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EVALUATING INDUCTIVE VERSUS DEDUCTIVE RESEARCH IN MANAGEMENT STUDIES: IMPLICATIONS FOR AUTHORS, EDITORS, AND REVIEWERS

Purpose – To address the imbalance between inductive and deductive research in management and organizational studies and to suggest changes in the journal review and publishing process that would help correct the imbalance by encouraging more inductive research.

Design/methodology/approach – We briefly review the ongoing debate about the ‘developmental’ versus ‘as-is/light-touch’ journal review modes, trace the roots of the prevailing developmental review to the hypothetico-deductive research approach, and contrast publishing deductive and inductive research from the perspectives of authors, editors, and reviewers.

Findings – Application of the same developmental evaluation and review mode to both deductive and inductive research, despite their fundamental differences, discourages inductive research. We argue that a light-touch review is more appropriate for inductive research, given its different logic.

Practical implications – Specific criteria for the light-touch evaluation and review of and some concrete suggestions for facilitating inductive research.

Social implications - Advancing knowledge requires a better balance of inductive and deductive research, which can be facilitated by light-touch evaluation and review of inductive research.

Originality/value – Building on the debate on journal publishing, we differentiate the evaluation and review of inductive and deductive research based on their philosophical underpinnings and draw implications of pursuing inductive research for authors, editors, and reviewers.

Keywords Deductive research; inductive research; developmental review; light-touch review

Paper type Viewpoint

EVALUATING INDUCTIVE VERSUS DEDUCTIVE RESEARCH IN MANAGEMENT STUDIES: IMPLICATIONS FOR AUTHORS, EDITORS, AND REVIEWERS

Introduction

There is a growing, frequently critical, literature examining peer-reviewed journal publishing in management studies, focusing on the roles of editors and reviewers and advising authors how to get their papers published (Baruch et al., 2008; MacDonald & Kam, 2007; Thomson and Kamler, 2013; Zahra & Neubaum, 2006). Discussion on these issues is ongoing with many current and past editors entering the debate, and rightly so: the publishing process is at the core of sharing and advancing knowledge (Bedeian, 2004; Miller & Van de Ven, 2015).

A central debate focuses on the editorial policy of developmental versus ‘as-is’ (or ‘light-touch’) evaluation and review, with the former being dominant, especially at leading management journals. In the developmental review mode, submitted manuscripts, seen as “diamonds in the rough,” get polished by the joint efforts of authors, editors, and reviewers, and finally published – or rejected – after several rounds of revision (Bergh, 2008). The as-is review has been promoted as an alternative where manuscripts are owned solely by their authors and are accepted or rejected after the first round of reviews. The light-touch approach operates similarly to as-is review, with authors making changes to the editor’s satisfaction, following a favorable set of reviews.

To better understand the origins of the debate and to contribute to it, we drew on arguments from the philosophy of science. We traced the roots of the developmental review in the hypothetico-deductive research approach (Nola & Sankey, 2007), which currently dominates in management studies (Johnson, 2015) and in social sciences in general. We argue that problems arise especially when developmental reviews are applied to inductive research; they in fact hamper

the publication of inductive research findings. Yet, inductive research is needed in order to advance knowledge (Harriman, 2010: 6) and to develop new theories (Eisenhardt, Graebner & Sonenshein, 2016; Locke, 2007). As one journal editor put it: “Research questions derived solely through deduction from even a thorough knowledge of the extant literature are likely to generate only incremental contributions to the field” (Konrad, 2008: 13).

In different areas of management studies, such as organizational behavior, organization theory, and strategy, inductive research often requires qualitative research methods¹, such as case studies and ethnography, that allow answering ‘how’ and ‘why’ questions and detecting organizational processes over time (Eriksson & Kovalainen, 2016, 42). Qualitative data and analysis provide the basis for ‘thick description’ that make new theoretical explanations possible (Eisenhardt, Graebner & Sonenshein, 2016).

