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The Study of Important Marketing Issues: Reflections

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Abstract
Kohli and Haenlein (2020) should be praised for tackling an issue
foundational to our purpose: How can academics in marketing study more
important problems? The motivation for this paper is not new. For years
indeed, scholars have deliberated whether the glass is half-full or half-
empty (see, Lilien 2011; Reibstein et al. 2009; Roberts et al. 2014a &
2014b (including commentaries by Lehmann 2014 and Winer 2014); Stremersch
et al. 2007; Stremersch and Winer 2019; Stremersch, Winer and Camacho
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On the one hand, there are clear signs that our research matters. Roberts,
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academic importance and practice importance. They found that some
marketing papers make important contributions on important questions (the
half-full glass). On the other hand, Roberts et al. (2014a) also found
that academic marketing papers only do so on rather rare occasions (the
half-empty glass). Thus, we should aim to raise the bar (or the water-air
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advocate and they do so at the right time.

Keywords: Academic Research; Impact; Marketing Academia; Marketing
Science; Marketing Thought; Marketing Domain

Introduction

Kohli and Haenlein (2020) should be praised for tackling an issue foundational to our purpose: How can academics in marketing study more important problems? The motivation for this paper is not new. For years indeed, scholars have deliberated whether the glass is half-full or half-empty (see, Lilien 2011; Reibstein et al. 2009; Roberts et al. 2014a & 2014b (including commentaries by Lehmann 2014 and Winer 2014); Stremersch et al. 2007; Stremersch and Winer 2019; Stremersch, Winer and Camacho 2020).

On the one hand, there are clear signs that our research matters. Roberts, et al. (2014a) made a list of 100 high impact papers, and ranked them on academic importance and practice importance. They found that some marketing papers make important contributions on important questions (the half-full glass). On the other hand, Roberts et al. (2014a) also found that academic marketing papers only do so on rather rare occasions (the half-empty glass). Thus, we should aim to raise the bar (or the water-air ratio, if you will), which is exactly what Kohli and Haenlein (2020) advocate and they do so at the right time.

Importance and Relevance: From Science Puzzles and Applied Science to Thought Leadership

An attractive component of Kohli and Haenlein (2020) is the tension they describe between importance and relevance and their suggestion for future research to lean more to importance than to relevance. They also provide a great check list on importance by conceiving it as the magnitude of change a new insight can bring among a large number of high-status stakeholders. Indeed, too much of the academic research in marketing brings small changes in behavior to few not so impactful stakeholders. In other words, scholars in our discipline study too rarely strategic issues that may guide behavior at the boardroom level and too frequently tactical issues that may guide tactics of rather junior managers down the company hierarchy; if they study issues relevant to practice at all.

This tension becomes clearer if, borrowing and adapting from Stokes (1997) and Tushman and O'Reilly (2007), I divide research studies in a two-by-two as in Figure 1 below, along importance – so elegantly defined by Kohli and Haenlein (2020) – and managerial relevance, which is well defined by Jaworski (2011) as "the degree to which a specific manager in an organization [this term may include other institutions beyond companies, such as public policy] perceives academic knowledge to aid his or her job-related thoughts or actions in the pursuit of organizational goals" (p.212).

The contrast of both dimensions leads to four types of research projects that could end up in a publication in a major journal such as *IJRM*, *Journal of Marketing* or *Journal of Marketing Research* (for second-tier journals this typology may look a little different):

