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DIALOGUE, DEBATE, AND DISCUSSION

Develop Mental Ideas for Management Journals? Commentary on Zhang and Chen

Michael Barnett

Rutgers University, USA
Email: mbarnett@business.rutgers.edu

Management scholarship is not for the faint of heart. It requires perseverance in research and writing processes that take months and years to produce an initial draft. And that is just the starting point of an arduous multi-year journal review journey thereafter, during which the researcher must endure negative feedback and frequent disappointment. My personal record is 10 years from idea to publication, replete with a decade's worth of intensive high-risk revisions and second-round rejections. Here's the capper: Even if you do eventually succeed in publishing, your work is unlikely to matter to practicing managers. The lack of relevance of our outputs is regularly bemoaned by the most prominent members of our field (Bartunek, 2003; Bennis & O'Toole, 2005; Hambrick, 1994; Mowday, 1997; Rousseau, 2006).

Well, that's not good! Enter Zhang and Chen (2024), who point to medical research as offering insights about how we might do better. Owing to differences in publication processes, they recently were able to rack up several dozen acceptances in mental health journals in the time it would take to get perhaps a single publication in management. Moreover, their publications had a significant impact on the mental health field. Overall, they found the mental health journal publication process to be better for both scholars and practitioners. Win-win!

Clearly, there is much room for improvement in the management publication process. But how effectively can the lessons that Zhang and Chen (2024) learned from their experiences with mental health journals be used to better management journals? Some of the ideas they present are worthy of implementation. Others could create more problems than solutions.

In contrast to management's 'theory fetish' (Hambrick, 2007) that pushes our research into esoteric and insular debates, Zhang and Chen (2024: 343) explain that medical and health research prioritize evidence and place primary emphasis on relevance, 'aiming to address practical problems'. They argue that management research should do the same, so that the field can 'align itself more closely with the goals of applied science to make meaningful contributions that address practical challenges faced by organizations and society'. Agreed! Theoretical advancement remains the rule, but some management journals are open to evidence-based, practice-focused work, as Zhang and Chen (2024) acknowledge. They overlook one shining example, however: *Academy of Management Perspectives* (*AMP*). As the editor-in-chief of *AMP*, I certainly cannot let that go! We recently revised our mission to ensure that we develop and publish only those papers that provide evidence-based solutions to important managerial problems (Barnett, 2025). It is not an easy transition, but we think it will catch on, and if we have our way, it will become more the rule than the exception in management. Get *AMP*ed!

On the other hand, Zhang and Chen (2024) note several aspects of their experiences with mental health journals that, for reasons I will next explain, should not be adopted in the management field or that will require significant adaptation to do so.

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Relevance requires rigor. Zhang and Chen (2024: 344) argue, 'The dichotomy of usefulness versus rigor presents a pivotal tension in academic research, which is dealt with in a more pragmatic manner in health sciences'. Specifically, health sciences 'often leans heavily toward usefulness and relevance', in contrast to management which 'tends to not relax rigor and credibility at the expense of practical utility in any situation'. OK. But it should stay that way. If anything, health sciences should learn from management, because rigor is not at odds with relevance; it underpins it.

I am a fan of relevance: perhaps a zealot at this point, given my editorship. But not at the expense of rigor. Quickly cranking out dozens of articles that are not adequately vetted is a recipe for confusion, not clarity. We see this in seemingly daily studies on various aspects of health that get significant press coverage but contradict each other. Coffee is good for you, then bad for you, then maybe sorta both. Then red wine … then sitting … then standing … and so on. Incorrect or at least incomplete findings, even if highly relevant in the moment, tend to further muddy the water as more complexities come to light. It is hard to know what to eat, how much to exercise, how much water to drink, and so on, as a result. Applying this to management, practitioners can easily dismiss articles or find counterarguments where the rigor is weak. What is needed – what is relevant – is robust work that is unlikely to be contradicted quickly. Weakly supported findings are not credible, and therefore not relevant. We do not need a greater volume of articles for any reason other than internal prestige and promotion games.

Speed is a trap. Zhang and Chen largely equate relevance or usefulness to speed. They note, 'One of the striking differences we encountered in health sciences was the importance given to the timeliness of research' (Zhang & Chen, 2024: 345). They point out that medical journals make decisions within days, or even just a single day, given the need for rapid dissemination of life-saving studies. They suggest that management journals should implement fast-track processes because 'in the management field … relevant research cannot wait and needs timely research dissemination' (346).

