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OPERATIONS, INFORMATION & TECHNOLOGY | REVIEW ARTICLE

Lakatos revisited: Innovation and 'Novel facts' as a foundational logic for the social sciences in an era of 'Post-truth' and pseudoscience

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Abstract: In what seems to be a "post-truth" era this paper argues that management theory development is necessary to provide insights into how to manage the academic research, or discovery system, itself, as it no longer seems clear that its explicit purpose, as per Lakatos's argument, is the development of theory that predicts "novel facts." This logic questions the extent to which the social science research system is inherently innovative. To explore these issues, the topic of pseudoscience is used as a heuristic, in order to derive an understanding of how the purpose of the discovery system seems to be understood by different stakeholders. In doing so, this paper explores how researchers may need to manage their research fields to negotiate contested academic terrain and develop what Lakatos refers to as progressive research programmes. It is argued that hypervigilance and overly conservative gatekeeping in response to threats of pseudoscience can run deep in the structure of academic engagement, contributing to a paradigm of constrained innovation. Drawing from post-normal science theory, a conceptual framework is identified for how these problems might be addressed by a focus on the development of scalability in the research process itself, without compromising rigor.

Subjects: Business, Management and Accounting; Philosophy; Philosophy of Science; Epistemology

Keywords: Management theory development; innovation management; knowledge management; innovativeness; pseudoscience; Lakatos



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PUBLIC INTEREST STATEMENT

In an era of "Post-Truth" and politicised scientific findings, one can lose sight of the consequences of a dangerous form of pseudoscience, defined by Lakatos as a failure of academic fields to produce novel facts, or innovative discoveries. Economic theorists explain why it is getting harder to do this, and evidence suggests that the pace of global innovation and technical progress is slowing down. The large-scale innovations of the years 1879–1970 have not been matched since irrespective of recent technological change. This paper uses Lakatos's ideas to better understand pseudoscience and to provide a novel perspective of how we currently create knowledge, and how this can be improved.

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1. Introduction

The Oxford Dictionary "Word of the Year" for 2016 was "post-truth," defined as "relating to or denoting circumstances in which objective facts are less influential in shaping public opinion than appeals to emotion and personal belief" (Peters, 2018, p. 563). Indeed, according to Peters (2018, p. 563):

> One major consideration about the shift to post-truth is the 'truth carrying capacity' of new social media and its propensity to disseminate fake news through Facebook, Google and Twitter, and thereby to create a 'bubble world' where algorithmically selected news sources simply reinforce existing prejudices thus compromising the capacity for moral thinking.

A recent and quickly growing literature explores how this post-truth era has emerged, and makes explicit its potential consequences (see Lewandowsky, Ecker, & Cook, 2017; McIntyre, 2018; Neimark et al., 2019; Rider & Peters, 2018). Some have highlighted the need for scientific research into misinformation itself and its consequences (Lewandowsky et al., 2017). Indeed, according to Lewandowsky et al. (2017, p. 353) "the post-truth world emerged as a result of societal mega-trends such as a decline in social capital, growing economic inequality, increased polarization, declining trust in science, and an increasingly fractionated media landscape." Theory development and practice in the field of management are not independent of these influences, and both management scholars and practitioners may benefit from an improved understanding of how these trends may influence the fundamental nature of the social and natural scientific research process itself.

Declining public trust in science has also occurred due to the proliferation of invalid, or junk science (Dadkhah, Lagzian, & Borchardt, 2018). The term junk science has its origins in criticisms of the quality of scientific evidence of expert testimonies in legal proceedings (Giannelli, 1993). Discussions of junk science seem to fall into the category of "pseudoscience." The deliberate or non-deliberate use of invalid science to further political agendas can also be described as pseudoscience.

Recent discussions of science stress how science seems to be increasingly politicised. Science "and research findings have become a battleground on which to influence the opinions and beliefs of the general public" According to Mainous (2018, p. 490)

> We are in an era where authority figures will manipulate and discount information being released to the public that doesn't fit their worldview or support their plan of action. We hear constant claims of 'fake news' for positions with evidence while simultaneously hearing support for policy positions that clearly have no scientific evidence. The Environmental Protection Agency (EPA) removed scientists from panel discussions on climate change so that the scientists were unable to publically present their findings. Further, although evidence indicates that childhood immunizations are not associated with autism, the persistent believe in the harmfulness of childhood immunisations and the corresponding information disseminated by anti-vaccine groups has led to low vaccine rates and measles outbreaks in places like the Somali community in Minnesota.

In light of certain suggestions that trust in science is declining (Lewandowsky et al., 2017), and the seeming advent of a post-truth era (Peters, 2018), the objective of this paper is to provide a critical review of the notion of pseudoscience, and to highlight its dangers for the scientific agenda associated with the discovery of novel facts (Lakatos, 1978). It is argued here that Lakatos's (1978) approach to pseudoscience is particularly important, as it highlights the costs and dangers to society of a failure of research to solve pressing research problems of societal importance. As will be explained in the sections that follow, Lakatos's approach differs from other treatments of the pseudoscience phenomenon, and the discussions here delimit themselves to this approach. In doing so, certain longstanding assumptions about how we create knowledge are revisited. It is also suggested that in an era of "Post-Truth" discourse, it may be easier to lose sight of the dangers of

pseudoscience that derive from Lakatos's seminal perspective- and the costs to society of declining returns to investments in research.

The paper takes the following structure. First, in Section 2, pseudoscience as it is used here is defined, and practical examples of the consequences of pseudoscience are considered. Next, in Section 3, Lakatos's (1978) reconciliation of the tensions between Popperian and Kuhnian theory is explored. In Section 4, the political characteristics of debates about pseudoscience are considered, and then in Section 5 they are related to the processes by which new academic fields develop. In Section 6, discussions then move on to the prevalence of hierarchies and network dynamics in the social and natural sciences. Finally, a theoretical framework premised on post-normal science is used in Section 7 to derive the argument that it is necessary to keep science aligned with Lakatos's core idea of the purpose of science, namely the continued discovery of "novel facts." It is concluded that much of the social scientific work we do does not conform to this bedrock rationale, and is therefore insufficiently innovative. Definitions and examples of pseudoscience are now introduced.

