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Point-Counterpoint

Pseudotheory proliferation is damaging the organizational sciences

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Summary In recent years, there has been an increasing emphasis on the role of theory in organizational behavior (OB) research. Authors are strongly encouraged to develop "theory" in their manuscripts and to make "theoretical contributions." This trend is in stark contrast to the process used in other fields of science. Our counterparts in those fields follow the scientific method and define theory as a concise and elegant hypothesis that has survived extensive empirical testing. Rather than being based on extensive empirical research, many of OB's theories are based on a limited number of primary studies (at best) or speculation and conjecture (at worst). OB researchers are discouraged from testing other researchers' theories or replicating previously published work. Consequently, many OB theories do not meet the criteria for a true scientific theory. We propose that OB researchers should re-embrace the scientific method and focus on creating a body of empirical research that could be used in the future to establish true scientific theories (through extensive hypothesis testing, empirical research, and conceptual replications) rather than concocting pseudotheories. Research in the personnel selection subfield of OB provides a reasonable exemplar of this model, yet it has been derided of late for being too atheoretical. Copyright © 2016 John Wiley & Sons, Ltd.

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The best approach to drawing valid conclusions in any science, including organizational behavior (OB), is to incorporate the scientific method in the study of organizational variables. Although OB researchers strive to be scientists, they are largely failing in this aspiration. In recent decades, OB research has become less of a science and more of an exercise in creative writing by its overemphasis on "theory" (Hambrick, 2007; McKinley, 2010; Pfeffer, 2007). Ironically, theory plays a critical role in science, yet our field has embraced an incorrect definition of theory, a definition at odds with most scientific disciplines. Additionally, problems with our current model for disseminating research results make it difficult to establish true scientific theories and evaluate the validity of many OB theories, especially as authors and journals shun the replication work that is needed to establish true scientific theories (Barends et al., 2012; Makel et al., 2012). In this paper, we address several questions that we have seen arise concerning theory. Specifically, we define and explain the following: (1) what a scientific theory is; (2) what makes a theory good or bad; (3) what a theoretical contribution is; (4) whether science can overemphasize theory; (5) whether researchers should have to make a theoretical contribution in their papers and presentations; (6) how papers with strong results but no strong "theory" should be viewed; and (7) implications for practice.

How Do You Define Theory? Or in Other Words, What Is Theory to You?

Many scientific fields show a strong consensus in definitions of theory. Unfortunately, OB, other areas of management, and most of the social sciences lack a consensus on a definition of theory. Sutton and Staw (1995, p. 372)

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noted a “lack of consensus [in management] on exactly what theory is.” Likewise, Corley and Gioia (2011, p. 12), noted that “there is little agreement on a universal definition” of theory in management. To help clarify a definition of theory for OB and management that is consistent with most scientific fields, Cucina, Hayes, Walmsley, and Martin (2014) reviewed descriptions of the scientific method from various fields of science and identified a general consensus for the following seven-step process:

1. “ Make an observation
2. Form a question
3. Write a hypothesis
4. Make a prediction (i.e. if the hypothesis is true, then the prediction will be true)
5. Test hypothesis using experiment or observation
6. If test supports hypothesis, then make new tests for hypothesis. If test does not support hypothesis, then revise or create new hypothesis
7. Repeat steps 1 through 6 many times. Only if a hypothesis is supported after many replications can it become a theory.” (p. 357)

Accepting their definition, we define theory as a well-studied, well-replicated, and well-supported hypothesis. It is a description or a model of a phenomenon that has been established empirically and shown to have a high probability of being correct. It is as close to absolute truth that we can get to in a science. Reserving the word theory for these types of findings is the predominant perspective in other fields of science (refer to Cucina et al. [2014] for specific examples from the hard sciences, life sciences, and interdisciplinary literatures). In the absence of a meta-analysis (with a large number of studies, a large cumulative sample size, and sensitivity analyses), it is quite unlikely that a single research study can yield a true theory; rather, it is most likely to yield a supported hypothesis, leading us only to step 6 in the scientific method.

