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Advocacy and the Search for Truth in Management Scholarship: Can the Twain Ever Meet?

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Abstract

Scholars have long debated the merits of advocacy-based research versus research considered from the quest for objective truth. Building upon reflections from multiple sources, a set of 11 brief reflections on three posed questions are presented. Tsang concludes our discussion with additional insights on how moving beyond the "interestingness" advocacy will be beneficial to the continued professional development of the management discipline.

Keywords

academic freedom, advocacy, interestingness, kindness, scientific discourse, truth

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Close to 200 years ago, the preeminent philosopher of science, Whewell (1847), stated that truth is the only viable end in science and that empirical science has both the capacity and duty to competently research the necessary or objective truth. At its core, this viewpoint remained dominant and basically unchallenged for many years. This perspective changed for many with the highly influential and widely cited advocacy by Davis (1971) for academic scholarship to primarily aspire to be novel and interesting. Tsang (2022) and Wright (2023) make the case that the focus of much research advocacy, especially politically-based in nature, can be highly detrimental to the continued professional development of management as a social science discipline.

Building upon Tsang (2022) and Wright (2023), the following 12 brief reflections by noted management scholars (including a follow-up reflection by Tsang) were presented with a choice of three questions to address pertaining to the truth and advocacy dilemma. The first question posed asks: Can our collective obsession with interestingness lead to dysfunctional research outcomes? Leading off, Kyle Emich proposes that we stop viewing our research through a highly competitive lens and switch to a collective sensemaking lens based upon a collective kindness framework. From the perspective of a journal editor and reviewer, Jone Pearce suggests that actively encouraging colleagues to critically think through and articulate why something is interesting constitutes an act of genuine kindness. Next, and through the use of personal example, Stratos Ramoglou competently and clearly explicates the foibles of granting sacrosanct status to interestingness to the relative exclusion of seriously seeking the truth.

The second question posed asks: Why do so many researchers obsess with the sole pursuit of the interesting? For Neal Ashkanasy, the true value of interesting must be considered from the framework of the level of expertise or knowledge of the members of the academic community. Using examples taken from evidence-based medicine and policy, Jean M. Bartunek, Sven Kunisch, and David Denyer contest the assumption that research rigor is necessarily opposed to the Davis (1971) notion of what is interesting. Through the use of example, Nicholai J. Foss and Peter G. Klein make the case that reliance on the interestingness criterion will certainly impede scientific inquiry in a number of ways. According to Sophia Town and John Hollwitz, an overreliance on Davis' (1971) approach to construct theories to supposedly stimulate reader interest can be counterproductive to the search for truth in our scholarship.

The third question posed asks: What happens when advocacy trumps the search for objective truth in our scholarship? Through the effective use of example, Chet E. Barney discusses a number of troubling negative consequences when politically based advocacy supplants the search for truth. For Peter Harms, many of the negative consequences of focusing on research that is novel, interesting and politically expedient, if left unattended, will be the end of management research as a legitimate discipline of science. According to Timothy P. Munyon, in order for science to continue to flourish, we must stand united to reject attempts to use coercive power tactics to foster compliance with prevailing ideologies. Using his research on the importance of character (cf., Seijts & Wright, 2021), Gerard Seijts supports the assertion that interestingness is not a virtue of good scientific theory, but rather is best considered as merely an accidental byproduct.

We conclude where we began and close with the thoughts of Eric W.K. Tsang. Through the use of examples pertaining to his work on superstitious decision-making, Tsang provides valuable insights into the interesting phenomenon. More specifically, while his research on Chinese superstitious decision-making behavior was not initially seen by colleagues as sufficiently interesting taken from Davis' perspective, Tsang found out firsthand that finding meaningful ways of breaking out of what he calls the interestingness “straight-jacket” approach opened up a number of new opportunities for future research endeavors.

As will become evident, each of the scholars raises very important questions and concerns for management researchers to seriously consider. At the core, do you believe that necessary or objective truth even exists, and if so, do you agree with William Whewell that this search for truth must be our research goal and form the basis of our scholarly endeavors (Wright & Wright, 2002)? These reflections follow in the order in which they were introduced above.

Posed Question 1: Will Our Collective Obsession With Interestingness Lead to Dysfunctional Research Outcomes?

We Build Truth by Being Kind

Kyle Emich

Tsang (2022) and Wright (2023) are correct. Optimizing counterintuitive interestingness, including the particular political interest associated with advocacy, is not just irrelevant but antithetical to the goals of establishing management science as a respected scientific discipline. They clearly summarize assertions backing this claim, so I do not rehash them here. Instead, I argue that these tendencies have caused and fed back into a collective egocentric (*breaking*) frame, which harms the field as a whole. Further, I suggest that switching to a collective sensemaking (*building*) frame, based on collective kindness, will help to alleviate many of these issues, although it will require substantial effort.

I refer to the current management science environment as existing within a breaking frame since it focuses on segmenting one's work from the remainder of the field. Although Davis (1971) certainly did not establish this frame, his piece positively did feed back into this trend. Our use of counter-intuitiveness as the central proxy for interestingness exemplifies this breaking frame. In reality, interestingness refers to an affective response determining attention. Seeing something unexpected is one way to generate this response but, as Wright (2023) notes, it is hardly the only way. As such, we reward scholars who come up with novel constructs and models (Emich et al., 2020). At one level, this makes sense since science grows from a more nuanced understanding of the world around us. At another level, it does not, because this understanding relies on establishing what we collectively know. Our current frame produces a strong force on the periphery of our field while neglecting the maintenance of its core. As such, it is difficult to assess how work builds on existing knowledge since we often do not know the state of our existing knowledge. From my own work, this could take the form of something as seemingly simple as how leaders should treat conscientiousness and proactivity when composing teams or how social emotions influence team processes (Emich et al., *in press*; Emich & Lu, 2023). Instead, researchers hope diffuse work will accumulate enough mass to advance their careers, while intermittent review articles and meta-analyses often simply acknowledge this heterogeneity. To remedy this, we need to stop viewing research competitively. We must understand that we are all in this together. Anyone's success is *our* success. We are trying to make sense of an important and complex multilevel open system. One person, or even a subset of people, cannot do this. It will require broader collective sensemaking.

