Proceedings of the 52nd Hawaii International Conference on System Sciences | 2019

Reconsidering the Role of Research Method Guidelines for Qualitative, Mixed-methods, and Design Science Research

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Abstract

Guidelines for different qualitative research genres have been proposed in information systems (IS). As these guidelines are outlined for conducting and evaluating good research, studies may be denied publication simply because they do not follow a prescribed methodology. This can result in "checkbox" compliance, where the guidelines become more important than the study. We argue that guidelines can only be used to evaluate what good research is if there is evidence that they lead to certain good research outcomes. Currently, the guidelines do not present such evidence. Instead, when it is presented, the evidence is often an authority argument or evidence of popularity with usability examples. We further postulate that such evidence linking guidelines and outcomes cannot be presented. Therefore, it may be time for the IS research community to acknowledge that many research method principles we regard as authoritative may ultimately be based on speculation and opinion, and thus, they should be taken less seriously as absolute guidelines in the review process.

1. Introduction

In the information systems (IS) field, there is a perception that only positivistic research methods are "legitimate methods for use in social science" [1 p. 343]. Such positivistic methods include "inferential statistics, hypothesis testing, mathematical analyses, and experimental and quasi-experimental design" [1 p. 343]. Although such views may not be truly positivistic [2], it is understandable that scholars engaged in qualitative IS research report significant difficulty in meeting these beliefs in publishing qualitative research. Many qualitative IS scholars reacted to these perceptions by writing methodological articles aimed at rendering qualitative research "scientific" or publishable [3, 4]. For example, Lee [5] proposed a case study methodology to meet the standards of the "positivistic" natural science model of scientific research. Klein and Myers [6] reported that, "while the conventions for evaluating information systems case studies conducted according to the natural science model of social science are now widely accepted, this is not the case for interpretive field studies" [p. 67]. To ensure IS acceptance of interpretive research, these authors proposed principles for conducting and evaluating interpretive research.

A similar trend has occurred for conducting and evaluating design-science research [7]. Later, similar guidelines were outlined for mixed-methods research by Venkatesh: "there is a dearth of mixed methods research in information systems" [8 p. 1] and a lack of "guidelines for conducting and evaluating mixed methods [research]." To increase publication of mixed-methods research, these authors proposed guidelines for conducting mixed-methods research in IS studies.

We do not doubt that the guidelines described above have helped IS scholars in publishing qualitative, mixed-methods, and design-science research. The downside is that the guidelines can also prevent the publication of such research, as reviewers can interpret them as absolute dogma regulating what is acceptable (rigorous) and what is not acceptable IS research. The guidelines can be easily read in this way. For example, it is noted that "guidelines should be addressed in some manner for design-science research to be complete" [7 p. 82]. To give another example, Venkatesh et al. [8] provided a normative view that "IS researchers should employ a mixed-methods approach only when they intend to provide a holistic understanding of a phenomenon for which extant research is fragmented, inconclusive, and equivocal" [p. 36]. This implies that, in other situations, mixed methods should not be used.

Given that the guidelines can be interpreted as normative, it is currently the case that IS scholars "produce knowledge that seeks to get through reviewers looking to check boxes on theory and method" [9 p. 275]. Fitzgerald [10] reported that, during the doctoral consortium of the International Conference on Software Engineering, "research method was mentioned just once (and that was by a student) and the focus was much more on the actual content of the research." He stated that, when he attended the doctoral consortium of the European Conference on Information Systems (ECIS), "more than 50% of the time involved discussions of research method issues. However, I do not necessarily think that this was time well-spent."

URI: https://hdl.handle.net/10125/60062
ISBN: 978-0-9981331-2-6

In the IS field, the perception that it is important to adhere to correct research method principles (RMPs) as a hallmark of science is understandable. Influential scientific thinkers, such as Comte, Bain, Jevons, Helmholtz, and Mach, thought that the scientific method is necessary for the success of science [11]. Logical positivists even suggested that the use of a scientific method distinguishes science from pseudoscience [12]. However, numerous philosophers, such as Feyerabend [13], have reported that strict methodological principles restrict innovation, thereby hindering scientific breakthroughs.