Articles in mainstream OB journals (e.g., *Academy of Management Journal*, *Journal of Applied Psychology*, *Journal of Management*, *Journal of Organizational Behavior*, and *Personnel Psychology*) now include lengthy “theoretical” discourses in the introduction and discussion sections, often drawing theoretical conclusions that cannot be supported by the data presented in the study itself. This is a new trend that has been documented in editorial statements, recent mission statements, and word choice in published articles in OB journals (Cucina & Moriarty, 2015). For example, Klein and Zedeck (2004) indicated that theory is developed and then tested—in other fields of science, the order is the opposite. Some journals go further (e.g., *Academy of Management Review*, *Harvard Business Review*) and eschew any empirical support beyond case studies (for which $n = 1$) or pure imagination (for which $n = 0$). Articles in these journals often appear to be selling one author’s personal viewpoints and beliefs on management and employees. Few of these offered theories are ever tested even once. This is evident by Edwards, Berry, and Kay’s (2014) investigation showing that most theories proposed in *Academy of Management Review* (*AMR*) were never tested in subsequent publications. Similarly, Kacmar and Whitfield (2000) observed that of those articles citing a paper published in *AMR*, the majority did not test the hypotheses proposed in the original article. Further, not even a single hypothesis (recall that *AMR* papers tend to propose multiple hypotheses) was tested for almost half of the original articles. Thus, the theories proposed in these outlets are not true scientific theories. Instead, these are pseudotheories, the scientific equivalent of fool’s gold. This is the complete opposite of what other fields of science require for a theory (i.e., large sample sizes and many replication studies).

Additionally, the hypotheses and theories in other fields of science are concise, elegant, parsimonious, and succinct, in contrast to the elaborate hypotheses and pseudotheories in the last decade’s OB literature (refer to Cucina et al., 2014 for examples). When an inelegant pseudotheory includes a large number of variables and propositions, it becomes difficult (if not impossible) to test the theory empirically. Instead, we are left with an elaborate set of hypotheses (labeled “filigrees” by Cucina et al., p.363) containing too many interconnected pieces, predictions, and paths to be proven or disproven easily. We need to return to our roots as scientists and create falsifiable hypotheses that could lay the groundwork for future theories in OB.

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What Makes for a Good Theory or a Bad Theory?

In most scientific disciplines, a theory is a concise, well-supported, well-replicated, and well-accepted hypothesis. Anything that we call a theory that is not concise, well-supported, and well-replicated is a pseudotheory. Although there are examples of good scientific theories in the OB and management literature (e.g., goal setting theory, expectancy theory, and equity theory), most theories are actually examples of pseudotheory. Consider the terms that have been used to describe theory in OB. It has been said that a good theory should be "interesting" (Davis, 1971; p. 309) or "surprising" (Corley & Gioia, 2011, p. 16) and that you know it when you see it. Good theories are often described as "propos[ing] new explanations," (Klein & Zedeck, 2004, p. 931), "offer[ing] more than a review or integration" of past findings (p. 932) of the existing literature, and an explanation of "*why*" not "*what*" (p. 931). In contrast, a bad theory is described as something that only "involv[es] correlations among phenomena… [and that fails to inform us of something] which everyone did not already expect," that are "obvious" (Davis, 1971, p. 322). Additionally, we are told that "in and of itself…a meta-analysis does not constitute good theory" (Klein & Zedeck, p. 932). These definitions of theory are unscientific and promote inappropriate practices that harm OB as a science.

In science, whether or not a well-replicated finding (i.e., a theory) is interesting is a subjective question that depends on one's research interests. Whether a scientific theory is surprising depends on the consistency between the theory and prior knowledge of a phenomenon. It should be rare that our prior knowledge (e.g., our pre-meta-analytic understanding of a set of primary studies or lay view of a phenomenon before being exposed to scientific research findings) is so inconsistent with aggregated empirical results that we are surprised. Instead, a good theory is one that summarizes the results of numerous studies and likely confirms something we already suspected. Thus, a theory is a description or a model of a phenomenon that can be evaluated empirically. Hypotheses and theories lie along a single dimension of credibility. Credible theories have empirical support through replications—both exact and conceptual. Psychologists are making advances in exact replication studies (Open Science Collaboration, 2015), yet conceptual replications are still lacking (Cucina & Hayes, 2015).

