Essays

That's Interesting! A Flawed Article Has Influenced Generations of Management Researchers

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Abstract

Davis's (1971) article "That's interesting! Towards a phenomenology of sociology and a sociology of phenomenology" is regarded by many management researchers as a classic work and a basis for guiding management studies; in the wake of its publication, an interesting research advocacy gradually emerged. However, from the perspective of scientific research, Davis's core argument that great theories have to be interesting is seriously flawed. Interestingness is not regarded as a virtue of a good scientific theory and thus has little value in science. Moreover, obsession with interestingness can lead to at least five detrimental outcomes, namely promoting an improper way of doing science, encouraging post hoc hypothesis development, discouraging replication studies, ignoring the proper duties of a researcher, and undermining doctoral education.

"I have reservations about the contributions of your study which ultimately impact perceptions of novelty and interestingness."
An anonymous journal reviewer

The above comment was made by an anonymous reviewer of my recently rejected journal submission. "Another guy poisoned by Davis's article," I sighed. I was referring to Davis's (1971) article "That's interesting! Towards a phenomenology of sociology and a sociology of phenomenology," which promotes the idea that great theories have to be interesting in the sense that they provide counterintuitive arguments: "What seems to be *X* is in reality non-*X*," or "What is accepted as *X* is actually non-*X*" (p. 313). An example in management research is Kerr's (1975) classic article "On the folly of rewarding A, while hoping for B," lamenting that organizations often reward one kind of behavior while hoping that organization members will behave in a different way, one that is preferred by the organization. For instance, "[s]ociety *hopes* that teachers will not neglect their teaching responsibilities but *rewards* them almost entirely for research and publications" (Kerr, 1975, p. 773) at least for large and prestigious universities. Given the fact that time is limited, faculty members must choose between teaching and research activities when allocating their time. When they spend more time on research, which is rewarded, the quality of their teaching is sacrificed. Kerr's argument is counterintuitive in that

while one expects the behavior rewarded to be the behavior preferred by the organization, this is often not the case in reality.

Although Davis's target audience was sociologists, as indicated by the title of the article, it has turned out—rather surprisingly—that Davis's article has been particularly influential among management researchers, who have hailed the article as a classic and a basis for guiding studies of organizational phenomena. The following remarks from four former editors of the *Academy of Management Journal* (*AMJ*) in their editorial essays clearly indicate their appreciation of Davis's argument:

1. "Davis's (1971) analysis showed that the most influential sociological theories become widely cited, not because they are necessarily 'accurate' or 'correct,' but rather, because they are 'interesting.' On the basis of an examination of the content and subsequent citation rates of various sociological theories, Davis concluded that in order to generate interest, a new

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theory had to violate at least some expectations of readers. If it did not, the readers' perception was that no value was added." (Eden & Rynes, 2003, p. 680)

2. "Davis's (1971) 'index of the interesting' is one useful way to describe how to arouse a reader's curiosity." (Colquitt & George, 2011, p. 433)

A former editor-in-chief of the *Academy of Management Review* also claims straightforwardly that "a good theory paper should also utilize existing literature to highlight what is interesting and different (Davis, 1971)" Kilduff (2006, p. 253). In a more recent issue of *AMJ*, the editor-in-chief acknowledges that Davis's paper is "one of the most influential articles in management" (Tihanyi, 2020, p. 329) although he proposes that we should shift our attention from interesting to important research.

Table 1 classifies Davis's (1971) citations by journal articles published up to the end of 2020 as captured by the Web of Science. Editorial citations refer to those citations appearing in editorial essays of the journals concerned. Many mainstream management journals, such as *AMJ*, belong to both the "Business" and "Management" categories in the Web of Science. To avoid double counting, I assigned such journals to the category that is earlier in the alphabetical order, i.e., the "Business" category. The same rule was applied to nonmanagement journals that belong to more than one category. If we combine the "Business" and "Management" categories, their ordinary citations contribute 61% of total ordinary citations and their editorial citations contribute 82% of total editorial citations. Admittedly, within these two categories, there are journals in nonmanagement

Table 1. Web of Science Citations of Davis (1971) up to the End of 2020.

Discipline	Ordinary citations	Editorial citations
Business	190	31
Communication	10	0
Computer science	31	5
Economics	7	0
Education and educational research	18	2
Ethics	6	0
Information science and library science	7	0
Management	115	14
Psychology	27	0
Social sciences	11	1
Sociology	23	0
Miscellaneous[*]	54	2
Total	499	55

[*]The "Miscellaneous" category consists of subject areas with less than five citations each.

domains, such as marketing and management information systems, although mainstream management journals are still the majority.

Modeled on Davis's article, the Aalto University School of Business created a "That's Interesting!" Award, which recognizes the Academy of International Business conference paper that most effectively pushes the boundaries of our existing knowledge in the field. Some of the criteria for the award are the extent to which the paper challenges taken-for-granted assumptions in the field and deny old "truths" as well as the ability to attract reader attention. These criteria are in line with what Davis's article promotes.

The above discussion indicates an obsession in the management community with the premise that good theories or research findings have to be interesting. Given the dominant influence of Davis's article among management researchers, a natural question arises: is there anything wrong with his core argument that great theories have to be interesting? Running the risk of antagonizing many of my peers who think highly of Davis, in this essay, I will reveal the flaws of his argument and debunk the long-held myth that good management theories or research findings have to be interesting. To set the backdrop for my critique, the next section addresses the question of whether management researchers are, in fact, scientists.

Are Management Researchers Doing Science?

Before discussing the problems with Davis's argument, it is necessary to first establish whether management researchers are really doing science. Historically, one of the early works that formed the foundation of the management discipline—Taylor's (1911) *The Principles of Scientific Management*—has a title that conspicuously reminds the reader of the scientific nature of its content. Reviewing the development of the management discipline from the 1920s to the early 1950s, about a quarter of century ago, Perrow (1994, p. 192) concluded that "there was widespread agreement that management was becoming a science." This so-called "widespread agreement" has been reflected in the names of some leading journals such as *Administrative Science Quarterly*, *Organization Science*, *Management Science*, and *Decision Science*. The most recent member of this group is *Strategy Science*, the first volume of which was published in 2016. In sum, many, if not most, management scholars want to uphold an image that they are doing genuine science.