This study reviews some of the research method guidelines (RMGs) in IS. We also review the philosophy of science regarding RMGs. We end by presenting a naturalistic approach to RMGs in IS, in which RMGs are regarded as either scientific hypotheses with evidence or idealizations. The first approach requires evidence that each principle leads to a specific outcome. We maintain that such evidence cannot be provided in indeterministic settings, such as in IS (qualitative) research. As a result, we postulate that RMGs are idealized and may have various benefits for educational purposes. Having said that, it is debatable whether they can be used to evaluate the quality of research.

2. Methodological guidelines for qualitative-oriented IS research

We first explain RMGs and RMPs, and then we review three guidelines for research in IS. We point out that these guidelines are outlined as criteria for *good* or *high-quality* research. Finally, we review what evidence they provide to back up their claim that they can be used as guidelines on how to conduct and evaluate research.

2.1. RMGs and RMPs

In the philosophy of science, RMPs and RMGs belong to the "theory of scientific methodology" [14 p. 3], which we review in section 3. There is no common definition for RMPs and RMGs in the philosophy of science. Roughly speaking, RMPs are concerned with "how scientific theories in general are appraised and validated" [14 p. 3]. What can be regarded as RMPs varies from one author to another in the philosophy of science. We characterize RMPs as any principles that provide normative guidance on how good research is conducted or evaluated (or both).

In IS, RMPs can vary from requiring certain tests, *p*-values, and sample sizes to requiring normative statements about which conditions of qualitative research are acceptable for mixed-methods studies. An RMG consists of one or more RMPs; thus, broadly speaking, an RMG is a collection of RMPs. For example, Klein and Myers [6] suggested nine principles for interpretive research; each of these principles is an RMP, according to our terminology, while the complete list of principles is regarded as an RMG.

2.2. Guidelines for conducting and evaluating high-quality, rigorous research

Considering that the IS literature is prolific when it comes to research methodology guidelines, in this section, we focus primarily on three commonly known and widely cited normative criteria. These are Klein and Myers's [6] criteria for interpretive field studies, Hevner et al.'s [7] criteria for design science, and Venkatesh et al.'s [8] criteria for mixed research. We discuss these guidelines below.

Hevner et al. [7] conducted a study "to inform the community of IS researchers and practitioners of how to conduct, evaluate, and present design-science research … by developing a set of guidelines for conducting and evaluating good design-science research" [p. 77]. Hevner [15] also reported that "it is vital that we as a research community provide clear and consistent guidelines for the design and execution of high quality design science research projects" [p. 87]. Similarly, Klein and Myers [6] noted that "[a]s the interest in interpretive research has increased … researchers, reviewers, and editors have raised questions about how interpretive field research should be conducted and how its quality can be assessed" [p. 67]. Mixed method guidelines have similar goals. For instance, Venkatesh et al. [8] offered "a set of guidelines for conducting and evaluating mixed methods research in IS … to initiate and facilitate discourse on mixed methods research in IS and encourage and assist IS researchers to conduct rigorous mixed methods research" [p. 2][1].

Grounded theory guidelines [16] are "guidelines for conducting and evaluating grounded theory studies in information systems" [p. 358], and they should "clarify what good grounded theory might look like" [16 p. 368]. These guidelines "address how the researcher might achieve the degree of conceptualization necessary to build a good theory" [16 p. 368].

To summarize, given the guidelines' statements, it is easy to understand that, in the hands of reviewers, when IS research does not meet these guidelines, the research—rather than the guidelines—is often blamed as being of low quality. When the guidelines are not met,

[1] Venkatesh et al. [8] also "offer a set of guidelines for IS researchers to consider in making decisions regarding whether to employ a mixed methods approach in their research" [p. 15].

the jargon used by reviewers, for example, may include the criticism of a "lack of methodological rigor." Furthermore, considering that these guidelines are advocated as procedures for evaluating and conducting *good* or *high-quality* (qualitative, interpretive, or design-science) research, we need to ask what evidence they are based on. This is considered in the next subsection.

2.3. Evidence supporting the use of guidelines

Typically, RMG articles require a legitimization strategy, which usually involves arguing that the proposed guidelines have the following characteristics: 1) they are *consistent* with previous research(ers); 2) *popular* among a group of researchers; 3) *used* by one or more published paper(s); and/or 4) *can be used* by future researchers. However, these RMG articles fail to mention that the guidelines do not provide evidence for better outcomes or performance compared with approaches that do not follow the guidelines. Below, we discuss these issues in more detail.