However, even with substantial empirical support through many replications, many of our field's favored theories will be shown to be somewhat or substantially wrong because research counter to the theory has been suppressed through some combination of author and editorial decisions (Kepes, Banks, McDaniel, & Whetzel, 2012; Kepes, Bennett & McDaniel, 2014). Authors often do not make papers available through journals or other accessible outlets when the paper does not support a current favored theory or does not speculate on a new theory that explains the "aberrant" findings. Journal editors and reviewers eschew papers that do not match their theoretical orientations (Cucina & Schmidt, 2015; Kepes et al., 2012; Kepes & McDaniel, 2013; Locke, Williams, & Masuda, 2015). Whereas only statistically significant results tend to be published, we speculate that any cumulative set of research findings tied to a well-accepted theory has population parameters that are about 20 to 40% overestimated (Kepes & McDaniel, 2015). For example, Kepes and McDaniel (2015) documented the moderate to large overestimation of the correlation between conscientiousness and job performance, likely because of small effect size studies being suppressed from the literature by authors, reviewers, and editors. Since higher impact journals are more likely to publish studies with novel or counterintuitive findings, and those with large effect sizes, these journals are more likely to overestimate effect sizes (Schmidt & Hunter, 2015). Our journals tend not to accept papers with null or nil findings, which are the types of papers that could discredit a theory (Kepes & McDaniel, 2013). In addition, our journals seldom accept replications, which are the ideal type of papers to either support or discredit a theory (Kepes & McDaniel, 2013). Thus, the validity of many of our theories is suspect because of an inability to properly conduct step 7 of the scientific method ("Repeat steps 1 through 6 many times. Only if a hypothesis is supported after many replications can it become a theory," Cucina et al. [2014], p. 357).

Sometimes, we hear that theory explains "why" a phenomenon occurs in organizations as opposed to just stating that the phenomenon occurs. The why aspect of theory is important in science. For example, chemists will identify the mechanism (i.e. the interaction between different atoms forming a new molecule) of a chemical reaction. However, just as in the other sciences, for a proposed mechanism to become theory, many empirical studies would

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need to be conducted to show that the mechanism is likely true—otherwise, it is only a hypothesis not a theory. We offer that a good default position is that all theories must be at least somewhat wrong. One goal of a scientist is to conduct research that can be used to make a theory less wrong or at least to identify the boundary conditions for where the theory is more correct and where the theory is more wrong.

Researchers should not only test the boundary conditions of theories and hypotheses but also conduct crucial tests of competing theories and hypotheses. In step 6 of the scientific method, it is common for scientists in other fields to design tests that could not only provide support for the working hypothesis but also refute it by providing support for an alternative hypothesis (Platt, 1964). This approach has been identified using different terms—Platt (1964) termed it "strong inference" and Latham, Erez, and Locke (1988) termed it the use of "crucial experiments." It is commonly used by applied scientists who are identifying certain specimens (e.g., determining the species a particular specimen of bacteria, fungus, or insect; identifying the chemical composition of a particular specimen). Essentially, a scientist devises tests that would not only support the working hypothesis but also rule out several alternative hypotheses. Two predictions might even be made for the outcome of a particular test, each based on different competing hypotheses.

A series of studies by Latham et al. (1988) provided an excellent example of applying this practice to OB research. Unfortunately, despite earning the distinction of a monograph in the *Journal of Applied Psychology*, the study by Latham et al. has not led to an upsurge of similar studies. In our experience, when authors aim to crucially test two competing hypotheses, editors and reviewers often view this as hedging one's bets (e.g., ensuring that the study results in a supported hypothesis) and ask that the authors commit to one hypothesis (often the one favored by the editor or reviewer). The *de facto* policy of requiring that all hypotheses be supported by a $p < .05$ for publication only exacerbates this issue as it prevents the publication of results supporting a null hypothesis (a possible alternative hypothesis). In contrast, a good theory is one that has survived the challenge of competing hypotheses.

What Is a Theoretical Contribution?

We make a distinction between three types of theory-related contributions: pseudotheoretical contributions, true theoretical contributions, and incremental contributions. Much of what is labeled as "theoretical contributions" in the OB literature is actually pseudotheoretical writing. The elaborate and untested hypotheses that we critique above are good examples of pseudotheoretical writing. When an author proposes an elaborate mechanism for an OB phenomenon or a complex web of interrelated variables, he or she is actually proposing a hypothesis, not a theory. The hypothesis does not become a theory until it has been well-tested using multiple studies. Thus, much of what is presented as a theoretical contribution in the OB literature are actually examples of poorly constructed hypotheses that are in need of pruning and further testing. Even when the hypotheses put forth by the authors in the study are tested, there is often too little replication for the hypotheses to become theory in much of the current OB research literature.