We argue that the light-touch review is particularly appropriate for evaluating inductive research, while the developmental review approach is not. We contribute to the debate about the publishing process by tracing its origins relative to the different research approaches – deductive or inductive – and argue that each type of research should be evaluated differently, and suggest how, highlighting some useful evaluation criteria for light-touch reviews. We do this by contrasting the publishing process between deductive and inductive research, from the author choosing a research question to the publication of the manuscript reporting the findings. We then draw implications for authors, editors, and reviewers for publishing inductive as opposed to deductive research. We conclude with a discussion of how deductive and inductive research advance knowledge in different ways and argue for encouraging more inductive research. To better

¹ Although most qualitative research is inductive, it can also employ deductive designs (Bitektine, 2008, as reported in Pratt, 2009; Eriksson & Kovalainen, 2016).

balance inductive and deductive research for advancement of knowledge, we see adjusting the dominant publishing process as a crucial first step.

The Publishing Process via Journals: Alternative Views and Philosophical Underpinnings

Developmental versus as-is/light-touch review

A central issue dividing opinion about the publishing process is whether manuscript review ought to be “developmental” or “as is” (Tsang & Frey, 2007). The advocates of developmental reviews envisage them as a means of teaching authors how to better frame their research questions/findings and develop their ideas to advance knowledge (Schminke, 2002). Developmental reviews frequently involve several rounds of revisions. Such a process, according to Bergh (2008: 122) “...can take a long time to complete and the eventual manuscript may reflect the reviewers’ thoughts just as much as the authors’.” Those favoring this type of review point to “peak reviewing experiences,” where an author rises to the challenge of apparently irreconcilable objections and conflicting demands from reviewers, across multiple revisions, to produce a manuscript that is eventually accepted and perceived to have an enlarged contribution (Rynes, 2006). Others see it as often taken too far, with developmental reviews argued to lead to a distortion of the authors’ ideas and findings, to “ghostwriting” by the editor and the reviewers (Bedeian, 2008), or to what some have termed “intellectual prostitution”: authors selling their integrity in order to get published (Frey, 2003). The end result may have little resemblance to “the paper [the authors] had set out to write” (Meyer, 1995: 267).

To avoid the issues created by extreme developmental review processes, ‘as-is’ reviews have been proposed as an alternative (Tsang & Frey, 2007). The advocates of the ‘as-is’ review start from the premise that authors are peers of reviewers – not their inferiors in need of education

as to how to conduct and write up their research (Romanelli, 1995; Starbuck, 2003). The proponents of the ‘as-is’ review argue that since authors are the owners of their ideas, the editor should make a decision – either to accept or reject – after the first round of reviews (Tsang & Frey, 2007). Within this process, a paper is either currently publishable in a particular journal or it is not. The reviewers may suggest improvements to the paper, but it is left up to the authors to incorporate them or not. Most authors would acquiesce, recognizing that it is in their self-interest to present their research and findings as clearly and interestingly as possible (Frey, 2003; Tsang & Frey, 2007).

Instead of ‘as-is’, we will use the term ‘light-touch’ review throughout the rest of this paper. The light-touch review, where authors make changes to the editor’s satisfaction after the first round of review, is compatible with the as-is review (Tsang & Frey, 2007) but we deem ‘light touch’ a more accurate description of how the process would typically be implemented. The only management journal (to our knowledge) that has a review process resembling this, is *Strategic Organization*, with its “only one major revision” policy.

While the debate about the appropriate publishing process goes on, it is instructive to ask what may have led to the different processes of review. In our view, the origins of the developmental review can be traced to the hypothetico-deductive approach to research or “normal science” (Daft & Lewin, 1990) that is now dominant in management studies (Johnson, 2015). Yet, we argue, inductive research is better aligned with the light-touch review. To show these connections, we first discuss briefly the evolution of these two research approaches.

Deductive versus inductive methods

Deduction entails moving from the general to the particular, as in starting from a theory, deriving hypotheses from it, testing those hypotheses, and revising the theory (Locke, 2007; Nola &

Sankey, 2007). Induction, on the other hand, involves moving from the particular to the general, as when making empirical observations about some phenomenon of interest and forming concepts and theories based on them (Locke, 2007). The first philosopher of science, Aristotle, held that induction was necessary to develop valid theories and thus logically preceded deduction, which was needed to test and further refine theories (Harriman, 2010: 235-236). In other words, induction and deduction were seen as complementary. But later developments in the philosophy of science created a divide between the two.