The first step in this process is developing programmatic theory (Cronin et al., 2021), or determining what we consider settled science (cognizant this will change). This provides a strong core from which to build out. Importantly, this will take being kind: *being nice to each other*. We must understand that we are all working hard to balance knowledge development, knowledge dissemination, and keeping our jobs. So, politely make people aware of what they do not know (developmental opportunities), try to fix problems instead of identify problems, and take time to actively listen to younger researchers who have different perspectives (Kluger & Itzchakov, 2022). Look at colleagues, editors, authors, and dare I say even reviewers as teammates. To build knowledge, we need to follow the data (data is data is data), generate falsifiable hypotheses, test alternative hypotheses, and reproduce our results, as Tsang (2022) states. I am not saying we need to slack. I am simply saying that being rigorous is not antithetical to being kind. In fact, being kind is how we move forward to establishing and refining our understanding of what is true. We need to focus on *building* that understanding together.

Obsession or Kindness?

Jone L. Pearce

Tsang (2022) has developed a strong critique of what he characterizes as an obsession or cult in the fields of management and business with Davis's (1971) widely cited article. He helpfully provided a table (Tsang, 2022, p. 151) reporting that references to Davis's thesis are substantially more popular in business and management editorial essays than those in a variety of related fields. As someone who has found herself serving as an editor and reviewer, I would like to provide my own view of why I think so many journal editors in our field refer to Davis's article, even to the point, as Tsang (p. 156) notes of listing interestingness as a criterion for acceptance in the *Academy of Management Journal*. I want to propose that the reason for this emphasis is not the result of an obsession with interestingness over accuracy but in reaction to the many submissions editors and reviewers receive that are not useful to anyone, scholar or practitioner.

My argument is based on the one criterion of interestingness that Tsang did not analyze in his otherwise detailed analysis of Davis' language and logic:

…an audience will find a theory to be interesting only when it denies the significance of some part of their present 'on-going practical activity' (Garfinkel, 1967) and insists they should be engaged in some new on-going practical activity instead. If the practical consequence of a theory is not immediately apparent to an audience, they will respond to it by rejecting its value until someone can concretely demonstrate its utility: 'So what?' 'Who cares?' Why bother?' 'What good is it?' (Davis, 1971, p. 311)

As I have argued elsewhere (Pearce, 2004; Pearce & Huang, 2012) too much of our research has no practical value to either educators or practitioners. This is a long-standing complaint in our field, and I suspect that Davis's article was particularly attractive to our editors because we care about the usefulness of research in our applied field, and he made this point about usefulness by appealing to scholars' vanity and ambition. That is, Davis did not scold researchers about this failing as I and so many others have done but simply reported his own observations and let the ambitious draw their own conclusions. Editors see submission after submission that have taken the authors so many hours of labor and energy to produce something that is no good for anything other than a line on the author's CV; so, it is understandable that editors would send those seeking to submit papers for review to read Murray Davis's engaging analysis. He suggests that a little more thought about why readers should care about the implications for others' actions and provides suggestions to help them avoid wasting time and trouble.

In any piece as complex as Davis's article it is only natural for each reader to focus on what was most resonant to them. For me, it was not that Davis tries to articulate why certain theories in sociology have had more impact than others' theories (that may have been equally true or not true), but his articulation of how our scholarship must have some meaning for the actions of others. That advice about how to be clearer about how the research can have implications for readers' new practical actions can be wide: to spark a new direction for readers' own research, to spur a detailed critique of the wrong-headed paper that an unfriendly reviewer cites, or practical advice that we can use in our classrooms or work with practitioners. Good research and theory must have practical action implications for at least some other people. Warning junior colleagues to think through and articulate what those implications for others could be before committing themselves to a labor-intensive research project does not make journal editors or PhD advisors members of an obsessed cult, it is a kindness.

On Intellectual Fun and Seriousness

Stratos Ramoglou

Davis's (1971) paper was one of the key papers I had to study when I started my PhD. I still remember my surprise at Davis's advocacy of "interestingness" as more important than truth. Why would it matter if scientific knowledge is interesting or not? With little doubt, it is welcome if scholarly advances that improve our understanding of the world *happen* to be interesting as well. But why would *interestingness* itself be a relevant—let alone prime—criterion for the assessment of research outputs? My takeaway from Davis's argument was that, if he was right, and interestingness indeed trumps truth, we should be particularly suspicious of interesting research.

Unfortunately, this was not the main takeaway for management scholarship. Instead, the notion that contributions must be interesting has become something of an orthodoxy. Thankfully, Tsang (2022) affords a much-needed criticism of our scholarship's obsession with “the interesting,” complemented by Wright's (2023) brave reminder that we are in the business of *objective* truth—not personal “truths.” I would wish to augment Tsang's and Wright's efforts by calling attention to a problem associated with our obsession with interestingness; namely, the prevalence of an intellectual climate that rewards extraordinary and counterintuitive theories at the expense of mundane yet realistic understandings that are nevertheless closer to truth.

I first witnessed the distaste directed toward mundane truths in my first publication attempt. It was unsuccessful *not because* my theoretical solutions were not plausible, but *precisely because* they were *obviously* true. I remain puzzled. Why not value mundane explanations if their absence creates the misplaced need for far-fetched explanations? Why dismiss uninteresting yet solid knowledge claims, if this knowledge has been forgotten in our escapist intellectual excursions? I suspect that the “sacrosanct status” (Tsang, 2022, p. 156) that interestingness has reached in management scholarship is the main culprit. Our intellectual culture's fascination with interesting and counterintuitive claims does not only make us discount the value of realistic reminders; even worse, I am afraid that it also favors the popularity of patently unrealistic theories. Consider for example the notion that entrepreneurs may possess some special genetic makeup (Ramoglou et al., 2020), the thesis that opportunities are observable entities (Ramoglou & Tsang, 2016), or the idea that entrepreneurs are world-makers (Ramoglou & Tsang, 2017).

All these perspectives emerge by turning a blind eye to fairly commonsensical ways of thinking about the world. The idea of a hidden “entrepreneurial gene” emerges against the backdrop of a theoretical picture in which entrepreneurial action is seen as a result of the causal interplay with “opportunity entities” (Shane & Venkataraman, 2000). In turn, it is the assumption that “nonentrepreneurs” must lack “what it takes” to “respond” to such “observable opportunity entities” that sustains the myth of a unique entrepreneurial makeup. Yet, a moment's sober reflection suggests that entrepreneurs *choose* to exercise action—they do not respond to supposedly existing “opportunity entities.” This means that it is fallacious to suppose that so-called “nonentrepreneurs” lack some “special genetic makeup”. They may—quite simply—have good reasons to doubt that what *appears* to be an opportunity may be nothing but wishful thinking. For, when economic actors contemplate opportunities, they are attuned to the fact that they are discussing *uncertain possibilities*—not readily detectable entities such as tables or stones (Ramoglou & McMullen, 2022, 2023). Moreover, the notion that entrepreneurship may be an act of “world-making”—in the sense that “complexity is a function of the mind and not the world” (Alvarez & Porac, 2020, p. 739)—runs contrary to the patently obvious truth that we live in an incredibly complex and hard to comprehend world. It also denies the truism that what our minds *believe* to be possible is simply irrelevant, if the complex array of real-world conditions is not "there" to allow the actualization of desirable worlds (Ramoglou & McMullen, 2022).

