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The Bible of publishing: a perspective inspired by Don Lehmann

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ABSTRACT

What constitutes good research, and how should it be evaluated and communicated in scholarly publishing? Drawing on nearly 25 years of editorial experience at leading marketing journals, we develop a unified perspective on the research and publication process, inspired by the legacy of Don Lehmann. We conceptualize good research as work that is interesting, important, and not wrong, and examine how trade-offs among these dimensions shape scholarly output.

We contend that prevailing publication norms often overemphasize methodological sophistication and formal metrics, at the expense of interestingness and importance. In contrast, impactful research prioritizes meaningful questions, aligns methods with the problem, and communicates insights clearly.

We want to stimulate a reorientation toward research that truly advances knowledge and practice. For researchers, we provide actionable guidance on how to craft work that is both publishable and influential. For editors and reviewers, we offer principles for decision-making and reducing type II errors—rejecting potentially high-impact work. For institutions, we challenge the reliance on imperfect metrics and call for better evaluation systems. Ultimately, we argue that the future of marketing scholarship depends not on maximizing publication counts, but on generating insights that are worth knowing, robust enough to trust, and consequential enough to matter.

1. Introduction

We represent collectively almost 25 years of editor-in-chief experience at *IJRM*. We also fulfill and/or have fulfilled VP of Publications, Editor, Guest Editor, Area Editor and Editorial Review Board duties at other leading journals in marketing, beyond *IJRM*, such as *Journal of Marketing*, *Journal of Marketing Research, Marketing Science,* and *Journal of Consumer Research.* From these front row seats,

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we have participated in and observed the publication process from many different angles. In this paper, we wish to share what we have learnt along the way.

We do so inspired by the memory of Don Lehmann, former co-editor-in-chief of IJRM (with Stefan Stremersch; 2006–2009), whose passing on December 23, 2025, prompted the writing of this paper. Don was a deeply influential scholar in our community. Despite his many honors, he wore his achievements lightly. Quite literally, guests that stayed in his home could find all his awards stored in boxes under the guest room bed. Don was also known to most, especially outside academia, as "Coach". He intensely committed to football coaching, as well as to coaching many of us in academia. This paper aims to codify publishing guidelines that we collectively learned over time that we feel are consistent with how Don saw the publishing world.

Our intent is to provide one collective and substantiated viewpoint on scholarly publishing, accepting that there are many others as well, without claiming our viewpoint as being the superior one. We believe offering that viewpoint can be valuable for multiple stakeholders.

First and foremost, for young scholars, such as PHD students, postdocs and assistant professors, publishing makes up a big chunk of their time allocation with big consequences on the career options they have and the bigger impact on the world they will or will not make. However, they navigate an ill-charted research and publication journey with distorted and contradictory signals. Our contribution to young scholars is to educate and inform them in a coherent manner so they can make more informed decisions.

Second, experienced faculty members may wonder about alternate views on publishing and in which considerations such alternative views are grounded. Thus, our paper is meant to challenge currently held beliefs among experienced faculty, which can redirect their task as reviewers, their own development trajectories or the influence they have on others.

Third, all of us have accepted once for the first time an editor-in-chief position. One reason for doing so is that it provides a learning opportunity to oversee the publishing field from an editor's perspective as we are all new in the job at some point and try to figure out how things really work. This paper provides prospective editors a heads up and allows them to come more informed to the start line than we maybe were when we started out.

Fourth, institutional stakeholders monitor, guide and reward (or punish) scholarly outcomes. Think of universities that employ scholars, funding institutions that provide grants, or associations that orchestrate professional and/or scholarly communities. Decision-makers of such institutions are not always fully informed on the publication process or on publishing in marketing specifically. Such lack of information may introduce randomness, or worse, systematic bias, in their guidance of scholars or in their decision-making on compensation (such as hiring, promotion and setting salary and research support). Our attempt is to improve their understanding and decision-making.

The paper is organized as follows. First, we discuss what constitutes good research, as research that is interesting, important, and not wrong. Then, we address how good research can be communicated effectively in well-constructed papers. Third, we examine scholarly careers and collaboration. Fourth, we adopt a scientometric perspective on evaluating scholarship, including the "Lehmann-number". Fifth, we turn to journal management and editorial decision-making. Sixth, we look ahead to the future of scholarship, publishing, and IJRM. We end with a brief afterthought.

2. Good research is research that is interesting, important and not wrong

As scholars, we all aim to produce good research. Lehmann's pragmatic take has guided several editorial teams including the ones at IJRM (= us), by asking three questions about the research that is submitted or presented: (1) Is it interesting? (2) is it important? and (3) is it not wrong? These three questions have guided generations of editors to probe the quality of a paper at IJRM as well as other leading journals.

This logic is a practical application of the distinction between the *q*-quality and the *r*-quality of a paper (see Ellison 2002). Interesting and important research has high *q*-quality, but such *q*-quality may come at the expense of *r*-quality (i.e., rigor). More interesting and bigger questions may be harder to study. At the same time, higher rigor may come at the expense of interestingness (i.e., it is easier to confirm with high reliability what we know than it is to disconfirm what we know) or importance (i.e., it is easier to isolate a small phenomenon enabling rigorous study than to do so with a big phenomenon).

The "not wrong" question labels the rigor dimension as a minimum bar to pass, not a dimension one should maximize on, and shifts the weight in favor of interestingness and importance. This recalibration of weights answers the calls of prior research in favor of importance (Kohli and Haenlein 2020) and interestingness (Stremersch et al. 2021). Next, we dig a bit deeper in each of these three questions, each in turn.

2.1. Is It Interesting?

Interestingness is more than mere novelty. Presenting research that has not yet been done before might be novel but not necessarily interesting. Something that is interesting catches and holds our attention and arouses curiosity (Oxford Dictionary). For example, in discussing the dimensions of strong theories, Carton (2025) describes interestingness as an attribute that draws the reader's interest, conveys an engrossing narrative, and reveals insights that challenge commonly held beliefs.

The latter component of interestingness was well elaborated on in an early landmark paper by Murray Davis (1971), which presents a taxonomy of the different ways in which a research study can challenge commonly held beliefs. Stremersch et al. (2015) examines this Davis' framework empirically in a content analysis of 659 articles from five major marketing journals and clarifies that the positive effects of interestingness are bounded. I.e., there is a positive effect on citation counts the more papers are challenging previously held beliefs, but beyond a certain level of interestingness, papers may see their impact decrease as they are considered absurd, more of an

anomaly, or a fluke impossible to replicate, consequently losing an expert reader's interest and curiosity. Therefore, interestingness can never come at the expense of the search for the truth or it backfires in the short or long term on the respective scholars, articles or institutions, such as journals or universities.

Many editors at IJRM have highlighted the publishing of unexpected insights as the ultimate goal of authors and journals (e.g., Dekimpe, 2009). In a similar vein, Don Lehmann noted in his founding vision for Marketing Letters, already more than three decades ago that "interesting papers will make readers say 'I learned something,' 'it made me think,' and 'I wish I had done this'."

We acknowledge that interestingness is, to some extent, a matter of taste, also when it comes to research. To better grasp the notion of interestingness for authors and reviewers, IJRM recently proposed the following three paper checks as inspiration (Schreier et al. 2021, p. 808):

(1) Would you enthusiastically take the paper to a bachelor's or master's level marketing class, hoping for a vibrant 30-minute discussion?
(2) Would you confidently raise the paper's findings over a joint lunch with a marketing executive or another important marketing stakeholder?
(3) Would you proudly report the gist of the paper during dinner with colleagues from other departments or schools from your university?

If the answer to (one or more of) these questions or questions along similar lines of thought is yes, then the research is likely to be interesting. However, a positive answer to these questions does not necessarily make the research important (e.g., the research may engage only a small specialized audience of which practices may not be much affected) or not wrong (e.g., plenty of behavioral research has shown to be interesting but not replicable and, thus, likely wrong). Therefore, let us turn to these two additional dimensions next, each in turn.

2.2. Is it Important?

Kohli and Haenlein (2020) conceptualize that research is more important as: (1) the number of affected stakeholders increases, (2) the status of affected stakeholders is higher; and (3) the magnitude of expected change in stakeholder behavior is larger. Stakeholders can be managers, but can also be academics, intermediaries (as in Roberts et al. 2014), societal groups, or policy makers.

Think of importance of research to managers as an example. When we refer to the managerial relevance of research, we refer to what Jaworski (2011, p. 212) called "the degree to which a specific manager in an organization perceives academic knowledge to aid his or her job-related thoughts or actions in the pursuit of organizational goals." According to this definition research can investigate a managerially relevant question (e.g., how to optimize a Facebook campaign) that is relevant to few stakeholders, with a low-level decision-power in a firm and with a small expected change in behavior. Stremersch (2020) calls this "applied science" and distinguishes it from "thought leadership", i.e., research that is both managerially relevant and important (e.g. how to become better at radical innovation?). A similar contrast can be identified among different target stakeholders – public policy or consumer advocacy groups – to ascertain that importance of a research study to such stakeholders represents a markedly higher and better bar for publication than its mere relevance.