Despite such famous champions of induction as Francis Bacon and Isaac Newton (Ormerod, 2009), deduction came to dominate as the means of scientific research and advancing knowledge, due in part to the influence of skeptics such as René Descartes and David Hume. Descartes and Hume claimed that our senses are not valid means of attaining knowledge, and therefore we need to rely on innate ideas or existing theories and deduce hypotheses from them. Induction was considered an inferior or invalid means of advancing knowledge.

These views culminated in contemporary philosophy of science through the writings of Karl Popper, who challenged the idea that knowledge is advanced by generalizing from empirical observations. Popper's solution to the "problem of induction" was to eliminate induction altogether, and to advance knowledge by deducing hypotheses from existing theories, testing the hypotheses, and eliminating theories by falsifying them, until only valid, more refined theories were left (Harriman, 2010: 189; Ormerod, 2009). This, of course, begs the question: where do the theories come from in the first place if they are not induced from empirical observations?

The publication of Popper's *The Logic of Scientific Discovery* in English in 1959, while generating debate, boosted his falsification approach and the hypothetico-deductive method in general (Johnson, 2015; Ormerod, 2009). This resonated with the emerging field of management

studies (e.g., strategic management, organization theory, organizational behavior) which was, at that time, seeking legitimacy (Hambrick, 2007; Porter & McKibbin, 1988) by emulating the natural sciences and their methods. The embrace of the deductive method also fostered the dominance of the developmental review mode.

Why the deductive method gives rise to the developmental review

The deductive method can start from any theoretical base, from which any number of alternative hypotheses could be deduced. Authors are expected to start their papers with substantial introductions, justifying their theoretical starting points and the hypotheses they have deduced. And since their starting points are not grounded in empirical observation, but on a proposed theory that often is not fully validated yet (Locke, 2007), they are vulnerable to the criticisms of the editor and the reviewers who each may favor a different theory and hypotheses as the starting point for the research. This likely contributes to the fact that complete agreement among reviewers is rare (Starbuck, 2003; 2005), as they tend to express concerns about research that counters their own and favor research that cites their published work (Starbuck, 2003: 347).

The presence of alternative published theoretical positions also instigates the negotiation process between the editor, reviewers, and the author(s) as to what the best starting point for the research is – and is a key driver of the perceived need for developmental reviews of a manuscript. This negotiation can involve several rounds of revisions, as the three parties collectively determine how best to frame the paper, and in some cases also what would be the most effective way to test the hypotheses. Since the starting point is not some observed phenomenon of interest and evidence about it (but a theory which is argued to still need further validation or development), naturally there may be some debate and differing views on what the paper should or should not include.

Inductive research, the developmental review, and implications for the publishing process

Despite the dominance of the deductive approach, it has been challenged more recently in several fields. According to Ormerod (2009), psychologists and cognitive scientists have concluded that deductive reasoning does not adequately capture how people actually think, and computer scientists have found that deductive logic cannot explain how people respond to their environments. Philosophers of science have found that the deductive approach does not adequately explain the scientific method (Harriman, 2010). In psychology and organizational behavior, inductive research has been argued to be a key means of advancing knowledge: developing valid theories requires painstaking empirical observation or experimentation over long periods of time (Locke, 2007).

Nevertheless, editors and reviewers, as well as authors, influenced by the developmental review philosophy and operating under the hypothetico-deductive research model, tend to view inductive research as insufficient by itself and often suggest adding deductive foundations, such as hypothesis testing to what in fact are inductive studies (Suddaby, 2006). However, inductive research does not start with a theory to be falsified (or confirmed) or refined but with unanswered questions about a particular phenomenon of interest. In other words, no foundational hypotheses are needed, as the authors focus on how their research question and research go beyond what is already known (Locke, 2007).