That said, I can understand that counterintuitive theories are not as "boring" as painstaking logical or empirical analyses can surely be. Views according to which there are no objective limits to what entrepreneurial agency can achieve, and that the world is a shell in the hands of crafty entrepreneurs are surely exciting (Brattstrom & Wennberg, 2022). But they are intellectually unserious. By the same token, few would accuse conspiracy theories of being "dull." Take, for example, the conspiracy theory about Bill Gates' role in the recent pandemic, according to which he caused it to control humanity by implanting chips into people (McVeigh, 2022). That is an easy theory to digest, a theory that provides the intellectual satisfaction of being able to understand what are, in reality, highly complex and scientifically demanding matters. As recently put by Gates himself, "Malevolence is a lot easier to understand than biology" (in Blanco, 2023). I am afraid that our field's obsession with interestingness may have opened the gates to equally counterintuitive yet no less preposterous theories. I confess that I am petrified by the idea that our academic field may be transforming into a space of intellectual "fun"; that is, from a scholarly domain where the search for realistic and correct understandings reigns to a space in which we can willfully escape the constraints of reality and entertain ourselves.

Let us embrace Tsang's and Wright's brave efforts and spoil this party. For the music of this party, fun as it may be, is not in tune with improving our understanding of our world. And, in a world suffering from poverty, inequality, war, and climate crisis, the light of hope can only be kept alive by reason and realism—away from the intellectual escapism fueled by the obsession with "the interesting."

Question #2: Why Do So Many Researchers Obsess With the Sole Pursuit of the Interesting?

What Do We Mean by "Interesting?"

Neal Ashkanasy

In this commentary, I focus on the nature and definition of the word "interesting," and discuss some of the implications that flow from this analysis. Davis (1971, p. 311) states that "the defining characteristic" of an "interesting theory" is that it is "engaging" in as far as "it stands out" as being "in contrast to the routinized taken-for granted world of … everyday life," and therefore represents "an attack on the taken-for-granted world of their audience" or "assumption ground." More recently, Tsang (2022, p. 154) challenged this definition insofar as he believed it does not define the term "assumption ground." Tsang argued further that Davis presents confusing and flawed arguments in support of his position. This is something that has long worried me, too, but from a different stance to that taken by Tsang. Like Wright (2023) I challenge the paradoxical "orthodoxy" of assuming people necessarily understand commonly accepted words like "interesting" and "engaging." This notion is paradoxical because the very challenge itself makes broad assumptions about the meaning of commonly used expressions like "interesting."

So, what does it really mean to say a theory is "interesting?" In fact, what Davis (1971) was referring to is a reader's *instinctual* responses to a writer's argumentative presentation; a kind of "gut feel." Thus, while a reader's sense of "engagement" (with a theoretical position they read in a published article) is essentially intuitive, the accuracy of their judgment depends upon whether their "attention" was a product of heuristic-based or experience-based intuition (Kahneman, 2013). In this case, the validity of Davis's (1971) argument assumes that the readers of scientific journal articles are experienced and qualified sufficiently to make an experienced-based judgment. Tsang (2022) and Wright (2023) appear to disagree with this assumption; and make a good case that they are correct in disputing this assertion. Tsang argues that "Interestingness is not regarded as a virtue of a good scientific theory" (p. 150), while Wright extends this idea by adding that "political advocacy" further skews the notion of interestingness away from the essential tenets of scientific rigor.

I argue here that the criticisms offered by Tsang (2022) and Wright (2023) are not necessarily always valid. Thus, while *heuristic intuition* may indeed be prone to the kinds of "unscientific" biases discussed in Tsang and Wright's critiques, *expert intuition* maybe not so much. Thus, experts in a particular discipline can temper the arousal they experience when they read an "interesting" theoretical position, even if their initial response suggests to them that the idea looks to be an exciting new theoretical development. Since an expert reader's intuition is based on expertise rather than heuristic "rules of thumb," she is more likely to question any new assertion that does not "look right."

This is the point made by Bartunek and her distinguished coauthors (2019) in their strident defence of Academy of Management President McGahan's (2019) decision to engage in political advocacy following President Trump's controversial EO 13769. Bartunek and her colleagues argue that McGahan's decision should "stimulate development of previously established conceptual perspectives" (p. 251) among the *knowledgeable* members of the Academy whose expert intuition and sense of arousal leads them to question established shibboleths (such as the Academy's tradition of shying away from issues that may be interpreted as a form of political advocacy).

In conclusion, the position I take is that Davis's (1971) stipulation (that research must be "interesting" to constitute a substantive contribution to the literature) must be understood within the broader context of the "expert" scholarly community it was directed to. Thus, while some (less expert) colleagues may be prone (via heuristic intuition) to accepting "interesting" theoretical positions uncritically, others (the experts) will use the idea to stimulate further theoretical development and innovation.

The Interdependence of Rigor and Interest

Jean M. Bartunek, Sven Kunisch, and David Denyer

In management research, *rigor* is often assumed to be in opposition to Davis's (1971) notion of what is *interesting*. We contest this assumption using the exemplar of *review research*.[1]

Davis argued that to be impactful, theories should be *interesting* to their *audiences*. He defined interesting as challenging some audience assumptions. Davis did not have much to say about social research (except that a good deal of it is dull, and certainly *not* interesting), and he definitely did not discuss review research. Nevertheless, his approach opens up important questions. Who are the audiences for management research and, particularly, review research? Is research like systematic reviews, the ultimate in rigor, uninteresting by definition?

The foundations of review research in management can be traced to evidence-based medicine and policy, as well as meta-analysis (cf. Kunisch et al., in press). With regard to evidence-based medicine, Archie Cochrane, a British Physician, wanted to distinguish the types of tuberculosis his fellow prisoners in a German prisoner of war camp during World War II were suffering, to treat them properly. The early development of evidence-based management was an attempt to assist policy and practice approaches the U.K. government was taking. Meta-analysis was fueled considerably by Professor Eugene Glass's desire to substantiate his belief that psychotherapy could be successful. The audiences for these initiatives included people whose interests were in effective practice and research. The intent was to collate and synthesize research in ways that would rigorously settle questions for each audience, not raise new ones. Over time, systematic reviews have gained well-deserved recognition as ways of supporting the rigor, comprehensiveness and trustworthiness of scholarly findings for practice and theory.