2.3. Is it not wrong?

In the wake of Don Lehmann, we jointly argue for assessing papers not on 'perfect', or even 'right', but on 'not wrong'. Like the 'not guilty' instead of 'innocent' verdict in law, this more modest framing acknowledges that certainty in social science is elusive as social science is – in the words of behavioral economist John List – muddy or messy. The "not wrong" criterion establishes a minimum threshold: before we invest collective attention in a finding, let alone advise decision makers, we need reasonable confidence that it reflects genuine patterns in the world rather than methodological artifacts, statistical flukes, or confirmation bias.

Don Lehmann would frequently insist researchers also demonstrate that the proposed effect exists in the simplest of model formulations. Often, this meant ordinary least squares (OLS) regression. If your theorized relationship disappears when estimated with OLS but emerges only with a baroque specification involving multiple instruments, obscure functional forms, or elaborate controls, Don would raise one eyebrow, or even two. The complexity, he suspected, might be doing the work rather than the underlying phenomenon.

This insistence on *simplicity* was not anti-methodological. Don published methodologically sophisticated work throughout his career and appreciated technical innovation. Rather, it reflected a particular philosophy about the burden of proof. Complexity should clarify genuine patterns, not create them. The OLS test serves as a kind of Occam's razor for empirical research. The effect should survive the blunt instrument of OLS, with all its limitations but also consistent robustness of directional impact to its (often unrealistic) assumptions. We can then deploy more sophisticated methods to better quantify the effect size and its uncertainty, address endogeneity and unobserved heterogeneity, or estimate causal effects more precisely. But if the effect only emerges from methodological complexity, we should proceed with caution. The finding may be an artifact rather than a discovery.

What, then, does it mean for research to be "*not wrong*"? First, it means the core empirical claims survive basic robustness checks, including estimation with simpler methods. Second, it means researchers have seriously considered and tested alternative explanations rather than cherry-pick specifications that support their preferred narrative. Third, it means the findings are presented with appropriate epistemic humility—acknowledging limitations, boundary conditions, and the provisional nature of empirical knowledge.

2.4. Putting the three dimensions together

While each dimension of good research seems noncontroversial on its own ("more interesting, more important, and/or less wrong" is better, ceteris paribus), it is very hard with one paper to get all points on all three dimensions. There is no such thing as a 'perfect paper'; in fact, most papers that have been influential have been so exactly because they were imperfect and needed follow-on research.

What is complicated is the overall assessment of research as being better or worse, while counterbalancing all three dimensions. Indeed, the three dimensions are likely inversely related to each other. As discussed in greater detail by Carton (2025), for example, interestingness might be negatively related with importance and importance might be negatively related with accuracy.

Many top journals in our field have been criticized for putting accuracy above all else. Even worse is the critique that journals, reviewers or authors have put analytical complexity above all else, using it as a proxy for accuracy (i.e., believing a more complex method delivers a higher accuracy). If this critique is valid, authors have an incentive to pursue research with complex methods in the search for accuracy, possibly at the expense of interestingness and importance. Introducing the notion of "not wrong," eases the burden placed on the third dimension and puts more weight on the first two. Indeed, our research efforts should first be guided by identifying the most interesting and important research questions and only then trying our best to "get the answers on them not wrong."

3. How to write a paper well?

3.1. What should an abstract say?

From a "marketing" perspective, the abstract is the most important part of a paper: (1) It gets the most eyeballs; (2) based on the abstract, readers decide whether to read the rest of the paper; and (3) for anyone reading the paper, it typically is the first paragraph they read and sets the tone and content expectations for the rest of the paper. In today's world, where science blends with social media, influencers on LinkedIn who provide a wider reach for scientific research may especially screen abstracts, rather than papers, to decide what paper is worth it to feature in a post… Yet scholars writing for academic journals often fall victim to the curse of knowledge, crowding their abstracts with specialist vocabulary and complex sentence constructions, thereby obscuring the central insight their work offers to a broader audience, rather than seeing the abstract as a "Netflix trailer".

Don introduced three simple questions that an abstract essentially should answer in a lucid manner:

1. *What have you done?* What theory did you develop, which empirical studies did you run, which models did you develop or implement? What is the phenomenon you study and how did you do it? Be clear, specific and do not use too much expert jargon.
2. *What did you find?* What are your most interesting and important findings? What are the effect sizes? How do they possibly differentiate from the insights prior literature offers or one would expect naively thinking about the phenomenon? Focus on your most important results only and do not clutter the messaging with side stories.
3. *Why does it matter?* Why should anyone care? Who should read your paper, what can they learn from doing so and how can it influence their behavior, decisions or views on the world? What is the magnitude of the impact of the insights you offer on which stakeholders? Also science needs to be sold to an audience. This is where you tightly argue – without much superficial inflation – why which audience should "buy" your research, while staying within the boundaries of what you can claim from a scholarly perspective.

3.2. How to Balance Theory and Findings: Theory-First and Empirics-First

For many so-called substantive papers, the two main sections are the theory and findings sections. A well-written paper strikes a good and honest balance between the two. The theory predicts what findings will show (Theory-First) or conceptualizes ex post what findings have shown (Empirics-First).

Don Lehmann's heritage on this balancing act between Theory-First and Empirics-First is to counsel authors to be pragmatic on the approach chosen and be high quality within the approach adopted. Very few (if any) studies immediately provide the ultimate answer to a research question, especially when that question is sufficiently important. Whether one starts with an established theory that is subsequently tested, refined, and again tested, or starts with the study of an empirical phenomenon that expands or challenges existing literature, the starting point (whether it is theory or data) ultimately becomes less and less important as the cycle is repeated multiple times (Lehmann 2020). Dogmatism about which of these approaches is the optimal one is not a very useful allocation of energy and intelligence.

Beyond pragmatism in terms of research design, what is required of an author is clarity and integrity in honestly setting the stage as TF or EF, steering away from practices such as HARKing (Hypothesizing After Results Are Known or claiming one developed a theory first), or p-hacking (strangling the empirics to fit a presumed theory expectation). We advise authors to always (i) clearly differentiate from the start the approach chosen (Pauwels et al. 2024), (ii) guide readers and reviewers with sound logic why one approach was chosen over the other (Rutherford and Friend 2025), and (iii) stay true to the selected approach not only in how the research is conducted, but also how it is reported (Golder et al. 2023).

In TF-based research, a theory is borrowed (often from psychology for behavioral research and economics for quantitative research), refined or developed after which hypotheses derived from that theory are tested. EF research, in contrast, (i) originates from a real-world marketing phenomenon, (ii) involves obtaining and analyzing data, and (iii) produces valid marketing-relevant insights

without necessarily developing or testing theory (Golder et al. 2023).

A key advantage of EF research is that it is not constrained to fit within a pre-conceived theory. This helps to dodge "convoluted efforts to fit what one is trying to do into a particular theory … while at the same time highlighting something novel and interesting about the research" (Lehmann 2020, p. 30). It also has a natural arc that bends more easily back to real-world implications, and while TF research is often satisfied with establishing the statistical significance and direction of a (causal) effect, EF research regularly aims to also quantify the effect sizes to inform marketing stakeholders about the economic and societal significance of the findings. These advantages of EF has caused it to rapidly gain traction (Pauwels et al. 2024). However, this growing popularity does not come without some caveats.

First, adopting an EF approach should not be an excuse to ignore relevant prior literature: "no research is without some theory at least with respect to what variables will be explored" (Lehmann 2020, p. 229). Empirics First does not mean Empirics Only. EF does not claim that theory has become irrelevant, nor that pure number crunching is sufficient. Such a narrow-minded interpretation of the concept is likely to slow, rather than facilitate, research progress.

Second, in the paradigm debate between both approaches, it is important to keep in mind that neither approach is expected to dominate in all circumstances, and that specific research projects may blend positive features of both. Because of that, the EF versus TF discussion should not be seen as a strict dichotomy, but as a continuum, with variations in the relative weight attached to theory and data/empirics during different stages of the research process. Because of that, there is no need to pledge an unqualified allegiance to either paradigm, but to pragmatically select the most appropriate one in function of the research question and research objectives at hand. Pauwels et al. (2026) suggest to make the theory-first or empirics-first question secondary to selecting the marketing phenomenon, and adopt instead a phenomenon-first approach.