Management scholars' embrace of the science label is by no means surprising. Chalmers (2013, p. xix) starts his popular book *What Is This Thing Called Science* with this statement: "Science is highly esteemed." It is natural

that, as a relatively young discipline, management is eager to be perceived by society as a member of the prestigious "science club." The science label also helps promote the adoption of management research results in practice. To students in the classroom or managers in the firm, a management theory surely sounds more authoritative and legitimate if it is presented as being the outcome of rigorous scientific research rather than the product of other endeavors such as anecdotal observations, storytelling, or imagination (Tsang, 2017).

When I mentioned the core ideas of this essay to a colleague, who is a senior scholar, his immediate reaction was along the lines: "yes, management is science, but it is a different kind of science compared to natural science and therefore shouldn't be evaluated based on the latter's standards." My colleague is not alone in holding this view. Consider, for example, the well-known, and somewhat nasty, debate aroused by Pfeffer's (1993) methodological essay "Barriers to the advance of organizational science: Paradigm development as a dependent variable." Adopting Kuhn's (1962) concept of paradigm, Pfeffer discusses the risks of theoretical and methodological pluralism in management and advocates that researchers attempt to arrive at some degree of consensus, which is a critical precondition for promoting paradigm development in the management discipline and raising its status relative to peer disciplines such as economics. Among others, Cannella and Paetzold (1994) and Van Manaan (1995) criticize severely Pfeffer's (1993) essay. For the former, "the test of publishability [of a research paper] should be coherent persuasiveness – an internal logic and cohesion capable of winning support" (Cannella & Paetzold, 1994, p. 338). As to the latter, "putting theory in print is a literary performance; an activity involving the use of language whose methods are ways of writing through which certain identifiable reader responses are produced" (Van Maanen, 1995, p. 135). Both comments intend to spell out important criteria for evaluating management research. What is remarkable is that neither is in line with the principles of scientific research.

Although Popper (1959) maintains that what is to be called a "science" is a matter of convention, this does not imply that the science label can be used arbitrarily; otherwise, the word "science" loses its proper meaning. (Can fortune-tellers claim that they are doing science?) In fact, Popper himself uses falsifiability as a criterion for demarcation between science and nonscience. For the sake of discussion, I adopt Thomas's (1979, p. 2) rough demarcation: "to call a study a science implies that there is an empirical constraint on the acceptability of its statements, that the testing of its statements against the world is at least one strong criterion for the acceptance or rejection of those statements." According to this demarcation, assessing the quality of a piece of scientific research is neither a popularity contest nor a literary evaluation, as the above comments of Cannella and Paetzold (1994)

and Van Maanen (1995), respectively, seem to suggest. That said, I do agree with their view that management scholars should present their arguments in a coherent, persuasive, and eloquent manner. However, these qualities of the presentation should be of secondary importance relative to other aspects of scientific research such as data collection, data analysis, and whether the results support the hypotheses (Tsang, 2017).

As to my colleague's comment that management is different from natural science and so should not be evaluated based on the latter's standards, management *is* a social science. One major difference between natural and social sciences is that it is far more feasible to conduct natural science research in a closed system where "a constant conjunction of events obtains; i.e., in which an event of type a is invariably accompanied by an event of type b" (Bhaskar, 1978, p. 70). Ideal experiments aim to create closed systems so that regular sequences of events can be observed. Conditions of closure are rarely possible, however, in social science research. The artificiality of laboratory experiments performed by social psychologists attests to this point (Harré & Second, 1972). The openness of social systems is mainly due to the fact that their configuration is modified continuously by human actions and that humans have the capacity to learn and change (Sayer, 1992). Since it is impossible to construct closed systems using laboratory experiments in social science research (Danermark et al., 2002), social theories are primarily explanatory rather than predictive in nature.

The differences between the natural and social sciences do not imply that the two should adopt very different sets of standards governing the conduct of research or evaluation of theories (Bhaskar, 1998). For example, although the principle of replicability is often regarded as the most important criterion of genuine scientific knowledge (Rosenthal & Rosnow, 1984), as elaborated below, replication has been ignored or downplayed by many management researchers. Tsang and Kwan (1999) argue lucidly that similar to natural science, replication is important for the advancement of knowledge in management research.

Davis's Arguments

When I discussed Davis's paper with some scholars, all of them could recall his core claim (i.e., that a theory has to be interesting or counterintuitive). But I got the impression that they either had read the longish paper rather casually or had heard about the core claim but had not read the paper at all. This is unfortunate because, before we accept Davis's claim, we should, in the spirit of academic inquiry, first examine his reasoning and the empirical ground that supports the claim. As I show below, a careful reading of the paper will probably change one's positive view of his arguments.

Davis's writing lacks the rigor normally found in an academic paper, not to mention a supposedly philosophy-based academic paper. After all, his paper was published in *Philosophy of the Social Sciences.* [1] This problem gives rise to errors and ambiguities throughout the whole paper, far more than what I find in an average term paper submitted by doctoral students. To illustrate my point, consider this complicated sentence: "Should the assumption which a social theorist tries to convince his audience that they actually hold be too discrepant from the assumption they do actually hold, his audience will accuse him of setting up a 'straw man' – an assumption which is easily blown over because no flesh and blood person ever actually held it – certainly no member of his audience." (p. 333). Regarding the first clause, I believe his intended meaning is to describe a scenario where a social theorist tries to convince his audience that they actually hold Assumption A, but the audience think they hold Assumption B. Assumption A is "too discrepant from" Assumption B. Unfortunately, it is not clear whether "straw man" in the second clause refers to Assumption A or Assumption B. Moreover, in logic, the straw man fallacy that Davis seems to be talking about actually refers to the case where one attacks an opponent's argument, but that argument was not actually presented by the opponent. There is no implication that the argument is bad, although it sometimes is (Finocchiaro, 2013). On the other hand, Davis's straw man assumption is one that "no flesh and blood person ever actually held," implying that it is a bad assumption.

Owing to space limitations, in this section, I only focus on the problems with the opening statements of Davis's paper and his arguments concerning interesting propositions. Interested readers may contact me for other errors that I identify in his paper. I defer critique of Davis's suggestions offered to researchers (pp. 332–341) to the section in which I discuss the negative consequences of using interestingness as a key criterion for evaluating a piece of scientific research.

Opening Statements

Davis starts his article with these two bold, eye-catching statements: "It has long been thought that a theorist is considered great because his theories are true, but this is false. A theorist is considered great, not because his theories are true, but because they are *interesting*." (p. 309). I cannot recall any other influential article that begins with such blatantly false statements. Some management scholars do not seem to be aware of the serious problems inherent in these sentences and so embrace them wholeheartedly. For example, Frank and Landström (2016, p. 55) rephrase the second sentence approvingly while Salvato and Aldrich (2012) quote both sentences as the opening sentences of their editorial essay published in *Family Business Review* (*FBR*). In their *AMJ* editorial essay, Bartunek et al. (2006, p. 10) quote the second sentence to support their point that "scholars who produce interesting research have more influence on others."