2. What is pseudoscience and why is Lakatos's perspective important?

In order to understand the nature and dangers of pseudoscience, it is necessary to define it. Pseudoscience can naively be defined as false or incorrect science, or even empirical rejectionism, the intentional rejection of empirical evidence. It is, however, a contested term, with many contested characteristics[1], and its delimited definition for the purposes of this work will therefore be explicitly framed in terms of Lakatos's (1978) reconciliation of the work of Popper and Kuhn. In other words, the definitional approach to the term pseudoscience taken here is strictly delimited to Lakatos's (1978) demarcation—of an academic research programme's failure to uncover "novel facts," a perspective that broadly accords with other work, such as that by Thagard (1978) and Rothbert (1990). An alternative, and broader, definition is provided by others, including Hansson (2009).

According to Hansson (2009, p. 237) a text is pseudoscientific "if and only if it (1) pertains to an issue within the domains of science, (2) is not epistemologically warranted, and (3) is part of a doctrine whose major proponents try to create the impression that it is epistemologically warranted." Whereas there are certain advantages to this definition, it does not bring scrutiny to bear on the implications of Lakatos's notion of a failure to generate novel facts. It is argued here that certain areas of the contemporary scientific discovery, or research, system are gridlocked—they are not producing novel facts quickly enough to solve serious societal problems.

Empirical evidence of this gridlock can be found in the relationships between research investments and declining total factor productivity. According to Gordon (2014, p. 2):

> ... advances since 1970 have tended to be channeled into a narrow sphere of human activity having to do with entertainment, communications, and the collection and processing of information. For the rest of what humans care about- food clothing, shelter, transportation, health, and working conditions both inside and outside the home- progress slowed down after 1970, both qualitatively and quantitatively. Our best measure of the pace of innovation and technical progress is total factor productivity (hereafter TFP), a measure of how quickly output is growing relative to the growth of labor and capital inputs. TFP grew after 1970 at barely a third the rate achieved between 1920 and 1970.

This paradox also seems to be reflected in "puzzling trends in industrial research, patenting, and productivity growth" whereby patenting has "been roughly constant as research employment has risen sharply over the last forty years" (Kortum, 1997, p. 1389). Similarly, according to Cowen (2011, p. 2), the "pace of technological development has been slowing down" as we are reliant on the advances of previous technological change. An empirical example of this gridlock relates to healthcare R&D: per billion US dollars of R&D spending, the number of new drugs approved has

halved about every nine years since 1950- falling around "80-fold in inflation-adjusted terms" (Scannell, Blanckley, Boldon and Warrington, 2012, p. 191). Certain economic models show—theoretically—why scientific breakthroughs are becoming increasingly harder to achieve (see Jones, 2009; Kortum, 1997; Segerstrom, 1997). It is this body of evidence and theory that highlights Lakatos's perspective as particularly useful in light of its insights. It needs to be acknowledged, however, that his perspective differs from many other seminal perspectives of pseudoscience.

For example, according to Hansson's (2017, p. 1) criticism, "Lakatos would classify a nonprogressive discipline as pseudoscientific even if its practitioners work hard to improve it and turn it into a progressive discipline." This criticism needs to be acknowledged here, in that the focus of this paper is delimited. As discussed, the focus of this paper relates to a primary concern of the knowledge creation literature—its failure to deliver societally important inventions and discoveries to those who need them the most. Given the importance of the definitional understandings of pseudoscience considered here, Lakatos's demarcation of science from pseudoscience is considered in detail in the following sections.

Developing scholarly knowledge of pseudoscience perhaps increasingly important. According to Mainous (2018, p. 490):

> In a world of misinformation, public understanding of medical research and advances has never been more important. Unfortunately, the general public is targeted with messages by groups who may not be providing the public with the most accurate information. Furthermore, scientists who only disseminate their findings to each other and write only for other scientists miss a key constituency- the general public.

An awareness of pseudoscience and its risks is therefore important to management scholars as well as practitioners, as organisations are nested within their economic and socio-political contexts. As already discussed previously, the political forces that seek to de-prioritise climate change research and its influence on the management of organisations is one example that highlights the importance of this knowledge for managers.

Other examples of the dangers of contemporary pseudoscience exist. Another is the notion (originally derived from a published academic paper) that vaccines can cause autism. This has led to measles outbreaks. A failure to vaccinate due to other beliefs has also led to the re-emergence of diseases like polio in certain regions and communities across the world.

White (2014) applies social network theory to illustrate how dishonest research, contradictory scientific results, mass media and public opinion contributed to a debate on vaccination and its potential links to autism. This debate in turn resulted in a fall-off in vaccinations and a seven hundred percent rise in measles outbreaks in the United States (U.S.).

Despite overwhelming scientific data and evidence in support of no link between vaccination and autism, certain studies have found *one in four* in the U.S. *continue to believe* vaccinations cause autism, and many refuse to vaccinate their children, causing a break down in herd immunity, or the universal vaccination benefits that typically result from complete vaccine coverage in a population (White, 2014).

At the roots of this debate, however, are the actions of an initial academic author of a 1998 study claiming evidence of the link. Media, however, "substantiated inaccurate science by publishing erroneous evidence, indulging in celebrity testimony, and balancing credible science with fear-based anecdotes" (White, 2014, p. 270). Rejecting rigorous scientific evidence the media "continued to cover politicians and celebrities who attacked science" (White, 2014, p. 270). Ironically, while seeking to continually "balance" both sides of debates, media-

constructed notions of science were effectively produced, ultimately legitimising pseudoscientific notions, with a cost in human life.

Another example of pseudoscience, this time in the South African context, is the large-scale loss of human life caused by scientific denialism and dissonant academic attacks on objective science that took place during the South African AIDS crisis. According to Nattrass (2008) this crisis can be traced back to academia in the U.S., as accomplished (highly respected at the time) academics, such as Peter Duesberg argued that AZT caused AIDS rather than treating or preventing it. The President at the time, Mbeki, convened a Presidential AIDS Advisory panel (half orthodox scientists, half AIDS denialists), and its denialist members asserted "AIDS would disappear instantaneously if all HIV testing was outlawed and the use of antiretroviral drugs was terminated" (Nattrass, 2008, p. 162). Little discussion or criticism seems to persist on these events.

These examples seem to highlight Kuhn's (1970) predictions, that science, and its applications can be shaped by human values and behaviours in a way that overshadows the (substantial) accumulation of objective evidence. These examples also challenge the assumptions made by some that empirical rejectionist behaviour is largely benign. Such examples therefore highlight the dangers of the selective use of academic ideas in different contexts.