3.3. Methods: The right tool for the research questions at hand

As we saw in the 'not wrong' section, the critical question isn't whether your method is cutting-edge, but whether it's the right tool at the right time for your research questions. As we evaluate submissions in a compensatory fashion, novel and fascinating topics with unique data don't always require methodological innovation. However, every paper must clearly motivate its methodological choices, and methodological choices vary with the problem at hand. If you only have one hammer, everything looks like a nail. Importantly, it is not necessary to always use the most sophisticated possible method. Sometimes less sophisticated methods may communicate more effectively, due to their simplicity and ease of understanding.

Methods also have a time-context. What is appropriate now may not have been in the past or vice versa. The worst defense of a method is that someone (long?) before you has used it and it was published. Maybe the field has moved on and discovered what was once appropriate is not anymore. Thus, your paper needs to convince an audience you chose the right method at the right time for the right problem. Within each chosen method, papers must reflect current best practices and document choices along the way transparently.

Increasingly, we have come to value multi-method studies or triangulation of research results. Don's ask for an OLS replication of results – even when not correct given context – is but a simple example thereof. Bringing in several datasets and methods – the (in) famous multimethod paper (Blanchard et al. 2022) can enhance reliability as well as advance understanding on the same phenomenon along distinct dimensions.

In sum, methodological excellence emerges when papers match research questions with appropriate techniques, execute those techniques following current best practices, document choices transparently, and triangulate across multiple methods to build more robust and ecologically valid insights.

3.4. Discussion: Future research and practice

Don used to say "Don't take yourself too seriously, we are not researching cancer!" but he made it a point to stress that a paper needs to translate results into a change – an impact. The discussion section is where an author makes this change tangible for an audience. It is useful to organize the Discussion section to answer three questions. Who should care? What should they do or change? And what happens if they do so?

3.4.1. Who are key stakeholders and audiences?

Who is the customer (i.e., buyer) or consumer (i.e., user) of your research? Most marketing papers have at least two primary audiences, researchers and practitioners/managers. But they could include others like policy makers and regulators, who care about consumer welfare and firm accountability, or investors, boards and senior leadership especially in papers focusing on marketing-finance interface, or platform designers focusing on online marketing, and so on. Researchers may care about a phenomenon that is being researched, but highlighting the other audiences provides a signal that the paper is useful for making decisions. It is also useful to highlight the boundary conditions – under what conditions the paper's results are useful for making decisions.

3.4.2. What should stakeholders change?

For researchers, the change is generally conceptual or phenomenon-based. The discussion section could make researchers (1) think about new perspectives for a conceptual relationship, (2) better understand moderators or boundary conditions, (3) teach a new methodology that could be useful in a marketing context, and so on. In general, the discussion should trigger new thinking and lead to new research as the focal paper may not be to be the last word on any topic.

For practitioners and managers, the change one expects to see is often behavioral. "How does the research change what managers do currently?" is a question that is generally asked in evaluating the managerial implications of a paper. For example, the paper could highlight how the current managerial practices are wrong, either due to wrong assumptions or wrong implementations, and why it fails, based on the mechanism the paper identifies.

For policy makers, the discussion of the results implications can highlight the unintended consequences of an existing policy, implications for consumer welfare that could imply a different policy either directed at firms or governmental regulations, etc. For example, the results of an analysis could imply new or different privacy or competition regulations.

3.4.3. What will be the outcome if stakeholders change?

The discussion needs to clearly explain what the changes in outcomes would be when stakeholders would adopt the implications of the research. For researchers, it could trigger new research ideas enabling more important, more interesting or more accurate research findings. For managers, the outcome could be improved performance in terms of higher revenues, profits, or customer value. For policy makers, the discussion could highlight improved consumer welfare or better regulation.

3.5. Replicability and the purpose of (Web) Appendices

Papers need to offer enough information to facilitate replication of the reported research. Claims gain epistemic standing not only because they were observed once, but because they can be observed time and again, ideally by independent researchers, using transparent methods and data practices. To facilitate replication, authors should disclose all information about their studies in the paper or in web appendices. Think of all experimental stimuli for behavioral studies, software code for quantitative models or standard errors on empirical estimates. Such vital details facilitate replication or meta-analysis – a special interest area of Don Lehmann who fondly promoted replication and generalization. For instance, if scholars do not report standard errors on their estimates, meta-analyses cannot effectively control for meta-analytic bias, such as publication bias (Korkames et al. 2026).

Journals need to discipline authors in providing said details and stimulate replication. In 2012, IJRM launched a replication corner to foster a replication culture. Inspired by Don Lehmann, IJRM editors Jacob Goldenberg and Eitan Muller believed that if performing replications will become routine, it would help in establishing more reliability, understanding better the real effect sizes and their boundaries. Don Lehmann was also one of the corner's editors. The concept was simple: any researcher can submit a replication plan to the corner explaining the importance of the phenomenon that is supposed to be tested, along with the design of the replication study. If approved the researcher performs the research and any result is accepted and a publication is guaranteed.

From the experience of IJRM with the replication corner and other replication efforts, it is clear replications offer the following advantages: (1) credibility and error detection (i.e., replications help identify false positives, analytic mistakes, and overfitted results), (2) cumulative knowledge and theory building (i.e., replications accumulate empirical regularities and establish boundary conditions), (3) methodological improvement (i.e., replication efforts create incentives for better measurement, clearer reporting standards, and more transparent workflows on stimuli, data and code), (4) training ground (i.e., a replication offers a useful training for less experienced scholars including PhD students).

There exist different types of replications: (1) direct replications (i.e., a reproduction of the original study (same operationalization, procedures, and analysis plan), (2) conceptual replications (i.e., testing the same underlying hypothesis using different operationalizations, measures, manipulations, or contexts), (3) replications with extensions (i.e., combining a replication with planned variations, such as additional moderators, populations, or outcomes), (4) reproducibility checks (i.e., computational reproduction of published analyses using original data and code).

Despite its value, there are naturally several limitations to replications. First, replications can be time-consuming and expensive, potentially displacing novel research, especially in fields with strong incentives for "new" results. Second, a failed replication does not uniquely imply that the original finding was false; it may reflect differences in context, measurement error, statistical power, or hidden moderators. Third, replications may be undervalued by journals and institutions, which reduces researchers' willingness to invest in them and may create reputational conflicts. Fourth, constructs and their relationships may shift over time or across settings, making "the same study" difficult to define. Fifth, despite their value, the publication of replications may decrease the impact factor of the publishing journal because they may not be cited at the same level as regular papers, as their novelty may be lower.

4. On scholarly careers and collaboration with others

Don Lehnmann loved collaboration with many different people on many different topics. In his mind, as well as in ours, the main selling point of the profession we chose is the immense variety of topics we can self-select to study with collaborators we can self-select out of a wide and global talent pool. The testament of Don Lehmann is to exploit this potential of our chosen profession and be broad in the domains we study to foster cross-pollination and collaboration.

4.1. Generalist or specialist? How to Avoid Being Bored and/or Boring

Nowadays, specialized knowledge seems to be *du rigueur*. It was not always like that. For much of human history, the *homo universalis* or "Renaissance man," the person with broad knowledge across different (sub)domains, commanded the highest respect. People like Aristotle, da Vinci, Newton, Leibnitz, Goethe, and von Neumann come to mind. The label also applies to Don, who made seminal contributions across a wide range of domains, from consumer behavior to management science. He was a true expert across all

fields of marketing and beyond. That did not mean his individual papers could not be highly specialized. Rather, it meant that across his entire oeuvre, he was a *homo universalis*.

Don deplored the increasing specialization and concomitant balkanization of our field. He believed it contributes to narrow scholarship, reinventing the wheel, and a loss of academic relevance to practitioners. That is why Don called for PhD programs to include substantial training in relevant marketing institutional knowledge (Lehmann et al. 2011, p. 161). We believe that a broader scope across fields also helps counter one of the most common problems in our field: post-tenure burnout.

The productivity of many marketing scholars drops substantially after they receive tenure. To understand this, consider why marketing academics do research. According to Self-Determination Theory (Deci and Ryan 1985), the source of the "why" is two-fold. The first reason is extrinsic motivation. The academic hopes to attain external rewards, including tenure, pay, and promotion. Once tenure is obtained, external motivation loses a lot of its power. The second reason is intrinsic motivation—personal satisfaction, curiosity, cognitive stimulation, and interest (Deci and Ryan 1985). It is focused on the enjoyment of the activity of research itself and the excitement generated by pushing the boundaries of our knowledge. After tenure, internal motivation should remain a potent force as long as the researcher remains cognitively stimulated, which is the opposite of boredom. Truly intrinsically motivated people may even become more productive or higher quality after tenure as they are liberated from the chains of tenure track expectations and constraints.

We believe a key reason why too many colleagues significantly reduce their research output after receiving tenure is that they become bored after studying the same, relatively narrow phenomenon, but are unsure or not well-equipped to venture into new directions. This obviously does not apply to all scholars. Some colleagues keep on doing important research in the same area. That is highly commendable. But this observation also suffers from sample-selection bias. We have forgotten those who disappeared because they grew bored.