Outcomes of review research are not expected to be *interesting* in Davis' sense, especially for practitioners. Physicians can be successfully sued for making medical decisions based on theories that challenge assumptions, but that are not supported by rigorous scholarly evidence. In such cases, problematizing assumptions seems the antithesis of rigorous systematic reviews. Of course, some assumptions of medicine have benefited from problematization. Until the 19th century, challenging bloodletting as an effective way to cure disease would have been thought absurd. Undoubtedly, there are equivalent assumptions held today.

Thus, we suggest recognizing *rigor* and *interesting* in research as comprising a *duality*, in which the apparently opposing elements do not compete with each other, but are interdependent, in a both/and relationship (Putnam et al., 2016). Rigorous research requires testing assumptions systematically. Being interesting requires rigorous challenges that can be recognized as credible, even if surprising. Further, such assumptions and challenges may differ substantially for different audiences such as academics and practitioners.

Consider a recent meta-analysis (Peng et al., 2021) showing that the relationship between transformational leadership and support for change is more positive in articles published in lower-tier journals than in higher-tier journals. For which audiences is this interesting, and for which does it matter?

In fact, recent advances in review research (cf. Kunisch et al., in press) demonstrate how interdependent rigor and interest are. Well-conducted systematic research reviews challenge some of Davis's tacit assumptions about challenging assumptions. At the same time, Davis's work highlights the crucial importance of interesting assumptions as a foundation for systematic review research. How has everyone missed how mutually beneficial the interdependence of rigor and interest can be?

"That Is Interesting" and the Scientific Process

Nicolai J. Foss and Peter G. Klein

We agree with Tsang (2022) that the interestingness criterion proposed by Davis (1971) can hold back scientific progress. Like Wright (2023), we are also concerned that the emphasis on interestingness can serve as a cover for introducing activist politics into management research. However, our main concern is that the interestingness criterion for assessing the value of a contribution can "jam" the process of cumulative learning that is the hallmark of a scientific field. Not only does it downplay the quest for clarity, insight, and truth as the purpose of scientific and scholarly activity, but also it discourages deep and thoughtful engagement with the subtlety, complexity, and nuance of important phenomena in management. It also promotes faddishness by linking the quality of research findings to currently fashionable topics, methods, and findings, not only within scientific communities but within the larger culture (including social and political trends, as discussed in Wright, 2023).

Management research, like other fields of inquiry, is a cumulative process that progresses via theory development, testing (both for logical consistency and empirical explanatory power), critique, and refinement. It can be understood as a process of *systematic error correction* through continual dialogue, discussion, criticism, and evaluation, a view famously articulated by Charles Peirce (Burks, 1946). Of course, as Kuhn (1962) and others have emphasized, this process operated within a larger set of assumptions—typically not tested—about what questions can be asked, what methods can be used, and so on. The process is far from perfect, and the evolution of science frequently manifests both Type I and Type II errors, partly for institutional reasons (e.g., how the evaluation process is organized and funded, how many hierarchical layers are needed to evaluate a contribution before it is accepted or rejected Sah & Stiglitz, 1986, the career concerns of scientists, and so on) and partly due to the bounded rationality of evaluators.

Evaluating scientific contributions is a complex task, not the least because of the many criteria involved such as falsifiability, internal and external consistency, simplicity, rigor, and fertility. Tradeoffs may exist among these criteria and it is not always obvious how the different criteria should be weighed. The emphasis on novelty exacerbates these problems. Davis was hardly the first to observe that ideas take hold for reasons other than scientific merit; the concept of the "growth of knowledge" (and the related literature in the philosophy of science; Lakatos, 1978; Popper, 1935) tends to privilege novel claims. What Davis added is the idea that novel ideas should also be interesting, even exciting.

Unfortunately, the interestingness criterion jams the process of error correction in science. Not only does it weaken the selection environment, but it also affects the variation and heredity side of the evolution of knowledge by prioritizing differentiation, incentivizing scholars to dress up otherwise mundane research as flashy and counterintuitive, and complicating the task of evaluating such research. The emphasis on novelty, excitement, and even surprise to stand out—as well as the common requirement at many journals that empirical papers also make a "theoretical contribution"—has likely contributed to the explosion of constructs and labels, mechanisms, and techniques over the last few decades, much of which has been adopted or promoted by self-styled "communities" (sometimes with their own standards of evaluation). The resulting complexity has further contributed making the process of evaluating knowledge claims.

This development also explains why the management literature includes fewer replications, reviews, meta-analyses, and shorter papers than other scientific fields—these are less "interesting." More mundanely, it explains the proliferation of "interesting" (often quite strained!) paper titles that use, for example, titles of popular songs to capture attention.

In sum, while we recognize the need to present and frame research in a way that highlights its potential contribution—and certainly do not advocate boring research as an ideal!—we have the same reservations about the "that's interesting" effect as Tsang (2022) and Wright (2023). While often styled (particularly in doctoral education) as a clever way of thinking about marketing one's research, the widespread adoption of interestingness as a criterion has problematic longer-run consequences. In terms of the evolutionary metaphor of the growth of knowledge, it reduces the proper selection for true claims to knowledge, leads to loss of memory within the disciplines and fields, and introduces variation that taxes the bounded rationality of scholarly assessment. Three cheers for the mundane!

The Paradoxes of Interesting Theory Building

Sophia Town and John Hollwitz

Davis's (1971) model for theory-building—and, consequently, what constitutes *truth* in management research—purports that "great" research is "interesting" research, and that interesting research must be counterintuitive. This view is canonical, a point Tsang (2022) laments in his thoughtful assessment of the risks that Davis's approach poses to scientific inquiry. We applaud Tsang (2022) and highlight three paradoxes of "interesting" theory construction that invite further discussion.

Paradox #1: Good Old Intuitiveness Is Interesting, too

Contradicting an audience's expectations resembles a formula for comedy dating back to Aristophanes. However, something's comedic value, that is, the interest it spurs in the audience, generally derives from two sources: (a) *contradicting their expectations* or (b) *affirming their beliefs.* Consider the 1933 Marx Brothers movie, *Horsefeathers*, a rollicking sendup of college faculty life. In the opening act,[2] the character Groucho leads a chorus of men draped in academic regalia in performing the song, "Whatever it is, I'm against it!" Many senior faculty who have served on academic committees can recall instances when that service has been relentlessly political and at times comedically contentious. Groucho's parody posits a theory of academic life: that academics are stuffy, pompous, and habitually dissident. The movie's popularity among professors suggests that people find the movie interesting and that they recognize something of themselves in it. Its intuitiveness spurs its interest. According to Davis, audiences would not be interested in the intuitive; thus, *Horsefeathers* would not be funny. Yet 90 years of audience engagement suggests that it is.[3] Both "counter-intuitiveness" and "intuitiveness" can capture people's interest.