1. *Thought Leadership*: A research study that can alter big decisions or the worldview of a large number of senior managers (if they would sufficiently be exposed to the study's content). To craft this study, a research team needs to evoke strong skills in conceptualization, research design and research execution, as well as deep socialization with practice, preferably at a senior level.
2. *Applied Science*: A research study that can alter small decisions of a select group of relatively junior managers. To craft this study, a researcher needs to be able to incrementally extend knowledge, typically by knowing the vested literature and empirical techniques very well in the area s/he seeks to contribute to, as well as contacts with junior managers at least occasionally.
3. *Science Leadership*: A research study that can alter the practices and worldview of a large number of senior scientists across many fields, such as senior analysts and professors. To craft this study, a research team needs to have strong methodological skills or substantive knowledge that surpass the own field (e.g., in psychology, economics, statistics, etc.).
4. *Puzzling Science*: A research study that solves a puzzle and of which one (often readers, occasionally honest authors) is puzzled what its value is. It adds incrementally to what is known, but does not alter anyone's decision-making. To craft this study, a research team needs to have state-of-the art methodological and substantive skills in a narrow field and be competent at paper positioning and reviewer diplomacy.

Figure 1: Importance and Relevance

There are quite a few examples of Science Leadership where marketing professors have made big contributions and these seem not to be in decline, such as eye tracking, fMRI, Bayesian estimation, or endogeneity corrections. Also, some of these science leadership examples will afterwards turn into thought leadership, as practical applications that may not have been present at start become apparent after some time (similar to the invention of unbreakable glass that remained without main application until iPhone or Liquid Crystals that only found their mass application in displays, such as flatscreen TV's and mobile devices).

Our main deficit in the field is indeed possibly in the Thought Leadership cell. In my mind, our main challenge as a community of scholars – this may be different though for the organizations, such as business schools that employ marketing scholars (see Stremersch and Winer 2019; Stremersch, Winer and Camacho 2020) – is not necessarily to destroy the rise we have seen in Science Leadership. Rather it should be to drive some of the Applied Science and Puzzling Science and the resources they use into the direction of Thought Leadership. Such transformation would already really move the needle very significantly.

Ability and Motivation Along the Project Funnel

Kohli and Haenlein (2020) propose several ways in which such transformation could be accomplished, extending the classic (see, e.g., Martin, Seta and Crelia 1990) ability – motivation framework to include awareness. I believe this embeds an important insight, namely that undertaking more important research does not only lie in the ability and motivation to undertake it, but also the project selection that is made, which is a process that indeed starts with awareness. Extending on this basic idea, one could configure a researcher's challenge to undertake important research over an idealized "project funnel" (see Figure 2). Such project funnel takes the researcher through four main phases (there are

many variations one could make with additional phases, subphases, etc., which I leave to personal preferences): (1) awareness, (2) consideration, (3) choice, (4) execution.

The *awareness phase* is a phase of immersion. Scholars search widely for information and should immerse themselves thoroughly in their topic of inquiry. They remain open-minded and, for sake of importance, try to identify key challenges that practice or science face and that meet the importance criteria one has defined (as per above).

The *consideration phase* is a phase where the researcher has a couple of project options on the table. This can include different substantive questions to be addressed in a domain or different research designs for one substantive question. In this phase, the researcher also validates some of her initial assumptions, such as gaps in the literature, magnitude of the problem, or feasibility of the research. This phase may also include social feedback, such as advisor-student conversations, research team divergent deep dives, or feedback by entrusted colleagues.

The *choice phase* is a phase where the researcher or research team decides on the project scope, in terms of substantive questions and research design. Preferably this is seen as a choice between several options, rather than a go-no go decision on one option. And preferably, the research team chooses the important project over the less important project, accounting for downsides in risk and execution horizon, among others.

The *execution phase* is a phase where the researcher executes on the chosen research project. During execution, new challenges typically pop up that generate again an iterative awareness-consideration-choice-execution cycle. Each of these iterations again is a balancing act between pros and cons, ultimately hopefully tipping in favor of importance.

To steer research to importance, it requires *ability and motivation* to do so, in *each of the four phases*. For instance, being able and willing to liaison with senior-level practice may be a driver of the awareness of important research topics. A scholar's project ideation skills may determine how many important research projects end up in the consideration set. In the choice phase, scholars with a greater incentive to do important work – rather than "bean farming"[2] – will more likely retain important projects they evaluated during consideration as the ultimate project. And scholars with greater execution ability will more easily overcome challenges in the execution of the project without endangering the importance of their work (e.g., they will be able to avoid incrementalization of their ideas in response to reviewer demands for greater rigor).