Thus, excessive specialization not only harms the field but also individual careers. Don Lehmann frequently shared his sentiment that "specialists either get bored or boring". Meaning repetitive specialist research may get boring to audiences outside the specialized niche, even if the specialist maintains great passion for his or her specialty. Plenty of specialists also lose that passion for the specialty and bore out.

The solution to remain interesting for your audience and interested in your own work, is to become an expert across fields. Don is the example. He kept on publishing until the very end. Becoming an expert in any field requires time, and it is unlikely that everything interests you. Thus, you need to be strategic in allocating your effort. The most important step is to find something new that looks interesting to you. Here are a few suggestions to identify new topics that might interest you.

- Talk to managers or consumers. Which questions do they have? Which problems do they want to address? Preferably even work with companies as a paid consultant. Being paid guarantees you are working on something valuable with the company (or they will stop paying you) and it steers your competency development in a direction valuable to the company. Worry later that maybe the assignment is not specifically on a research paper. In many cases, the paper can follow after the consulting assignment (see Roberts et al. 2014 for examples of high-impact papers that followed that path).
- Read widely such as insightful newspapers, books and magazines, such as the *Financial Times*, *Wall Street Journal*, and *The Economist*. It will not only help you bring real-world problems into the classroom, but it will also give you ideas for your research.
- Go to general conferences where you see presentations and run into people who do very different things. The EMAC conference and the AMA Winter Conference are among the two options available, but so are a multitude of practice conferences.
- Go out in the field. If you are abroad, visit retail stores and see what is being offered, how people behave, and what assortments look like. You will be amazed at what you can learn from direct observation of the marketing environment. You can, of course, also do that in your own country, but when you are in a new environment, you are more likely to be inspired because many things are different. You cannot study gorillas unless you meet the beast…

4.2. Collaboration with others:

4.2.1. Importance of scholarly collaboration: The Lehmann number

Scholarly collaboration gives you access to specialized expertise and enables the production of higher quality research than would have been feasible without the collaboration with others. Second, collaboration may enhance a scholar's social influence (the influence a scholar garners because of *who* she or he works with), over and above her/his universal influence (the influence a scholar garners for *what* she or he publishes) (Stremersch et al. 2007). Don Lehmann was well-known for his excellent collaboration competencies, which made him even into the most central person in the entire network of marketing scholars. Thus, as an analog to mathematicians expressing their centrality to the field with the Erdos-number, marketing scholars have expressed their centrality in marketing by the "Lehmann number".

Highly central researchers, such as Don Lehmann, tend to occupy brokerage or hub positions that expose them to diverse ideas and methods, which can foster creativity and interdisciplinary recombination. Peripheral scholars, in contrast, may be more insulated but also more constrained in terms of diffusion of their work. Thus, network position often predicts downstream outcomes—citations, editorial appointments, and agenda-setting power—over and above individual ability or past productivity.

Second, collaboration networks function as channels through which methods and theories spread. Dense local clusters facilitate rapid diffusion and convergence within subfields, while bridging ties enable ideas to travel across intellectual communities. This structure helps explain why some innovations diffuse quickly while others remain localized, and why certain research streams become dominant while others stagnate. In this sense, academic networks provide a micro-foundation for diffusion processes typically modeled

at the aggregate level.

Third, ego-centered network analysis reveals how individual careers are shaped by their immediate collaboration environment. An ego network highlights not only the size of a scholar's collaborative reach but also its composition—seniority mix, institutional diversity, and connectivity among coauthors. Scholars embedded in cohesive ego networks may benefit from trust and coordination, whereas those whose ego networks span otherwise disconnected groups can act as intellectual brokers. These structural differences are often associated with distinct research trajectories, risk profiles, and long-run impact.

Finally, collaboration networks illuminate path dependence in academia. Early collaborations can have persistent effects, as initial network positions influence future opportunities through preferential attachment and reputation effects. This perspective helps explain the "Mathew Effect": how early career advantages—such as access to prominent coauthors—can have long-lasting positive consequences. Such positive consequences have been documented on the number of citations scholars' work garners (Karpienko et al. 2026; Stremersch et al. 2007).

Fig. 1 depicts who was the most central scholar in the marketing field. For data and method, we recommend readers to consult Goldenberg et al. (2010). Since 1999, Don Lehmann was the most central scholar in our field. Consequently, for this period, scholars could express their own centrality in the field of marketing, by counting their distance from Don Lehmann. If they co-authored with Don Lehmann their "Lehmann"-number is 1. If they co-authored with a co-author of Don Lehmann, their "Lehmann"-number is 2, and so on.

The black segment shows stabilization at very low average separation, consistent with Don Lehmann's scholarship that bridged consumer behavior, metrics, and strategy. Lehmann's position reflects consolidation rather than expansion—indicative of a mature, integrative role in the field. It may be also an early prediction for collaboration between quant and behavioral researchers – clearly his work with the two silos within marketing made him a broker (high betweenness centrality) that allowed him to possess the highest closeness centrality at the time.

4.2.2. Scholarly collaboration in practice

The first decision in collaboration is who to collaborate with. A senior researcher may bring a lot of experience in publishing. For example, how to position the paper, frame the contributions and how to navigate the revision if the paper is invited back. A junior collaborator often brings in a lot of enthusiasm, urgency and bandwidth that could help to push the paper forward.

Regardless of whether collaborators are senior or junior or whether the team is large or small, it is important to ensure that all collaborators share the passion for the problem that is being researched and are motivated to move the project forward. Nothing can be more frustrating than collaborating with someone who is slow to respond, does not have the same sense of urgency as the other author (s) or (even worse) stays on the paper without contributing. While you may be flattered when asked to join a team, ask yourself: Do I really find the research question interesting? Can I make a unique contribution to the team? And, importantly, will I have the time to do so in a timely fashion? If the answer to *any* of these questions is negative, you may want to eschew the invitation. And during the process, make an honest assessment whether you really make a contribution, and that you do not unduly delay the process. Unless the answer to *both* questions is positive, you may want to withdraw from the author list and take on the role of sounding board or friendly, yet critical, reviewer instead.

It is always a good idea to have an informal "contract" early in the collaboration. This is not a legal document, but a clear agreement about expectations. Things such as what roles each collaborator plays, how often to meet, checkpoints, and deadlines for internal work

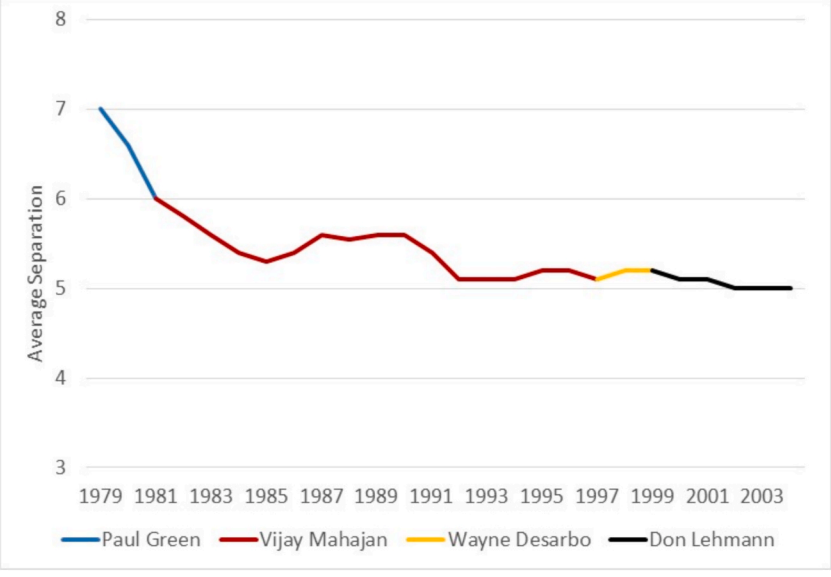

Fig. 1. Average separation of the lowest closeness centrality researcher, 1979–2004.

and for submissions to journals. Authorship and author order should reflect actual contribution and responsibility. The contract is a living agreement. Projects change and evolve and so flexibility is essential. It is always good to discuss the changes explicitly rather than drifting silently into resentment (which is quite common, by the way!)

In practice, collaboration works best when there is a clear driver. Often the first author serves as the project manager, writing the initial plan or draft, maintaining the master file (or Google doc), integrating edits, tracking open tasks, and setting deadlines. One useful practice is to assign ownership of each section with dates attached. Another is to always make the next step visible. At the end of each meeting or interaction, it is a good idea to agree on some concrete deliverables, decide who owns them, and agree on when they will be done. Prolific authors often have good project management skills.