There are two key issues here. First, Davis's intention is not to confine his arguments to sociology in particular or even social science in general. As he comments, "I will confine my inquiry to *social theories* … I suggest, however, that the level of abstraction of the analysis presented here is high enough for it to be applicable equally well to theories in all areas of social science and even to theories in natural science." (p. 310). Therefore, based on his *suggestion*, the two statements are applicable to both social *and* natural sciences. Second, they are stated as *universal* statements about theories and theorists, similar to a typical universal statement like "All swans are white." Just like the existence of a nonwhite swan falsifies the statement "All swans are white," a single example of a theorist who is considered great because his or her theory is true rather than interesting is good enough to falsify Davis's claim. In fact, one can easily come up with not one but numerous examples. Consider household names like Copernicus, Galileo, Newton, and Einstein, all of whom are irrefutably great scientists-cum-theorists. They are considered great not because their theories are interesting but simply because their theories are true not to mention advanced for the stage of scientific development extant at the time they were conceived. Finally, the two statements are, to a certain extent, an insult to scientists' quest for truth because they seriously understate the importance of a theory being true while grossly exaggerating the importance of being interesting.

Interesting and Non-Interesting Propositions

As mentioned, Davis equates "interesting" with "counterintuitive": "What seems to be *X* is in reality non-*X*," or "What is accepted as *X* is actually non-*X*" (p. 313). This is probably the most frequently cited/quoted and presumably the most accepted point of Davis's paper. For example, Alvesson and Sandberg (2013a, p. 131) state: "if a theory doesn't challenge some of an audience's assumptions significantly, it is unlikely to receive attention and become influential – even if it has been extremely rigorously developed." Davis elaborates his point using different wording:

> … an audience finds a proposition "interesting" not because it tells them truth they did not already know, but instead because it tells them some truth they thought they already knew was wrong. In other words, an interesting proposition is one which denies some aspect of the assumption-ground of its audience … (p. 327)

Davis does not define "assumption-ground" and uses the term casually. He states, "In *The Index of the Interesting* we have categorized the various aspects of this assumption-ground which can be denied." (p. 327). The first item under the category "organization" in that section is: "What seems to be a disorganized (unstructured) phenomenon is in reality an organized (structured) phenomenon" (p. 313). It seems that this aspect of the assumption-ground refers to the feature of a phenomenon that the phenomenon is disorganized (unstructured). In other words, an aspect of the assumption-ground refers to a feature of a phenomenon. Davis gives this example in illustration: "Ferdinand Tonnies' assertion in *Community and Society* that the relations among people within all societies were considered at the time he wrote to be manifold and indeterminate, can in fact be organized around two main types "*Gemeinschaft* and *Gessellschaft*)" (p. 313). That is, the aspect of assumption-ground here is that the relations among people within all societies were manifold and indeterminate. Ferdinand Tonnies denied this by showing that relations could be organized into two main types. Another category of aspects of assumption-ground consists of relations among multiple phenomena, such as "What seem to be unrelated (independent) phenomena are in reality correlated (interdependent) phenomena" (p. 322).

As I discuss below, Davis subsequently uses "aspect of the assumption-ground" in an ambiguous way. He also seems to use "aspect of the assumption-ground" and "assumption" interchangeably, as indicated in this sentence: "Interesting theories are those which *deny* certain assumptions of their audience, while non-interesting theories are those which *affirm* certain assumptions of their audience." (p. 309). Moreover, he uses the term "assumption" in a lax manner. More often than not, he actually refers to belief or knowledge. For example, in proposing a pedagogical approach, he argues that "a course would begin by articulating the student's common-sense assumptions about the subject matter, would continue by refuting these lay assumptions and replacing them with expert propositions …" (p. 338).

His usage of "assumption" is inconsistent with that in the theory and methodology literature. Mäki (2000), for instance, distinguishes between core and peripheral assumptions. The former is about the major causes postulated by a theory while the latter refers to the minor causes. A core assumption, such as opportunism in transaction cost economics, is a constituent of the mechanismic explanation proposed by a theory (Tsang, 2006). It is obvious that Davis did not use "assumption" with these meanings in mind.

As a remedy, Davis's discussion would have been clearer if he had avoided using "assumption" or "assumption-ground," and used "belief" or "knowledge" instead, as he did in these two sentences: "His assertion is interesting because it counters his audience's previous belief about phenomenon (a)." (p. 341), and "Theory construction, in other words, should not be treated as an independent logical or empirical enterprise separate from, and unrelated to, what the audience already 'knows' about a given body of data …" (p. 317). To avoid creating further confusion, I adopt Davis's usage of "assumption"; that is, I use "assumption" when in fact I mean "belief" or "knowledge."

Davis describes three general types of propositions that the audience would consider non-interesting. The first type is that "the proposition affirms some aspect of their assumption-ground" (p. 327). This type can be regarded as a corollary of the abovementioned definition that "an interesting proposition is one which denies some aspect of the assumption-ground of its audience." A problem here concerns Davis's example "Husbands often influence their wives' political behavior" (p. 327) used as an illustration. While the proposition describes a specific phenomenon, Davis does not state which feature of the phenomenon (i.e., which aspect of assumption-ground as categorized in *The Index of the Interesting*) he is referring to. The use of the term "aspect of assumption-ground" is even more problematic in the other two types of non-interesting propositions.

The second type is that "the proposition does not speak to any aspect of this assumption-ground at all" (p. 327) and so is considered irrelevant. This point is confusing and Davis's example "Eskimos are more likely than Jews to …" (p. 327) is perplexing. He has to explain how it is possible that a proposition does not speak to any aspect of the assumption-ground. But what does "speak to" mean here? As a remedy, he might replace "aspect of assumption-ground" by "assumption." For instance, people often assume, all other things being equal, that increasing R&D expenditure by a firm will increase the number of patents it applies for. The proposition "Turnover of research scientists in a firm will increase the number of patents granted to the firm" is irrelevant with respect to this assumption, although both are about R&D. However, it does not imply that the proposition is non-interesting. Suppose people often assume that the turnover of research scientists will hurt a firm's success of patent applications. Then the proposition denies this second assumption and thus is interesting, according to Davis.