Resistance to scientific thought itself has a long history. Contestations between empirical rejectionist belief systems and science, and the catastrophic costs in human life caused by belief-system resistance to science throughout history, is well documented (see Callaghan, 2015). According to Kuhn (1970), science does not progress neutrally, but its progression is based on the acceptance of sets of ideas and beliefs in the form of "paradigms," which typically results in the rejection of new ideas that are in conflict with a dominant paradigm, until countervailing evidence accumulates sufficiently so as to overturn an existing paradigm. For Kuhn, this process represents "normal science."

Another baffling example of pseudoscience is the experience of Semmelweiss (Best & Neuhauser, 2004). Semmelwiess discovered how to drastically reduce maternal mortality by introducing antiseptic hand washing prior to surgery. Despite empirical evidence of his success, he was shunned by the medical fraternity for this innovation. They implemented previous practices, with mortality rates returning to their previous levels. This rejection of evidence can be taken to be a further example of pseudoscience. The continued use of the QWERTY keyboard might reflect the strength of human resistance to innovations. Its typeface dates back to a period when human typing needed to be slowed due to allow keys to operate sequentially, so that they did not jam. The typeface that now allows the quickest biometrical typing speed is still not the general standard. The discussion of these ideas may be particularly important in a post-truth context in which political forces may have the ability to shape research agendas, and managers might take in pseudoscientific information, such as that which seeks to suggest that environmental concerns are not supported scientifically.

How then have seminal theorists differentiated between science and pseudoscience? One of the most respected scholars of scientific thinking is Lakatos.

3. Insights from lakatos into the differentiation of pseudoscience from science

For Still and Dryden (2004, p. 273), Kuhn's theory "seemed to put a distance between nature and scientific practice, and to undermine Popper's principles of demarcation" as this "apparent threat to science's reputation triggered energetic attempts to reconcile Kuhn and Popper, and to re-establish boundaries between science and pseudoscience." Still and Dryden (2004) point to the reconciliation of Popper and Kuhn's theories that Lakatos attempted, and the importance of Lakatos's work in this regard.

Although Popper's falsifiability criterion may have established the importance of empirical evidence in debates about the role of science (natural and social), Lakatos (1978) disagrees with

the notion that Popper's ideas can be used to different science from pseudoscience. With regard to the differentiation of pseudoscience from non-pseudoscientific science Lakatos therefore asks the following (1978, p. 2) (certain sentences are bolded, to highlight their importance for the continued discussions here):

> **Is, then, Popper's falsifiability criterion the solution to the problem of demarcating science from pseudoscience? No. For Popper's criterion ignores the remarkable tenacity of scientific theories**. Scientists have thick skins. They do not abandon a theory [merely] because facts contradict it. They normally either invent some rescue hypothesis to explain what they then call a mere anomaly and if they cannot explain the anomaly, they ignore it, and direct their attention to other problems. Note that scientists talk about anomalies, [recalcitrant instances,] and not refutations. History of science, of course, is full of accounts of how crucial experiments allegedly killed theories. But all such accounts are fabricated long after the theory has been abandoned. [Had Popper ever asked a Newtonian scientist under what experimental conditions he would abandon Newtonian theory, some Newtonian scientists would have been exactly as nonplussed as are some Marxists.] **What, then, is the hallmark of science? Do we have to capitulate and agree that a scientific revolution is just an irrational change in commitment, that it is a religious conversion?** Tom Kuhn, a distinguished American philosopher of science, arrived at this conclusion after discovering the naivety of Popper's falsificationism. **But if Kuhn is right, then there is no explicit demarcation between science and pseudoscience, no distinction between scientific progress and intellectual decay, there is no objective standard of honesty. But what criteria can he then offer to demarcate scientific progress from intellectual degeneration?**

To avoid all potential misunderstandings here (which seem to pervade much of the contested literature on pseudoscience), evidence of Lakatos's arguments are included here in the form of quotes of his verbatim text. Indeed, Kuhn's (1972) logics, taken to extreme, suggest that it is the tide of human values that forms the basis for how science, both natural and social, progresses. A management scientist might question the extent to which scientific fields are in fact shaped by the commercial agendas of their practitioner fields. To innovate away monopolies, for example, might work against such agendas.

Another seminal author that has written extensively on pseudoscience is Bunge. Bunge (1984) acknowledges that attempts to characterise science, and pseudoscience, by a single feature, have typically failed. Candidates for characterisations as pseudoscience have typically been arrived at on the basis of consensus, empirical content, success, refutability, or the use of the scientific method, for example. Bunge suggests that both science and pseudoscience are too complex to be characterised without a consideration of their different and multiple features. Both fit the description of a cognitive field, a sector of human activity that seeks to gain, diffuse, or use knowledge, irrespective of whether this knowledge is true or false. However, for Bunge, these fields differ according to whether they are research fields (such as the humanities, mathematics, basic science, applied science and technology, which includes medicine and law), or belief fields (such as religions, political ideologies, and pseudosciences and pseudotechnologies). Here, beliefs are the identifying characteristic of pseudoscience.

More specifically, Bunge (1984) suggests that 12 characteristics of fields typically jointly describe pseudoscience. Such fields largely exhibit *limited amounts of change*, and a general *unresponsiveness*, except to controversy and external pressures. They are typically comprised of *communities of believers*, who might label themselves scientists but do not typically conduct quality scientific research. Certain of their phenomena under study are *not certifiably real*. The free search for truth is not prioritised, and *dogma, even deception*, prevails. *Logic* and mathematical models are rarely present, and if they are, then they are *not empirically testable. Little is learned from other cognitive fields, and few interlinkages exist* with other fields. *Practical problems* are more often the focus than cognitive ones. Their bodies of knowledge are typically stagnant and are primarily comprised of *untestable or false hypotheses which conflict with external well-supported hypotheses*. There is little in the way of *development of laws* or their use in understanding and predicting facts. Their

methodologies are typically not falsifiable, and *criticism is not welcomed*. Unlike the genuine sciences, there is *no system* of pseudosciences with overlapping networks, and every pseudoscience is practically isolated. Bunge's (1984) characterisations seem to accord with Lakatos's (1978) observations of Kuhnian normal science, which at the extreme is not anchored to empirical roots but to the shifting values of human beings. According to the Kuhnian perspective, all these characteristics might fit certain academic research programmes, as there is no inherent overriding necessity for a scientific endeavour to conform to Popper's logics of falsifiability.