These days, collaboration is frequently long-distance, which makes structure even more important. Emails and shared documents are good practice but are not substitutes for progress. One has to be careful about "out of sight, out of mind" dynamics and create some ways to make everyone accountable. Finally, collaboration is not only about dividing labor, it is also about sustaining a relationship and sharing the outcomes, happiness or disappointment, in the publication process! Generosity, reliability, and transparency matter. The aim is not just to coauthor papers, but to build a working partnership that makes ambitious research possible.

4.2.3. High Rigor, Low Ego: The 10 Commandments from a Great Collaborator

Collaborating with Don Lehmann was a constant exercise in intellectual humility and high-velocity thinking. To his co-authors and students, Don was more than a "founding father" of marketing metrics; he was a masterclass in how to be a serious scholar without taking himself too seriously. That, in part, was key to his enduring success. If we believe that true scholarship and meaningful success go hand in hand, here are a few precious lessons on how to live a research life in the most complete way.

1 Be a "Great Citizen" of Science

To be Don's colleague, co-author or co-editor was to understand the meaning of academic citizenship. In an era of growing specialization and siloed research, Don's door and mind remained always open, and inviting to discuss anything. He didn't just collaborate with peers; he was among the most prolific mentors of junior faculty and doctoral students in the history of the field. His feedback style became its own "Lehmann-ism": incredibly fast, brutally honest, but never unkind. He was known for returning drafts within 24 hours, often adding more insight into the margins than most reviewers deliver in three pages. His preferred medium was a hard copy (printed by his assistant) covered in editorial changes, usually in blue ink or pencil.

2 One Talk - One Page of Thoughts

Don had a habit of approaching speakers after seminars and handing them a single page of comments, in addition to his questions during the talk. To him, feedback was a path to progress not just for the speaker, but also for the feedback giver. His PhD students were expected to provide written feedback on departmental talks, especially in brown bag sessions.

3 Generalizing First: Finding the Physics of Marketing

Don was deeply committed to empirical generalizations and meta-analysis. He played a central role in moving marketing from anecdotal storytelling toward a more rigorous, predictive science. For him, research mattered not just when it was novel, but when it contributed to reliable generalizations. Developing knowledge is a team effort, and the team includes the entire research community. University status or journal ranks didn't matter because each paper, according to this perspective, is just another data point.

4 "Cut The %$%#@ And Show The Model"

Who cares about the literature review if the model is wrong? The litmus test of a paper, is the model and the findings. There would always be time for a rigorous literature review, later. We noticed this often at research talks also, where even more theory-focused faculty may tell speakers "Just show us the data and model!".

5 The "Napkin" Filter

Don was legendary for his ability to find the signal in the noise. While others became enamored with complex methods and technical flourishes, Don had a rare gift for reducing a problem to its essentials. He often viewed marketing through the lens of linear regressions, a powerful way to cut through complexity. His "napkin " instincts let him anticipate the results of massive datasets long before the computer finished its first iteration.

6 A Scientific Paper Must Be Correct (i.e., not wrong)

You shouldn't just aim for your paper to be publishable, you should aim for it to be true. This mindset grounded many young scholars in rigor rather than chasing fleeting trends.

7 Believe and Doubt Your Hypothesis at the Same Time

Don taught us not to blindly commit to our hypotheses. Be ready to revise them the moment you find an error, but don't abandon them at the first non-significant result. Good science lives in the tension between belief and doubt.

8 Exercise Intellectual Humility Every Day

Humility is one of the most powerful tools in a scholar's toolkit. When you're genuinely seeking truth, humility prevents blindness, and ensures you're never alone in the search. Being humble is a way of life, you can't be humble only in one aspect.

9 The Perspective of "Found Time"

"Found time" was a recurring theme in Don's advice to junior faculty. He often spoke about the stochastic nature of academic schedules, such as canceled meetings, and late arrivals. What do you do with those unexpected 15–30 minutes? Don used them to return reviews, respond to colleagues, or advance a paper, and he often did it within 24 hours. His point wasn't that he was super-human, it was that anyone could use found time wisely.

10 Being a Scholar is a Profession

Don believed that while the "Basics of Marketing" matter, the basics of life such as kindness, humor, perspective, and resilience matter more. If you live like a scholar, success will follow. Ultimately, collaborating with Don meant learning that the best way to

shape the future of the field is to build it with people you enjoy. His lack of pretension, his sharp humor, and his unwavering focus on the big picture became a model for how one person can shape the culture of marketing departments across the world.

4.3. Professional service

Professional organizations serve communities of collaboration. Therefore, academic excellence extends beyond own and collaborative research and teaching to encompass service to our scholarly communities, our institutions, and society at large. Service activities span a wide spectrum. Think of serving on editorial review boards, organizing conferences, mentoring junior colleagues, running professional associations, and engaging with practitioners and policymakers. While these contributions rarely appear on a CV with the same prominence as publications, they form the essential infrastructure that enables our field to function and flourish.

Scholarship is fundamentally a collective enterprise. Every paper we publish benefits from anonymous reviewers who donated their time and expertise. Every conference we attend relies on volunteers who organized sessions, coordinated logistics, and curated programs. Every journal we read depends on editors who stewarded the review process. When we serve, we pay it forward and invest in the next generation's opportunities to contribute.

This logic extends from serving academic associations to professional or boundary-spanning institutions. Think of the AMA, for instance, that has an academic and professional chapter and that effectively connects both communities. The American Marketing Association has a very large practitioner member base, so it is important for the journals to connect to practitioners. At AMA, that is typically the *Journal of Marketing*. That means that even if there is pressure within the academic community to focus on method (e.g., quantitative or behavioral), it is essential to also have at least one journal that focuses mostly on substantive topics and practical considerations. Also the Marketing Science Institute is an important forum where practitioners and marketing scholars meet around a jointly set research agenda that funds and steers what we publish in our major journals. It is also not a coincidence that most of the Editors writing this paper have close ties with industry. Several of us have founded or run our own company(s) to remain closely tied to practice.

Don Lehmann exemplified this ethos of service throughout his career. Within the marketing academic community, his contributions were extraordinary in both scope and impact. He served as co-editor of the International Journal of Research in Marketing from 2006 to 2009, where his editorial philosophy of seeking research that is “interesting, important, and not wrong” shaped a generation of scholars' approach to publishing. He founded Marketing Letters in the early 1990s with the vision that “interesting papers will make readers say 'I learned something,' 'it made me think,' and 'I wish I had done this'.

Beyond his editorial roles, Don served on several editorial boards, reviewed thousands of manuscripts, mentored doctoral students and junior faculty across institutions, and contributed leadership to the Marketing Science Institute as an executive director and other professional organizations. His service wasn't merely dutiful—it was generous, thoughtful, and aimed at advancing the field's collective knowledge. This professional service paralleled his commitment to the local community as football coach. This dual identity of scholar and coach captures something essential about Don's character: a belief that expertise creates obligations to serve, that teaching extends beyond classroom walls, and that building capability in others represents the highest form of contribution.

Don's example reminds us that service and scholarship reinforce rather than compete with each other. His editorial work didn't distract from his research productivity; he remained one of marketing's most cited scholars. His coaching didn't diminish his academic impact: it likely enriched his understanding of motivation, teamwork, and development that informed his research and mentoring. The same qualities that made him an effective scholar made him an effective servant of both his scholarly and local communities.

In remembering Don, we honor not only his research contributions but also his model of academic citizenship. He showed us that great scholars don't merely produce knowledge—they build communities, develop talent, and create infrastructure that enables others to flourish. Service, in this light, isn't an obligation we grudgingly fulfill between research projects. It's an integral part of what it means to be a scholar, recognizing that our individual achievements rest on collective foundations that we have both the privilege and responsibility to strengthen.

5. A Scientometric role model

Don Lehmann was fond of metrics. In Scientometrics as a field we develop metrics to evaluate science. Such evaluation can serve career or institution goals, support promotion and tenure decisions or guide funding. Don’s guiding example can best be described as (1) focused on A-journals, while not dismissive ex ante of work in non-A-journals; (2) guided by broader impact as expressed in citation patterns, while acknowledging humbly the many limitations of citations as a popularity, rather than a quality, metric; (3) crowned by awards as a clear indicator of quality and impact, while recognizing we have too many awards with inflated self-perceived importance.

In sum, Don’s example is a humble view on the merit of scholarship as in a Balanced Score Card with compensatory dimensions all with their own measurement problems.

5.1. The Role of A-journals: A Messy Signal of Quality Beyond Beans

Whether we like it or not, A-journals play an important role in our field. All too often, the first thing Promotion & Tenure Committees look at is the number of A-publications. Most external reviewers also make the number of A-publications a key element in their letters. But what is an A-publication? Don had a simple rule: “A-journals are journals we all agree upon as being A.” The point is that we do not all agree on this, with some exceptions such as: *Journal of Marketing*, *Journal of Marketing Research*, *Journal of Consumer Research*, and *Marketing Science*. They are included in any list of journals we are aware of.