These just serve as a couple of examples. One could, if more space were devoted, develop a list of such factors for each stage. A lack of ability or motivation at each of these phases may lead to the regrettable conversion of a "potentially important paper" to an "actually less important paper". An interesting empirical question is in which phase of the funnel such conversion is most likely to occur and for which reasons.

Figure 2: Ability and Motivation Along the Project Funnel

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[2] With "bean-farming", I refer to the production by some scholars of just as many papers as possible regardless of quality, importance, creativity or involvement, as an analogy to "bean counting", which is the practice of merely counting the number of papers regardless of quality, importance, creativity or involvement.

Leaps to a Greater Importance

Kohli and Haenlein (2020) also have some good suggestions on how we could address this challenge and accomplish more thought leadership. I subscribe to a fair amount of what they say and what they say has inspired the following further thoughts in my mind.

Socializing with Practice

To achieve thought leadership in a certain area socializing with practice is fundamental (as suggested and demonstrated empirically in Roberts et al., 2014a). But there are some side notes, as follows.

1. *Learning by not doing is difficult*. Sometimes suggestions to socialize with practice include: (1) consult sources such as MSI research priorities (Desai et al. 2012); (2) do some executive education (Tushman et al. 2007; Vermeulen 2007); (3) 2-week practice immersions or externships (Wiegand et al. 2020). And these are great suggestions, but are they really enough? Somehow, it makes us believe we can do this in a cheap, time-efficient, way; that we can learn by "not-doing". Personally, I believe we need to "do-more". Or as Vermeulen (2007) calls it we need to "pay the price".

    Some of my most rewarding consulting work has included collaborations with companies over multiple years or deep immersions into specific industries. Roberts et al. (2014a) cites anecdotal evidence from scholars such as Rick Staelin and Jordan Louviere along the same lines. In many occasions, these engagements gave me a good sense of how companies work and what they find useful. Thus, while of course company engagements can give you a research idea, I would say they are even more useful to educate you as a scholar on how companies operate and what their challenges are.

    But be thorough about it!! When you want to learn about boxing, getting into the ring is more informative than reading about it. Or when you study gorillas, you can't do it by being one nor by just reading about them, you'll have to get into the mountains now and then and "smell the beast" (Vermeulen 2007). This deep immersion is common in many fields. A theology colleague once shared with me some details on *Gregorianum* – the leading theology journal that is "on the bed stand of the pope" (my theology colleague is a frequent guest of the Vatican). The editorial board consists of leading theology scholars, but when the Vatican takes a long Summer off, they all return (or immerse) to their clergy duties in their original parishes.

2. *Seniority rules*. Quite a few papers link socializing with practice to the doctoral program (e.g., Lehmann et al. 2011). And that is good. Indeed, doctoral programs need to ensure students understand what a company looks like and how managers in the respective area of study think. At the same time, doctoral programs should train core research skills and accept students from foundational disciplines to marketing (such as psychology and economics). The reason is simple. It is easier to learn about marketing theory than it is to learn about economics, statistics or psychology. The more foundational the discipline the earlier one best lays the foundations.

    Moreover, I wonder, if one considers importance as defined above, to what extent C-level functions of multinational companies want to talk to doctoral students – regardless of their training and origin. Thus, senior scholars need to ensure socialization with practice and onboard junior scholars with strong research skills as propagated in Roberts et al. (2014a).

3. *Not all are created equal*. As scholars, we do not all need to be alike. In fact, diversity has on many occasions shown to be the most fertile ground on which science can develop (see, for an example, Steele and Stier 2000). Like stated earlier, we have some great economists, statisticians and psychologists in our midst. We should continue to welcome and nurture them. That being said, I do worry about balance. We need to ensure we still are enough about marketing rather than seeing marketing merely as an application field.