Paradox #2: Using Interpretivism to Argue for Postpositivism (in Service of Epistemological Narcissism)

When Davis (1971) proposes a "sociology of phenomenology and a phenomenology of sociology" he creates a false distinction between *phenomenology* and *ontology* and neglects the *epistemological* grounding of his argument. Davis is concerned with accessing objective (what he considers real) truth. He refers to this as *ontology* and positions it against subjective (what he considers unreal) truth, which he refers to as *phenomenology.* However, these are crude categorizations that fail to capture the essence of these methodological concepts. For ontology, a more precise definition is *the nature of reality* and, for phenomenology, *the exploration of direct and conscious experience* (Craig & Muller, 2007). By using *ontology* as a proxy for objective reality as opposed to its more accurate use as a way of perceiving reality (including the multiple kinds of realities accessed by various research paradigms), Davis reveals but fails to acknowledge a *postpositivist* ontology to his argument. Postpositivist scholars perceive reality as *a priori* and independent of the observer. Alternatively, phenomenology stems from an *interpretivist* ontology in which reality is subjective. These are two different paradigms, which Davis fails to address.

More concerning, however, is Davis's confusing *epistemology*, that is, his conception of the nature and purpose of knowledge (Anderson & Baym, 2006). For Davis, this purpose appears to be objective truth. However, he encourages an audience-focused approach to theory (and therefore truth) construction—a perspective we find epistemologically problematic. In fact, Davis' argument poses an ontological and epistemological paradox: *How can we access objective truth by searching for ways to construct theories that contradict each other in the imagined eyes of our future readers?* Davis claims ontological post-positivism by recommending a highly interpretivist approach—albeit an approach that keen interpretivists would likely eschew. Tsang (2022) reports that management researchers are particularly tempted by this approach. As scholars in the field of business, we believe this may be due in part to our field's profit-focused paradigm, as opposed to other fields' science-focused paradigm. The result? Paradoxical research that results in, and from, an epistemology of narcissism.

Paradox #3: The "Pursuit Paradox"

We propose that scholars embrace what we are terming a *pursuit paradox*. In his assessment of Davis (1971), Tsang (2022) makes clear that "it has never been my intention to promote non-interesting or boring research" and in a parenthetical sidenote admits, "other things being equal, interesting research is certainly better than boring research" (161). In other words, there may be value to interestingness—so long as it is not the goal. To make sense of this contradiction, we turn to the practice of mindfulness which is, in and of itself, replete with paradox. In meditation circles, novice practitioners are taught that letting go of their goals is the only way to achieve them; moreover, letting go is both the outcome of success, and the path to it (Wright, 2017). This paradox can be found in the oft-cited joke, "you are perfect just as you are … and, there is always room for improvement." We see a similar pursuit paradox with doing research that is both good and interesting: *Do research (X) for quality (Y) and you will get both quality (Y) and interestingness (Z) but only if you do not focus on getting interestingness (Z). However, do X for Z and you may get Z but you will likely not get Y.*

In conclusion, for 50 years, management scholars have followed Davis's (1971) advice to construct theories that pique the interest of imagined readers. As Tsang (2022) notes, a reliance on this advice can serve more as self-aggrandizement than as a search for truth. This advice is grounded in an epistemology of narcissism, one that threatens the robust scientific inquiry that we management scholars are eager to defend. This temptation has resulted in several paradoxes—at least two of which problematize Davis's approach and one that offers (an admittedly counterintuitive) path forward.

Question #3: What Happens When Advocacy Trumps the Search for Objective Truth in Our Scholarship?

Will Academic Freedom Continue to Exist in the Era of Politically Based Advocacy?

Chet E. Barney

As a young undergraduate 20+ years ago, the idea of academic freedom was presented to me. From what I experienced in my old college classrooms, academic freedom could be seen as university professors presenting historical facts intertwined with their own ideas about the designated topics. During that era, those professors would then encourage students to go forth and seek the truth about that particular topic before formulating their own personal beliefs. This type of teaching is what attracted me to academia. Back then, I was challenged by my professors to study, investigate, and think critically so that I could both learn the assigned materials as well as form my own sentiments on those topics. Fast forward more than a couple of decades to today. I now stand at the front of the lecture hall, imploring students to use facts and research before forming their own opinions … but it appears that times have changed since I was a young undergraduate. During class lectures, I encourage open dialog about organizational topics, and during those discussions, I can tell that students have personal opinions on certain issues (e.g., diversification of teams, power and influence, etc.), yet most students remain silent … even those students who are generally openly vocal on a regular basis. However, I do not think I realized why students shied away from speaking up regarding certain subject matter until more recently and after reading Wright (2023).

Wright (2023) presented a scenario about truth and the science of management where he had taught a class in which the students refused to simply define the words “bias,” “prejudice,” and “stereotype.” It seems that the students were pre-conditioned to believe that by refusing to discuss these terms (by remaining silent) they would actually be demonstrating personal anti-racism. Wright successfully taught that particular group of students that by simply defining those specific terms, one does not advocate racism. In fact, an open dialog might actually add to the descriptive learning on the subject matter.

For those of us who have been in academia for a while, we have seen a cultural shift away from people having open dialogs in the classroom and during academic meetings. Perhaps all too often we are seeing an attempt to not offend others to the point that it discourages faculty, staff, and students from openly discussing interesting topics that could potentially have opposing viewpoints. For example, in a not-too-distant past meeting that I attended, the topic was brought up about the shift away from the long-standing Academy of Management (AOM) policy to remain politically neutral (AOM, n.d.). The AOM was seemingly becoming a politically visible organization with the president of the AOM publicly expressing political beliefs on behalf of the organization (Bartunek et al., 2019). The overall consensus in my meeting was to simply not get involved in a public discussion regarding academic organizations advocating for political topics. If this is where we are at in academia right now in our meetings, are most academics also teaching students that open dialog might not be the best idea?

In my mind, the differing viewpoints of authors such as Davis (1971), Tsang (2022), and Wright (2023) are what makes academia great. In fact, this is what academic freedom is all about. If we are able to have discourses on various subject matter, such as what makes “great” or “interesting” theories, discussions could unfold leading us into much richer learning experiences. Would that help or hurt academia? One of my vocational concerns is that new and emerging academic professionals might limit their research, classroom topics, and public discussions of *relevant* subject matter simply because they fear that their careers might suffer by expressing academic freedom. What would the future of academia look like if we were to once again encourage students to go forth and seek the truth? Perhaps we will never know.

