' awareness of the breadth and richness of marketing issues.

We note that there are at least two counterarguments to this recommendation: First, some may argue that adding substantive courses/seminars will require doctoral students to drop some of the courses/seminars they take currently (or spend more time in their programs). Second, and relatedly, the job market appears to value deep expertise in methods or theories in the so-called foundational disciplines more than expertise in marketing theories, findings and methods. As we argue later, these concerns reflect an unfortunate value system among some in the marketing community, which undermines the discipline and warrants collective rebalancing through a variety of ways discussed later.

3. Ability to study important marketing issues

Researchers are more likely to choose to work on an important research issue/question if they believe they have the ability to do the research well, and that knowledge gatekeepers (editors, reviewers) have the ability to evaluate the research appropriately. Conducting (and evaluating) research well requires several types of abilities. Prominent among them are (i) conceptual ability, (ii) methodological ability, and (iii) the ability to make judicious trade-offs among research design elements. We submit that methodological abilities of marketing authors and reviewers are strong; however, our conceptual abilities and abilities to make judicious trade-offs among research design elements appear to need strengthening, as discussed next.

3.1. Conceptual ability

Issues that are more important—e.g., those that are relevant to high status stakeholders, or affect a greater number of stakeholders—generally are more "messy" and ill-structured. For example, the issue of what is customer experience and what factors enable some organizations to deliver better customer experiences is messier and less structured than, say, the issue of the relative effectiveness of mobile advertising in retail stores versus non-store environments. Research on "messy" issues requires strong conceptual abilities for structuring them in terms that lend themselves to rigorous inquiry and scrutiny. These abilities include explicating the marketing phenomena, abstracting from the specifics of phenomena, delineating similarities and differences across abstract constructs, and relating constructs to build theories about their antecedents, consequences and moderators (MacInnis, 2011).

A number of indicators suggest that there is a need to strengthen the conceptual abilities of marketing scholars (including reviewers). For example, relatively few doctoral programs offer seminars focused on developing conceptual abilities. Unsurprisingly, MacInnis (2005) and Yadav (2010) point to the precipitous decline of conceptual articles in major marketing journals. Moreover, the marketing discipline is often described as an "applied discipline". This is frequently interpreted as calling for the application of theories from other disciplines to the marketing setting/context, rather than as calling for viewing the discipline as a setting/context with distinctive phenomena that require the development of organic/indigenous theories to explain, predict and control the phenomena. This likely explains why marketing scholars frequently borrow theories from disciplines such as psychology, sociology and economics, and "apply" them to marketing contexts. Instances of marketing scholars constructing organic/indigenous theories are relatively sparse despite calls for them by several major journal editors (Kohli, 2011; Moorman et al., 2019; Rust, 2006).

What might researchers and school administrators do to enhance marketing scholars' conceptual abilities? Today, relatively few doctoral programs offer seminars aimed at developing students' conceptual abilities. Moreover, many of the marketing theory seminars offered tend to focus on philosophy of science issues rather than construct development, abstraction, differentiation, synthesis, and theory building (see MacInnis, 2011). In absence of a forum for developing conceptual abilities, it is difficult for doctoral students to acquire them efficiently. In recent years, the *Academy of Marketing Science Review* under the editorship of Yadav has played a useful role. However, one only has to contrast the attention paid to developing conceptual abilities with the time and resources devoted to developing empirical capabilities to appreciate the stark imbalance.

We recommend the use of several mechanisms for developing conceptual abilities of doctoral students and enhancing those of faculty at large. These include introducing seminars on conceptual development and theory construction in doctoral programs (see also MacInnis, 2011; Yadav, 2010), running special sessions on these topics at conferences, and holding stand-alone workshops at schools, consortia of schools and conferences. More broadly, we urge scholars as well as administrators to develop a supportive culture at schools and academic conferences by encouraging risk-taking that is inherent to early stages of conceptual work and organic theory development. This means encouraging doctoral students and colleagues to undertake organic/indigenous theory building, being tolerant of looseness in thinking likely to be present during the early stages of projects, and offering suggestions

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for addressing those weaknesses. As experienced scholars recognize, organic theory building almost always goes through a phase of weak, ill-formed ideas before they are developed into coherent sets of rigorously defined constructs and theories.

