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Good science, bad science: Preventing paradigm paralysis and method-bias malaise

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ABSTRACT

This essay attempts to describe the potentially positive or negative outcomes of a research methods issue. I draw upon three lessons for scientists found in the field of medical science. These three lessons are applied to the Accounting Information Systems (AIS) field. Finally, I suggest a few ways by which AIS researchers can collectively make the most of this issue to thrust the field forward.

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1. Introduction

Any special issue on methods holds both promise and peril. On the one hand, it provides a promising opportunity to explore new ways of researching. Learning creative new methods can help researchers generate new ideas for using those methods. Innovative methods may enable new research questions or new theoretical frameworks to be studied—some that could not have been pursued using existing methods. Publishing a set of papers on new methods can be like sending a dentist to a convention at which many new practices and procedures and tools are being presented. I recall my own dentist raving about such a convention, which filled him with new ideas that he claimed positively transformed his practice and improved customer service. A methods issue holds similar potential.

On the other hand, a methods special issue may stunt or limit the field. If the new methods are merely method 'tweaks' and largely reflect what past researchers have done, or if the methods all come from one's own discipline, then the special issue communicates that the way things have always been done is the 'right way.' It would subtly suggest that we should all conform to existing methods or risk having our work

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rejected. In this way, the field and its norms may become more sharply defined in a manner that limits how well it is able to assimilate good ideas from other researchers and other fields.

Another peril is that one may think the new method is good enough to be a magic solution. A researcher with a new tool may apply it to more phenomena than wise and may not apply it correctly. Good science should not be driven so much by the tool as by the research question and theory. Tools should fit the research question, and must be used properly to obtain valid, meaningful results.

Whether this methods issue produces promise or peril for the Accounting Information Systems field will be left to the reader to decide. But this essay raises some of the issues of how a methods issue, or even a discipline's trends, can either lead to strength and vitality or weakness and malaise in a research domain. We introduce three lessons for a scientific discipline using examples from Gary Taubes' book Good Calories, Bad Calories, which critiques the science of diet and health. Taubes also draws on a number of philosophers of science to support his arguments. These lessons are then illustrated and applied to both the Information Systems and Accounting Information Systems fields.

2. Lessons for scientific disciplines

2.1. Lesson #1: Good science stays wary of the current paradigm

Thomas Kuhn (1970) observed that scientific effort tends to progress in waves or paradigms. Researchers tend to ride the wave (i.e., research paradigm) in vogue at the time. Taubes (2008) finds this in diet science.[1] He finds scientists in this field tend to follow the accepted low-fat, high carbohydrate diet theory so closely that they ignore or reinterpret contradictory evidence. Even funding tends to be awarded to those who follow the paradigm instead of those who dare to step outside. Taubes chronicles the paradigm stickiness of research waves in diet, heart, and diabetes issues.

A paradigm provides a way of viewing what belongs within an academic discipline. A paradigm is “largely a matter of implicit social consensus” that occurs over time (Banville and Landry, 1989: 50). A paradigm can define, by gradual consensus, “what should be studied, what questions should be asked,” and what methods and problems belong to a scientific discipline (Banville and Landry, 1989: 49).

In a field as young as Accounting Information Systems (AIS), it could be argued that no set paradigms yet exist. However, one can discern patterns of topics and methods pursued even in a young field like AIS. For example, researchers defined early on what is and what is not AIS research (e.g., design science ‘is’), and have more recently broadened it to be more inclusive (Sutton and Arnold, 2002).

One problem with paradigms is that they can blind you. The cartoon character Pogge said that a way of seeing (i.e., a paradigm) is also a way of not seeing (Van de Ven, 1989). By blinding, we mean paradigms can limit which topics are deemed acceptable and which research methods are used in a discipline. The well-known paradigm of experimental behaviorism drove psychology into a somewhat narrow focus for decades before the cognitive revolution began to blossom in the 1980s. Papers that lay outside the behaviorism paradigm were harder to publish because of an inward focus that excluded other research lenses. This can occur via a “not-invented-here” bias (Baskerville and Myers, 2002).

When management theorists debated the issue of their own paradigms, Harold Koontz advocated disentangling and narrowing what he saw as a “confused and destructive jungle warfare…” (Banville and Landry, 1989). In response, Herbert Simon said that confusion may be another name for progress, and that “science…does not lend itself very well to neat blueprints, detailed road maps, and central planning. Perhaps that's why it's fun” (Banville and Landry, 1989).