2.3.1 Consistency is not evidence of outcomes. Many guidelines use the rhetoric of being consistent with some articles or researchers. For example, Klein and Myers [6] justify their guidelines as follows: the "proposed principles are consistent with a considerable part of the philosophical base of literature on interpretivism and hence an improvement over the status quo" [p. 68]. (However, they do not show concretely that their principles "are consistent with a considerable part of the philosophical base of literature on interpretivism.")

Readers should understand that showing consistency is not good practice for scientific justification. Consider the following well-known thesis: The earth is flat. Then, consider the following argument: The earth is flat because this view is consistent with Carpenter [17]. (William Carpenter [17] advocated the theory that the Earth is flat.) It is true that this argument is consistent with Carpenter [17]; however, who would accept this as evidence that the Earth is flat?

In scientific research, it is not a good justification practice for researchers to base their arguments on references that are merely consistent with their opinions. This is because justifying claims by stating consistency with a previous study does not require the presentation of evidence for or against the claim. By analogy, proposing a principle for conducting and evaluating high-quality research, be it qualitative, mixed method, or design science, should require the presentation of available evidence for and against each principle. We argue that scientific research should provide the evidence for and against something. This should not be replaced by someone's opinion (without evidence) and references that are consistent with these opinions.

2.3.2 Evidence of use or usability. As exemplified by [6], [7], and [8], RMGs commonly present examples demonstrating the applicability of guidelines [2] by providing evidence supporting their use. However, evidence for use should not be confused with the quality of research or demonstrating cause and effect. We illustrate this in the examples below.

It was once thought that cancer was contagious (contagious cancer theory), spreading from one individual to another. The practical implication (preventive treatment) of the contagious cancer theory was that patients should be isolated to avoid the spread of the cancer. Let us presume that one follows this practice, that is, isolates the patients. This procedure—the act of isolating the patient successfully—does not mean that isolating patients with cancer is proof that cancer is contagious. To give an even simpler and more provocative example, let us presume that one is diagnosed with cancer, and the treatment advice is to walk one mile every day. If one can do that, it demonstrates that the actions are doable (for this person at least); however, the fact that one is able to walk for one mile is not evidence that this is a good cancer treatment. Similarly, how many times the paper is cited should not be conflated with evidence of the outcome. The popularity of a claim is not evidence that the claim is true.

The takeaway point is that many IS RMGs—at least those reviewed here, namely [6], [7], and [8]—either provide some evidence that an RMP has been used or demonstrate how an RMP can be used. These measures can be useful for pedagogical purposes, but they are not evidence on cause and effect or good outcomes. Such information cannot be used to claim that these guidelines are appropriate for evaluating and conducting *good* or *high-quality* (interpretive, mixed-method, or design-science) research.

2.3.3 Contradictory statements create confusion. Readers of the guidelines may find some statements confusing. For example, on the one hand, Hevner et al. [7] "advised against mandatory or rote use of the guidelines" [p. 82]. On the other, they suggested that "[r]esearchers, reviewers, and editors must use their creative skills and judgment to determine when, where, and how to apply each of the guidelines in a specific

[2] Klein and Myers [6] used "three published examples of interpretive field research from the IS research literature … in order to demonstrate how authors, reviewers, and editors can apply the principles" [p. 79]. Hevner et al. [7] also used three examples: "we use the proposed guidelines to assess recent exemplar papers published in the IS literature in order to illustrate how authors, reviewers, and editors can apply them consistently" [p. 78].

research project" [ibid, p. 82]. Similarly, Klein and Myers [6] cautioned that "principles are not like bureaucratic rules of conduct, because the application of one or more of them still requires considerable creative thought … [I]t is incumbent upon authors, reviewers, and editors to exercise their judgment and discretion in deciding whether, how, and which of the principles should be applied and appropriated in any given research project" [p. 71]. While this indicates some flexibility in guideline application, these researchers also noted that, "while we believe that none of our principles should be left out arbitrarily, researchers need to work out themselves how (and which of) the principles apply in any particular situation" [p. 78]. Omitting principles is an arbitrary action if no guidelines are provided. In addition, Klein and Myers [6] reported a set of guidelines for how "interpretive field research should be conducted and how its quality can be assessed" [p. 67] that "require considerable creative thought" [p. 78] and application based on individual perceptions of individual cases. However, this raises the following question: If their application requires considerable creative thought case by case, then how can these principles provide guidelines for how research "should be conducted and how its quality can be assessed" [p. 67]?