We argue that this should then become the starting point of the review: the reviewers' and the editor's task is to assess such manuscripts on one straightforward criterion: does the manuscript add substantially enough to what is already known? This, of course, has a few sub-criteria: is there sufficient evidence to support the new knowledge claims (Jonsen & Jehn, 2009; Konrad, 2008; Pratt, 2009)? Can the observed phenomenon not be adequately explained with existing theorization? Do the methods used fit the research question(s)?

Although it is possible for the reviewers and the editor to disagree on their assessment across these criteria, such disagreements are likely much easier to resolve than those on competing theoretical starting points and the hypotheses selected for deductive research. Such disagreements, when presented and supported in detail by reviewers, are likely easier for an editor to referee than disagreements on theoretical starting points or hypotheses common in evaluating deductive research, especially when the publishing process does not involve several rounds of review. Also, the evaluators of inductive research can always refer back to the empirical evidence presented in a manuscript, whereas those assessing deductive research do not have that basis when deciding which yet to be tested theories or hypotheses should be the starting point of the research.

Since deductive and inductive research play different roles in advancement of knowledge and follow different logics, these research approaches also have different implications for various stages of the publishing process. We demonstrate these differences by contrasting the two approaches in each stage.

Stages of the Publishing Process: Contrasting Inductive versus Deductive Research

The different stages of the prevailing publishing process present different choices for authors, editors, and reviewers in the context of inductive and deductive research, which we contrast below.

The following distinct stages of publishing mirror elements (some of them combined) presented diagrammatically by Clark, Floyd and Wright (2006: 656):

- 1) *the author* chooses a question to study, carries out the research, finds a journal to target, writes up the research, and submits the paper,
- 2) *the editor* assesses the paper and decides whether to send it for a review, assigning reviewers if this occurs,

- 3) *the reviewers* agree/decline to do the review after having received the paper (or an abstract); complete the review, and send it back to the editor,
- 4) *the editor* assesses the reviews and makes a decision (to reject, to invite a revision and resubmission, to conditionally accept, or to accept), and informs the author (with possible guidelines, in the case of revision or conditional acceptance),
- 5) *the author* decides how/whether to respond (returning the process to stage 2 (in case of resubmission) or 6 (in case of minor revisions),
- 6) acceptance of the paper by the editor, and publication.

1. Author(s) chooses the question to study, carries out the research, identifies a journal to target, writes up the research, and submits the paper. An inductive researcher² starts with an observed phenomenon of interest, typically framed as a question that has not been answered, partially or fully. Locke (2007: 884) cites several potential sources of questions or research ideas: "...an interesting finding in the literature, an unexpected finding in one's own research, an integration made from existing data, a finding in another field, etc. There are no fixed rules here so long as one is tied to reality." Although not highlighting inductive research directly, Daft and Lewin (1990) cite Davis (1971) and argue for examining interesting problems, such as explaining outliers, contrarian findings, or examples that challenge accepted assumptions.

A deductive researcher, in contrast, starts with an existing theoretical base (or sometimes, with a particular method to test hypotheses). For example, the researcher selects the resource-

² We use the terms "inductive researcher" and "deductive researcher" as convenient labels to refer to the kind of research is being submitted to a journal. These terms are not suggesting that a researcher could not vary or mix modes.

based theory of the firm or the agency theory, and identifies aspects of it that she would like to refine. She then deduces hypotheses from the theory, identifies or collects data, and applies methods, usually statistical, to test the hypotheses. Finally, she discusses how her findings confirm or modify the existing theoretical base.

An inductive researcher would like to find a journal that recognizes inductive research for what it is and assesses it accordingly. A deductive researcher needs to find a journal open to publishing papers based on the theory that he has chosen to test or method he wants to use. Most editors and reviewers want manuscripts to offer something new: a new topic, a new theory, or new evidence (Bergh, 2008). Therefore, finding the right journal to target is a fine balancing act for the deductive researcher: offering something new, but nothing that is too challenging or divergent to existing theories, lest those are the ones favored by reviewers and editors.