For a closer-to-home example, the Information Systems (IS) field went through a well-known debate in the early-to-mid 2000s on what research topics should be studied—or even accepted in its top journals, which arguably showcase what the IS field is all about. Benbasat and Zmud (2003) wrote an article published in a leading IS journal, MIS Quarterly, suggesting that in order to further the discipline's legitimacy, the field should be more narrowly defined to those tasks, structures, and contexts surrounding the IT (information technology) artifact (i.e., computing hardware/software). A large number of articles and book chapters debated this stance, a few in its favor and many against it (Agarwal and Lucas, 2005).

[1] Note that Taubes' book has also been critiqued by others (e.g., Bray, 2008).

Few opposed the general idea that a discipline needs to stake out a topical domain. But fewer agreed with how narrowly Benbasat and Zmud's article seemed to define the field.

The perils of narrowing the discipline's scope were stated in strong terms like: “misguided,” “potentially dangerous” (Myers, 2003, 582), and even: “Such singularity of focus is dangerous, because it leads to rigidity and dogmatism” (Hirschheim and Klein, 2003, 260). Critics suggested that taking a narrow focus reduces the relevancy of the research (McCubbrey, 2003, 554) and the currency or interest of its topics (DeSanctis, 2006, 360). They suggested the discipline would become healthier by pursuing “diverse and novel research” rather than focusing on the core IT artifact (Ives et al., 2004, 108). More than one argued that IS both began and continues as a cross-disciplinary field, and that it can demonstrate strength by positioning itself as a discipline that “transcends traditional functional boundaries” (Agarwal and Lucas, 2005, 390).

Accounting Information Systems (AIS), in both name and practice, spans the Accounting and Information System disciplines, and has already staked out a territory of its own. On the one hand, it has defined a practical set of boundaries by what it has published over time. Perhaps its beginnings in REA/SMAP (resource event agent/semantic modeling of accounting phenomena) have defined a core or foundational focus for the AIS domain (Sutton and Arnold, 2002). However, the question remains, “What else qualifies as AIS research?” A cursory look at a fairly recent monograph about the AIS field (Arnold and Sutton, 2002) indicates that the field has expanded into such areas as electronic commerce, ethics, system user participation, group or individual decision aid technologies, assurance services, and knowledge management. Reference disciplines for the field are said to include not only computer science for the design science parts of the field, but also psychology, sociology, and philosophy (Sutton and Arnold, 2002).

While the definition of the AIS domain is still a work in progress, it appears accepting of new research types as well as studies using the original paradigm(s) that created the field. Assuming this is true, this methods issue holds the promise of introducing new ways to analyze AIS phenomena. But it is possible that the methods introduced here will not be the only ones AIS researchers should consider. The most innovative methods may not have been submitted to this special issue because of the subtle blocking-power of current paradigms. For the same reason, methods that are able to address unique, innovative, and AIS boundary-expanding topics or research questions may not have been submitted, and, if submitted, may not have cleared the review process to publication. Therefore, it is appropriate to search other sources for new methods as well as this methods issue. The trouble with staying with the status quo is that it not only may not solve the next problem that appears, but it may not be the best answer to the current problem. Taubes commented, “‘There is always an easy solution to every human problem,’ H. L. Mencken once said—‘neat, plausible, and wrong…’” (Taubes, 2008: xvii, xxi) What occurred in one segment of the early Information Systems field was that a number of researchers followed a tidy, rigorous paradigm of using experiments to study cognitive style in the systems design domain. After numerous paradigm-following studies were published, an influential review article suggested that this large body of research missed the key issues, labeling it “much ado about nothing” (Huber, 1983). It is heartening to see that AIS is continuing to hold an internal dialog both about the field's boundaries (Sutton and Arnold, 2002) and about what constitutes well-done and valuable research (David et al., 1999).

2.2. Lesson #2: Good science does not become biased by accepted theories or methods

When we find a theory or method that suits us, we tend to focus on it and advocate it at the expense of other theories or methods. This can lead to bias. Taubes (2008) wrote about theory-advocating bias: “Believing that your hypothesis must be correct before all the evidence is gathered encourages you to interpret the evidence selectively. This is human nature.” (2008: 25) Taubes found evidence in the science of heart disease that scientists tend to become biased toward confirmation of their own theory and against its refutation. Similarly, our success using one method can prejudice us from examining other methods. In the review comments on our own papers, my co-authors and I have occasionally been told that our use of XYZ method was not appropriate when in fact it was widely accepted in many circles as valid for answering the question posed. On occasion, the reviewer appeared very devoted to one body of methods to the exclusion of almost any other. I doubt our experience is unique.