Indeed, as Lakatos (1987) observes, according to Kuhnian logics any branch of social science can defy any attempt at quantifying its quality. This gives rise to the further question of what characterises science (both natural and social) itself, if Kuhnian logics prevail (and cannot be logically contested)? It is here that Lakatos makes explicit his solution to this problem. Lakatos (1978, p. 3) explains further as follows:

> In the last few years I have been advocating a methodology of scientific research programmes, which solves some of the problems which both Popper and Kuhn failed to solve. **First, I claim that the typical descriptive unit of great scientific achievements is not an isolated hypothesis but rather a research programme**. [Science is not simply trial and error, a series of conjectures and refutations.] 'All swans are white' may be falsified by the discovery of one black swan. But such trivial trial and error does not rank as science. Newtonian science, for instance, is not simply a set of four conjectures—the three laws of mechanics and the law of gravitation. These four laws **constitute only the 'hard core'** of the Newtonian programme. **But this hard core is tenaciously protected from refutation by a vast 'protective belt' of auxiliary hypotheses**. And, even more importantly, **the research programme also has a 'heuristic', that is, a powerful problem-solving machinery, which, with the help of sophisticated mathematical techniques, digests anomalies and even turns them into positive evidence** ... Now, Newton's theory of gravitation, Einstein's relativity theory, quantum mechanics, Marxism, Freudism, are all research programmes, each with a characteristic hard core stubbornly defended, each with its more flexible protective belt and each with its elaborate problem-solving machinery. Each of them, at any stage of its development, has unsolved problems and undigested anomalies. **All theories, in this sense, are born refuted and die refuted. But are they equally good?** Until now I have been describing what research programmes are like. **But how can one distinguish a scientific or progressive programme from a pseudoscientific or degenerating one?** Contrary to Popper, the difference cannot be that some are still unrefuted, while others are already refuted. [When Newton published his Principia, it was common knowledge that it could not properly explain even the motion of the moon; in fact, lunar motion refuted Newton.] Kaufmann, a distinguished physicist, refuted Einstein's relativity theory in the very year it was published. **But all the research programmes I admire have one characteristic in common. They all predict novel facts, facts which had been either undreamt of, or have indeed been contradicted by previous or rival programmes.**

For Lakatos (1978), therefore, a pseudoscientific programme is one that is degenerating because it *fails to predict novel facts*. Some have suggested that the current research, or discovery system, is failing to innovate, or to produce the breakthroughs that are required to solve growing societal problems.

For example, a persistent lack of progress in producing biomedical research outputs (Grasela & Slusser, 2014; Munos, 2009), notwithstanding explosive technological advances in underlying scientific fields (Hayden, 2014), raises questions about the current state of science on the basis of Lakatos's notion that progressive research programmes should be defined primarily by their production of novel facts. It is been recently argued that the proliferation of technology may be driven by an explosion of data and information that has not been matched by a commensurate explosion in knowledge and wisdom, which can also produce inequality in access to the benefits of science (see Callaghan, 2018, p. 2019). From this perspective, certain societal problems are being created or intensified by emergent technologies, and it might only be by increasing the innovativeness of the discovery process that these knowledge asymmetries might be addressed.

In light of the importance of these arguments, and in order to ensure the precision of these arguments, Lakatos's (1978, p. 3/4) words are again revisited here, in their verbatim unambiguity:

> Thus, in a progressive research programme, **theory leads to the discovery of hitherto unknown novel facts**. In degenerating programmes, however, theories are fabricated only in order to accommodate known facts ... To sum up: [The hallmark of empirical progress is not trivial verifications: Popper is right that there are millions of them. It is no success for Newtonian theory that stones, when dropped, fall towards the earth, no matter how often this is repeated. But,] so-called 'refutations' are not the hallmark of empirical failure, as Popper has preached, since all programmes grow in a permanent ocean of anomalies. **What really counts are dramatic, unexpected, stunning predictions**: a few of them are enough to tilt the balance; where theory lags behind the facts, we are dealing with miserable degenerating research programmes. **Now, how do scientific revolutions come about?** If we have two rival research programmes, and one is progressing while the other is degenerating, scientists tend to join the progressive programme. This is the rationale of scientific revolutions. But while it is a matter of intellectual honesty to keep the record public, it is not dishonest to stick to a degenerating programme and try to turn it into a progressive one.

How then does science move forward, if not by Kuhnian revolutions, and if not according to Popper's logics? And what is the importance of the differentiation between science and pseudoscience? And what if slow or non-innovative scientific responses to crises are also taken to be unethical (Fenton, Chillag, & Michael, 2015)? As discussed previously, some have argued that the productivity of the research system itself has fallen off since the 1970s (See Gordon, 2016 for a full discussion of this as well as evidence of this decline). Other theoretical models predict that scientific breakthroughs are becoming increasingly harder to achieve over time (see Kortum, 1997; Segerstrom, 1997; Jones, 2009) for models from the field of economics which offer reasons for this).

Is our current discovery system dominated by degenerating research programmes? How well does our scholarly communication (publishing) system accommodate breakthrough ideas? It is known that some of the highest cited papers in academic history, and some of its most influential books were first rejected by journal reviewers and editors, including works that were eventually awarded the Nobel Prize in the fields of Physics, Chemistry, Physiology and Medicine (Campanario, 2009). Having contested Popper's logics, Lakatos (1978, p. 4) turns to more fully contest Kuhns's (again, the unambiguity of his verbatim eloquence serves to advance our understanding of this contested terrain):

> As opposed to Popper the methodology of scientific research programmes does not offer instant rationality. One must treat budding programmes leniently: programmes may take decades before they get off the ground and become empirically progressive. Criticism is not a Popperian quick kill, by refutation. Important criticism is always constructive: there is no refutation without a better theory. Kuhn is wrong in thinking that scientific revolutions are sudden, irrational changes in vision. [The history of science refutes both Popper and Kuhn:] **On close inspection both Popperian crucial experiments and Kuhnian revolutions turn out to be myths: what normally happens is that progressive research programmes replace degenerating ones**. The problem of demarcation between science and pseudoscience has grave implications also for the institutionalization of criticism. Copernicus's theory was banned by the Catholic Church in 1616 because it was said to be pseudoscientific. It was taken off the index in 1820 because by that time the Church deemed that facts had proved it and therefore it became scientific. The Central Committee of the Soviet Communist Party in 1949 declared Mendelian genetics pseudoscientific and had its advocates, like Academician Vavilov, killed in concentration camps; after Vavilov's murder Mendelian genetics was rehabilitated; but the Party's right to decide what is science and publishable and what is pseudoscience and punishable was upheld. The new liberal Establishment of the West also exercises the right to deny freedom of speech to what it regards as pseudoscience ... **All these judgments were inevitably based on some sort of demarcation criterion. And this is why the problem of demarcation between science and pseudoscience is not a pseudo-problem of armchair philosophers: it has grave ethical and political implications.**

Lakatos (1978), in refuting both Popperian and Kuhnian logics, offers us a mechanism through which social and natural science progress, but this mechanism reduces to his logics of novel facts. It does not address the problem of human agendas that can shape scientific ones. The answer Lakatos offers to those seeking to transcend a post-truth era is that over time progressive research programmes will come to dominate degenerating ones. But what if powerful interests subvert this slow march to progressive science? He acknowledges here the fact that the story is not really all about novel facts. It is therefore not all about innovation, or the rate at which breakthroughs are attained. They are also political.