The increasing pressure at many business schools to publish in the field's leading journals (MacDonald & Kam, 2007) complicates finding an appropriate journal for both types of researcher. Most leading journals in management studies have adopted the developmental review ideal and haven't differentiated the process for inductive and deductive research. Authors wanting to publish in the 'top' journals can anticipate, besides several rounds of reviews, the requirement for a deductive framing of their inductive research. Sutton's (1997) discussion of keeping qualitative data in the closet represents an example of such adjustment, as does Clark & Wright's (2007) statement on conforming to at least some 'genre constraints.'

2. Editor assesses the paper and decides whether to send it for review; assigns the paper to reviewers. The editor's task in assessing inductive research is more clear-cut, especially if she adopts the criteria suggested above for assessing manuscripts (sufficient empirical evidence, no existing theory to explain the phenomenon, and sound methods). When manuscripts clearly do

not meet these criteria, a desk rejection ensues. If the manuscript meets the criteria, reviewers can be promptly assigned, based on their expertise on the subject or the methodology.

The evaluation of deductive manuscripts can be trickier because of the potential reviewer disagreement about theories and hypotheses and no standard for deciding between them. (The exceptions, of course, are manuscripts with no clear new findings or fatal methodological flaws, or not fitting the journal's aims and scope that can be rejected at the editor's desk.)

The assignment of reviewers is a crucial step in the process which many authors have come to consider the "luck of the reviewer draw" (Bedeian, 2004). For inductive research, the editor must find reviewers who are familiar with inductive research in general besides being experts on the subject matter and/or on the methods used. One way of sensitizing reviewers to this would be to have separate guidelines for reviewing inductive and deductive research (cf. Pratt, 2009). Separate guidelines should make the reviewers' decision to agree or decline to review clearer.

A challenge for selecting developmental reviewers for deductive research is to achieve in-depth evaluations, based on understanding of the chosen, and perhaps alternative, theoretical perspectives and not colored by the extent to which the deductive study builds on or confirms the reviewers' own previously published research. Otherwise, finding agreement across reviewers is less likely and will prove more problematic for the editorial decision that follows the reviews.

3. Reviewers agree/decline to review; complete the review and send it back to editor. Some have argued that the reviewers' task is perceived as one of finding flaws in the manuscript and reasons for rejecting it (Barley, 2008: 41; Harrison, 2002), or to do what has been labeled SLAM (Stressing the Limiting Aspects of Manuscripts) reviews (Van Lange, 1999). This attitude is more likely to apply to assessing deductive than inductive research, because by having several choices regarding

theory and hypotheses, a deductive researcher is more vulnerable to criticisms and varying reviewer opinion about the acceptability and development potential of the submitted manuscript.

Inductive research, on the other hand, requires primarily that reviewers make a recommendation as to whether the manuscript provides a sufficient contribution to knowledge and is therefore publishable with ‘light-touch’ editing, or whether it should be rejected. Naturally, the reviewers could have preferred a study on a different phenomenon; however, that should not be an assessment criterion for inductive research (as long as the studied phenomenon is not trivial). Nor should views against qualitative research serve as justification for rejecting the insights such empirical evidence may hold (Sutton, 1997).

4. Editor assesses the reviews, makes a decision about the manuscript, and informs the author.

The editor’s task in evaluating inductive research might seem easier, as the empirical evidence about an interesting phenomenon should speak for itself. As already discussed, disagreement among reviewers about theoretical perspectives, hypotheses and relevant concepts in deductive research is more likely and makes the editor’s task more difficult. Where all submissions to a journal are subjected to a developmental review, similar challenges can emerge with inductive research, as the editor and authors face the issue of incorporating additional/alternative concepts and perspectives to what is likely to already be a rich data presentation and analysis.

The majority of manuscripts fall into the “revise and resubmit” (R&R) category (Starbuck, 2005: 185), which is considered the most difficult by editors, as they have to resolve the reviewers’ often divergent feedback due to different epistemological and theoretical premises (Eden, 2008). Unlike with inductive research, the editor cannot use the deductive manuscript’s empirical evidence for assessing the validity of its theoretical starting points. The editor needs somehow to reconcile the divergent reviews based on the reviewers’ different theoretical preferences and to

prioritize the feedback to the author, who would otherwise face the nearly impossible task of satisfying the reviewers' conflicting suggestions.