To summarize, the guideline proposals create confusion in several ways. First, if principles require situational adaptations, but no guidelines are provided to make such adaptions, then the principles cannot help provide adequate evaluations of qualitative studies. Principles lose value for research evaluation when situational adaptations based on personal judgment are required. For instance, Klein and Myers's [6] suggestion that principles should "leave open the possibility that other authors may suggest additional sets of principles" [p. 68] seems to oppose the evaluation of the qualitative research's quality. Second, if principles "require considerable creative thought" and case-by-case consideration, then how can they be used to determine research quality? Before we present our view on the role of these guidelines in providing norms for conducting and evaluating research, we briefly review what philosophy of science has to say about RMPs.

3. Philosophical foundations for research methods

This section reviews the philosophy of science underlying RMPs.

3.1. The necessity of an absolute research method

In this section, selected attempts to build an absolute research method are presented, and the perspectives that have inspired arguments against logical positivists are discussed to show how building an absolute methodology for scientific research has failed. Some objections have come from logical positivists themselves (i.e., Carnap and Neurath) [2]; however, the most well-known objections have been provided by outsiders, including Quine's [18] dogma of reductionism, Kuhn's [19] methodological subjectivity, and various theses by Hanson [20], and Feyerabend [13]; see [2].

3.1.1. Aristotle: Critical thinking leads to absolute certainty. Aristotle considered that scientific knowledge can be separated from opinion and superstition with absolute certainty through critical thinking [11]. For Aristotle, scientific knowledge was absolute and infallible [21]. However, later scientific progress cast serious doubts on this view [22, 23], as new scientific theories and studies kept challenging existing scientific views once regarded as infallible or self-evident [23, 14].

3.1.2. Comte, Jevons, Helmholtz, and Mach: Can the scientific method explain the success of science? When philosophers realized that scientific knowledge cannot be certain, specific sciences, such as physics, were seemingly highly progressive [11]. This raises the question of why they were successful. Given that Aristotelian infallible critical thinking was not the scientific method explaining the success of science, philosophers like Comte, Jevons, Helmholtz, and Mach suggested other candidates for the scientific method [11]. However, they could not agree on what this scientific method was [11]. Even more problematic was that Duhem [24] showed that proposed RMPs were either not used or were violated by successful scientists. As science continued to make breakthroughs in physics and medicine, interest in understanding this success was high [11]. Motivated in this way, logical positivists (e.g., Schlick, Neurath, and Carnap) suggested that the scientific method not only explains the success of science, but it can also be used to differentiate science from pseudoscience [2]. For example, Schlick [12] put forward an absolute method known as the verification method. Logical positivists' ambition to establish an absolute and objective method to separate science from nonsense attracted a lot of criticism, which ultimately clarified that no method is truly objective. Such criticism is discussed below.

3.1.3. Insider critique of logical positivism: Neurath and Carnap. Criticism of absolute RMPs first emerged in the Vienna Circle. Neurath, and later Carnap, argued that absolute RMPs were impossible [25], and Carnap [26] noted that no methodological norm provided "objective validity" because norms cannot "be empirically verified or deduced from empirical propositions; indeed [norms] cannot be affirmed at all" [p. 237]. In other words, Carnap deemed the acceptance of any RMP a matter of taste. This does not mean that Carnap lacked RMP preferences, as he certainly had them. Rather, it signifies that Carnap viewed it as impossible to justify that one RMP is ultimately better than another [27]. Therefore, for Carnap, RMPs were “proposals, which no one was obligated to accept” [28]. Similarly, logical empiricist Reichenbach [29] noted that the aims of science, including the choice of methodological norms, are ultimately a matter of taste.

3.2. The relativistic critique: Quine, Kuhn, Hanson, and Feyerabend

The critique outside the logical positivism was better known than the logical positivists own critique was. However, this outsiders’ critique was not fatal to the logical positivism, because positivists had already left their views behind [27, 30]. Ironically, while the positivists’ mission of developing an absolute methodology failed, it also inspired a number of philosophers to show how the best science was ultimately based on speculative metaphysics [2]. These views aimed to illustrate the following: 1) no method could be absolutely objective [18, 20]; 2) acceptance of RMPs was irrational, subjective, and a matter of fate similar to acceptance of religious views [19]; 3) RMPs were worse than useless [13]; and 4) RMPs were tacit knowledge that was impossible to present as written principles [31].