Now comes the third type: "instead of denying some aspect of their assumption-ground, the proposition denies the whole assumption-ground" (p. 327). Davis argues that "the audience's response to propositions of this type will be: 'That's absurd!'" (p. 327). Again, his example "Social factors have no effect on a person's behavior" is perplexing. How does it deny "the whole assumption-ground" (which I suppose means denying all aspects of the assumption-ground listed in *The Index of the Interesting*)? This example seems to illustrate that the proposition denies people's strongly held assumption (instead of assumption-ground) that social factors have at least some effect on a person's behavior.

Since the assumption is very reasonable, the proposition sounds absurd. My interpretation here is based on Davis's restatement of the distinction between the interesting and the absurd in the conclusion section:

> There is a fine but definite line between asserting the surprising and asserting the shocking, between the interesting and the absurd. An interesting proposition, we saw, was one which denied the *weakly held* assumptions of its audience. But those who attempt to deny the *strongly held* assumptions of their audience will have their very sanity called into question. (p. 343)

Putting aside this problem concerning terminology, his point is flawed. Since he equates "interesting" with "counterintuitive," when a proposition totally denies the audience's assumption-ground (or assumption), the audience should experience a higher level of counterintuitiveness than when the proposition only partially denies it. In addition, his point that "those who attempt to deny the *strongly held* assumptions of their audience will have their very sanity called into question" (p. 343) (as well as his elaboration of the point in endnote 7 on p. 344) is unfounded. In science, whether and how far an audience thinks that the proposition is absurd depends on the proposition's empirical support. Einstein's theory concerning time is a perfect example.[2] Before Einstein proposed his theory, people's concept of time was that time was absolute; that is, the rate at which time passed remained the same whatever the circumstance. Einstein's theory completely overturned this concept. When I first heard about the theory, I was a teenager and had studied Newtonian mechanics. I was stunned by Einstein's argument about the relativity of time and had a very keen interest in learning more about the theory. Yet, while few people (or few educated people) consider the theory absurd, most people consider it counterintuitive in the sense that it contradicts our daily experience of time on earth. In the domain of judgement research, people generally believe that professionals, such as judges, physicians, radiologists, and fingerprint experts, make pretty accurate judgements. However, Kahneman et al. (2021) provide convincing empirical evidence to overturn this belief. For instance, the same expert may not even make self-consistent judgements over time. Despite the serious flaws of the third type of non-interesting proposition, some management scholars seem to accept it without question. For example, Van de Ven (2007) takes a step further and restates Davis's point as: "Interesting theories deny weakly-held, not strongly-held, assumptions of the audience" (p. 111).

My critique is consistent with Davis's apparent realist stance. As quoted above, he argues that "an audience finds a proposition 'interesting' not because it tells them truth they did not already know, but instead because it tells them some truth they thought they already knew was wrong" (p. 327). According to the correspondence theory of truth, truth is a property of a statement (such as a proposition or premise) and is taken to consist of correspondence between that statement and the real world. As such, many philosophers deem that a statement "p" is true just in case p is a fact (Moore, 1959; Russell, 1906). If a theory, such as Einstein's, is supported by empirical evidence and so is true, people won't find it absurd even if the theory completely rejects their assumption. Rather, it is refusing to accept a true theory without any valid reason that is absurd.

In sum, Davis's discussion of the three types of non-interesting propositions is confusing. One major cause of this is his use of the term "aspect of assumption-ground," which refers to a feature of a phenomenon or a relationship among multiple phenomena. As such, serious problems arise when he states that a proposition does not speak to any aspect of the audience's assumption-ground (for the second type of non-interesting propositions) or that it denies the whole assumption-ground (for the third type). This is a fatal flaw because the distinction between interesting and non-interesting propositions is the core of his whole paper.

The Interesting Research Advocacy

When Davis's paper was first published around half a century ago, it was concerned with interesting theories or theoretical propositions. As its influence grew among management researchers, its core arguments were expanded to include empirical findings. For example, after rehashing Davis's idea about interesting theoretical propositions, Salvato and Aldrich (2012, p. 127) argue that "in the case of *empirical* works, challenging established assumptions and theory through counterintuitive research questions is also regarded as central in making an article interesting." Similarly, in a *Journal of Management Studies* editorial essay, Corbett et al. (2014, p. 9) propose that "although Davis speaks to theorists and theory papers, the same concept holds true for empirical work and any type of research we might conduct." This broadening of scope is unsurprising given the fact that management journals publish a far greater number of empirical papers than conceptual papers. Since journals tend to accept papers in which hypotheses are supported (i.e., the "file drawer problem," see Rosenthal, 1979), in order to include counterintuitive hypotheses in a paper (so that the related arguments sound more interesting), authors have to first produce findings that (at least mostly) support these hypotheses. In brief, this obsession with interestingness pervades the entirety of management research.

In the wake of publication of Davis's paper, the promotion of interesting research has emerged gradually as an advocacy in management and related disciplines, as indicated by the abovementioned "That's Interesting!" Award conferred by the Aalto University School of Business; a methodology

book titled *Constructing Research Questions: Doing Interesting Research* (Alvesson & Sandberg, 2013b); journal papers devoted to the topic of making research more interesting, such as Das and Long (2010) in management, Frank and Landström (2016) in entrepreneurship, Cachon (2012) in operations management, Smith (2003) and Voss (2003) in marketing, and Gray and Wegner (2013) in psychology; as well as a number of editorial essays devoted to the same topic (e.g., Baba, 2016; Salvato and Aldrich, 2012; Shugan, 2003). Of particular note is a 2006 *AMJ* editors' forum, titled "What Makes Research Interesting?", in which three essays were published: Bartunek et al. (2006), Barley (2006) and Dutton and Dukerich (2006). The forum was related to a survey that had been conducted in the autumn of 2004: all members of *AMJ*'s editorial board "were invited to nominate up to three empirical articles related to management from any academic journal over the past 100 years that they regarded as particularly interesting and to describe why they saw them as interesting" (Bartunek et al., 2006, p. 11).[3] The survey, in turn, constituted an initiative that had been undertaken "to encourage the development of more interesting research at *AMJ*" (Rynes, 2005, p. 13).

Another, more significant, initiative was to make "interestingness, innovativeness, and novelty" an explicit rating category on the *AMJ* reviewer rating form. In other words, every manuscript submitted to *AMJ* would be evaluated against that criterion. *AMJ* is surely not alone in this respect. Recently, I reviewed a manuscript for the *Journal of Management Studies*. The first item on the Reviewer Evaluation Form is "Is the paper interesting, innovative and/or novel?". Such review criteria have helped interestingness to reach sacrosanct status.