4. Are debates about pseudoscience not themselves inherently political?

What if the term pseudoscience is itself wielded in support of political agendas? In response to the pseudoscience literature, McNally (2003) stresses that "[t]he term 'pseudoscience' has become little more than an inflammatory buzzword for quickly dismissing one's opponents in media soundbites." He suggests that this is a problem particularly acute in certain fields, for example, sociobiology and evolutionary psychology. Given the need for novel theory development, and the fact that the contestation of ideas underpins innovation on all fronts, wielding "heavy" metaphorical weaponry in the form of academic censure through use of the term pseudoscience might also work *against innovation in science.*

Historically, targets for the pseudoscience label have included, amongst others, movements such as psychoanalysis, biological psychiatry, Marxism, and Nazism, and certain historical political adventures serve to illustrate the dangers of pseudoscientific claims (Still & Dryden, 2004). But, arguably, the wellspring for most pseudoscientific ideas has been academia. As well it is expected to be, given academic freedom and the role of the academy in generating all types of ideas, without limit. But then how has the academy itself considered these issues? What then are the landmark "seismic episodes" in academia, or historical debates and turning points in scientific thought most relevant to pseudoscience and its potential for harm? Are political forces not fully present in the contestations that occur between academic fields, when have these forces met, head to head, in contestation? The scientific wars of the 1990s are perhaps illustrative in this regard.

Still and Dryden (2004, p. 278) describe the "scientific wars" of the 1990s as a narrative account of conflict between scholars, some of whom sought to "restore science to its privileged position in relation to truth." This group attacked sociology of scientific knowledge as well as what they described as an "academic left" that had been "infiltrated by a postmodernism with a foothold in the whole range of Arts subjects. (p.278)" This attack included an argument that "humanity itself is in danger when postmodernists try to seize possession of the tree of knowledge, and obliterate the distinction between science and pseudoscience" (Still & Dryden, 2004, p. 278). Again verbatim quotes allow for no ambiguity in understanding the vitriol, in a way that paraphrasing would surely euphemise.

Taking recourse to fundamentalism about truth, authors such as Wolpert sought to "restore the demarcation between science and pseudoscience" representing those who broadly felt civilisation itself "is under threat, not from terrorism but from academic charlatans who peddle anti-science and pseudoscience" (Still & Dryden, 2004, p. 278). Still and Dryden (2004, p. 278) argue that these attacks reflect a "tree of knowledge" perspective which takes science to be like a tree, with one source of truth, yet despite the "famous victory of Sokal's Hoax, the hostilities petered out indecisively," mostly for lack of response.

It is suggested here, however, that attempts to delegitimise other fields are unhelpful, and can simply activate values configurations which de-prioritise those prioritised by "oppositional groups" (Hagan, 1962), where a field's values configuration literally can become a reversed mirror image of another's. Following Kuhn (1970), notwithstanding Lakatos's (1978) arguments, and taking into account other theory which predicts values can take form and relative structure (Schwartz, 1992), the logic of oppositional groups does not seem unrealistic.

Many academic fields (and movements) may have developed historically as much through oppositional dynamics as from other influences driving their development. In much the same way that nature abhors a vacuum, and air molecules tend to fill up empty space, ideas may have sought to populate academic contexts. Thus, we have the incommensurate paradigms of Burrell and Morgan (1979), and its oppositional premises, that recognise fundamental differences between fields. The question of novel facts, or the inherent innovativeness of these fields does not dominate in such discussions.

According to Lakatos's (1978) perspective, even degenerating research programmes engage in an elaborate process to protect their hard cores, and it is not clear how they might not be perpetually successful at this protection. Lakatos himself describes the mechanisms of protection but it is not clearly evident that degenerating research programmes will not be successful in this. Similarly, given Lakatos's descriptions of these mechanisms, if these protections are effective then one (as either an insider or an outsider) might not even be aware which are degenerating and which are progressive, if they manage their machinery with their own resources and interests at heart.

The implication of this is that innovativeness might have been subordinated to other agendas in many fields that have experienced degeneration that is not visible to outsiders. Without being too harsh on fields, one can envisage a context akin to the Economists' notion of perfect competition, where if all ideas are contested immediately, without given time to develop, fields would perhaps be less likely to develop in different directions or to develop at all. With insider knowledge, one can perhaps map the incentive structure of development of different academic fields, and it does seem that building and tending entrance barriers can serve their interest, particularly with respect to goals other than fostering innovation.

These challenges seem characterise the normal science paradigm, and essentially reduce to Kuhnianpredictions, that it might be the shared values of human beings that seem to account for many of the changes, and developments, in academic fields. Further consideration of the literature relating to academic fields also suggests that their development is not necessarily premised on maximising innovativeness.

5. The genesis and development of academic fields

Theory is required, to understand how academic fields come into being and develop. Frickel and Gross (2005, p. 204) stress the lack of theory explaining why and how "disciplines, subfields, theory groups, bandwagons, actor networks, and other kindred formations" emerge to change the "intellectual landscape," and offer a general theory of scientific/intellectual movements (SIMs) and their development.

Tight academic labour markets in certain disciplines have effectively enabled the creation of new fields, an example being the exodus of German academics to start new fields in different areas, such as physiologists entering philosophy or experimental psychology (Ben-David & Collins, 1996; Frickel & Gross, 2005). Similarly, new fields can gain legitimacy in the wake of changes, and novel technologies are creating opportunities for new methodologies to emerge.