There's Nothing Magical About Make-Believe Science

Peter Harms

The prioritization of interesting research over that which is accurate or practical (Tsang, 2022) and of political values and ideas over scientifically-grounded, but uncomfortable research (Wright, 2023) represent critical threats to the organizations literature, but they are not the only ones. It is also necessary to acknowledge the proliferation of misleading research in our discipline and take steps to counter it. Surveys of lay people suggest they are losing their faith in science. And why shouldn't they? The combination of politicized research, academic misconduct, and the widespread failure to replicate gives them more than enough reason to question us.

Rather than incentivizing rigorous and relevant research and the development of critical-minded and competent scholars, our publish-or-perish culture has become coupled with demands from journals and institutions for media-friendly headlines and results (Harley, 2019; Ledgerwood & Sherman, 2012). At the same time, we see efforts to justify the usage of quick and dirty studies utilizing questionable samples (e.g., Walter et al., 2019) and equally questionable measures (e.g., Matthews et al., 2022). These trends, along with the increasing requirements for ever more complicated models have resulted in a field that is increasingly dominated by published findings that are unlikely to be replicated and even less likely to inform organizational practice (Saylors & Trafimow, 2021). The hard work of refining research over time, using robust multimethod approaches is forgotten. Instead, we get special issues of journals centered around buzzwords like "paradox theory" and filled with methodological artefacts employed to document the exceedingly unlikely (e.g., humble narcissists). Junior scholars who successfully master these strategies are more likely to publish in top-tier journals, win awards, get invited to present at paper-writing PDWs, and go on to jobs at prestigious institutions where they too can train others that the best way to get ahead may be by cutting corners, creating ever-increasing populations of scholars generating questionable research (Smaldino & McElreath, 2016; see also Cortina, 2019; Gupta & Bosco, 2023; Tsui & McKiernan, 2022).

How prevalent is this problem? At a recent editor panel at SIOP, the head editor of a premier journal suggested he believed that misreported results were relatively unimportant because they would be swamped by other studies in future meta-analyses. This shows a lack of understanding how persistent newly introduced ideas can be, even if they are critically flawed (Greenwald et al., 1986). Look no further than the unfolding implosion in the field of leadership research to get a clearer picture of the consequences of building a literature using poorly validated measures, shaky theories, and questionable reporting of results (Atwater et al., 2014; see also Alvesson, 2020; Gottfredson et al., 2020; Van Knippenberg & Sitkin, 2013). The conjecture that a few bad studies will be swamped by the many good ones is dependent on most researchers reporting honestly and accurately. Yet reviews of reported results in top management journals repeatedly find that the rates of misreporting of critical statistical information such as p-values and fit statistics are shockingly high (Credé & Harms, 2015, 2019; Harms et al., 2018). Although some of these errors could be excused as resulting from statistical incompetence or simple computational error, many such errors are demonstrable mathematical impossibilities.

One possible avenue for course-correction would be to follow other disciplines and embrace post-publication peer-review as a means of detecting and removing problematic articles (Harms et al., 2018). However, many editors seem resistant to relinquish their roles as being the final arbiters of truth or being forced to admit that they have made poor decisions. Perhaps nothing better illustrates this than an interview on Retractionwatch.com where the head editor of a premier Management journal suggested that the reputations of fellow scholars should be prioritized when the accuracy of their research is questioned because, unlike in the medical field, misreported results in the organizational literature are not likely to have significant real-world consequences. Another prominent editor decried individuals who publicly raised concerns about articles after publication on blogs as being "self-appointed data police" and engaging in "methodological terrorism" (Gelman, 2016). These public statements by important gatekeepers send a dangerous signal to new researchers and serve to undermine faith in our field. The relative lack of retractions and corrigenda in management is not a sign of a healthy discipline, but rather a sign of a metastasizing and unaddressed disease.

But, as pointed out, editorial policies at our major journals continue to call for the novel, interesting, and politically expedient rather than work that is accurate, robust, or of practical value. This is the perspective of a privileged class that has lost touch with the ultimate goal of management science as a discipline. We are not meant to be ruthless careerists, generating endless streams of unreadable papers full of unreplicable results. Rather, our focus should be on facilitating a more productive, more cohesive workplace, making our work accessible and practical to those outside the academy. And it is critical that we hold ourselves and others to the highest ethical standards.

Conversations Unsaid: Coercive Power and the Erosion of Scientific Discourse

Timothy P. Munyon

Science has always functioned within the dominant power paradigms of its day. This is illustrated in the account of Galileo and the Roman Catholic Church. Galileo—often characterized as the father of modern science—developed new telescopes and an experimental approach supporting the heliocentric perspective of Copernicus. At the time, the Church advocated for a geocentric position where the heavenly bodies orbited the earth.

Interestingly, Galileo was also friends with Pope Urban VIII, who suggested that Galileo discuss his model as a hypothesis, but not as fact. Galileo compromised with the Pope (apparently an early postpositivist), but also penned a book in 1632 that extrapolated his findings and embarrassed the Pope (who he referred to as "Simplicio"—an Italian play on words meaning "simple-minded"; Reville, 2020). As a consequence, the Roman Catholic Church offered him the opportunity to recant his position or face death. Not surprisingly, Galileo formally recanted and was spared—albeit in house arrest—until his death in 1642. Since then, Galileo has been vindicated, and the Roman Catholic Church offered a formal apology in 1992, but the incident cites an important use of coercive power as a constraint on science.

Coercive power is one of the five original bases of power proposed by social psychologists French and Raven (1959). They defined power as influence manifesting "changes in behavior, opinions, attitudes, goals, needs, values, and all other aspects of the person's psychological field" (p. 260). Coercive power is rooted in the ability of an actor to punish another, up to and including the destruction of that individual. In his commentary, Wright (2023) illustrated how coercive power has been used to encourage political advocacy and limit scientific discourse.

First, Wright highlights how coercive power censors scientific discourse, including troubling instances where scholars suffered physical harm or professional injunction because they presented rigorously conducted evidence viewed as offensive by others. Ironically, this coercive censorship undermines the development of alternative theoretical explanations, functional replications, and generalizability tests that could potentially refute offensive evidence.

For example, in 2020 the *Strategic Management Journal*—one of management's top journals—accepted a paper whose original theory and hypotheses were rooted in racist logic. Although the paper's empirics appeared robust, the online pre-publication version of the paper contained racist overtones that obscured its potential contributions, and the journal was asked to reconsider publication of the paper. However, rather than retract the paper, the journal allowed the authors to remove the inflammatory language and publish an updated manuscript.