Davis's article has also affected how we train the next generation of management researchers, as suggested by this glowing review of his article: "When taking a broader view of theoretical insights, Davis's (1971) classic, *That's Interesting*, is an article I've read yearly since my graduate school days that provides a number of concrete ways that works can provide novel interest by establishing counterintuitive observations." (Short, 2009, p. 1315). Similarly, Podsakoff et al. (2018, p. 518) claim that their own experiences of working with doctoral students indicate that Davis's suggestions are "useful ways to generate good research ideas." I know a colleague who included Davis's article in his doctoral seminar as a required reading and in his comprehensive examination questions. As such, the article has influenced the mindset of many budding researchers as to how research should be conducted.

Within the emerging interesting research advocacy, there are different views among advocates concerning the importance of interestingness as an attribute of good research. For instance, in the summer of 2004, *AMJ*'s editorial board members were surveyed to assess the journal's strengths, challenges, and opportunities. A key finding was that "while board members viewed *AMJ* as unparalleled from a standpoint of publishing technically competent research that simultaneously contributes to theory, empirical knowledge, and practice, they also believed that it was both possible and desirable to raise the proportion of articles published in *AMJ* that are regarded as important, competently executed, and *really interesting*" (Bartunek et al., 2006, p. 9). In this way, interestingness was regarded as one attribute of good quality papers. There are even more extreme views than this, however. For instance, Landström and Harirchi (2019, p. 507) begin the abstract of their paper with an eye-catching claim: "In order for a work on entrepreneurship to be published and attract attention, it must be interesting." In other words, it was imperative that a paper be interesting.

Benefits of Interesting Research

A natural question for one to ask is: "What are the benefits of doing and publishing interesting research?" Bartunek et al. (2006) describe three main benefits, each related to a different stakeholder. First, reiterating the point made by Davis, they argue that scholars producing interesting research have more influence on others. Second, they cite psychological research results indicating that materials perceived as interesting produced a higher degree of learning by readers. Thus, journal articles that are more interesting to readers are more likely to attract attention and be read, understood, and remembered. The last benefit is concerned especially with potential doctoral students in that interesting research will help attract, motivate and retain talented, enthusiastic doctoral students. Retention is particularly important for doctoral programs because the attrition rate is generally high.

Within the broad discipline of management, there are additional benefits that sub-disciplines may gain from promoting interesting research. For example, in their *FBR* editorial essay, Salvato and Aldrich (2012, p. 126) maintain that making family business research more interesting helps "family business specialists break out of the narrow confines of their field and carry their message to adjacent fields," and also enhances the visibility and impact of the research as an autonomous scholarly field. Similarly, Frank and Landström (2016, p. 51) argue that in the case of entrepreneurship research, "the field as a whole benefits when it is perceived as interesting because it will attract researchers from other fields who will contribute to the renewal of the research field." In fact, Fayolle et al. (2016, p. 480) contend that the successful growth of the entrepreneurship field is because "entrepreneurship has been regarded as an 'interesting' field of research (Davis, 1971)." Such sub-disciplinary views are understandable because various sub-disciplines compete for research grants, talents, and recognition.

Making Research Interesting

A concomitant of advocating for interesting research is a variety of ways of making research more interesting that is not only based on but extend beyond Davis's ideas. For instance, based on survey responses from 915 entrepreneurship scholars, Landström and Harirchi (2019) suggest piecemeal improvements vis-a-vis interestingness for each of the five aspects of research, namely research question, theoretical framework, research design, research output, and writing (see Table 9). One suggestion under "research question," for example, was: "There are always 'hot topics' within the field that are regarded as interesting." (Landström & Harirchi, 2019, p. 526).

Alvesson and Sandberg (2013a) contend that the shortage of interesting and influential studies in management is due to the almost total dominance of incremental gap-spotting research—identifying a gap in the existing literature and trying to make a contribution by filling the gap. They identity three key drivers behind this research style, namely institutional conditions, professional norms, and researchers' identity constructions. In addition to suggesting changes to these drivers, they propose two methodologies that may encourage and facilitate more innovative research. One methodology is to problematize certain dominant assumptions in existing research, a method in line with Davis's suggestion. This involves challenging the underlying assumptions of not only others' meta-theoretical positions but also researchers' own positions. The other methodology concerns the use of data, or what Alvesson and Sandberg call "empirical material." It requires "a more active construction of empirical material in ways that are interesting, and not just waiting passively for data to show us the route to something interesting, as is typically the case in more conventional research" (Alvesson & Sandberg, 2013a, p. 146).

The Value of Interesting Theories in Science

Since interestingness seems to be so highly valued by management researchers, who are supposedly doing science, one may ask: is interestingness an important virtue of a theory in science? McMullin (2008) proposes a list of virtues of a good theory. Empirical fit and explanatory power are the two *primary virtues*. The former refers to the extent that a theory can "account for data already in hand" (p. 501) while the latter is "the persuasiveness in general of the underlying causal structure postulated by the theory" (p. 502). There are also three categories of *complementary virtues*, namely internal, contextual, and diachronic. Surprisingly—and disappointingly as far as Davis's fans are concerned—whether or to what extent a theory is interesting, counterintuitive or novel is not one of the complementary virtues. That is, the interestingness of a theory is

regarded to be of little value in science. This outcome is not at all surprising. Nothing in the ideas of Thomas Kuhn and Karl Popper—two of the most influential philosophers of science—"values novelty for its own sake" (Cohen, 2017, p. 3). Why? The answer lies in the objectives of scientific research.

Putting aside its inherent controversy (see Gooday, 2012), the distinction between pure and applied science suggests two main objectives of scientific research, namely explaining and problem-solving. The main purpose of pure science is to find an explanation for a phenomenon that happens in the world, such as explaining solar eclipses. In contrast, applied science attempts to find a solution to a problem that affects human life, such as developing a drug that cures a disease. The two objectives, though distinct, are sometimes closely related to one another. For example, explaining the occurrence of earthquakes helps the prediction of their occurrence. Both objectives are only remotely related to interestingness. Regarding the objective of finding an explanation, even if the phenomenon in question is interesting, it should be distinguished from an interesting theory that explains the phenomenon. Davis's argument is focused on the theory, not the phenomenon. Obviously, a theory explaining an interesting phenomenon is not necessarily interesting (in the sense of being counterintuitive or novel) in and of itself. More importantly, whether the theory is interesting is simply irrelevant; what *is* relevant is whether it can provide a satisfactory explanation.