While we do not dispute the quality of these four journals, several problems arise with this view. First, we likely agree that the best work does not always find a home in these four journals. Leading journals may be risk-averse or dogmatic in their approach and reject the most novel (deviant) and risky work. Leading journals may also uphold high standards on embeddedness in the literature (the expert's curse) or reliability of evidence (needing isolation of a phenomenon from its messy context). Both may limit leading journal's ability to some of the best work in the field. In the words of former editor-in-chief Steve Shugan at *Marketing Science*: the Book of Genesis would likely be rejected at a major journal because there was no prior literature section and no empirical proof.

Even if we agree, simply for the sake of argument, that the best work is indeed published in these four journals, two additional issues arise. That the best work is published in these four journals is not the same as saying that the work published in these four journals is the best work. In fact, leading journals publish rather few home-run papers. Most papers they publish are good, but not great, papers. In fact, they may even publish not so good papers, because of selection errors or because also leading journals need to fill their pages; the proportion not so good papers is likely smaller than in non- leading journals thanks to the risk aversion and higher *r*-quality of leading journals. Given any leading journal publishes great papers, (very) good papers and a small proportion not so good papers, simply counting the number of A-level publications of a scholar does not tell you anything about the distribution of the scholar's work across these three types of papers.

Moreover, one can question why one would want to dichotomize what is in essence a messy quality signal. What is published in non-A journals is certainly not of zero quality or relevance. Second, the top four feel like a guild in Medieval Europe. It is no coincidence that all four are published by U.S. associations. Half a century ago, that might have made sense, but given the state of scholarship worldwide, it no longer does. The most obvious omission is the *International Journal of Research in Marketing*. All of us have been Editor-in-Chief of *IJRM*, and we can attest to its quality. Some of our most highly cited papers have been published in IJRM. The only reason it is not a generally recognized A-journal, we believe, is that it is not published in the U.S. From a philosophical perspective, dichotomizing journals into A versus non-A leads to a loss of information (Fitzsimons 2008, Irwin and McClelland 2003).

The solution is twofold. First, we should expand the range of A-journals beyond the U.S., or, alternatively, add recognition to non-A journals in career evaluations, on top of the superior recognition devoted to A-journals (non-A journal publications are complements not substitutes of A-journal publications). Second, when making career decisions, we should read the articles to assess the *r*- and *q*-quality of papers generated by a person rather than simply count the number of A's, or citations (as advocated in Stremersch et al. 2021) and give more weight to *q*-quality (which captures the contribution inherent in the main ideas in the article) and less to *r*-quality (executional elements such as exposition, literature review, robustness tests, extensions, methodology) (Steenkamp et al. 2026). After all, we are in the business of knowledge creation, not in the business of filling journal space or counting beans.

5.1.1. The Role of Citations: A Distorted Signal of Popularity rather than Quality

A second metric we often look at, for instance in resource allocation or promotion and tenure decisions, is a citation count. Citation counts are vaguely informative as a categorical variable. A scholar with less than 100 citations (we are here using the most inclusive Google citations) is at the start of her or his career and probably does not merit promotion (yet). When scholars approach 500-1000 cites, they are seemingly gaining influence in the intellectual discourse. When scholars cross 1000-5000 cites, they may be considered for full professor positions at good research universities, as they now have been influential in the intellectual discourse of the field. Scholars with citation counts approaching 10,000 cites or more can be considered as having a prime voice in the field and be leading scholars. When scholars approach or reach citation counts of 100,000 or more, it concerns really unusual scholars in the popularity that their work has reached.

To dive a bit deeper in distortion, prior research has shown that only 15% of all citations in marketing are signaling intellectual indebtedness of citing to cited paper; meaning that such citing papers apply, affirm or negate components of the cited paper (Stremersch et al. 2015). The other 85% of citations are mere mentions because a cited paper belongs to the same field (i.e., a review citation; 53%) or it concerns occurrences where content analysis of both papers cannot reveal why the paper was cited by the citing paper (i.e., a perfunctory mention; 32%). Thus, the notion that citations are a signal of intellectual influence is messy at best, distorted at worst. Moreover, citations are driven by visibility (Stremersch et al. 2007), but quality does not correlate always with visibility. Just like so-called experts on news shows are not always the best experts.

Citation work also empirically shows that the biggest determinants of citation scores are, in fact, not related to quality. The biggest predictors are the passing of time (often in a quadratic fashion), the domain a scholar is active in (some domains are larger or more prone to incestuous citation gaming than others) and the methodology a scholar employs (some methods are more accessible than others) (see Stremersch et al. 2007). Other important drivers of citation counts are the social position of scholars in a network, as in Merton's Matthew Effect (see Goldenberg et al. 2010) and the readability of the published work of the scholar (Warren, et al. 2021).

Just like music or movies, popularity is more easily measurable than quality but it is a quite different metric. Not that it is unimportant or does not deserve inclusion in a balanced scorecard, but it should be looked at while balancing it together with other considerations. For instance, Stremersch et al. (2021) propagates a system to study the first page of a scholar's publication record in Google Scholar ranked on citations. One should not require the first ten papers to be all published in A-journals but if the first ten papers of a scholar as ranked on citations are all in non-A journals or the scholar has never been a first author on any of these 10 top-cited papers it may lead one to exert more caution and look at the track record of the scholar in a bit more detail to verify if the scholar under scrutiny is in fact seen as a reputable high-quality participant in intellectual discourse.

5.1.2. The role of awards: A socially constructed indicator susceptible to inflation

Awards act as public signals that research is not only of high rigor but also is likely interesting and important. Awards deliver a different and unique quality signal that other metrics, such as publication counts, citation counts and even reading at times cannot

deliver. At the article level, an award or whether the article is selected as a lead article is a stronger quality signal than its citations, although they positively correlate. At the author level, awards often signal the elevation of a scholar by his or her most reputable peers, which is again different from an author's collective citation score. It is also a metric that deans of business schools are/should be considering more in recognizing the value of research (Stremersch et al 2021).

Awards also shape how knowledge diffuses within the discipline. Award-winning papers are usually included in doctoral seminars, bringing attention to the research in a crowded field. Award-winning papers act as exemplars for other researchers to emulate. It is important to realize that while award-winning articles are almost always exceptionally good papers, not all great papers win awards. So, it helps to recognize winning awards as signals, but not an ultimate goal, in and of itself.

Awards should be treated as byproduct of writing a paper that excels on the different dimensions that predict award-worthiness. These dimensions often include importance of the problem, clarity of the paper, rigor of the methodology, overall contributions of the paper – theoretical and/or managerial – and path breaking, in the sense of opening up new pathways in research, be it theoretical or methodological.

At the same time, also awards are limited purely as metrics. First, whether a paper or scholar receives an award or not it is a dichotomous variable with sparse occurrences and thus provides poor information and few signals (few 1's many 0's). Second, most awards are voted on by a committee or by a full editorial board. Thus, they also potentially fall prey of favoritism. In the worst case, one can best understand why a scholar received an award by looking at the committee composition. Third, our field has a dazzling number of awards leading to award inflation. Whenever an AMA conference has taken place, one's LinkedIn feed is flooded by the many people posting with their plaques.

6. On journal management: An editorial role model

Editors can have a large impact on the progress of the field, when they see themselves as more than just paper pushers. Going back to Don's emphasis on papers that are interesting, important, and sufficiently rigorous, it is easy for Editors (especially younger, less-experienced ones) to focus almost exclusively and excessively on rigor, and sacrifice importance. Soon, authors and reviewers see the review process as simply certifying rigor.

6.1. Composing editorial boards

It really is true that a diverse team is best, and that the biggest potential mistake that can be made when assembling a review board is to choose one that is not broad enough (Shugan 2006). The team needs to be diverse in subject area, methodological expertise, area of formal training, practical experience, geography, ethnic background, and gender, just to name a few important variables. It also should represent the full reach of the author base.

As an incoming editor it is good to keep a couple of "personas" in mind you want to get on your board, even if overrepresented compared to the submitting author population:

1. *Decisive constructivists*: the decisive constructivist is the ideal AE. S/he can easily see new and improved papers develop out of possibly poor initial submissions. S/he takes a constructive stance but is decisive. S/he accounts for reviewer input but is not a counter. Thus, this AE steers typically more papers in a positive direction than a reviewer vote-counter would do, thereby rescuing more papers and more easily siding with papers that are interesting and important but may be unusual or less rigorous (e.g., conceptual) by conventional standards.
2. *Library rats:* Library rats are reviewers that can safeguard your journal from disastrous mistakes. They know the papers that authors "forgot" to cite and can detect fraudulent practices such as (self-)plagiarism. When one of us served as editor it was one of these library rats that saved *IJRM* from publishing a paper that was an English translation from another already published paper in a language different from English avoiding a direct copyright violation.
3. *Modeling simplicity experts:* This is the true modeling expert. True experts understand methods to a level they can make it simple for everyone involved. They do not need jargon, they do not raise endogeneity issues without precisely stating what they are concerned about and what solutions may be. They also use simple methods as dummy tests of more complicated models. They are there to figure out what is the easiest way to get a proof that is not wrong for a broad readership, beyond analytical complexity for the sake of complexity.