Aside from the matter of conceptual abilities, there appears to be the perception that it is easier and less "risky" for authors to apply theories from respected disciplines such as psychology or economics to marketing contexts. Reviewers and editors may feel more comfortable accepting such work compared to a novel theory specific to marketing phenomena developed for the first time by authors. A primary focus on borrowing from other disciplines, however, inhibits the development of organic marketing theories and new insights about important issues that may be unique to marketing contexts. Furthermore, a predominant "borrowing orientation" comes in the way of the marketing discipline establishing its own distinctive knowledge base and reputation. As such, we recommend that reviewers evaluate the conceptual rigor of papers based not on whether they incorporate a currently fashionable or prominent theory from another discipline, but rather based on the intrinsic soundness of the conceptual work (e.g., integrity of abstraction, precision of construct definitions and theoretical propositions, and persuasiveness of arguments supporting those propositions).

3.2. Research design tradeoff ability

Many important (and messy) issues are more time consuming and difficult to study with as much precision as well-structured issues that may be less important. For example, it is more time consuming and difficult to obtain longitudinal data about organizational variables in order to infer causal orderings among those variables (compared to, say, data on sales promotions and revenues). Similarly, it is very difficult to conduct field experiments with random assignments of strategic actions (e.g., different channel designs) to treatment and control groups (McAlister, 2016).[10] The high cost of studying important issues in terms of time, effort and dollars is likely to dissuade scholars from studying them if productivity is measured simply by the number of articles published, an issue we address in the next section.

Moreover, these significantly greater costs are likely to dissuade scholars from studying messy but important issues unless they believe that reviewers and editors can make appropriate trade-offs across various research design elements, including between methodological rigor (e.g., accuracy of estimated effect sizes) and the importance of an issue examined in a paper.

We underscore that virtually *all* research projects involve making trade-offs. For example, lab experiments trade off external validity for internal validity; studies with archival data tend to trade off validity of measures with external validity; small sample ethnographic studies trade off external validity for depth/novelty of insights. Broadening the notion of research design to include the importance of an issue examined in a research project, Rust (2018) argues that while it sounds nice to say that a leading journal article should have both rigor and relevance (i.e., importance), a trade-off between the two is inevitable. It therefore behooves scholars to have the ability to make judicious trade-offs between rigor and issue importance.

The ability to make sound trade-offs is especially important because papers vary in their research aims. As Kohli (2017) suggests, these call for different levels of methodological and conceptual rigor for the papers' conclusions to be valid. Specifically, in papers that test a new theory, typically the research question is whether a variable influences another variable or not (i.e., whether an effect is present); in other papers the research question focuses on the *size* of the effect. The two research questions call for differing levels of accuracy/precision in the estimated effect sizes, and hence different levels of methodological rigor.

For example, it is critical to estimate a well-specified econometric model to engender confidence in the conclusions of a research project designed to assess the effect size of advertising on new product sales. In contrast, a complicated econometric model is less important for the conclusions of a research project aimed at testing a new theory proposing the presence of an effect of consumers' regulatory focus on the number of products in their shopping baskets. Conversely, conceptual rigor (sound arguments for the proposition) is critically important for the latter project, but less critical for the former.

Some reviewers appear to use the absence of the latest methodological refinement in a paper as a "fashionable" heuristic to reject papers (see also Ellison, 2002). In doing so, they appear to perpetuate a tendency (in Economics) chided by the Nobel laureate Paul Krugman: "… there's still an overvaluation of mathematical razzle-dazzle relative to real insight."[11] To hold each research project to the highest possible standards of novelty as well as all types of validities and precision is tantamount to a call for rejecting *all* research projects (see also Lehmann et al., 2011). Instead, we argue that the methodological and conceptual standards used to judge a research project must take into account the *aims* of the project and the importance of the issue it examines. As scholars have previously noted, questions of methodological rigor become more difficult to solve when addressing problems relevant to marketing managers at higher hierarchical levels (e.g., Roberts et al. 2014b).