As to the other objective concerning problem-solving, consider the case of HIV/AIDS research. Since the discovery of HIV as the cause of AIDS in 1984, thousands of scientists have been working hard to develop drugs that can cure HIV and AIDS. If these scientists were to be asked whether their theories or research results are interesting, I'm sure they would be perplexed by the question. They are finding an *effective* drug, not a drug that is based on any *interesting* theory. The current COVID-19 pandemic is another great example. Scientists in various countries are working day and night to deal with the epidemic and don't have the luxury of thinking about the interestingness of their findings. In fact, in this kind of emergency, does anyone really care about interestingness? For both objectives, if the resultant theory, theoretical proposition, or solution is interesting, it is an accidental byproduct, not an intended outcome.

To conclude, interestingness (or counterintuitiveness or novelty) is not a virtue of a good scientific theory and thus has little value in science, despite the huge emphasis that many management scholars place on interesting theories or theoretical propositions. Here I state a fact based on the philosophy of science literature. Whether it is worth promoting something that has little value is itself a value judgement and beyond the scope of my discussion. Instead of helping the field of management research to progress, the emphasis on

interestingness has had detrimental consequences, as discussed in the next section.

Consequences of Obsessing with the Interesting

Although interestingness has little value as far as scientific research is concerned, it does have some benefits as discussed above. For instance, interesting theories and findings may help a sub-discipline, such as entrepreneurship, attract researchers' attention, and compete for resources. That said, promoting interesting research gives rise to the following detrimental consequences.

Doing Science in a Weird Way

Whether something is interesting is in the eye of the beholder. That's why Davis advocates a weird, audience-driven way of conducting research: "the social researcher who wants to be certain that he will produce an *interesting* theory about his subject must first familiarize himself with what his audience already assumes to be true about his subject, before he can even begin to generate a proposition which, in denying their assumption, will attract their attention" (p. 337). His paper painstakingly discusses how assumptions may be held differently by different segments of an audience and suggests ways to deal with the complicated situation (pp. 328–334). There are three main problems with this. First, the objective of coming up with a proposition that will be considered interesting by the majority of an audience, not to mention the whole audience, may be unachievable. For instance, Davis admits that "the assumptions about a topic held by both laymen and experts may be too diverse or too amorphous for any proposition about this topic to be found universally interesting" (p. 329). Management researchers often want their papers to be read by people in different countries. There are various dimensions of diversity in such a huge audience. Take culture as an example. A proposition that violates an assumption held by people in an individualistic culture may be completely consistent with a different assumption held by people in a collectivistic culture. Davis actually makes a similar point: "As an audience is often segmented along various social lines, the assumption about a topic held by one audience segment is likely to be at variance with the assumption about that topic held by another audience segment" (p. 329). To a certain extent, he admits that his audience-driven approach is unrealistic.

Second—and more importantly—this is not the way scientists actually do their work. Usually, a scientific research project is either phenomenon-driven (corresponding to the objective of explaining) or problem-driven (corresponding to the objective of problem-solving), or both. Davis's audience-driven approach is unheard-of in the history of science. Before Einstein developed his theory of relativity, he didn't whisper to himself (in German), "Hmm, since people have an absolute concept of time and space, let me create a theory that overturns it. Then I'll be famous." (See Isaacson, 2008 for a detailed account of how relativity theory was created). In psychology as well as in management, the research results of and theories proposed by Daniel Kahneman and Amos Tversky are certainly interesting (by Davis's standards). Yet, they did not start their research with the intention of upending traditional notions of rationality in human thought (see Lewis, 2017 for a biography of the two scholars and an account of their collaboration). In short, none of these great scientists gave a damn what their audience assumed.

Third—and unfortunately—some management scholars faithfully follow Davis's audience-driven approach. So his suggestion does have an impact on management research practice. For instance, after rephrasing Davis's description of interesting theories, Van de Ven (2007, p. 111) recommends that "the more we engage and the better we know our audience, the better we can select and frame our conjectures to the prevailing assumptions of the intended audience of our work." Davis's approach is not a proper way to do science. It makes little sense to start one's research by surveying the assumptions held by different segments of the audience with the objective of creating an interesting theory or theoretical proposition (even if we ignore the fact that it is likely to be a futile attempt because of the diversity of these segments, as discussed above). Doing so won't help achieve either of the abovementioned main objectives of science, namely explaining and problem-solving. If a theory turns out to be interesting, that attribute is an accidental byproduct and should by no means be set as the target.

Encouraging the Practice of Hypothesizing After the Results are Known (HARKing)

As shown in Table 1, most of the editorial citations of Davis's article came from management journals. Most of these citations are positive; that is, they agree with Davis's argument. For example, "to encourage the development of more interesting research at *AMJ*," the journal "made 'interestingness, innovativeness, and novelty' an explicit rating category" on its reviewer rating forms (Rynes, 2005, p. 13). In the domain of empirical research, a serious consequence of journals demanding interesting findings is that there will surely be a supply of such findings, whether it be through legitimate or illegitimate research methods. This is especially so because management journal editors and reviewers have such a dominant influence on how authors present their ideas and what to include in their manuscripts

(Tsang & Frey, 2007). As Cortina (2016, p. 1144) keenly observes:

> When reading published papers, be on the lookout for exceptionally counterintuitive findings, that is, the sort that make you think to yourself that you would have guessed the exact opposite. I will bet you 10 dollars for 1 that that hypothesis is supported by the data in that paper.

Most quantitative papers published in management journals adopt the method of null hypothesis statistical testing (NHST)—to test a hypothesis (and its underlying theory) by checking whether its null counterpart can be rejected at a certain level of significance (usually $p < .05$). These studies are presented in a format suggesting that hypotheses are first deduced from related theories. Then data are collected and analyzed for hypothesis testing using the method of NHST. At least, this is the order in which materials are presented in a manuscript. Yet, this positivist hypothetico-deductive approach promoted by Hempel (1965) may not reflect actual practice. Bettis (2012) recounts an incident of his visiting another university and asking a second-year doctoral student, "So what are you studying?":

> His reply of "I look for asterisks" momentarily confused me. He proceeded to tell me how as a research assistant under the direction of two senior faculty members he searched a couple of large databases for potentially interesting regression models within a general topical area with 'asterisks' (10% or better significance levels) on some variables. When such models were found, he helped his mentors propose theories and hypotheses on the basis of which the 'asterisks' could be explained. (pp. 108–109).