3.2.1. Quine: Verification theory and reductionism. Quine’s [18] critique pointed out that verification cannot test a single statement or hypothesis isolated from its underlying assumptions. That is to say, any test or observation, no matter how simple and obvious it may sound, is always associated with a number of underlying presuppositions that are not empirically testable and must be assumed. Quine [18] maintained that, when a claim is tested, a complex web of assumptions and presuppositions are also tested; thus, he concluded that any hypothesis can be accepted by revising the underlying assumptions. Quine’s critique applies to any test for RMPs.

3.2.2. Kuhn: Fundamental method decisions are irrational. Kuhn [19] argued against positivists’ absolute views. Kuhn argued that different paradigms in one scientific discipline have radically different methodological norms for assessing theories. These norms are subjective beliefs rather than evidence-based assertions [19]. Kuhn [19] maintained that, by definition, the worldview and languages of each paradigm are so different that one paradigm cannot communicate methodological rules outside of it. Kuhn claimed that a change in methodological thinking in physics does not occur through rational discussions or objective evidence, and it has nothing to do with the verification method suggested by positivists [12]. Instead, methodological changes for assessing theories are irrational, a “leap of faith,” or comparable to a religious “conversion experience” [12].

3.2.3. Hanson: Theory-laden observations. The positivists’ verification method was based on observation [2]. Hanson [20] took up this point and argued that all observations are theory laden. For example, when microscopic images from a biochemistry journal are viewed, those who have doctorates in biochemistry see different things in the picture than those who lack such education [2]. Hanson [20] presented examples of how, even within one scientific discipline, different scientists may see different things based on the same observational evidence available.

3.2.4 Feyerabend: Universal method principles are worse than useless. Feyerabend [32] examined breakthroughs in physics. He argued that there are no universal, predefined, or common methodological rules in science. He argued that if he had to give one such rule, it would be “anything goes”; this became his famous slogan [33], [34][3]. See also Treiblmaier [35]. Feyerabend [13] presented evidence that the best scientists made up their own RMPs as they proceeded with their research. Importantly, Feyerabend [13] noted that breaking the rules for appraising research was not limited to exceptional cases. Instead, he emphasized that the scientific elite not only broke all the common and predefined RMPs, but they also did so frequently [13, p. 23]. Feyerabend’s [13] other important point was that RMPs restrict theory development: “Science needs people who are adaptable and inventive, not rigid imitators of established behavioral patterns” [13, p. 163]. For him, theory development was an invention that “depends on our talents and other fortuities circumstances” [13, p. 155], and rules just limit talented people [13, p. 156]. Moreover, Feyerabend [13] noted that any test or instrument for observation comprises (speculative) beliefs that are inculcated in us through education and upbringing.

[3] Feyerabend [34] noted: “*anything goes* does not express any conviction of mine, it is jocular summary of the predicament of the rationalist: if you want universal standards, I say, if you cannot live without principles that hold independently of situation, shape of world, exigencies of research, temperamental peculiarities, ties, then I can give you such a principle. It will be empty, useless, and pretty ridiculous-but it will be a "principle." It will be the "principle" "anything goes” [p. 188].

3.2.5. Polanyi and Hesse: Scientific expertise is tacit and cannot be written as methodological rules. Polanyi [36] claimed that "[n]o rules can account for the way a good idea is found for starting an inquiry, and there are no firm rules either for the verification or the refutation of the proposed solution of a problem" [p. 27]. For Polanyi, scientific activities are intuitive insights and tacit knowledge, which cannot be written as rules. Somewhat similarly, Hesse [37] noted the impossibility of setting rules for science. He maintained that, whenever such rules exist, they reflect individuals' scientific upbringing.

4. RMPs as hypothetical, instrumental, and revisable

Despite the drawbacks mentioned above, logical positivists' lack of ability to establish absolute normative rules does not mean that all methodological rules are opinion-based or that RMPs are irrational [28]. Rather, Laudan [38] suggested that RMPs are like any other empirical and conceptual problems in science. According to his view, RMPs and RMGs should be as open to testing, like any other scientific theory, hypothesis, or proposition. In addition, scientific theories, hypotheses, and propositions are not "fixed once and for all" [p. 353], but instead, may be revisable in light of the evidence. Similarly, RMPs and RMGs should also be tentative and revisable. Research methods are theory laden [20] and subject to Quine's [18] problem of reductionism. This explains why two scholars can disagree about a certain RMP even if they are considering the same evidence and share the same scientific aims.