Embracing Residual Ambiguity

Residual ambiguity is a very vivid concept in medical sciences. Clinical testing of new drugs typically includes a final pre-approval phase (phase 3) of 1,000-10,000 patients (in some cases, e.g. vaccines, phase 3 testing may include several tens of thousands of subjects). Thus, any adverse effects that materialize in less than 1/10,000 patients are likely to remain undetected in pre-approval clinical studies, but may become apparent once the therapeutic intervention – think of a Corona virus vaccine – is administered to millions of patients, even with the best scientific methods employed. This is residual ambiguity.

Some scientific fields, just like some therapeutic categories, face more residual ambiguity than others. "Hard" sciences typically face less residual ambiguity than "soft" sciences. Or, in some sciences it is easier to demonstrate a fact with certainty than in others. The harder the science, the more befitting the scientific method is and the more definitive findings can be. Bennis and O'Toole (2005) question the applicability of the science method to management, which is inherently a (very) soft science. Within management, fields such as finance are substantially "harder" than marketing or management. And research that addresses the most difficult questions managers may have, likely also faces the most residual ambiguity.

This original view of Bennis and O'Toole (2005) implied that there would be a tradeoff between rigor and relevance, in a similar vein as Kohli and Haenlein (2020) hint at a tradeoff between rigor and importance. A discordant view on this has been developed by Vermeulen (2005) and Gulati (2007), who (1) argue that research that is not rigorous cannot be relevant, (2) recommend to accept that rigor and relevance are not opposites and (3) recognize that lots of opportunity exists for conducting research that is both rigorous and relevant.

The following practices may help us in becoming better at embracing residual ambiguity, which is, without doubt, needed when studying important problems:

1. *Don't just do "enough", do the best you possibly can*. Studying messy, ambiguous, ill-structured problems cannot be an excuse for sloppiness. Sometimes, self-reported survey research will be the best we can do, but then we should do it in the best way it can possibly be done. Sometimes, corrections for endogeneity are not impossible, but just very difficult and time-consuming, but that in itself is not a good reason not to implement them. So rather than recommending editors to tradeoff importance with rigor, I feel editors should evaluate whether given the research question, the researchers have done the best that one possibly can do. I.e., did they maximize on rigor given the question they had? (which of course is different than only studying questions one can rigorously study). Or, also, did the authors do the best they can given the stage of evolution of the field they aim to contribute to, given high rigor is typically easier in mature fields as compared to nascent fields?
2. *Better define the limits of the residual ambiguity*. All studies have boundaries, including important studies. As a field challenged by research integrity and reproducibility allegations, we

probably can do better in better defining the boundaries of where reasonable certainty ends and where residual ambiguity enters. As long as these boundaries are well defined in a paper, I do not see why important papers would be disadvantaged.

3. *Embrace ambiguity, you will not stand alone*. I believe the editorial processes in several marketing journals are embracing residual ambiguity (at least for the journals, I can most easily evaluate this for such as *International Journal of Research in Marketing*, *Journal of Marketing (JM), Journal of Marketing Research (JMR)* and *Marketing Science*). In fact, I have never witnessed a potentially important paper that was not wrong (the "Lehmann test") being rejected in the first round. I do think there are too few "important" submissions, despite the odds of acceptance being in favor, rather than against, important work.

Do Not Get Boring or Bored From Hyperspecialization

In response to the criticized "balkanization" of our field in behavioral, strategy and modeling (Reibstein et al. 2009), Kohli and Haenlein (2020) recommend scholars to develop self-identities with a specific marketing phenomenon, based on the belief that deep immersion pays off more than a broad view across disciplines. This recommendation for scholars to narrow and "balkanize" even more in smaller fields (i.e., marketing domains) requires a deeper analysis.