The student's reply indicates the practice of what Kerr (1998) labels as "HARKing." With sufficient effort, patience, and some luck, it should not be too difficult to generate interesting findings that satisfy journal editors' obsession, especially if one has a large dataset. The declining cost of computing also aids tremendously this kind of "fishing expedition": "In the 1980s and 1990s, expanded access to computing power led to rising concerns that some researchers were carrying out growing numbers of analyses and selectively reporting econometric analysis that supported preconceived notions – or were seen as particularly interesting within the research community – and ignoring, whether consciously or not, other specifications that did not." (Christensen & Miguel, 2018, p. 931).

HARKing is the opposite of the hypothetico-deductive approach; in HARKing, hypotheses are formulated based on post hoc reasoning to accommodate research findings, but these hypotheses are presented as if they were a priori. HARKing has a number of harmful outcomes, such as reporting falsely significant relationships, reporting greatly exaggerated levels of statistical significance, precluding the identification of plausible alternative hypotheses, and developing flawed theories (Bettis, 2012; Bettis et al., 2016; Kerr, 1998).

Downplaying Replication Studies

> In 1989, chemists Stanley Pons and Martin Fleischmann made headlines with claims that they had produced fusion at room temperature – "cold" fusion compared to the high temperatures the process was thought to require. It was the kind of discovery that scientists dream of: a simple experiment with results that could reshape our understanding of physics and change lives the world over. (Understanding Science)

In addition to being an enormous breakthrough in the advancement of clean energy, this discovery was interesting because it rejected the strongly held assumption of chemists and physicists, as well as laymen, that the process by which two atoms fuse has to take place at extremely high temperatures. Contrary to Davis's claim that "those who attempt to deny the *strongly held* assumptions of their audience will have their very sanity called into question" (p. 343), the scientific community did take the discovery seriously instead of treating Pons and Fleischmann as lunatics. At the peak of the saga when the hope of finding a new energy source had not yet been dashed, Maddox (1989, p. 701) remarked that "no doubt the general opinion will depend on the outcome of attempts at replication;" that is, whether Pons and Fleischmann's results could be replicated by other scientists. As we all know now, they couldn't. This scientific blunder indicates clearly that if an empirical finding can't be replicated, it won't be accepted by the scientific community regardless of how interesting it is. The incident also refutes squarely Davis's claim that "a theorist is considered great, not because his theories are true, but because they are *interesting*" (p. 309).

As mentioned, Popper (1959) uses falsifiability as a criterion of demarcation between science and nonscience. Replication is also proposed as such a demarcation criterion (Braude, 1979). Reproducibility is referred to as the cornerstone of science and as "the best and possibly the only believable evidence for the reliability of an effect" (Simons, 2014, p. 76). As such, in many natural sciences, it is a common practice to replicate empirical findings of previous studies. Unfortunately, replication is much less common in the social sciences in general and management in particular. With few replication studies published in management journals, concerns have been raised about the reproducibility of empirical results. In the domain of strategy research, for instance, Goldfarb and King (2016) analyzed 300 articles published in top outlets and estimated that 24%–40% of significant coefficients would become insignificant at the 5% level if each study were replicated. In other words, these articles likely reported false positives. Given this looming reproducibility crisis, recently, there have been calls for an

increased role of replication in sub-disciplines of management research, such as international business (Harzing, 2016), organizational behavior (Wright & Sweeney, 2016), and strategic management (Bettis et al., 2016).

While a number of factors may be contributing to the current situation of downplaying replication studies by management researchers, the obsession with interesting results is definitely a crucial factor. There is an intrinsic tension between the demand for interesting results and the need to conduct replication. Generating interesting results often requires creativity and novelty, whereas replication seems to be antithetical to these values (Tsang & Kwan, 1999). In their critique of Davis's article, Pillutla and Thau (2013, p. 192) pose a cautionary note: "A discipline that does not encourage replication and instead values the novel and the interesting invites ambitious academics to publish interesting one-off findings without any concern about scrutiny."

Note that an interesting result does not imply that it is more likely to be replicated. Tsang and Yamanoi (2016), for example, conducted the first replication of Barkema and Vermeulen's (1998) study of international expansion by Dutch firms based on a comparable dataset of Singapore firms. The original study won the *AMJ* best article award and presumably consists of some interesting findings and/or theoretical propositions. Tsang and Yamanoi found that the original study misinterpreted the regression coefficients for hypothesis testing and only two of the four hypotheses were actually tested. For these two hypotheses, one was supported in neither the original study nor the replication, while the other was supported in the former but not the latter. For the other two hypotheses (that were not tested in the original study), Tsang and Yamanoi found support for one of them. In sum, Barkema and Vermeulen claimed that all four hypotheses were supported, whereas only one was supported in the replication. This replication outcome is not surprising. When researchers strive to generate interesting findings in order that their study be accepted by a top journal like *AMJ*, they are motivated to engage in HARKing that, as discussed, will probably lead to invalid or unreliable findings.

Ignoring the Duties of a Researcher

In reviewing the state of sociological research at that time, Davis (1971, p. 336) remarks, "The common critique of most contemporary social and especially sociological research is that it is *dull*, that it says what everybody knows or what nobody cares about." In commenting on the research domain of moods and emotions in organizations, Brief (2001, p. 136) follows Davis and makes the following observation:

> The subordinates of a supervisor who treats them unjustly tend to be more annoyed and hostile at work than those subordinates of a fair supervisor. Employees high on the personality trait

agreeableness report more positive moods at work than those exhibiting less of the trait. These are pretty boring assertions. They are not particularly interesting because, for example, they are not especially novel in the sense of failing to contradict what we think we already know or to alter the conceptual background against which they appear (Davis, 1971, 1999).

As far as scientific research is concerned, Davis's comment "it is *dull*" or Brief's comment "These are pretty boring assertions" is novel but bizarre. What's wrong with a boring theoretical proposition that can adequately explain a phenomenon or solve a problem? Indeed, there is nothing wrong with the proposition but something *is* wrong with those who complain about boring stuff because they seem to forget that as management scholars, we are *paid* to do research as well as to write and review research papers. Whenever I am reviewing the so-called "boring" manuscript and somewhat lose my patience, I remind myself that I am paid for the task and should never complain. On the contrary, I should be grateful for being paid to work on research topics in which I am interested even if such research does not generate interesting findings. For those who want to read something novel, they should read a novel, not a research paper. Management scholars are *not paid* to entertain themselves or their audience through producing interesting, counterintuitive or novel theories, theoretical propositions or empirical findings.