If RMPs are hypothetical and revisable in light of evidence regarding how effectively they promote the goals of science, then the key issue for each RMP is its evidence [28]. Different methodological principles can be effective for promoting different aims; therefore, each RMP is linked to and promotes a specific goal. For Laudan [39], selecting a RMP *rationally* requires choosing a method that best promotes a specific goal in science. Moreover, for Laudan, an RMP is not chosen in a comparative vacuum. Instead, RMPs are selected based on a variety of criteria for acceptance, testing, and use, by using available evidence [40]. Laudan [38] suggested that a decision is rational when an individual perceives that certain RMPs are more likely to realize the goals of an inquiry than the alternatives are. In the next section, we discuss the challenges of applying Laudan's program in IS RMGs for qualitative study, design science and mixed methods.

5. The challenges of Laudan's normative naturalism in IS

Laudan [41] suggested that the principles of research methods are hypothetical imperatives formulated as follows:

> "If actions of a particular sort, m, have consistently promoted certain cognitive ends, e, in the past, and rival actions, n, [have] failed to do so, then assume that future actions follow the rule. If you[r] aim is e, you ought to do m[, which is] more likely to promote those ends than those actions based on the rule: if you[r] aim is e, you ought to do n." [28]

Doppelt [42] criticized Laudan, stating that all principles of RMGs are hypothetical imperatives. [44] maintained that, due to Quine's underdetermination, there are basic methodological standards that are justified, even if they lack conclusive empirical evidence for being the most effective means of achieving the research aims. If this suggestion is accepted, then not all principles of RMGs are hypothetical imperatives and some are basic methodological standards.

Laudan [43] and Resnik [44] found this reasoning wanting, as do we. Principles of RMGs can be underdetermined based on evidence. The implication of underdetermination, if accepted, is that researchers can challenge that a certain principle, X, is better than Y for achieving an outcome; however, this does not challenge the idea that RMGs are hypothetical imperatives [43], [44]. A practical concern of normative naturalism is that scholars cannot survey all the available methods to select the one that is best-suited to a specific study; we simply lack the cognitive competence for this task [45].

We see three other concerns in applying Laudan's normative naturalism [43] in IS. First, while the specific yet various aims in (say) physics or cancer research may be well understood, it is not clear that this is the case in IS. We are afraid that the primary "aim" in IS is "how to publish in top IS journals," and the guidelines are intended to help with this.

The second challenge, which is not specific to IS, is that the methods may distort the reality. For example, "all statistical models include a number of assumptions about the underlying data generating process, sampling and the observed distributions, that are, strictly speaking, false" [46 p. 441]. This means that methods (generally speaking) are theoretically restricted. These assumptions and restrictions are not understood in IS, perhaps because the RMGs in IS are aimed at showing some RMPs as accepted. Thus, the aim is not to deeply understand their underlying assumptions, which distort the reality. However, they need to be understood if we want to use them as evidence for promoting certain cognitive goals. As a concrete example, statistical significance is not the same as practical significance. For example, the

American Statistical Association (ASA) announced an official warning for authors who employ statistical significance tests uncritically: "Statistical significance is not equivalent to scientific, human, or economic significance" [47]. In medicine [48], "[t]he non-equivalence of statistical significance and clinical importance has long been recognised" [p. 311]. As another example, an article [49] noted, "Statistical significance at any level does not prove medical, scientific, or commercial importance" [p. 325].

The third issue concerns Laudan's [39] normative naturalism and its implications for cause and effect. This issue is also important for any IS guideline, which suggests that certain RMPs can be used to evaluate good research. For Laudan, "[i]f one's goal is y, then one ought to do x" [p. 24] and "all methodological rules … can be re-cast as [a] contingent statement of … [the] connections between means and ends" [p. 25]. We argue the following: If RMPs are normative, for example, then they claim to say what is good qualitative research or when mixed methods can be used. This claim implicitly assumes cause and outcome (effect) relationships. If design-science or interpretive research principles are required for doing good science, then there is an assumed causal relationship between RMG principles and outcomes. Similarly, those reviewers who use the guidelines to evaluate what acceptable science is implicitly assume cause and outcome (effect) relationships, where RMPs are the cause and good or acceptable research is the outcome. Are there such causal relationships? Is this even possible? To clarify this point, let us consider the three following commonly recognized causal capacities (or types of causation): 1) deterministic, 2) random, and 3) probabilistic causation [48 p. 522].