This need for speed in management is overstated and even dangerous. In contrast to health sciences, management publications rarely if ever have the potential to save lives that are in imminent danger. I speak from personal experience. I am an active emergency medical technician (EMT) with several hundred 911 calls of a very diverse nature under my belt. Not once have I pulled *Administrative Science Quarterly* or *Academy of Management Journal* from the shelves of an ambulance to save a life. I'll go so far as to admit that we do not even carry management journals on our ambulances, nor have I seen them in the many hospitals that I have delivered patients to. Rather, the influence of management research, which I do not entirely mean to belittle, takes time to unfold, as it should. Just as the US Constitution was designed to work, with slow, deliberative advancement entailing sober review and requiring broad agreement to forge change, so is and should be the management field. As we see in current US governance, when this slow, deliberative process is replaced by rapid-fire executive order, things become problematic. Rash decisions requiring repeated rehashing replace reason and reckoning. The reality is, even in much of health sciences, the pace of change is slow, and not the immediate result of a single sudden publication. As new data emerged, EMTs gradually ended long-established practices of using rigid backboards by default, prioritized chest compressions over ventilations in CPR, and so on. Evidence-based research eventually even showed us that literally speeding to the hospital in ambulances and running to the scene of events proved counter-productive and dangerous overall. Turns out, speed kills.

Coordination is the soul of brevity. Zhang and Chen (2024: 345) found that in health sciences, 'Utilizing established instruments and following standardized procedures fosters consistency and ensures that research findings are comparable, reliable, and replicable'. To clear up the incomparability problems in management and to enable studies to build on each other, Zhang and Chen (2024: 345) 'strongly encourage management scholars to develop more standardized expectations'. Moreover, to aid in both practical and scholarly uptake, Zhang and Chen (2024: 346 & 347) encourage 'emphasis on clear

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writing over polished prose' and 'focusing less on stylistic perfection and more on the clarity and substance of the evidence'.

Brevity is the soul of wit, and perhaps wisdom as well, and so standardization and conciseness in managerial articles are indeed welcome. At *AMP*, we ask authors to avoid jargon and write in ways that reduce article length by at least one-third relative to most other management journals. Many high-prestige science journals are much shorter yet: some only a page or two. But such brevity requires agreement on the paradigms of a field so that core issues can be taken as given rather than argued anew, paper by paper. In management, we lack such paradigms, so authors must devote many pages to carefully convincing reviewers and editors, as well as eventual readers, that the paper is addressing an important topic.

What could be the soul of brevity in management? Zhang and Chen (2024) briefly address the importance of functional research teams, but only to encourage more credit for co-authorship. Yet research teams can also serve as part of a broader plan – research roadmaps – that enables brevity. In Barnett (2016), I called for coordinating research efforts by creating shared plans and agreeing to distributed teamwork to provide answers to open managerial questions. That is, scholars can agree on roadmaps that specify what needs to be studied and who will study it. This would enable research teams to focus on rigorously conducting the studies, more so than repeatedly convincing others that the studies are worthy of being conducted. Moreover, in a nod to health sciences research, I outlined how such coordination could lead to the production of a sort of *Managers' Desk Reference*, akin to the *Physicians' Desk Reference*, that would provide managers with quick and clear evidence-based insights to guide practice.

In summary, it is important to learn all that we can from other fields, as Zhang and Chen (2024) have artfully done. Indeed, there are important lessons to be learned from the health sciences that can enrich managerial studies. However, not all the lessons learned by Zhang and Chen (2024) can or should be applied to our field. In particular, we must be careful to account for the relevance of rigor, non-need for speed, and curtness through coordination.

I have stepped into what has turned into a multi-sided conversation a bit late, though, and so I need to compare, contrast, and perhaps contort the views I have just shared against what others have already written. Davis (2024), Friedman (2024), Meyer (2025), and Tsang (2025) have previously provided commentary, and Zhang and Chen (2025) have already drafted up a response. With the benefit of having seen these various takes, I offer a few more points for your consideration.

Davis (2024) tough-lovingly encourages us to get over ourselves and not assume our ideas can live on forever. He has no time for timeless insights, instead encouraging us to recognize that our work is transient: inherently time- and place-bound. Agreed. We are all just dust in the wind and whatnot. Zhang and Chen's (2025) response affirms the importance of identifying the contingencies that determine when and where our inherently temporary findings hold. But none of this negates the need for rigor, at the pace required of rigor. Acknowledgement of temporality and contextuality is not a license to be quick and dirty. Let's not pile up time-limited papers like fast fashion trash heaps. Instead, let's carefully craft works that meld together the best of what's available at the time. If it is of high enough quality, then even as time and context change, it can be adjusted instead of wasted. We want works that bear the 'right to repair' rather than designing them to be scrapped as conditions change. And rigor need not be glacial; it just need be thorough. For sake of perhaps a few weeks or months, let's not risk confidence in our work and push over-production of transient ideas.