It is not surprising that Davis's fans tend to take a self-centered view of a researcher's role. A key message I got from reading Davis's article is that researchers should try to catch the attention of their audience by producing interesting theories. For instance, he describes what will happen when students can distinguish between "interesting theories" and "uninteresting theories":

> … they will find that their theories will then make their readers literally "sit up and take notice." Their theories will then be discussed among colleagues, examined in journals, confirmed or denied in dissertations, and taught to students as the most recent instances of "progress" in their profession. (p. 310)

Whether such interesting theories can, in the domain of sociology, really improve social policies and make society better is not his concern. Rather he cares more about how researchers can advance their own career: "The best way to make a name for oneself in an intellectual discipline is to be interesting – denying the assumed while affirming the unanticipated" (p. 343). This is also hailed as one of the three main benefits of making research more interesting by Bartunek et al. (2006) mentioned above. If most management researchers have an attitude similar to Davis's (and I hope this is not the case), the discipline is doomed.

Undermining Doctoral Education

One of the three benefits of making management research interesting cited by Bartunek et al. (2006) is that it helps attract, motivate, and retain talented and enthusiastic doctoral students. Interestingly, the authors quote a piece of anecdotal evidence as support for their view:

> "I notice from reading the many applications to our Ph.D. program. . .that very few people aspire to become business academics with the intention to publish journal articles that will only be read by other academics (at best); rather, these applicants are much more inspired by the thought of gaining and developing truly relevant knowledge that might change the world of organizations." (Vermeulen, 2005, pp. 980–981)

Here, it appears that Bartunek et al., equate interesting research with research that generates "truly relevant knowledge." This view is perplexing because interesting research (in the Davis sense) and relevant research are two completely different concepts.

My experience with two of my own doctoral students sheds light on this issue. Both students joined our doctoral program enthusiastically and were diligent in learning the "tricks of the trade." After having papers rejected repeatedly by conferences and/or journals (as is to be expected in this type of career), they noticed a rather frequently cited reason for rejection: their hypotheses and related findings were not interesting, counterintuitive, or novel enough (i.e., a reason similar to the one referred to in the opening quote of this essay). Consequently, they were in essence "forced" to engage in the above-described process, dubbed by Bettis (2012) as "searching for asterisks." After persisting for a period of time with this type of research, the students ended up going through a wrenching soul-searching process, eventually telling me that they had lost interest in doing "interesting" research and had become disillusioned. In line with Vermeulen's (2005) observation quoted above, the students' main concern was that a great deal of effort was being spent on developing fancy hypotheses and producing the sort of counterintuitive findings that might create an "aha" moment for reviewers but were often unrelated to business practices. They could find no meaningful purpose other than churning out publications so that they could stay in an academic career. As their mentor, I frankly admitted that in the current atmosphere of obsessing with interestingness, I could not offer them a satisfactory solution. Fortunately, they have not changed their academic career and I do hope that they will strike a balance between publishing journal papers and doing meaningful research. These two separate emotional incidents triggered a soul-searching process on my part and strengthened my motivation to write this essay.

In contrast, I have also encountered doctoral students who have a more pragmatic attitude—"since the market demands interesting research, I'll try my best to produce it in order to survive." Such students don't usually have much passion for an academic career, simply treating academia as a stable job (after gaining tenure) with decent pay. I believe our field needs the former, not the latter, kind of scholars to sustain its development.

Conclusion

The interesting research advocacy generated by Davis's article has influenced generations of management researchers. This phenomenon itself is interesting, according to Davis's own definition of interesting, because it contradicts people's usual assumption that, in addition to other merits, an influential article should be well written. Yet Davis's argument is full of errors. My critique is also interesting because under the influence of Davis's argument, many management researchers have taken for granted that a good article should be interesting. However, my critique contradicts this intuition by arguing that interestingness should not be one of the criteria for assessing the quality of an article. While interestingness has little value in science, obsessing with it leads to such harmful consequences as promoting an improper way of doing science, encouraging the practice of HARKing, discouraging replication studies, ignoring the proper duties of a researcher, and undermining doctoral education.

The interesting research advocacy seems to have been fueled more by emotion than by scientific consideration because probing into the nature of science will reveal that interestingness in fact has little scientific value. This saga illustrates that management researchers should take a cautious approach toward engaging in advocacies, especially political advocacies. A recent case in point is the response of the Academy of Management (AOM) to Executive Order 13769 signed by President Donald Trump on January 27, 2017 to bar citizens from seven Muslim-majority nations from entering the United States. AOM's response, in the form of political advocacy, altered the organization's longstanding "no political stands policy" and thus aroused a heated debate among management scholars (see Stackman et al., 2019 for a brief description of the incident). The controversy was somewhat expected given that the advocacy was inconsistent with AOM's identity as an academic organization concerned with researching management, instead of political, phenomena. Should the AOM refrain from political advocacy unless the organization itself is threatened? As an AOM member, my answer is "yes" because I prefer AOM to remain politically neutral.

It has never been my intention to promote non-interesting or boring research. (*Other things being equal*, interesting research is certainly better than boring research.) Rather, I hope to debunk the widely held myth that whether theories, theoretical propositions, or empirical findings are interesting is an important attribute of management research. My

argument is based on the premise that management is a (social) science discipline. If any management scholars think that they are not doing science, then my argument doesn't apply. Sure, if one is writing a management story, an interesting story will sell better than a boring one. But as management researchers, we have to ask ourselves: is this the kind of business we are really in?

In this essay, I review the development of the interesting research advocacy and its detrimental consequences, which far exceed its benefits. This is an unfortunate episode in the development of the management discipline, defeating our effort to strive for society's recognition of management as science. On a positive note, the question of why this advocacy emerged provides an opportunity for our collective soul-searching that hopefully will prevent similar mistakes from happening.

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Notes

1. It would be interesting to know how Davis's article could get through the supposedly rigorous review process of *Philosophy of the Social Sciences*. His style of writing suggests that he might intend to present aphorisms instead of logically coherent arguments perhaps because he thought that the former would be more likely to attract readers' attention. This writing style seems to be acceptable to the journal half a century ago. Moreover, it was published in the first volume of the journal and the review criteria were probably less stringent.
2. The same argument applies to space; that is, Einstein's theory completely rejected people's concept of absolute space. Kant offers an insightful analysis of our intuition of time and space (see Allais, 2015).
3. Bartunek et al. (2006) summarize the reasons given by *AMJ*'s editorial board members for their nomination into six categories, namely counterintuitive, quality, good writing, new theory/finding, practical implications, and impact. "Counterintuitive" is exactly Davis's meaning of interesting while "new theory/finding" is related to novelty. Their other four categories are remotely related to the meaning of interesting according to Davis or our daily usage.

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