A deterministic causation "is one which, under specifiable circumstances, always produces its effect" [ibid]. Deterministic causality requires the existence of true, 100% exceptionless laws. In psychological and social phenomena, such as qualitative research, there are no deterministic laws [50]. It is highly questionable to claim that the RMPs are deterministic, that is, that they cause the effect, namely the outcome of good research, with no exceptions. This leaves us with two options: The causation is probabilistic or random. "[P]robabilistic capacity also operates only sometimes, but the strength of the tendency to produce the effect is nomologically fixed … [I]f there are genuinely random capacities, it is obvious that there are capacities that cannot be reduced to quantitative probabilities" [39 p. 522]. None of these guidelines show probabilistic evidence, that for example, with 85% likelihood, employing a certain RMP leads to better results than following some other principle. This is not a criticism of these guidelines: We doubt that producing such evidence is possible at all. For example, Thagard [51] suggested that causality in medical research and psychology is complex, changing, and above all, random. What is random causality? "A random capacity sometimes produces its effect and sometimes does not, but nature does not determine how often or how regularly it does so" [39 p. 522]. To summarize, if there are no deterministic or probabilistic causes between the RMPs and the outcomes because the phenomenon is indeterministic, then no philosophizing or tests can establish it. This does not imply that the qualitative, mixed-methods, or design-science research is "unscientific." Rather, it means that we should omit the idea that certain RMPs produce or guarantee (i.e., cause) good research. The implication is that the RMPs or RMGs should have little normative effect on our journal review process. Moreover, this does not mean that they are useless; rather, they can have a pedagogical function, for example, in PhD training.

6. Discussion

Next, we discuss five key problems emanating from the "normativization" of methodological guidelines.

6.1. Clear and consistent guidelines

Hevner [15] noted that "it is vital that we as a research community provide clear and consistent … guidelines … for the design and execution of high quality design science research projects … to establish the credibility of IS design science research" [p. 87]. The problem with the requirement for consistent guidelines is that it may force design-science research into a uniform format that does not allow for variety. This was noted by Klein and Myers [6]: "[T]he complete literature of interpretive philosophy comprises so many varied philosophical positions that it is unlikely to yield one consistent set of principles for doing interpretive research" [p. 70]. An additional concern is the prohibition of creativity and out-of-the-box thinking, which can hinder scientific innovation.

6.2. Checkbox compliance

Klein and Myers [6] noted, "it is better to have some principles than none at all, since the absence of any criteria increases the risk that interpretive work will continue to be judged inappropriately" [p. 68]. There is also a risk that these guidelines will pave the way for inappropriate judgements. The idea that, by reading an article on a set of principles, readers can conduct and evaluate research in the area is misleading and can lead to checkbox compliance.

First, these guidelines may create a situation where reviewers, without any hands-on expertise on interpretive, design, or mixed-methods research, take on the role of gatekeepers by simply using these guidelines as a checklist. Second, if paper acceptance in leading IS journals requires compliance with these principles (to meet "methodological rigor"), then what is required for publication is not gained expertise on qualitative research, but instead, how to demonstrate compliance with the guidelines. If this is true, then it may lead to a situation where the required methodological principle is complying with the guidelines and not the mastery of, say, interpretive research. Consequently, we may have IS scholars whose primary qualitative "training" is reading the guidelines, with a focus on strict compliance (to maximize paper acceptance). We are afraid that this is already the case.

Finally, what happens to creative and unexpected research and any settings or circumstances that do not fit the guidelines[4]? More precisely, there is the risk that noncompliance with the guidelines will be viewed as a "lack of methodological rigor" or "flaw," thereby leading to rejection. If reviewers' "challenge is to find the fatal flaws" [52], then we are afraid that the bet is to find a setting or circumstances where you can have 100% compliance with a guideline. It could be that important cases and settings are those complex and dynamic cases that do not match well with any established guidelines.