To be clear, we are not advocating publishing theories/findings that are wrong. However, as is well known in the Philosophy of Science literature, it is not possible to treat a theory or a finding as being 100% true or false based on the results of one empirical study, or even multiple empirical studies. This is because of the problem inherent to induction discussed in this literature (e.g., Popper, 1982, pp. 86, 88), and the potential invalidity of assumptions inherent in every empirical method (see Popper, 1959, p. 50). For this reason, conclusions about a theory or findings based on an empirical study cannot be characterized as true or false with certainty, but rather held with higher or lower levels of confidence depending on the methodological and conceptual rigor of the research.[12]

[10] It is important for the discipline to address strategic issues as well as operational/tactical ones.

[11] Krugman, Paul (2019), "Gold bugs for Trump," New York Times, July 13.

[12] The idea that conclusions from a piece of research are tentative is clearly reflected in the use of *p*-values to indicate the level of confidence that may be had in the results of an empirical study.

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How might scholars and reviewers/editors take into account the above-noted considerations to make judicious trade-offs in research design? Lehmann et al. (2011) persuasively argue that the marketing field should embrace two criteria for evaluating a research project: (1) "Is it interesting?" and (2) "Is it not wrong?" The "not wrong" criterion refers to a research study's conclusions. We develop the notion of "not wrong" further, and suggest that it may be easier to assess the *probability* of a study's conclusions being false (wrong) than to categorize them as being either true or false with certainty. Moreover, we argue that it is important to take into account the *aims of a research project* (estimating effect sizes of well-known effects versus testing new theory) in assessing the probability of its conclusions being false given the varying needs of conceptual and methodological rigor for research projects with differing aims as discussed earlier. Based on the preceding discussion, we suggest that a sound evaluation of a research project calls for making a tradeoff between (1) the importance of the issue investigated in a study and (2) the *probability* of the study's conclusions being false *given the aims of the research.*[13] Doing this would more accurately reflect a study's "research value" and the extent to which it merits publication.

It may seem paradoxical that we advocate trading off probability of false conclusions for issue importance. One might argue instead that the more important the issue, the higher the stakes, and hence the higher the confidence one should like to have in the research conclusions. To the contrary, we note that *not* making this trade-off has the perverse effect of the more important issues being ignored altogether by marketing scholars (i.e., not studied). Furthermore, making a trade-off does not imply that research on important issues should be published even if its conclusions are likely to be false; rather, it implies that research on important issues should be publishable as long as the probability of its conclusions being false is not high (i.e., they are likely to be true) even if its methodological rigor is not at the latest state-of-the-art.

When a substantive topic/area is relatively new or under-researched, there is a greater need for research on issues related to the topic, and as such "Issue Importance" is likely to be high. Several scholars argue that when a paper proposes interesting new theory, as may be the case for new or under-researched topics, the standards used to evaluate its empirical work should be relaxed, not strengthened (e.g., Sutton & Staw, 1995). This is consistent with the idea that it is in the best interests of a discipline to accept a somewhat higher probability of false conclusions for such research compared with the more extensively researched topics/issues. As knowledge about the topic builds up, the issues relating to the topic/area tend to become more structured and smaller in scope and importance. This in turn, calls for accepting a lower probability of false conclusions for publication purposes. Hence, the relative importance of these two components may vary over time, depending on the state of maturity of the specific research area/question.

4. Motivation to study important marketing issues

The greater the *intrinsic* satisfaction a scholar derives out of working on important issues, the greater is the probability of his/her choosing those issues for study. Similarly, the greater the *extrinsic* rewards a scholar expects from working on important issues, the greater is the probability of his/her choosing to work on those issues. Current practices in most schools and in the discipline, however, lead to lower intrinsic as well as extrinsic motivations to study important issues. We discuss three such practices – the practice of identifying ourselves as behavioral, quantitative or strategy scholars, hiring practices, and promotion and tenure practices.