The post-normal science paradigm relates to conditions under which facts are uncertain, values are contested, and stakes are high but decision-making is urgent (Funtowicz & Ravetz, 1994). Theory may develop further to integrate new technological opportunities with theory that explains how new fields can emerge that prioritise the discovery of novel facts.

Epistemic cultures (Knorr-Cetina, 1999), or "repertoires of thought, action, and technique, that shape the organisation of scientific and intellectual inquiry in a given field or fields" vary primarily between individualistic and collective cultural orientations. Individualistic contributions are typically valued if path-breaking and divergent from other intellectual agendas. Collective

contributions entail working collaboratively to build on the work of other pioneers (Frickel & Gross, 2005, p. 218). This differentiation reflects important differences between individualistic versus collectivistic cultural values (House, Hanges, Javidan, Dorfman, & Gupta, 2004) and between individualist self-enhancement and self-transcendence values orientations (Schwartz, 1992). These differences might have implications for innovativeness, or the discovery of novel facts.

A tension might exist, between individual path-breaking values and those needed to prioritise the collective in the form of the field. At extremes, this tension might reflect eccentric path breaking versus groupthink behaviours (Janis, 1971), or even cult behaviours (Iannaccone, 1992). Academia is by definition fertile ground for idea development, no matter its agenda (as it should be). Collectivism, however, forms the basis for exchange relationships, where loyalty to a field may be part of a trade-off for payoffs such as employment or other rewards. This literature also supports Kuhn's (1970) thesis that that subjective shared values and beliefs of groups of scientists can thwart innovative progress, placing cultural forces at the centre of constraints to academic innovativeness under the normal science paradigm.

According to Gieryn (1983), the "intellectual ecosystem has with time been carved up into 'separate' institutional and professional niches through continuing processes of boundary-work designed to achieve an apparent differentiation of goals, methods, capabilities and substantive expertise." According to this conception, successful scientific fields have over time won out in boundary disputes against other "knowledge producing activities, such as religion, art, politics, and folklore" (Gieryn, 1983, p. 783).

These changes may however reflect changing patterns of payback, or changing incentives for membership of these networks. Following Kuhn's (1972) theory, this process of boundary management may have other objectives than innovative scientific progress, whereby when these objectives conflict, innovation might be subordinate to the needs of boundary management. Hence, innovativeness might be subordinate to the need to create "monopolies" in boundaried knowledge domains. Even Lakatos's (1978) description of a progressive research agenda attracting members does not discount the possibility that these members may be attracted through the lure of resources and prestige created through other means than the discovery of novel facts.

Are these fields ever expected to necessarily show convergence with knowledge generation, and "real" scientific progress, as described by Lakatos (1978) as the generation of theory that predicts novel facts? If progress in natural science is constrained by behavioural constraints to innovation, as predicted by Kuhn (1970), then it is perhaps to social science that we need to look for insights into how to manage or eliminate these paradigmatic constraints. These constraints may therefore reflect the paradigm of normal science.

6. Hierarchies versus networks

According to Gieryn (1983, p. 792/3), descriptions of science "as distinctively truthful, useful, objective or rational may best be analysed as ideologies: incomplete and ambiguous images of science nevertheless useful for scientists' pursuit of authority and material resources." Kuhn's (1972) theory cannot be one of absolutes, however, and evidence should exist as to graduations in the extent to which science is evidence based or not. For Peterson (2015, p. 1202), the "apparent lack of advancement in the social sciences has fostered a widespread belief in a "hierarchy of sciences," in which:

> [S]ome fields are judged to be more scientific than others. Natural sciences like physics and chemistry sit proudly at the top, while the social sciences muddle feebly at the bottom. However, empirical research has produced only mixed results trying to correlate this intuitive hierarchy with the products or social organizations of different fields.

The process of scientific discovery is nonetheless dependent on processes which are the focus of study in the social sciences. For example, according to Peterson (2015, p. 1203), "bench-building" occurs "when scientists at the unstable and ambiguous research frontier concentrate their efforts on the production of reliable effects through an iterative process whereby they incorporate new techniques and technologies."

This process of bench-building, when successful, "extends the horizons of enquiry and produces practical consensus as a byproduct, as researchers incorporate new methods to maintain their place at the cutting edge of their field" (Peterson, 2015, p. 1203). There seems to be a conflict highlighted here however between the notion that "more scientific" fields have more of a monopoly on truth, akin to scientism (Bannister, 1987; Birkinshaw, Healey, Suddaby, & Weber, 2014).

What seems more plausible is that core differences are premised by some instead on the "measurability" of their phenomena under study, or the extent to which they can be quantified. And mathematical quantification offers perhaps the most precise specifications of relationships. From such a reductionist perspective, at the pinnacle may be physics, and the mathematical sciences. Economics would contrast with management, with different understandings of rigour characterising each field. These would also differ from fields such as actuarial science. Thinking of academic fields as horizontal networks would belie perceptions of status differentials between fields, and notions of oppositional logics discussed previously. Understanding the innovativeness of academic work would therefore need to develop knowledge of the tensions between horizontal relationships across and within fields, as well as the vertical networks of power and prestige that run through these networks.

But this cannot be the whole story. Taking aside this natural hierarchy of specification, what remains in the literature that specifically relates to what is potentially "not science," or pseudoscience? And how might innovativeness be "harmed" by exposure to these debates, and oppositional logics.

The Sokal affair, or Sokal hoax, brought debates over issues concerning scientific rigour to the fore of academic discussion; a hoax article was submitted and was accepted for publication by a top social science journal (Sokal, 1996). Sokal (1996, p. 4) argues:

> [The journal's] acceptance of my article exemplifies the intellectual arrogance of Theory- meaning postmodernist literary theory- carried to its logical extreme. No wonder they didn't bother to consult a physicist. If all is discourse and "text," then knowledge of the real world is superfluous; even physics becomes just another branch of Cultural Studies. If, moreover, all is rhetoric and "language games," then internal logical consistency is superfluous too: a patina of theoretical sophistication serves equally well. Incomprehensibility becomes a virtue; allusions, metaphors and puns substitute for evidence and logic. My own article is, if anything, an extremely modest example of this well-established genre.

Sokal and Bricmont (1998, x/xi) take issue with epistemic relativism, or the idea that "modern science is nothing more than a 'myth', a 'narration' or a 'social construction,'" or what they term the postmodernist and cultural studies penchant for "misappropriating ideas from the philosophy of science, such as the underdetermination of theory by evidence or the theory-ladenness of observation, in order to support radical relativism" and "mystification, deliberately obscure language, confused thinking, and the misuse of scientific concepts" as a set of intellectual practices. One must ask, however: is the postmodern academic topic area not an appropriate space for radical thought of this nature?