This lesson is clearly a caveate about the methods presented in this methods issue. We ought not to allow this issue to invite or enhance methodological bias. Van de Ven (2007: 89–91) points out that researchers often use the same biased judgment as everyone else. Unless we consciously fight them, biases can arise as we narrow the list of research methods to something we feel we can control. Every method or methodological approach has both strengths and limitations (McGrath, 1981). McGrath points out that the strength of each research method also represents its weakness on some other dimension (see his Fig. 2). For example, he shows that when we maximize generalization to the population (e.g., via careful sampling surveys), we cannot maximize control and measurement of the variables involved because we have given up experimental control. Similarly, when we maximize contextual realism via a field study, we score low on measurement precision. Hence, "Not only is there no 'one true method' or set of methodological choices, that will guarantee success; there is not even a 'best' strategy or set of choices for a given problem, setting and available set of resources" (1981: 179). Each method is best at explaining one part of the picture but not another.

For a simple example, researchers using one-time-period methods have found trust to be a good predictor of transacting business via e-commerce (Gefen, 2000). But longitudinal methods have shown that trust's effects become attenuated over time (Gefen et al., 2003). Similarly, survey methods that find usefulness leads to intention to use systems have answered what leads to what, but have not addressed the questions of how this occurs over time or why it occurs. Explaining "why" is one of the hallmarks of a good study (Whetten, 1989). So is explaining "when" something works. A design science model of how something is to work does not test how it might work in a specific practice context unless someone follows up the modeling with a field study. For a positive example, a large number of field studies have followed up the original REA model paper (McCarthy, 1982), both validating it and expanding on it (Geerts, 2008).

Because no one method answers every research question, and because no one method can maximize both internal validity and external validity, we should refuse to become methodological bigots. Every method type has its place. Often, a stream of research can be benefitted by the use of multiple methods—either within a study or across several studies. For example, Payne et al. (1988) proposed a theory about decision strategies and then conducted a Monte Carlo simulation of their theory. Next, for practical purposes, they performed a controlled experiment to see how their simulated theory worked with 'real' decision makers. Although each method by itself could justifiably be critiqued, when one viewed the study across the two methods the levels of both measurement precision and realism were relatively high. Similarly, Sal March has produced both design science pieces about data modeling in various contexts (e.g., Boulanger and March, 1989) and has studied experimentally which modeling formalism is preferred (Kim and March, 1995). Seeing the phenomena across two methods enlightens understanding far better than seeing it with one method.

This lesson is especially important in fields that have traditionally used multiple methods for understanding various phenomena. This is certainly true of both IS and AIS. Information System researchers have used multiple methods ever since the field's inception, because early IS researchers came from different departments and colleges and were trained in pluralistic ways (Banville and Landry, 1989). The use of multiple methods in IS has continued, which Agarwal and Lucas (2005) argued is one of the strengths of the IS field. The AIS field's recent monograph (Arnold and Sutton, 2002) shows a similar plurality of methods. Each method has its own uses, but we should give researchers credit *a priori* for thinking through their method approaches. In this way, more research questions can be addressed and more evidence about a phenomenon can be added to the literature.

2.3. Lesson #3: Good science never compromises rigor; but neither does it compromise relevance

It is the rigor and argumentation and criticism among scholars that produce valid and reliable results. Taubes quotes Claude Bernard to point out a related problem in medical science: "In medicine, we are often confronted with poorly observed and indefinite facts which form actual obstacles to science, in that men always bring them up, saying: 'it is a fact, it must be accepted'"(2008: 3). Rigor takes us much further. The public merely wants to know the answers. Real scientists, by contrast, "want to know that what they know is really so, "(Taubes, 2008: 449) says Robert K. Merton sociologist of science. Otherwise we have what Taubes characterizes as pseudo-scientists who become part of "an enterprise…that purports to be a science and yet functions like a religion"(2008: 452) by striving to convince people to accept their prescriptions without hard evidence.