4.1. Self-identities of marketing scholars

The field of marketing is widely viewed as divided into three sub-fields, loosely labeled behavioral, quantitative and strategy (MacInnis, 2005). Scholars tend to identify themselves as interested in behavioral, quantitative or strategy issues, which has a subtle but powerful effect on their interests and motivations. Scholars who identify themselves as behavioral researchers tend to be interested in learning about new developments in psychology or sociology (the so-called "parent" or "basic" disciplines). Similarly, scholars who identify themselves as quantitative researchers tend to be interested in learning about new developments in statistics, economics and econometrics. Strategy scholars tend to be interested in learning about developments in the management and organizational psychology/sociology disciplines.

While these broad self-identities ("behavioral," "quantitative," or "strategy") encourage the desirable practice of learning from other disciplines, they can lead to a primary identification with other disciplines and reduce marketing scholars' interest in learning about issues related to phenomena that are uniquely or largely marketing (e.g., branding, positioning, channels, segmentation, customer experience, delivering customer solutions, developing hybrid products, customer relationship management, and developing marketing doctrines). This hurts marketing scholars' ability to generate new insights about marketing phenomena because the process to do so requires *deep and sustained immersion* into the phenomena. As Kohli (2017) argues, such immersion and reflection leads to the generation of various conjectures, their critical evaluation and rejection, generation of more conjectures, and so on, until a set of plausible novel insights emerge. Arguably, the most innovative thinking stems from a deep immersion in one area combined with some knowledge of other areas. (There is some risk, of course, that if marketing scholars become immersed

[13] It may be possible to combine these two components into a numeric summary index of the extent to which a study merits publication. For example, one could score issue importance on a scale from 1 to 7 and divide it by the probability of false conclusions (ranging, say, from 0.01–1). This would result in a publishability index with a range of 1–700. The higher the index score, the greater the case for publishing the research. Marketing journals could include this index as a part of review reports, and ask reviewers to explicitly consider its components. Over time and with data from across reviewers, norms are likely to develop for index scores of different types of research papers, and help make decisions about whether to publish a paper being reviewed.

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exclusively in marketing phenomena they could end up ignoring ideas in other disciplines to the marketing discipline's detriment. Given where we stand today, however, the risk of this happening any time soon is very small).

Moreover, when a scholar's self-identity is not related to a marketing phenomenon (but to a general "behavioral" or "quantitative" or "strategy" orientation), s/he may come to think of the "parent" or "basic" disciplines as having a higher status than Marketing (see Gunther McGrath, 2007). This would make immersion in a marketing phenomenon less fulfilling, reduce the scholar's intrinsic motivation to engage in the phenomenon at a deep level, resulting in lower likelihood of the scholar generating deep insights about the phenomenon.

We recommend that marketing scholars develop self-identities that focus on specific marketing phenomena (e.g., marketing channels, segmentation, branding, pricing, customer relationship management, and so on) rather than the overly broad categories of behavioral, quantitative and strategy. Doing so is likely to not only increase their awareness of important issues pertaining to those phenomena, but also their intrinsic motivation to develop new insights into those issues as well as their ability to do so. A number of successful marketing scholars have developed self-identities associated with marketing phenomena. For example, Marnik Dekimpe and Inge Geyskens are identified with retailing and channels, Kevin Keller with branding, V. Kumar with customer lifetime value, Don Lehmann with valuing/ managing marketing assets (among others), Kent Monroe with pricing, Werner Reinartz with customer relationship management, Jan-Benedict Steenkamp with global marketing, Louis Stern with channels, Berend Wierenga with marketing management support systems, and Valarie Zeithaml with service quality. Such examples, however, are few and far between. More scholars in the field, including junior scholars, could benefit by developing substantive self-identities.

4.2. Hiring, reward/recognition criteria, and individual research strategies

In recent years, many schools have increased the proportion of hires from PhD programs in psychology, economics and statistics (compared to hires from marketing doctoral programs). While many such hires are interested in marketing, others have little intrinsic interest in learning about or studying issues of importance to marketing stakeholders, and some actually prefer to continue to primarily identify with their previous disciplines. This reduces the probability of their choosing to study important marketing issues. We recommend screening faculty (and doctoral) candidates for intrinsic interest in marketing phenomena, especially for hires from non-marketing doctoral programs. This is a critical ingredient for increasing the likelihood of marketing scholars choosing important marketing issues for study. Importantly, intrinsic interest should not be based on what candidates say or the journals in which they publish, but rather on the actual topics and issues on which they work.