Indeed, according to Searle (1990), in common with "taxation" and "relations between the sexes", higher education is an area of continual contestation, and alarmist approaches typically yield high populist payoffs. Searle (1990) explains further as follows:

> There are, indeed, many problems in the universities, but for the most part, they tend to produce silliness rather than catastrophe. The spread of "postructuralist" literary theory is perhaps the best known example of a silly but noncatastrophic phenomenon.

To consider postmodernism as pseudoscience is a serious charge, which perhaps misses the point, that academic fields might not necessarily be more inclined to innovation than other collective systems. Searle (1990) seems closer to the point; that these movements are "noncatestrophic" or essentially cannot do harm. In a normal science paradigm, ideation should in all instances be given free rein in the academy, and voices such as Wolpert, Sokal and Searl as well as their opponents are then simply free expressions of ideas, with the net result that more contested ideas have more "counter" ideas associated with them. Science might then be seen to progress on the basis of generalised agreement on ideas. Admittedly, this seems perilously close to Kuhn's (1970) logics, and a state of gridlocked innovation might be a natural state under a normal science paradigm. These are difficult issues to contend with in trying to understand the inherent purpose of the academic system, if Lakatos (1978) is taken to be correct in that this fundamental purpose is the production of theory that predicts, or uncovers novel facts.

7. Is extreme gatekeeping conducive to the discovery of novel facts?

An increasing number of journals pride themselves on their rejection of submissions, with some claiming up to 98% rejection rates. Is such extreme gatekeeping really the gateway to increasing our access to novel facts? It is clear that as the billions in the developing world, for example, clamour to access the limited supply of the world's top journals, the structure of academic publishing seems to increasingly seem to exhibit the characteristics of a bottleneck. Is this system producing a monopoly in prestige and status? Does this represent the "tacitification" of the academic career system? Is it tacit knowledge that becomes separated from explicit knowledge through the centrifugal effects of the journal publishing system? How many of us management scholars will ever publish in the Academy of Management Review journal, or the handful of elite journals at that "level." Is this not even more difficult for those with English as their second or third language?

How much of the rejection process in these elite journals actually is a check on pseudoscience? How much knowledge fails to get "out there," as it is rejected for other reasons that those of poor quality methods?

It might be argued that the need to protect the academic system against Type II errors (acceptance of false facts) in the normal science paradigm has perhaps engendered an excessively conservative stance reflected in very strong gatekeeping. Such an academic system may persist in a state of gridlocked innovation if gatekeeping is biased to reject novel facts that are true (Type I errors) at a higher threshold in order to protect against Type II errors. Academia is surely an area in which ideas need to be continually contested, and this contestation might be seen as a characteristic typical of normal science.

The tendency of the academic field to bias itself toward conservativism in review processes seems to exist to limit diffusion of pseudoscientific ideas. However, overly conservative reviewing can itself simply play into the hands of the Kuhnian phenomenon, where an academic game emerges where reduction of variance becomes the goal itself in academic publication. Getting published in the world's top journals might then become more a game of benchmarking secret languages (and having articles look similar to others, perhaps in a quest to then win in the top journal publication lottery system) than actually providing breakthrough insights which advance science, social or otherwise. The ideas in this paper are no less subject to these constraints.

Other academic bias exists, such as largescale non-publication of null results, on account of the 'file drawer problem", where significant results are typically a prerequisite for publication, and studies failing to reject the null hypothesis literally remain in file drawers (De Long & Lang, 1992). This might result in a disproportionate amount of Type II errors as over time those associations that fall into the significant 5% when 95% fall into the null category. This may be another

characteristic of the normal science paradigm of scientific research. Are nulls not novel facts, according to Lakatos's (1978) logics? How do we deal with the problem of pseudoscience without stifling innovation? According to Bunge (1984, p. 44):

> There is always the fear that some gold nuggets may lie hidden in a pile of pseudoscientific rubbish, but the latter may be nothing but protoscience, or emerging science. Such fear is quite justified in the beginning, particularly since an extremely original theory or technique- an unorthodoxy- may smack of pseudoscience just because of its novelty. But scepticism must succeed caution, and scepticism must in turn be replaced with denunciation if the novelty fails to evolve into a full-fledged component of science at the end of, say, a half century. Indeed whereas the protosciences advance and end up by becoming sciences, the pseudosciences are stagnant pools on the side of the swift current of scientific research.

Does an elite context of top journals that boast of 97% rejection rates provide a useful climate for the development of protosciences? If it is necessary to enter these journals to gain the impact factor advantages necessary to spread innovative ideas, then what is the fate of the newest generations of protosciences? The hard questions need to be asked. It is not useful to shy away from them. Have we created a system that is inherently biased against innovativeness?

Extremes are shown in Figure 1, whereby a circumplex structure is used to illustrate the arguments made here, that at the extremes, pseudoscience and extreme gatekeeping have the same effect.

The current lack of a strong probabilistic relationship between scientific R&D inputs and the ability to solve outright certain societally important outcomes is taken to be a feature of normal science, and in Figure 1 this state is considered to represent innovation gridlock. Historical models