Friedman (2024) – the artist, not the Milton – tells us that management is an art, not a science, and in our squishy domain, stories sell. Who doesn't love a good story? But does that really mean that, in management, our facts are merely 'facts', as Friedman (2024) argues, because different storytellers tell the story differently? As a matter of fact, what we accept as fact will be fleeting. That's a feature of science, though; fact is, facts change as science progresses. That should not mean that, as management artists, we accept that our facts are at the whim of the latest storyteller. We have rigorous processes to underpin our storytelling – again, rigor is essential, even though it takes time. Let's uphold them.

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Friedman (2024: 700) dismisses our ability to draw objective conclusions in management because 'For a manager, there is not a core task' – they must 'build a team, encourage innovation, think about organizational design, figure out how best to hire people, solve logistical problems, deal with conflicts between employees, set priorities, and more', whereas for physicians, they have a clear core task of diagnosing and treating patients. Diagnosing and treating patients also has a long subset of activities associated with it, and in which it is embedded. Furthermore, management could also be simplified to, say, 'diagnosing and treating organizations'. Thus, I find this a weak argument for excusing management from scientific rigor. At minimum, we can find shelf-stable solutions to problems associated with each individualized and contextualized task, even if we cannot provide a singular answer about how to manage in general. All that said, I will openly admit that I often say to co-authors, tell me what the data say, and I'll write the story to support it. The story I tell changes to best convey the results of the analysis – the facts. But the facts do not change to match the story. The facts are constant and inform the story; the story does not inform the facts.

Meyer (2025), like Friedman (2024), discourages us from aspiring to lab coats. Instead, we should think of ourselves as akin to the humanities, which are 'discursive, rarely constrained by rigorous rules of evidence, and often disorganized' (Meyer, 2025: 2). He sees our purpose not as being practical and useful, but as sparking ideas that may trickle down into practice eventually – or not. Then again, Meyer (2025) also notes his pride in some of his work that proved quite practical and useful. He admits that he thus offers a mixed message. This mix reminds me of exploration and exploitation (March, 1991), the combination of which is necessary for the long-term survival of organizations. We disagree on the appropriate mix of the two for management scholarship. Meyer (2025: 2) loads up on exploration, only sprinkling in a dash of exploitation here and there, declaring: 'mental playfulness and experimentation needed to cultivate new ideas … may be our main mission'. Perhaps in the base disciplines from which management scholarship draws, favoring exploration is appropriate. But we are an applied discipline and so should exploit our skills and insights to develop managerial solutions primarily, not secondarily.

Tsang (2025) believes that we should prioritize explanation over prescription because prediction is too difficult and perhaps even impossible in the messy, open-system world of management. Though Tsang (2025) at least lets us hang on to the pretense of being scientists, he reminds us that we are social, not natural scientists. Unnatural as we are, we are best served by 'mechanism-based explanation' (Tsang, 2025: 3). Curiously, in their rejoinder, Zhang and Chen (2025: 1) 'unequivocally concur'. They 'do not contest the superiority of explanatory theorizing and agree that theoretical explanation remains the paramount objective of management scholarship' (Zhang & Chen, 2025: 2). I am surprised by the total capitulation; the balance is once again in the wrong direction. As scholars, we add little value when we focus our energies on placing established managerial actions in a shiny new box with a catchy label. Managers need only look in the mirror to see what they are already doing. Moreover, the problems of the dominance of theory fetish in the management field are well established (Hambrick, 2007). Instead, explanation should serve as a pathway to prescription, and prescription should be the end goal. By contrast, Zhang and Chen (2025: 3) seem pleased to have just the occasional prescriptive paper slip through the tight grip of explanatory theory, only questioning 'whether every individual paper must prioritize novel theoretical explanation to merit publication'.

I close by noting that this format of point-counterpoint that I am now participating in is something that I rejected when I took over as editor at *AMP*. I know – awkward! I feel that practitioners do not know how to make sense of debate exchanges that play out in this tit-for-tat fashion. It seems to me that one scholar countering another only serves to confuse the reader, not enlighten. They all make good points, so who is to be believed? More to the point, this format allows scholars to frame the debate as they wish, often leading debaters to talk past each other. I created a new format called Constructive Confrontations (Barnett, 2025), in hopes of abating rather than endlessly debating. In this format, those holding opposing positions cooperate on a joint new study that they agree will provide resolution to some aspect of the debated issue. Perhaps some of the authors here, or others,

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would like to pursue that path. As editor, I cannot participate, but I sure as hell can encourage – so please consider it!

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Michael L. Barnett is Dean's Research Professor at Rutgers Business School and Editor of *Academy of Management Perspectives*. Mike's research focuses on the interrelationship between the sustainability of business, society, and the natural environment. His publications are available at profmikebarnett.com.

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