A true theoretical contribution would occur when a researcher proposes a hypothesis and provides enough support for that hypothesis to become a theory. This type of contribution is almost mythical as it is rare that a researcher can propose a new hypothesis and complete steps 1 through 7 of the scientific method in a single paper. Of course, there are instances in which researchers have proposed a new hypothesis and showed that a synthesis of the results of previous studies (which were viewed as supporting other hypotheses) supports the new hypothesis to a degree that it can become a theory. However, these types of groundbreaking events in science are rare. Additionally, for a hypothesis to become a theory, there usually must be a consensus among the scientific community that the theory is empirically supported. Some of the great well-supported theories in science actually underwent multiple rounds of follow-up testing and debate before being accepted as a true scientific theory. Schmidt and Hunter's (1977) theory of validity generalization of general mental ability is a good example of a groundbreaking scientific theory that began as a new hypothesis and was then tested using a synthesis of previous studies viewed as supporting situational

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specificity (the opposite of validity generalization). However, validity generalization was initially met with skepticism and required a number of follow-up publications before being accepted (Cucina & Schmidt, 2015; Schmidt, Hunter, Pearlman & Hirsch, 1985).

What is perhaps a more common theoretical contribution is for a researcher to propose a hypothesis and provide some initial support for it. As additional tests are conducted, the hypothesis eventually becomes a theory, and the concept behind the theory is attributed to the author of the hypothesis. Locke and Latham's (1990, 2002) goal-setting theory, for which the research program was described as a "35-year odyssey," (Locke & Latham, 2002, p. 705) is a good example of this type of theory. Albert Einstein is known as perhaps the greatest developer of theories in science; however, his ideas would still just be hypotheses if it were not for the empirical support that exists. In fact, several of his hypotheses and ideas turned out to be wrong or lacking in support. For example, he proposed that a factor named lambda allowed the size of the universe to remain constant (Wright, 2004) and initially dismissed the big bang theory, telling its creator "your grasp of physics is abominable" (Midbon, 2000, p. 18). His 30-year effort to create a unified field theory that combined the laws of electromagnetism and gravity was described as a "fruitless quest" by Tretkoff (2005). Let this be a cautionary tale to management theorists who think they are emulating great scientists by developing pseudotheories.

The third type of contribution is incremental, and most scientific publications fall into this category. The scientific method is an incremental process, especially steps 6 and 7. Ordinarily, many articles would need to be published before there is evidence that a hypothesis is a theory, and even then, additional tests of the theory can be published. Thus, every well-executed primary study contributes in a small way toward the development of theory. Cumulative research (e.g. meta-analysis) presents a stronger contribution to the development of new theory as it tests whether or not a hypothesis is well-supported. A hypothesis begins to achieve the status of theory when its meta-analytic support has small credibility and confidence intervals about its point estimates of the effect size (or robust evidence for a moderating effect). However, if other more elegant hypotheses can explain the results of the meta-analysis, then the first hypothesis is no longer a theory. Researchers who initially propose a hypothesis could possibly make a stronger eventual contribution to the development of a new theory. However, their hypotheses are of little value and cannot become a theory, if they are untested.

Much of what is now labeled as a "theoretical contribution" in the OB literature is antithetical to the label. It is neither theoretical (instead it is poorly conceived hypotheses) nor a scientific contribution (in that it does not follow the scientific method). Nevertheless, journal editors ask that authors provide theoretical contributions in their manuscripts and authors attempt to comply. Consequently, journal articles have become longer. Spector (2015) has shown that the samples of articles in *Journal of Applied Psychology* (*JAP*) have increased from 28 paragraphs in 1971 to 68 paragraphs in 2015. Highhouse (2014, 2015) has also noted the increase in article length over time. Longer articles leave less space available for replications, and readers must wade through an expansive morass of pseudotheory in order to make sense of a journal article.