5. *Author decides how to respond to editor's letter.* With most journals taking a developmental approach to reviewing, both the inductive and the deductive researcher will receive, if not an outright rejection, an R&R letter. If rejected, their responses are similar: revise the manuscript based on feedback and submit to another journal (one more welcoming to inductive research, or one more compatible with the deductive researcher's theoretical premises).

An R&R, however, is a risky proposition, more so to the deductive researcher, as it is uncertain whether the author can satisfy divergent reviewer requirements that led to the R&R. The inductive researcher is likely to face fewer rounds of review even in the current system, as there are no hypotheses and their theoretical premises for the reviewers to contest.

Implications of Pursuing Inductive Research for Authors, Editors, and Reviewers

Most research in management studies is deductive (Johnson, 2015; Locke, 2007), and the typical journal review process is developmental and geared toward deductive manuscripts. Since inductive and deductive research are complementary for advancing knowledge, we argue that more inductive research should be encouraged. However, as presented above, inductive research will not thrive under a developmental review process; instead it requires a light-touch review. In the following sections, we summarize the implications of pursuing inductive research for authors, editors, and reviewers, highlighting areas in which the implications differ from those of deductive research.

Implications for authors

We see the implications for authors of pursuing inductive research as two-fold: those relating to choosing the topic and methodology, and those relating to writing up the research (as a journal article).

As we have indicated, the starting point for inductive research is an observed phenomenon of interest, as opposed to an existing theory. If one considers describing and explaining of phenomena the primary tasks of research – and once these have been accomplished, developing theory that predicts events and their consequences (von Wright, 1971) – inductive research would be the preferred approach. This is especially true of young fields such as management studies where there are few established theories (Locke, 2007), and these cover relatively narrow areas.

The variety of interesting phenomena is broad; the primary guideline to researchers is to focus on observed or stated practice in choosing questions to study. In management studies, one of the primary means to do so is to study questions or problems with which managers and other employees are grappling, such as motivation, productivity, value creation, competitive advantage (Eden, 2008: 247-248). Another means to focus on actual problems is to address questions raised but not yet answered by other researchers (Bergh, 2008).

Writing up inductive research as a journal article requires inductive researchers to persuade the readers that the question addressed is interesting and important to the field (Davis, 1971). If they manage to do so, their chance of getting their research published is higher than when their contribution to the existing literature is incremental, such as testing additional hypotheses (Bergh, 2008: 116; Konrad, 2008: 13). The authors' primary task is to show that they have studied an interesting new, or poorly understood, phenomenon.

Another element crucial in writing up inductive research is to demonstrate the soundness of the methods employed. The authors need to show what their methods fit their research questions

and that their findings provide answers to the questions. Since inductive research does not involve hypothesis testing, statistical methods are frequently not used. Rather, researchers need to take care to display their often qualitative data and link them to their study's conclusions (Miles & Huberman, 1994; Strauss & Corbin, 1990). While this is also true of deductive research, inductive researchers must be particularly vigilant in this regard, given that most editors and reviewers are more accustomed to evaluating deductive research.

Implications for editors

It has been suggested that the editor's work is "to create, maintain and extend the body of knowledge in the field" (Konrad, 2008: 3). Although it is not a single-handed task – authors also play a key role – it is demanding work nevertheless. Even so, we suggest an added demand: apply different evaluation criteria to inductive and deductive research and assign reviewers accordingly.

In some ways, assessment of inductive research simplifies editorial decisions (but can also make them more challenging). With inductive manuscripts, we see the primary assessment criterion is clear-cut: the paper either adds to knowledge (in the form of a new question or new evidence) or not. We argue that with inductive manuscripts the desk rejection rate may be much higher than with deductive manuscripts, as editors are able to assess the extent of added knowledge and methodological rigor of most manuscripts (as opposed to determining whether the author had chosen suitable theory and hypotheses as the starting point of his research). Given the volume of research submitted to most journals, desk rejections speed up the publishing process. (The authors also benefit: manuscripts of insufficient quality are returned, letting authors decide sooner what to do with them next, based on the key editorial feedback).