While administrators and promotion & tenure committees profess allegiance to a faculty candidate's research impact/importance rather than the mere number of publications, actual practices are unclear, and probably represent a mixed bag. It is safe to say that many faculty members believe that in the end it is the *number* of publications that explains raises, promotions, and tenure. This results in a lower probability of their choosing important marketing problems for study, primarily because it represents a more rational trade-off from their standpoint given the greater time, cost and effort it takes in general to complete projects addressing more important issues.

We urge a shift on part of administrators and promotion & tenure committees towards evaluating scholarly productivity in terms of importance of research issues addressed rather than merely the number of publications. While this idea is often repeated, it is unclear that it is always heeded. Perhaps this is because it is difficult to judge the importance of a piece of research. To that end, we suggest using the three criteria for assessing "Issue Importance" developed in this research. We submit that if faculty members are aware that administrators and promotion & tenure committees (and reviewers) use these criteria for assessing research, they are more likely to choose more important marketing problems for study. Relatedly, promotion & tenure committees and others should evaluate the importance of the work rather than the stature of the journal in which it is published.

A simple way to encourage administrators and promotion & tenure committees to focus more on the importance of the research done by a scholar is to limit the number of papers they consider for promotion and tenure decisions to 1–2 papers at each promotion stage (e.g., assistant to associate professor, associate to full professor).[14] The general idea of placing a maximum on the number of papers that a candidate may submit is also raised by Harmon (2006), Gunther McGrath (2007) and Stremersch and Winer (2018). It is likely to serve several purposes. It would encourage scholars to identify important questions/issues, and provide them more time to do in-depth and rigorous research on those issues (because they would need to develop fewer papers). It would also reduce the reviewing burden on administrators and promotion & tenure committees. In addition, it would reduce the burden on external evaluators. Scholars could still work on less important issues and publish them for realizing other benefits such as external visibility, and those outputs could still be considered for general (e.g., annual) performance reviews, but not for promotion purposes.

We recognize that there may be instances in which exceptions may need to be made for scholars engaged in programmatic research on a topic, publishing multiple papers with small contributions that add up to a strong contribution. However, in such instances the scholars should be asked to show how their "small" papers build on each other and cumulatively make an important contribution.

Finally, even if administrators and promotion & tenure committees fail to always evaluate faculty research based on its importance, we recommend individual researchers pick at least one truly important marketing issue to work on along with other

[14] A school interested in doing this probably would need to gradually phase in this practice over several years so as to give sufficient lead time to all involved – faculty, and committees at the college and university levels.

potentially less important issues. This “portfolio strategy” will balance out the greater effort, time and risk of the more important project with other more “bread and butter” projects. This would reduce the overall risk but allow for high returns as and when the high-risk project bears fruit.

5. Conclusion

Spurred by the rising concern that an increasing proportion of research in marketing is not particularly useful, we undertake a systematic examination of its likely causes and offer ideas for arresting the trend. We suggest that assessing the usefulness of potential or finished projects in terms of their “relevance” rather than “importance” may lead to inflated evaluations of a project's usefulness. We argue that researchers are likely to address important marketing issues if they (1) are aware of them, (2) have the conceptual, methodological, and trade-off abilities for addressing them, and (3) have the intrinsic and extrinsic motivation to address them. In multi-researcher teams these three qualities may be distributed unevenly across the team members, but need to be present.[15] We identify specific variables in each of these categories that influence the likelihood of scholars studying important issues. It is our hope that the framework helps identify additional specific variables that influence the likelihood of marketing scholars studying important issues.

Conducting and publishing research on important marketing issues not only is inherently desirable but also enhances the stature of the marketing discipline. Table 1 includes a summary of our recommendations to that end. We hope our observations in this paper stimulate further discussion, and our recommendations lead to concrete action towards increasing the proportion of published papers that address important issues in marketing.

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[15] When paper(s) being evaluated by a promotion & tenure committee are co-authored by a candidate and several other researchers, a separate assessment of the candidate's contribution to the paper(s) may be needed. This may require, for example, asking the candidate and co-authors for the specific contributions made by the candidate to the paper(s).

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