Innovation literature finds that diversity – not hyperspecialization – is key to innovation, both radical and incremental, for quite a few reasons (for "a primer" list of reasons and empirical evidence, see Wuyts et al. 2004). Scientometric literature in marketing promotes diversity (as in the diversity of disciplines a paper builds upon) for three reasons: (1) it provides a richer understanding of a marketing phenomenon; (2) it encourages original thinking that could foster a new paradigm; and (3) it stimulates scientific progress (Stremersch and Verhoef 2005; Tellis et al. 1999). Scientometric literature outside marketing demonstrates that important papers often arise at the intersection of previously unconnected, but well established, fields (Uzzi et al. 2013). This view underlies the widely influential theory of structural holes (Burt 1995). It seems that the innovation and scientometric literature advise us to become broader, boundary-spanning and less locked up into ghettos, rather than more as proposed by Kohli and Haenlein (2020).

The assumption that Kohli and Haenlein (2020) make that marketing is not at great risk of ignoring or being ignored by other disciplines is unsupported by data. In fact, the Article Influence Scores (AIS) of marketing journals are typically lower relatively speaking than those of other business disciplines such as finance or management. The AIS corrects Journal Impact Factors for the weight of a citing journal. The more important the journals that cite marketing journals the higher the AIS. In sum, marketing journals (AIS of JM = 2.64) are cited by less important journals than finance (AIS of Journal of Finance = 7.46) or management (AIS of Academy of Management Journal = 5.61) journals. Thus, ignoring the risk that marketing scholarship may be irrelevant to all other sciences but itself, seems imprudent.

Beyond data, also the cases that Kohli and Haenlein (2020) mention, do not seem to fit well. Jan-Benedict Steenkamp cannot be bucketed, he is the multi-domain specialist "par excellence". He mastered diverse fields such as, not only global marketing, but also retailing, branding, structural equation modeling, channels, to name just a few. Where would one even begin to list all of Don Lehmann's specialties. I had the company of Don Lehmann as my co-editor at IJRM (2006-2009). We used to share with one another what we saw as the consequence of immersion in only one subfield of marketing: "You get bored or boring".

The ability argument also does not stick here. Of course, diversity comes at a higher cost (see Wuyts et al. 2004). Thus, it is very efficient to focus on one marketing phenomenon but it would stifle innovation. Therefore, I think for most of us it is better to maybe publish a bit less (i.e., be less efficient in our research), but to publish something that is more important or innovative. And if Marie Curie can win two Nobel Prizes in two different sciences (Physics and Chemistry), I am confident that (on average, less brilliant) marketing scholars can still master two or more marketing phenomena or domains. Thus, I feel the recommendation of Kohli and Haenlein (2020) is best accompanied by a cautionary note that scholars should try to develop multiple self-identities at the same time, combining the best of both worlds – immersion and diversity.

Another way to still view this problem is the demise of strategy in the marketing triangle – strategy, modeling, behavioral – which has impoverished our field. It is fine for modeling and behavior to define itself based on different theoretical (economics versus psychology) and methodological (statistics versus experimental data) primitives. In fact, it makes the breakthroughs made in this field within marketing more fundamental and more appealing to foundational disciplines. But, with the demise of marketing strategy as a field, the bridging function it fulfilled has crumbled. Thus, another take beyond multi-specialty identities could be to re-ignite marketing strategy as a subfield of marketing with strong interconnections with behavioral research or modeling. This befits the rise of general management supported by strong functional expertise as an orientation in many leading business schools.

Envoy

At the end of their paper, Kohli and Haenlein (2020) express the hope that their framework is helpful for scholars to study important issues. I believe they, no doubt, succeeded in that. Of course, academics never agree and I recommend scholars to take away their bits and pieces from it and freely alter it. I see the search for important papers as a personal learning journey for all of us, one on which we can continuously improve. Kohli and Haenlein (2020) provide an excellent base from which to self-assess your own trajectory.

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	Not so managerially relevant	Managerially relevant
Important	Science Leadership *e.g., How to correct for endogeneity of managerial decisions?*	Thought Leadership *e.g., How to make an innovation portfolio more customer centric?*
Not so important	Puzzling Science *e.g., How to remain diplomatic when creating a typology?*	Applied science *e.g., How to optimize a Facebook campaign for an FMCG brand?*