The editor's second decision – assigning reviewers – is also relatively straightforward, with the added proviso of choosing reviewers that are not fundamentally biased against inductive

research (as those sharing Popper's views would be). Having a sufficient pool of reviewers that meets this criterion could be challenging; however, that should change if and when inductive research starts to gain more currency (Locke, 2007; Ormerod, 2009). The other selection criteria for reviewers are simply their knowledge of the research area and/or of the methods used, as with deductive research. We suggest that if an additional reviewer is required for an inductive manuscript, she could represent an "average reader," i.e., a scholar who may not be an expert in the research area or methodology but who could assess an author's claim about adding new, interesting knowledge.

The third editorial decision is either to accept or to reject the manuscript after the author has responded to the reviewers' comments. Again, this is a relatively straightforward assessment, based on the three criteria (sufficient evidence for new knowledge, no existing theory, a sound methodology). There would be little need to balance the divergent viewpoints and theoretical preferences of the reviewers. If the reviewers do not agree in their recommendations to publish or not, the editor can cast the deciding vote based on his own considered judgment.

With such a 'light-touch' review process (and the editorial decision after just one round of reviews), a developmental review that provides guidance for framing the paper with the "right" theories and choosing the "right" hypotheses to test is not needed.

Implications for reviewers

Reviewers play a critical role in the academic publishing process as peer experts (whether the manuscript they assess employs a deductive or an inductive approach). However, the burden placed on reviewers of inductive research, particularly in contrast to the leading journals where the hypothetico-deductive model and developmental reviews dominate, should decrease. Reviewers of inductive research are not expected to become co-developers of knowledge (Bergh, 2008: 122).

Their primary task, like the editor's, is to assess whether a manuscript adds sufficient new knowledge. (This role, of course, is not insignificant). Their secondary task is to provide constructive feedback to authors on why they view the novelty or rigor as lacking, which could entail suggestions for enhancing the quality of their manuscripts (in terms of methods, data, and presentation).

With that main difference, the reviewers of inductive and deductive research have similar initial responsibility: an honest self-assessment of their suitability to undertake the review. In other words, the reviewers must ask themselves: Do I have sufficient expertise/knowledge of the subject area? Am I familiar enough with the methods? Am I interested in the topic? (Cf. Barley, 2008: 44).

Summary and some concrete suggestions

Our examination of the publishing process began by reviewing the ongoing debate about it, particularly regarding the developmental versus 'as-is' reviews. We traced the debate's origins to contrasting perspectives present in deductive versus inductive research approaches. We then argued that the developmental review process, prevalent particularly at the leading management journals, is based on the dominance of the hypothetico-deductive methodology.

We also argued that to advance knowledge, a better balance of inductive and deductive research is required. Since the deductive approach has been dominant for so long (Daft & Lewin, 1990), it is likely that changes in the publishing process are necessary for more inductive research to be conducted and published in management studies. This would potentially help connect the field back to the issues faced by practicing managers (Hambrick, 2007). More research starting from phenomena for which causes are not (fully) understood, as opposed to elaborating or testing

existing theory, would provide a novel foundation from which to further advance our theories. For example, Locke (2007: 879) explains the development of his and Latham's well-known theory of goal-setting: "It was only after 25 years of research and some 400 studies by ourselves and others that Latham and I felt ready to actually develop a theory. It was done strictly by induction. There was no advance theory."

One of the ways to promote a balance between inductive and deductive research is to recognize the differences between the two types of research – including in the publishing process. We claimed that while the developmental review mode might be suited to assessing and furthering hypothetico-deductive research, it is not conducive for assessing inductive research. In fact, developmental review may hamper inductive research. If we want inductive research to flourish, it must be assessed differently.

Given the ongoing debate about the review process and changes implemented by some journals, there are reasons for optimism that inductive research will expand and play a greater part in advancing knowledge in management fields. For example, although the developmental review mode has been presented by some as an ideal and tends to dominate, there is room for refining the publishing process.