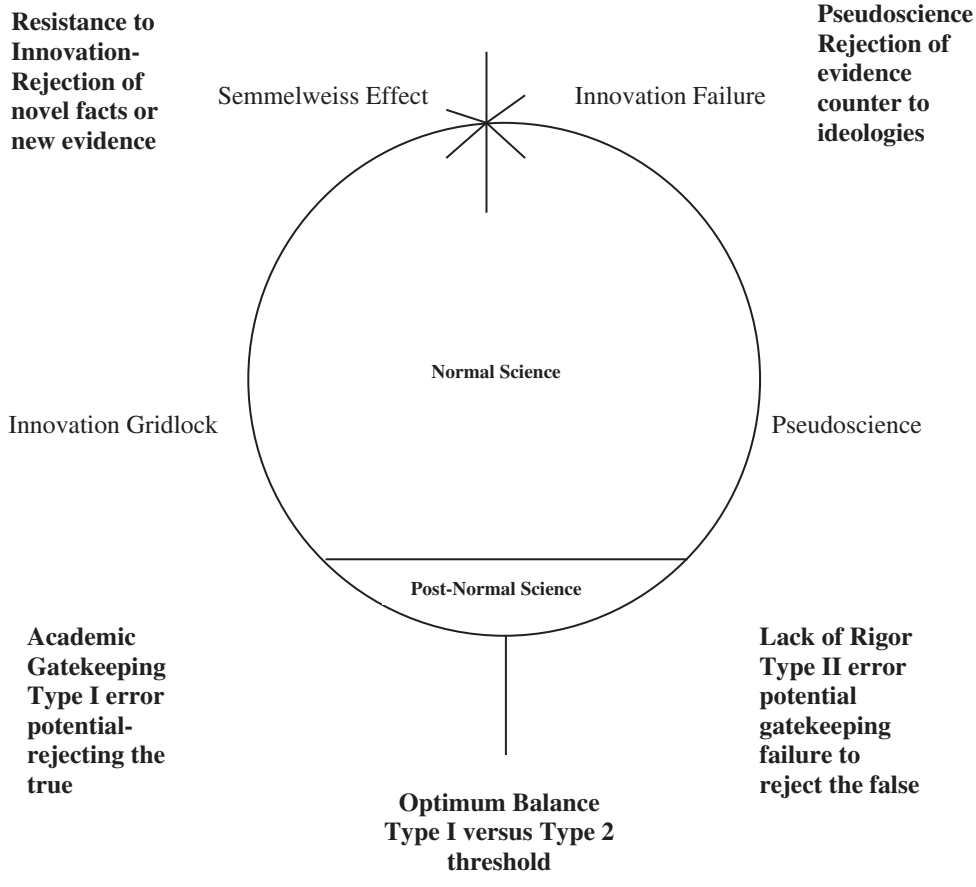


Figure 1. The Pseudoscience Innovativeness Circumplex.

of R&D seem to have been in a state of innovation gridlock because of the need to increase the chance of making a Type I error, so as to counter the greater threat of accepting false hypotheses, or of making Type II errors.

Thus, the historical paradigm of social scientific research seems to have been skewed toward conservatism, due to the need for constant vigilance and the ever present threat of accepting false hypotheses, or pseudoscientific hypotheses.

The error frontier of normal social science (where decisions are made that need to balance the costs of making Type I or II errors) may be set at the point at which innovation gridlock is a likely state of the academic system. In contrast, the error frontier under conditions of post-normal science are arguably at a more optimal point as the variance associated with error frontier decisions is reduced, on account of multiple sources of data and greater transparency and accountability of the research process itself to stakeholders.

The citizen science ethical framework might extend stakeholder theory (Freeman, 1984), as emergent technological advancements result in the democratisation of science and give greater voice to populations affected by research, and particularly to populations that are currently marginalised as unattractive markets for privately funded scientific research. The maximisation of transparency and accountability is perhaps the most realistic safeguard against pseudoscientific practice as well as excessively conservative gatekeeping.

The idea that principles of post-normal science might mitigate the problem of innovation gridlock caused by an over-cautious or overly tacit gatekeeping system amongst elite journals may seem utopian here. Nevertheless, it is argued that if Figure 1 can be seen as a clock face, then the innovativeness "hand" of the clock that represents the current state of affairs in our elite journals may be pointing to the state of innovation gridlock, or even, in some instances, to the extreme of the circle labelled "Semmelweiss Effect."

If the world faces serious geopolitical and social problems, and the social sciences research system is largely failing to solve them, then is it possible that the system is out of balance. Even if new ideas such as those offered by post-normal science are not able to help us find a way out of this innovation trap, surely there must be ways to make new ideas more accessible to the scientific communication system of social science. If elite journals are the primary mechanism of communication of these protoscientific ideas, is it not clear that 97% rejection rates based on criteria other than innovativeness poses some kind of problem to societal problem solving. Many might agree that the world is currently facing serious geopolitical and societal challenges that are simply not being solved.

How can it be that the natural sciences deliver technologies and capabilities that allow millions to use air travel daily, for example, and scientific work that can provide information about galaxies we cannot see with the naked eye, yet we continue to live with high levels of socioeconomic problems that seem to reflect the inability of social science to solve them. The ideas presented here are not new, as one might see from a reading of the work referenced here. Nevertheless, it might be necessary to ask the difficult questions that might be necessary to make the social sciences research system more responsive to the urgent needs of human populations at this time. It may be time to move toward an era of Post-Post-Truth, to re-focus on Lakatos's imperative, namely the development of theory that can predict urgently needed novel facts, before it is too late.

In light of the findings here, it is necessary to pose a final question—is pseudoscience really the major deeper methodological problem associated with slow research (and its societal costs), or is there a more important viewpoint for the development of modern philosophy of management.[2] Incremental research- or that which does not seek to produce novel facts—is surely still important,

and where its importance can be justified, it could not be classified as pseudoscience. Nevertheless, given the literature discussed here, it would seem that the failure of the discovery system to generate much needed novel facts is important enough to warrant further research, particularly within the field of modern philosophy of management.

It should also be noted again that the discussions here are delimited in scope, as they build on Lakatos's (1978) work. As discussed previously, there are a great many different perspectives of pseudoscience (Hansson, 2017), and further research might extend the work of this paper to understand its other societal consequences, over and above its relationships to knowledge creation and the generation of novel facts.

8. Conclusion

The objective of this paper was to make the argument that the world is facing serious problems that the current system of social science has not been able to solve, due to inherent constraints to its innovativeness, or its ability to develop theory to predict novel facts. It was argued that discussions of the inadequacies of this system would benefit from revisiting Lakatos's (1978) ideas about what the fundamental role of science should be. To ground its arguments, practical examples of the consequences of pseudoscience were first discussed. This provided the context with which to extend discussions into a consideration of Lakatos's (1978) reconciliation of the tensions between Popperian and Kuhnian theory. Drawing from Lakatos's (1978) logics, a model was derived, illustrating the tensions between pseudoscience and the academic response to it that may have resulted in innovation gridlock in the social sciences system itself. It is concluded that excessive gatekeeping on the basis of criteria other than methodological rigor might create a barrier to the transmission of protoscientific ideas needed for the development of problem-solving theory required to solve certain serious social problems of human populations at this time.

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Notes

1. For a more complete review of these debates and characteristics than the one provided here (which is delimited to Lakatos's (1978) work), see Hansson (2017).
2. I am grateful to an anonymous reviewer for raising this important question. It needs to be addressed, in order to position the research in relation to other literature germane to the discipline of modern philosophy of management.

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