It is even more difficult for readers to make sense of the body of OB literature as there are too many competing theories and few empirical tests of the theories themselves and the overlap among the theories. The use of computerized text analysis to identify redundant constructs and ideas in previously published theoretical articles is an interesting reactive approach to reconciling this issue (Larsen & Bong, in press). A proactive approach would be to refocus on the scientific method and spend more time developing elegant well-thought out hypotheses about different constructs and rigorously testing, revising, and retesting them. This approach will eventually yield sound non-redundant scientific theories and make a contribution to organizations and our literature about them.

Is it Possible for Science to Place too Much Emphasis on Theory?

It is impossible for science to place too much emphasis on true scientific theory. The final step in the scientific method is the establishment of theory (step 7). Many scientific fields have well-established theories, and this

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information leads to a solid knowledge base. In contrast, the psychology and management knowledge bases include valid (e.g., operant conditioning, goal setting theory), invalid (e.g., multiple intelligences theory, some leadership theories), and untested theories (e.g., many of those put forth in *AMR* and the introduction sections of articles in the *JAP* and *Personnel Psychology*). Consequently, consulting industrial psychologists and OB researchers are left with only a few valid theories to put into practice. The focus on novel ideas and the lack of rewards (e.g., publications, tenure) for testing someone else's hypotheses and conducting replications means that psychology and OB are overemphasizing the wrong type of theory (i.e., pseudotheory). In many ways, recent trends in our literature are leading us to become organizational alchemists rather than organizational scientists. Now is the time for change.

In contrast to theory, it is possible for a scientific field to place too much emphasis on pseudotheory. The OB, industrial/organizational (I/O) psychology, and management research fields are examples of fields of science that are currently placing too much emphasis on pseudotheory. Articles that contain large numbers of new hypotheses are more valued than those testing previously stated hypotheses. Nowhere is this more evident than in the "review" journals (e.g., *AMR*), which only publish speculative ideas. These outlets are essentially stuck at step 3 of the scientific method. Although these journals are well cited and apparently well read, the hypotheses rarely make their way into empirical studies. Many of the journals that allow the presentation of empirical data require the creation of new theories for each article. Thus, the scientific method is allowed to proceed from step 3, but it stops at step 5. By placing too much emphasis on hypotheses and pseudotheory, the OB, I/O psychology, and management research community is preventing itself from following the scientific method. Consequently, true scientific theories cannot be developed, leading to a poor understanding of the true nature of organizational phenomena.

Should Papers and Conference Presentations Be Required to Make a Theoretical Contribution?

We do not believe that papers and presentations should be required to make theoretical contributions. We should be modeling our OB and management research on the top journals in science, like *Science* and *Nature*. Articles in *Science* and *Nature* have strict word limits, and the writing focuses primarily on the empirical work. There is no pseudotheorizing that plagues OB research like an ever-encroaching fungus. Instead, they follow the scientific method. Kuncel and Hezlett (2007) and Owen et al. (2010) are some good examples of behavioral studies in these journals.

Unfortunately, many (perhaps most) of the new theories in management and OB are not credible (i.e. are not theories in a scientific sense) because they lack repeated replication. Oftentimes, only part of the new theory is tested in the publication that proposed it. Rather than replicating a potentially theory-relevant result, the behavior that is most rewarded is to come up with a new theory. Our journals tend to reward the new idea rather than replication efforts to find knowledge that is robust. Our authors behave in a manner to get the rewards. This reward structure encourages publication bias in our literature (Kepes & McDaniel, 2013; Rothstein, Sutton, & Borenstein, 2005). In sum, the publication process in OB is dysfunctional and often a detriment to the trustworthiness of our cumulative knowledge.

The only way to truly make a brand new theoretical contribution in science is to suddenly discover, summarize, and consolidate a large body of pre-existing research or to devote many years toward conducting multiple studies. Schmidt and Hunter's theory of validity generalization (1977) is one example that has involved decades of research. These types of contributions typically come once in a lifetime, and they involve multiple papers with repeated replications. The practice supported by OB and management journals is best described as one that typically generates one pseudotheory per paper. Journal policies, which focus on the new rather than replications that help establish the true (Pfeffer, 2007) hamper the establishment of true scientific theories in OB and management.

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What Can Be Made of a Study that Has a Strong and Reliable Empirical Finding but no *a priori* "Theory" Besides a Plausible Hypothesis?