Many editors appear to have adopted an activist role and do not let conflicting reviews leave a manuscript in a stalemate, but instead resolve the conflict by their own judgment (Clark & Wright, 2009; Jacobs, 2008). Also, switching to a light-touch review mode for inductive manuscripts should not meet with too much resistance, as it does not diminish the power of editors or reviewers (e.g., Hargrave & Van de Ven, 2006; Miller & Van de Ven, 2015). The editors still decide whether manuscripts are published or not, and the reviewers provide their expert opinion, as when assessing deductive manuscripts. What should diminish is the workload for all parties and

the time from the completion of a manuscript to its publication, as publishing decisions on inductive manuscripts would be made after one round of reviews.

Concrete suggestions for encouraging inductive research

Appeals for inductive theorizing have already been made and guidelines to researchers given by others (Eisenhardt, Graebner & Sonenshein, 2016; Locke, 2007; Ormerod, 2009; Pratt, 2009). We make our plea primarily to journal editors, as they have the most power (with the approval of their publishers) to implement changes that will help encourage more inductive research. We have three concrete proposals for editors wanting to promote inductive research: 1) establish a different – light-touch – review process for inductive research, including separate reviewer guidelines, 2) make reviews open, and 3) provide a separate forum for critique and debate.

To encourage inductive research, editors should convey that inductive manuscripts are welcome at their journals and will be recognized as such, and that they will be reviewed in a manner suited to assessing inductive research. The authors will then know at the outset that their submissions will be evaluated based on their knowledge contributions, and that they will receive the editor's decision after the first round of reviews, or after no more than one revision. Such explicit policy is likely to attract high quality submissions and to make the publishing process faster, thereby advancing knowledge more quickly.

To educate reviewers about inductive research, a separate set of reviewer guidelines for assessing manuscripts based on such research should be developed. Such guidelines would help both authors and reviewers: the former to design their research and develop their manuscripts to maximize their knowledge contributions, and the latter to assess manuscripts based on the same criteria.

Double-blind reviews of inductive research may also not be necessary, as such research is less likely to directly challenge someone's favorite theory. And since no hypotheses will be tested, there are no arguments about the preferred theories and hypotheses (although reviewers could point out existing theories providing alternative explanations). Open reviews from the outset would have the benefit of encouraging only qualified reviewers to agree to review, and would give them public recognition for the valuable work that they do. More importantly, if reviews are published alongside the manuscripts (or online), other researchers benefit from the feedback and can learn from them to improve their own projects. Open feedback is also likely to be more constructive and thus help advance inductive research.

An example of open reviews is *Implementation Science*, a journal publishing articles relating to the uptake of research findings into routine healthcare. The journal has adopted a practice that all peer review "is open, meaning that reviewers' names are included on the peer review reports, and secondly, that, if the manuscript is published, the reports are published along with the article (Implementation Science, 2017). The interdisciplinary online publishing platform Public Library of Science (PLOS) operates similarly to Implementation Science.

While editors' feedback has never been given anonymously, opening up reviewer feedback would require greater clarity from editors, particularly when manuscripts are rejected. Such clarity of standards can only be of benefit to authors of inductive research and help them strive towards those standards.

While publishing manuscripts based on inductive research after a light-touch review is likely to speed up advancement and sharing of knowledge, providing separate fora for presenting informed opinion, critique and debate can also facilitate inductive theory building. These could be modeled after the So!apbox and Point-Counterpoint sections in *Strategic Organization* and

Journal of Management Studies. In journals that also target practitioner audiences, practitioner comments could help to facilitate inductive research. Practitioners initially contributed extensively in journals such as *Long Range Planning*; however, their involvement has waned over its forty-year history (Cummings & Daellenbach, 2009).

In sum, we hope that our suggestions may stimulate ideas to editors for facilitating additional inductive research, and that our arguments may persuade readers about the importance of inductive research and encourage its pursuit. Achieving a better balance between inductive and deductive research will, in our view, lead over time to fully fledged, broader theories and advance knowledge in management studies.

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