6.3. Do the RMGs meet the standards they impose?

As exemplified by [6], [7], and [8], many RMGs/RMPs propose "validation" or "evaluation" guidelines and ask the authors to validate their research with different "rigorous" tests. At the same time, readers can hardly find such "rigorous" evidence supporting the RMPs in these articles. This makes readers wonder whether the RMG articles meet the standards they set for empirical research. The answer is that they do not meet their principles. For example, Hevner et al. [7] suggest comparisons with rival approaches, but they do not consider doing this for their guidelines.

6.4. How should we regard the RMGs and their principles?

One option concerning how we should regard the RMGs and their principles is in line with Kuhn's [19] suggestion that the decisions about which RMPs are used by a research community are irrational, so that a shift in such norms is comparable to a religious "conversion experience" [19]. As for those who agree with Polanyi [31], they would perhaps deny the whole business of proposing a set of principles (and claim that the research method competence is tacit). Laudan's [28], [38] approach would subject these principles to scientific study. According to this view [38], research on "methodology is the study of how to conduct inquiry effectively" [p. 349]. According to Laudan [39], the key question for scholars examining research methodologies is understanding that "methodology rules are … statements about instrumentalities, about effective means for realizing cherished ends" [p. 24]. Given that RMGs are like any other theories or set of propositions in science, RMPs are testable to the same extent as any other theories, hypotheses, and propositions in science [38]. However, such tests have not been reported in IS. Ultimately, if we cannot show any evidence that these principles lead to better outcomes than their competitors, then why should we require them?

6.5. The "authority" and "consistent with" arguments

As a final point, many RMGs justify either individual principles or the whole RMG with an authority argument, such as "in our opinion" or "based on our insights." An authority argument is, for example, when one declares an opinion without presenting the available evidence to justify it. Readers may wonder whether the RMPs, especially when they are used to regulate research, are too important to be a matter of authority or opinion. If RMGs contain rules that regulate what "rigorous" practice is, and reviewers require authors to follow them, then should such principles not only be testable claims on how the RMPs are empirically successful in achieving the specific goals? Instead, should they also present evidence for (and against) the approach? As elaborated on above, RMGs typically use the "consistent with" argument, which is a questionable approach to justification.

7. Conclusion

Methodological guidelines have been proposed for both conducting and evaluating qualitative, design-science, and mixed-methods research. While these guidelines require rigorous testing and validation, they themselves do not meet these requirements. The "evidence" for the guidelines consists of the authors' opinion and showing that the principle is consistent with some previously published research. The guidelines give examples on the applicability of the principles, showing

[4] For example, Venkatesh et al. [8] "summarize seven purposes for mixed methods research that [the authors] adapted from prior research" [p. 5], indicating that only these seven purposes are possible. Moreover, "if researchers fail to provide and explain meta-inferences, the very objective of conducting a mixed methods research study is not achieved" [p. 19]. Or consider, "IS researchers should employ a mixed-methods approach only when they intend to provide a holistic understanding of a phenomenon for which extant research is fragmented, inconclusive, and equivocal" [p. 36]."

how they can be applied or how a study has applied them. Neither focus counts as evidence for "conducting" and "evaluating" good or high-quality research.

If the method guidelines are used for evaluation—as they claim—then papers' acceptance should not be based on the authority argument, usability, or showing that the principle is consistent with some papers. Moreover, there is a risk that the guidelines have paved the way for 'checkbox' compliance, where research that meets the principles are accepted and research that does not is regarded as lacking methodological rigor. Our experience suggest that this situation is common.

It is also important to ask why IS journals need to present method guidelines when there are many research method journals[5]? Do the IS guidelines add any value? Finally, the philosophy of science regarding the scientific method offers important lessons for IS. One option is regarding each RMP as a tentative and revisable principle that enjoys evidence for and against. This view means that RMGs are similar to hypotheses in science and have limited generalizability. This option, however, seems to be impossible to achieve in the IS context of mixed, design-science, and qualitative methods. Alternatively, the Kuhnian approach is regarding RMGs as dogmatic and irrational conventions. Our proposal is considering the guidelines as idealizations, which are useful for pedagogical purposes. This does not mean that IS guidelines are useless, but rather, that they should have less weight in evaluating what is an acceptable use of method.

8. References

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