Such a study should be valued and efforts should be made to replicate it. Oftentimes, many iterations of the scientific method were required before a finding was discovered. We need to recognize that many of the major findings in science have started with a reliable empirical finding (e.g., Wootton, 2010). Reviewers, readers, and editors often fear that results from such a study may have stemmed from "cooking the data" to find a significant result. Requiring an elaborate theory does not eliminate this risk. Instead, replication is a better solution. Unfortunately, OB journals tend to discourage the publication of results from endeavors that went through multiple rounds of the scientific method (e.g. testing different hypotheses before arriving at the correct one). Nevertheless, this approach is completely appropriate in science and had led to some of science's greatest contributions. For example, Watson and Crick (1953; Watson, 2001) tested many different models for the structure of DNA before arriving at the correct one. Today, an OB or management journal would likely reject the paper because there is no theory and the results were not hypothesized in advance. As long as this continues, we will continue to be a pseudotheory discipline. Ryan and Ployhart (2014) noted that personnel selection research is "eroding" from the OB literature as it is viewed as "too atheoretical" (p. 710). This was corroborated by Aguinis, Bradley, and Brodersen (2014) who surveyed Fellows of the Society for Industrial and Organizational Psychology and observed that personnel selection research is often viewed as "less theoretical" than other areas of organizational research. Because the top OB and management journals look for authors to make theoretical contributions (Cucina & Moriarty, 2015), research that is viewed as non-theoretical (e.g., personnel selection) is "less likely" to be published in these outlets according to Aguinis et al. It is ironic that an OB subdiscipline (i.e., personnel selection) is being pushed out of mainstream OB research journals mainly because it is too much in tune with the scientific method.

Should Practitioners Implement Theories Consisting of Propositions and Hypotheses (Without any Empirical Support) or Wait for Well-Established Theories with Strong Empirical Support?

Ostensibly, a key goal of the OB and management literature is to produce knowledge that leaders and organizations can implement to make their workplaces more effective. Nevertheless, we are in a situation where there are few real scientific theories to implement. Implementing a theory that does not yet have support runs the risk of wasting an organization's money and destroying our scientific credibility. We need to accept the fact that OB, with its preoccupation with pseudotheories, has relatively little more than pseudotheory fads to offer managers (Kepes, Bennett, & McDaniel, 2014). There are many examples of unsupported pseudotheories, fads, and well-intentioned, but unsupported beliefs and practices being implemented in applied settings:

- Generational differences,
- Emotional intelligence as a unique construct,
- Multiple intelligences and practical intelligence in educational settings,
- Learning styles in education and training,
- Myers–Briggs Type Indicator in work settings,
- Much of the diversity training in organizations, and
- Team development exercises using games like paintball, falling backwards off of a ladder, etc.

Through OB's pseudotheories, some researchers have become promoters of snake oil rather than scientists. Nevertheless, managers need to make decisions every day. These decisions will be made regardless of the status

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of management theories. It is unfortunate that managers implement pseudotheoretical ideas because an expert told them that it was backed by published research. What will the OB and management researchers of the next century think of our implementation of incorrect theories and the money that is wasted on practices that are as effective as a sugar pill placebo? To narrow the science–practice gap, we need to work collectively to adopt the scientific method and develop true theories that yield actionable findings for organizations and provide recommendations based on solid empirical evidence. We need a scientific renaissance in OB and management research. Such an effort could take decades; however, the end result (i.e., independent variables that improve the workplace) will likely be of more interest to society than the "interesting" pseudotheory with which we now are confronted. Unfortunately, today's organizations cannot wait until we have strong empirical support for an idea before implementing it.

Conclusion

In our quest for theory, OB and management journals publish too many papers containing ideas that have no scientific basis and contribute almost nothing to improve the world of work. Scientific theory building is an incremental process that requires a large number of empirical studies. This is in stark contrast to suggestions that theoretical contributions in the management literature are stronger when they are a "revelation" rather than being "incremental" (Corley & Gioia, 2011, p. 15–16). We do not anticipate much progress in OB, management, and related disciplines until the fields eschew the neo-Freudian approach of creating pseudotheories and instead adopt the theory building approach common to most scientific disciples. Our current pseudotheories continue to proliferate and place the OB discipline in a clown car that meanders to no good end.

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