6.2. Vote counting

The easiest thing for an Editor to do is to simply count up the "votes" by the review team. There are only two major things wrong with this. First, most reviewers are negative, and most reviews are rejection recommendations. Younger reviewers are overrepresented in this pack Thus, editors should ensure that experienced, mature, scholars are adequately represented on their board. Some reviewers are also consistently negative on any paper you send them, thus making the summation of reviewer recommendations representative of the reviewer delegation on the paper, not the quality of the paper itself.

A related major problem is that even very good papers—those that eventually win major article awards—often receive very negative reviews. A journal looses very little by publishing a low-impact paper. Most papers have little impact. The major risk to the journal is *rejecting* a potentially high-impact paper (Type-2 error). Thus, a capable Editor considers potential impact when making a decision on the paper. Some of us have even kept papers in the review process (giving the paper one more chance), even with a

unanimously negative review, or have fired entire review teams, only to see the papers they subsequently steered to publication win best paper awards. Of course, at cocktail parties the originally negative reviewers seem to have "forgotten" their own firm stance and sing praise of the award-winning paper once it received the aura of publication and awards. An editor even needs to be willing to go against accusations at times even of co-editors of preferential treatment of papers to be able to publish award-winning papers.

Editors also differ in their decision-making style. Some are more prone to take risks, led by Don's three questions: Is it interesting? Is it important? Is it not wrong? Others might be more cautious. Reviewers and associate editors provide critical input for the editor's decision but theirs is a *recommendation*; they do not *decide* the manuscript's fate. We have heard the suggestion that reviewers and AEs no longer provide an overall recommendation but only their comments. This makes sense if the editor is a vote counter, but that is neither the type of editorial decision-making we advocate nor common among editors of leading marketing journals. Editors weigh the comments made by the review team in the light of the editor's own assessment. Not infrequently, the editor overrules the review team, either by extending a revision or by rejecting the paper.

As an example, one of us was one of the reviewers on the original Service Dominant Logic paper by Vargo and Lusch (2004). That paper received a very negative review, but this reviewer dissented and said it was a potential award winner. The Editor, not counting votes, saw the potential loss from not publishing an award-winning paper, and decided to give the paper a chance. It ended up winning two major article awards from the *Journal of Marketing,* and has garnered over 25,000 citations.

7. Looking Forward

Don Lehmann's scholarship provides a uniquely powerful lens for thinking about the future of marketing research and publishing in general. As a sequence of (former) editors of *IJRM*, we extend his legacy in this section and look forward into the future of *IJRM*.

7.1. The Field: What Would the Reincarnation of Don Lehmann study?

As the above illustrates, Don Lehmann was one of marketing's foremost scholars. One approach to peak into the future is to run a thought experiment. What if a reincarnated Don Lehmann would start his scholarly career now, what would he study? For over five decades, Don asked four fundamental questions that still resonate today:

- How do people choose?
- How do innovations spread?
- What is a marketing asset really worth and, thus, how should we manage it?
- How can we formulate empirical generalizations of what we know?

If we were to project his intellectual lens forward, a young Don Lehmann today may likely focus on four major pillars:

- The automation of decision-making
- The disruptive innovation potential of AI and new marketing technologies
- The valuation of "synthetic" marketing assets
- The meta-analysis of the digital age

We believe these four areas are important new areas of discovery that will bring truly important and interesting research forward.

7.1.1. The Automation of Decision-Making

One of the most consequential shifts is the integration of artificial intelligence into both firm decision-making and consumer experiences. AI is increasingly embedded in targeting, pricing, service, content generation, and product design. For consumers, AI influences search, recommendation engines, social feeds, and shopping journeys. Don would likely argue that this shift creates a new research agenda: one that extends from viewing AI as a tool to examine AI as an actor in markets. Future marketing research will need to theorize how algorithmic agents reshape:

- Choice architecture
- Perceived persuasion
- Trust and consumer welfare

On the firm side, the focus will be on when AI improves decision quality vs. when it amplifies bias or promotes fragile strategies. On the consumer side, as AI becomes more integrated into decision processes, research must grapple with the joint entity of human + agents. Don would likely be studying the emergence of the automated consumer as well as the automated marketing decision-maker. He would ask questions such as: "How do we model choice among alternatives when the chooser is an algorithm?" or "How do we manage our brands and customer relations if the marketing decision-maker is an algorithm?"

7.1.2. The disruptive innovation potential of AI and new marketing technologies

Don would recognize that these forces of AI promise to be highly disruptive to marketing, which we are already witnessing today. Consider the complete overhaul of advertising and ad agencies. Don likely would be interested in questions such as:

- How disruptive will the adoption of AI be for firms?
- Does the rise of AI promise to generate enduring changes in market leadership?
- How does the rise of AI change the role of marketing in the firm and the competencies required of marketers?

Don would also likely get inspired by the many innovations that surface in marketing technology:

- How will chatbots and agent-agent communication alter customer relationships?
- How do you keep control over your brand if all communication is automated and relationships are platformized?
- What are the opportunities Augmented Reality (AR) and Virtual Reality (VR) present for marketing and interpersonal interactions?

The young Don Lehmann may also question some societal problems that may arise, such as the behavioral consequences of the loss of privacy as a result of consumers' expanding digital footprint and the desire for real-time personalization.

7.1.3. The Valuation of Intangible, "Synthetic" Assets

A core part of Don's legacy is the Brand Value Chain and the management of marketing assets. But in today's digital ecosystem, those assets are shifting. Platforms are no longer neutral intermediaries, they govern discovery, access, and monetization. A young Don Lehmann would have a keen interest in studying platforms and see them as an important study object. Don would likely also devote effort to the valuation of Personal AI Companions, digital agents that carry a consumer's preferences, habits, and history. If every consumer is paired with a trained AI agent, that agent becomes a strategic marketing asset.

Don might ask:

- How do we calculate Customer Equity when the customer interacts only through an agent?
- What is the ROI on training a Large Language Model (LLM) to reflect a brand's specific personality and values?
- What value do platforms bring in connecting supply and demand agents? Which type of platforms drive value?

7.1.4. Beyond Single Studies: The Meta-Analysis of "Big Data"

Don was a lifelong champion of empirical generalization and knowledge accumulation. He often criticized the field for being overly formulaic and for prioritizing one-off novelty over robust, replicable findings. He would likely view the *replication crisis* not as a threat, but as an opportunity; a chance to embrace what he always championed - meta-analysis.

In a digital world where firms like Amazon or Google run millions of A/B tests, Don would study the "digital file drawer" problem and push for what we might call "Generalization 2.0".

He would probably ask:

- Do the foundational laws of diffusion still apply in a world of real-time, global contagion?
- How do we extract robust truths from high-dimensional, unstructured data (e.g., text, video, social signals)?
- How can we continue to push for triangulation, combining methods to ensure findings are not artifacts of trendy tools but reflect meaningful insight?
- How can we validate digital twins and synthetic consumers before we trust such constructs in theory or practice?

He would see these questions as high-impact areas for future inquiry and we agree.

7.2. How will scholarship and publishing evolve?

Scholarship and publishing are entering a period of structural change, driven by technology and institutional reform. Generative AI (GenAI) has already altered how marketing research is conducted—from literature synthesis and question generation to instrument design, data collection, coding, and manuscript preparation (e.g., Peres et al. 2023). What previously required substantial time and technical expertise has become more accessible and scalable.

The implications are nontrivial. Evidence from over 40 million scientific publications suggests that AI-augmented scholars publish more, receive more citations, and reach leadership positions earlier (Hao et al. 2026). Productivity gains appear particularly strong among early-career researchers and scholars from non-English-speaking countries (Filimonovic et al. 2025), suggesting a democratizing potential. At the same time, research agendas may concentrate around data-rich domains, and the long-run role of human scientists remains uncertain. Speculative but increasingly plausible scenarios—such as "lights-out labs" working 24/7 without the need for human-researcher involvement and AI systems capable of Nobel-level science (Adam 2026)—raise fundamental questions about what comparative advantage human scholars will retain.

Beyond technology, open science reforms are reshaping norms of credibility. Preregistration, now common in behavioral research, increases confidence in internal validity and guards against *p*-hacking (Simmons, Nelson, and Simonsohn 2021). Yet preregistration does not ensure generalizability. Marketing has historically prioritized novelty over replication (Hubbard and Armstrong 1994). Encouragingly, replication efforts—both exact and conceptual (Lynch et al. 2015)—are gaining traction across leading journals.