Rather than incite or inflame racist rhetoric, the controversial paper has stimulated additional research, including challenges of its original conclusions in other top tier journals, including research in the *Strategic Management Journal* (e.g., Jeong et al., 2023). Indeed, the primary conclusions of the paper have been forcefully refuted by subsequent empirical research. Thus, although coercive power could have been used to retract the paper, civil discourse and robust science helped refute racist stereotypes and prejudice.

Unfortunately, coercive power can also limit scientific discourse in more subtle ways, including eroding psychological safety. For example, recent empirical evidence suggests that a majority of Americans engage in self-censorship for fear of isolation or retaliation (e.g., Burnett et al., 2022), and the costs for those who do speak up are significant, including “cancelation” in academia (see Stripling, June 21, 2023 for examples). The resulting lack of discourse can lead to false consensus effects.

This point was highlighted in 2022 when a petition opposing the U.S. Supreme Court's *Dobbs v. Jackson Women's Health Organization* decision was widely distributed throughout the *Academy of Management*. The Supreme Court's decision argued that abortion is not a constitutional right and relegated abortion right decisions to individual states. The petition asked the *Academy* to formally oppose the decision and also avoid holding meetings in any state where abortion rights are unavailable to women. Five hundred and ten members eventually signed it. Interestingly, all but 11 petition signatories included their names and institutional affiliations, and many included their statuses as fellows of academic associations or editors of leading *Academy* journals.

Yet rather than stimulate discussion and rigorous empirical inquiry, little to no discourse has occurred since the petition was distributed. Abortion is a polarizing topic in the United States, with roughly half of the population for and half against the practice (Gallup, 2023), and the risks of engaging with this topic have arguably limited its investigation by the field of management. The *Academy of Management* made no official position on Dobbs following the petition, even as no Academy meetings have been held in states where abortion rights are limited, nor are there plans to hold meetings in these states in the future. Similarly, this year's 2023 Academy Annual Meeting Program featured no discussion of Dobbs, and only seven sessions discussed women's rights in general. Thus, rather than encourage discourse and rigorous empirical inquiry, the field has been crippled by self-censorship and a lack of psychological safety.

Like Galileo and others before, modern scientists face an environment in which discourse is limited and potentially dangerous (see Ekins, 2020 for discussion), and political advocacy threatens to exacerbate these tensions. Although some academics hold what may be considered offensive and extreme perspectives, science is advanced when these perspectives can be civilly discussed, debated, and empirically tested. So how can we restore a psychologically and physically safe environment conducive to discourse?

First, it's far too easy to tell students what to think, rather than how to think. As academics, we need to fairly present multiple angles of an issue and allow students to draw their own conclusions. This implies that we will have to take on topics with greater depth, which necessarily trades off against the breadth of topics that one can discuss in the confines of class. However, advances in critical thinking would seem a fair trade.

Second, universities should begin to sponsor public debates and protect participants, including stringent policies on codes of conduct for students and other attendees. Coercive power often thrives in ambiguous and unprotected environments, and the proactive use of legitimate power is an important hedge ensuring discourse can occur.

Third, as scientists, we must be cautious not to depersonalize our subjects. Depersonalization is a process through which human traits are removed from individuals. Although we often present findings that are depersonalized, we must also recognize the potentially injurious consequences of our findings and communicate in a manner that maintains the dignity and respect innate for all humans. For example, in the *Strategic Management Journal* article cited above, the authors erred in their original manuscript by depersonalizing the black subjects of the paper. By contrast, the compassionate and proactive communication about subjects helps improve discourse by humanizing those we study. It also reduces the motive to use coercive power.

Scientists have always faced pressure to conform to the prevailing dogmas of the day, including political ideology. Yet, science also has the potential to powerfully bind humanity together and guide us toward greater insights about the world around us. However, civil discourse is the necessary condition to realize these gains. We can do better.

The Short-Sightedness of Sticking With the Interesting Research Advocacy

Gerard Seijts

I read with great interest Eric W.K. Tsang's (2022) insightful essay as well as the additional thoughts offered by Wright (2023). I believe both scholars raised valid points on how the article by Davis (1971) has the potential to contribute to detrimental outcomes in the pursuit of scientific research. I would like to offer some additional thoughts—in support of Tsang and Wright—to encourage further discussion on this important topic. I agree with their assertion that interestingness is not a virtue of good scientific theory—and, rather, should be seen as an accidental byproduct, not an intended outcome.

I am a strong believer in the science—practitioner model. For example, at my institution, the Ivey Business School, our brand mantra is real world leadership. While this may be broadly interpreted by my colleagues, the central tenet to which we adhere is to ensure that our stakeholders—from undergraduate students to senior executives—understand the context and application of research discoveries. It also speaks to actionable research: scientific discoveries that are seen as practical or useful in addressing business challenges.

For example, the global financial crisis of 2008 was a powerful demonstration of the importance of character in leadership. Regretfully, until that time, character's vitality to leadership excellence and success was not prominently featured in the leadership discourse or literature. Thus, together with several colleagues, I established an Institute in 2010 around leader character in order to generate research that was both rigorous by academic standards and relevant to the practice of management. As such, we invoked an engaged scholarship approach to ensure the voice of practitioners was captured in our studies.

However, at no time did we start our research program with the idea that it should somehow be interesting and different—that is, to deny old truths and challenge taken-for-granted assumptions. We sensed that the global financial crisis provided a critical opportunity for business schools to reevaluate their role in teaching leadership and in developing leaders for the public, private, and not-for-profit sectors. For example, at my institution, there was a lot of reflection on and examination of the changes required on how we educate leaders today to ensure that they make a more positive difference in the world tomorrow. We believe it was and continues to be a timely and relevant challenge. For if we fast forward to today, it is hard to miss the relevance of character in leaders and citizens alike in addressing the COVID-19 pandemic and other global crises. The pandemic, in particular, has not only starkly displayed the character of leaders, but research also exposed its critical role in their success or failure.

Character education used to be a vibrant part of academic institutions. Over a century ago, most university administrators and faculty members would have said that cultivating students with an integrated sense of self was their most important task. Yet somehow, we let that go. It has been a challenge to publish our work on leader character—which we believe is based on good scholarship—in leading academic journals. Of course, I realize that our work may get rejected for various reasons, but the idea that our research is not novel or interesting, as some scholars communicate to us in their reviews, jars and frustrates me. This is because we have inarguably found that organizations in the public, private, and not-for-profit sectors see our work as strongly applicable. So, indeed, character has been within the academic arena since Aristotle, but never before has it been researched precisely for its application to areas such as strategy development, culture-building and corporate purpose, executive recruitment and development, EDI, risk management, and other key corporate activities. Also, if we take a more existential view, character has been generally accepted as a foundational component of the human condition, but never before has the character of individuals, organizations, or societies had to contend with such deep and converging global crises—the pandemic, climate change, social and economic inequity, to name a few. In many ways, character is slightly paradoxical in that it is perennial and constantly evolving at the same time, and yet, it is because of this very fact that it requires perennial and evolving research to remain relevant and applicable within our current context.

Many scholars have been reflecting on our impact as a profession, bemoaning the fact that research appears to have little influence on, or relevance to, the world of practice. The article *On the road to Hell: Why academia is viewed as irrelevant to practicing managers* by Gioia (2021) in *Academy of Management Discoveries* is just one of several articles published in management journals that challenges us to do better—to reconsider our basic assumptions about what we do and how we do it.

Maybe too often organizational researchers are obsessed with interestingness and as such predominantly chase the shiniest new object—the one that is bound to attract attention. I believe Bandura (2005) got it right. He argued that among the most important criteria for evaluating a theory is social utility. He wrote: "… surprisingly little attention is devoted to their social utility. For example, if aeronautical scientists developed principles of aerodynamics in wind tunnel tests but were unable to build an aircraft that could fly, the value of their theorizing would be called into question. Theories are predictive and operative tools. In the final analysis, the evaluation of a scientific enterprise in the social sciences will rest heavily on its social utility" (p. 31). Of course, as Gioia articulated, we want to be inspired by new ways of seeing things. But this should never come at the expense of providing usable knowledge.

Moving Beyond the "Interestingness" Advocacy

Eric W. K. Tsang

I was thrilled when the associate editor, Thomas A.Wright, told me that he planned to invite scholars to contribute curated pieces on the topic of advocacies in management research in general and the "interestingness" advocacy in particular. I have benefited from reading the curated pieces even though I may not agree with all of the ideas presented here. In this essay, I attempt to supplement the discussion by briefly answering this question: If interestingness is no longer a key objective for researchers to aim for, what kind of research would benefit the development of our field?

One natural answer is that researchers should work on projects that produce useful results for guiding managerial practices especially because there have been criticisms that management, as a practical subject, fails badly in this respect (Gioia, 2021). While these are valid criticisms, it should be noted that there is a rough distinction between pure and applied research in science and both are needed for a healthy development of a scientific discipline. Unlike natural science subjects, it may be difficult to conceive pure research in management. One interpretation of pure research that I propose is to investigate important yet underresearched management phenomena with the objective of understanding the phenomenon per se. The results of such research may or may not generate any managerial implications.

A good illustrative example is my study of superstitious decision making. When I was working at HSBC, Hong Kong, before switching to my current career, some of my clients admitted frankly that they sometimes engaged in superstitious activities, such as consulting fortune-tellers or praying in a temple, in order to improve the quality of their strategic business decisions. In fact, this has been a well-known practice in Chinese business communities worldwide and the phenomenon is surely important given the crucial role played by private Chinese firms in Hong Kong, Taiwan, Southeast Asia and now mainland China. Yet to my surprise, when I started my research during the late 1990s, there was not even a single academic study on the topic of superstition *and* business decision making. With the benefit of hindsight, my surprise was somewhat unwarranted. First, the phenomenon is embedded in a specific cultural context and management researchers not familiar with Chinese culture may have missed it. Second, the phenomenon cannot be represented in an archival dataset. As such, it rules out the possibility of analyzing a dataset for the sake of generating interesting (in the sense defined by Murray Davis) findings and then formulating hypotheses based on these findings (i.e., post hoc hypothesis development). Finally, and related to the preceding point, qualitative research based on a kind of grounded theory approach is the most sensible method for investigating the phenomenon, with the risk of not producing any interesting findings. A natural outcome is that few researchers would be willing to invest their precious time and effort in this kind of risky research project. When I mentioned my study of the topic to a Taiwanese scholar at a conference, her knee-jerk reaction was: "Oh no! This sort of topic can't get into a good journal."

I embarked on my research without any theoretical preconceptions with the aim of answering a single question "Why do Chinese managers engage in superstitious activities when making strategic decisions?" My fieldwork included dozens of interviews with fortunetellers and Chinese businessmen in Singapore and Hong Kong as well as a simple questionnaire survey in Singapore. I published my results in *Organization Studies* (Tsang, 2004a) and a practitioner version of it in the *Academy of Management Executive* (Tsang, 2004b), both of which were the first outlet I attempted. The key finding of my study could be linked to two important concepts in the decision-making literature—rationality and uncertainty: "Superstition helps Chinese businessmen cope with uncertainty by providing a sense of certainty and alleviating the anxiety associated with uncertainty. Although superstition is often regarded as irrational and unfounded, practitioners try to justify it on the grounds of superstition's substantive validity or instrumental value." (Tsang, 2004a, p. 923). By Davis's standard, this finding is not particularly interesting, but it does satisfactorily answer the research question mentioned above and enhances our understanding of the phenomenon.

I was encouraged by some incidents during and after my research of the topic. One of the three anonymous *Organization Studies* reviewers of my manuscript commented that it "could become a classic *Organization Studies* piece in the spirit of its founder, David Hickson, who believed that rigor and boredom did not need to go together. It is pieces like [this] that often make a single *Organization Studies* issue more interesting to me than the entire year's crop of *AMJ*s." Although it was never my intention to set interestingness as a goal, I was pleased by this reviewer's appreciation—who do not want their papers to be considered interesting by their peers? "*Other things being equal*, interesting research is certainly better than boring research" (Tsang, 2022, p. 161). After my study was published, I received unexpected notes from researchers in Brazil and Mexico, saying that a similar phenomenon of superstitious decision making existed in their own country. Another surprising development is that over the years, my study has attracted citations from various non-management disciplines ranging from marketing (Wang et al., 2012), psychology (Huang & Teng, 2009), economics (Fortin et al., 2014), and finance (Gurd & Or, 2011) to even studies of death and dying (Wong, 2012).

The above example also shows that pure research may stimulate further research that is more applied in nature. For instance, Wang et al. (2012) explore the role of superstitious beliefs in consumer information processing and evaluation of brand logos. They have a rather substantial discussion of their study's implications for corporate branding. To conclude, breaking out of the "interestingness" straitjacket will open new opportunities for more fruitful research.

Notes

1.
The three of us, with Markus Menz and Laura Cardinal, have edited a special issue of *Organizational Research Methods* on *review research*, a term that refers to rigorous systematic investigations, such as systematic literature reviews and meta-analyses (Kunisch et al., in press). We have written this commentary from the perspective of our roles as editors of that special issue.

2.
Groucho's routine can be viewed at https://www.youtube.com/watch?v=29E6GbYdB1c

3.
At the time this essay was accepted, the Groucho Marx excerpt had approximately 314,000 views.

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