A related structural issue is publication bias (Korkames et al. 2026). As Rosenthal (1979) observed decades ago, the file drawer problem obscures how many null results go unreported. The consequences are substantial: distorted effect estimates, fragile literatures, and misaligned expectations among doctoral students. Remedies range from emphasizing effect sizes to rethinking null hypothesis

testing (McShane et al. 2024), but one particularly promising format is the registered report. By reviewing and conditionally accepting research proposals before data collection (Chambers and Tzavella 2022), registered reports reduce incentives for selective reporting. Although leading marketing journals now solicit such submissions, widespread adoption will require cultural adjustment and clarity about which research questions warrant this format.

Two additional formats merit consideration. First, megastudies—large-scale experiments testing multiple interventions simultaneously (e.g., Milkman et al. 2022)—leverage scale efficiencies and reduce selective reporting risk. Second, a "no file drawer" paper format could require authors to report all studies conducted on a focal question. Third, more careful reporting and submission enforcement of databases, enables more careful meta-analysis enabled by better corrections for meta-analytic bias (Korkames et al. 2026).

Finally, the role of journals in knowledge dissemination is under pressure. Preprint platforms (e.g., SSRN, arXiv) allow immediate dissemination, while alternative models such as "publish, then review" (Eisen et al. 2020, 2022) change the certification process that has been the prerogative of top journals. For time-sensitive domains—GenAI being the obvious example—the advantages are clear. At the same time, it is in our best interest to safeguard the quality gatekeeping of the traditional publishing process. As social media show in the general public, traditional media with high journalistic standards remain important to protect us from disinformation and fake news. In a similar vein, the traditional models of our top journals remain vital in protecting quality-based science against a popularity-based science.

7.3. What will IJRM look like 20 years from now?

Many changes are unfolding in the world of scholarly publishing.

1. **Democratization of insight creation and consumption**: Scholarly research is democratizing and globalizing. Scholarly journals aim to be more inclusive to different topics, methods and geographies. Also, scholarly journals are more accessible all around the world than before. Journals themselves are also being supplemented increasingly with other publishing channels, such as open source publishing, blogging, podcasting, and social media posting. For instance, increasingly scholars and universities come out with PR on research before the journal review process, while before it was more customary to do that once research was reviewed, forthcoming or published.
2. **Artificial intelligence will transform editorial workflows while preserving essential human judgment.** AI tools will increasingly handle initial manuscript screening for plagiarism, formatting compliance, and methodological basics, freeing editors and reviewers to focus on assessing substantive insights. Reviewer matching algorithms will become more sophisticated, though the peer review process itself will remain fundamentally human-centered.
3. **Content formats will become more dynamic and interactive.** The static PDF will evolve to include interactive data visualizations, video abstracts, and links to living datasets that update as new information becomes available. Micropublications are very likely to emerge for rapid sharing of single validated findings, including null results that currently struggle to find homes. This would permit more immediate debate between authors and author groups. These format innovations will coexist with traditional articles, expanding rather than replacing the ways we communicate marketing science.

Against this backdrop of change, *IJRM* is uniquely positioned to offer stability in its core. IJRM has always been defined by three core strengths that we believe will always remain distinguishing features: Diversity, Innovation, and Speed (DIS).

1. **Diversity** has been IJRM's hallmark from its founding. We are one of the only truly open leading journals in marketing welcoming quantitative modeling, qualitative inquiry, behavioral experiments, and mixed methods, all on equal footing. We publish marketing strategy, consumer behavior, inductive scholarship, and digital marketing. We draw authors and editors from every continent in a balanced manner recognizing that important marketing problems transcend geographic and cultural boundaries. While IJRM is EMAC's flagship journal, IJRM is distinctly not "Europe-first" in its approach, but remains truly global in reach. This inclusive approach positions IJRM as the natural home for the field's most interesting and important work, regardless of where or how it was produced.
2. **Innovation** runs through the journal's DNA. We have consistently championed research that is "novel, visionary or pathbreaking" rather than incrementally safe. Several examples of papers published in IJRM became home run papers that other leading journals found too risky to publish. We pioneered rapid publication processes, embraced new methodologies, and published papers that challenged conventional wisdom. As AI and new technologies transform research methods and publication formats, IJRM will continue leading rather than following, experimenting with interactive content, dynamic peer review, and novel ways of communicating marketing science while maintaining our commitment that research be "interesting, important, and not wrong."
3. **Speed** reflects our recognition that knowledge should be released as soon as it becomes available. Our editorial processes prioritize timely decisions and rapid publication without sacrificing rigor. As the pace of marketing practice accelerates, the lag between discovery and dissemination must shrink. IJRM's commitment to speed ensures that our published research remains relevant to the rapidly evolving problems practitioners and policymakers face.

These three pillars position IJRM to realize its full potential over the next 20 years. The journal has maintained consistently high quality for decades, publishing work that advances marketing science and shapes practice. The global recognition of IJRM and the quality it offers is rapidly catching up and will continue to do so. As research assessment becomes more sophisticated, IJRM's persistent

excellence will shine, as none of its recognition progress has been driven by gaming the system.

We envision that within two decades, IJRM will continue to be universally recognized as the best international journal in marketing, but also as the only international journal that perfectly matches the best US-based journals in the field. IJRM's inclusiveness allows us to capture the full richness of marketing science from every corner of the world, every methodological tradition, and every substantive domain. Its innovation will keep it at the frontier rather than the trailing edge of how scholarship is created and shared. And its speed ensures it remains relevant as marketing science and practice evolve at ever faster speed.

8. An Afterthought

When Stefan Stremersch and Don Lehmann set out as editors of IJRM, at the request of Jan-Benedict Steenkamp, who led the search for new editors in the period 2006-2009, they made quite a few major changes to IJRM. One of the most quirky ones was the afterthought section. In this section, Don and Stefan allowed authors to include statements outside the review process after at least 3-4 years studying a topic, things the authors had learnt while doing the study that they could not prove, sometimes even not conceptually.

Nothing is a more appropriate homage to Don Lehmann who fondly stimulated many authors for an afterthought than for some of us to leave their own.

"When you fall hard, sometimes it is best to act as if it did not happen." (Roland Rust)

When Don's health was already seriously failing, and he was in a wheelchair when he wasn't using his cane, at one point, he insisted on walking down a stairway on his own, and fell hard. Ming-Hui Huang and I saw it happen, and we knew exactly what Don would want. We gave him the time to gather himself and continue the conference as though nothing had happened. He was one of the toughest people I ever knew.

"Your reputation is what people think of you as a human being." (Martin Schreier)

In an interview with Arnaud De Bryn for his "Little nuggets for academic wisdom", Don said that "It turns out that your reputation is not just the objective value of what you do, a lot of it is what people think of you as a human being." For both reasons, Don's reputation has always been awesome. We miss him.

"Don't become boring or bored." (Stefan Stremersch)

In many conferences, we sat next to one another. On countless occasions, Don would say: "I heard this person speak about this already so often. Always the same… So boring for us, but I can't believe s/he is not bored either…" We thought about so many things so similarly, we never needed a lot of words to entertain a conversation. The joint editorship of IJRM was a true blessing I believe for both of us. We frequently hung out together without saying much. As Don used to say when we edited IJRM together: "Thank God we have something to talk about now…" Don, we won't talk at least for a while (I hope), but luckily we can still hang out together in spirit without saying anything.

CRediT authorship contribution statement

Stefan Stremersch: Conceptualization, Methodology, Project administration, Supervision, Validation, Writing – original draft, Writing – review & editing. **Marnik Dekimpe:** Conceptualization, Methodology, Project administration, Supervision, Validation, Writing – original draft, Writing – review & editing. **Jacob Goldenberg:** Conceptualization, Methodology, Project administration, Supervision, Validation, Writing – original draft, Writing – review & editing. **PK Kannan:** Conceptualization, Methodology, Project administration, Supervision, Validation, Writing – original draft, Writing – review & editing. **Eitan Muller:** Conceptualization, Methodology, Project administration, Supervision, Validation, Writing – original draft, Writing – review & editing. **Koen Pauwels:** Conceptualization, Methodology, Project administration, Supervision, Validation, Writing – original draft, Writing – review & editing. **Roland T. Rust:** Conceptualization, Methodology, Project administration, Supervision, Validation, Writing – original draft, Writing – review & editing. **Martin Schreier:** Conceptualization, Methodology, Project administration, Supervision, Validation, Writing – original draft, Writing – review & editing. **Jan-Benedict Steenkamp:** Conceptualization, Methodology, Project administration, Supervision, Validation, Writing – original draft, Writing – review & editing.

Declaration of competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Data availability

No data was used for the research described in the article.

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