One important aspect of rigor is the practice of eliminating plausible alternative explanations (Cook and Campbell, 1979). This holds true especially in social science, in which scientists often work with imperfect or even indeterminate models. Taubes chronicles how medical science has tried to prove that high dietary salt leads to high blood pressure without eliminating alternative explanations (2008:145 ff.). First, Taubes finds studies in this area produce very weak results. Further, the studies tend to focus on salt alone instead of testing whether high intake of refined carbohydrates also causes high blood pressure. Evidence for this alternative cause has been found for over 100 years. Thus, Taubes questions the salt ➔ high blood pressure results because this research did not eliminate the next most plausible alternative. Taubes suggests medical science was not following Karl Popper's advice to use "severe attempts to refute" its own hypotheses (Taubes, 2008: 25).

In the IS field, the TAM predictors have been studied fairly thoroughly in a number of contexts and both TAM and its sisters (TRA/TPB) have used largely the same basic theory and methods. More recently, researchers have considered some innovative alternatives to TAM, such as habit models (Limayem et al., 2007) and theory behind hedonic systems (van der Heijden, 2004). Habit and hedonic variables have proven to be plausible alternatives to TAM variables. However, how TAM, hedonic and habit variables work over time is still not clear. New methods used longitudinally may help scientists ferret out these unresolved issues. New methods often open avenues for eliminating additional plausible alternatives.

I find many in the IS field who express similar concerns about rigor. For example, Avison and Elliot (2006) say the IS field can lose credibility if it performs poorly on academic rigor. IS researchers may, for example, apply concepts from another discipline uncritically, as was done with culture or systems theory. We may use these concepts too simplistically, not applying their full meaning and depth. Ives et al. (2004) suggest that the IS discipline should strive to enhance creativity without sacrificing rigor. Power (2003) suggests that rather than adopting an overriding paradigm, we should foster rigorous research relevant to information systems. Similar calls to rigor are found in AIS, such as that of McCarthy et al. (1992) and David et al. (1999), who also proposed specific standards or guidelines for rigorous research.

However, tradeoffs also exist with rigor. Rigor and relevance are seen as interchangeable (McCarthy, 1987). Relevance is important because AIS, by nature, studies problems related to professional accounting system practice. McGrath's (1981) analysis shows that whenever we value both rigor and relevance we are caught on the horns of a dilemma. Each research approach will emphasize either rigor over relevance or the reverse. One cannot maximize one without reducing what can be done with the other. Therefore, in justifying our methods, our research objectives should be clearly communicated. If we are trying to produce something practice can use immediately, it makes sense to state it. If we are doing lab work that will not be applicable for years, then we should state that. We can explain the tradeoffs between rigor and relevance that take place whenever we choose a particular research method or approach.

We should not characterize this kind of purposeful tradeoff analysis as constituting lack of rigor. Let us be aware that accusations of lack of rigor/relevance may occur because we hold too dear our personal biases favoring either rigor or relevance and apply those biases during the paper review process. Instead, we should be accepting of the lower degree of relevance of our most rigorous work and the lower degree of rigor of our most relevant work—because the purpose for each type of research is different. And each type has its place in the literature.

Since the AIS field is young, it may still be groping for the proper balance. The levels of rigor in specific areas cannot be applied to others. Experimental controls do not easily apply, for example, to field work. Nonetheless, field work has its own set of standards for what is rigorous and what is not. For example, field quasi-experiment validity of various types can be both designed into the methods and tested (Cook and Campbell, 1979). These standards may not apply directly, however, to a qualitative (e.g., anthropological) study (Kirk and Miller, 1986). Instead, the key anthropological standard might be the length of time spent in the field, such as the "six months" living with research subjects that one anthropologist stated as a validity criterion.[2] Hence, AIS researchers should be careful regarding how they apply standards of rigor to various types or styles of research.

3. Conclusion

How do the above arguments relate to progress in the AIS field? And how may this methods issue and similar publications propel the field toward progress? Kuhn points out that periods of paradigmatic "normal

[2] Personal conversation with anthropologist Frank Miller.

science" do not usually move the field towards new or surprising explanations (Kuhn, 1970). Rather, the paradigm can blind us to novel explanations and novel methods, since "believing is seeing." (Banville and Landry, 1989: 51, quoting Weick, 1984). Rather, periods of non-paradigmatic "extraordinary or revolutionary science"(Banville and Landry, 1989: 51, quoting Kuhn, 1970) provide the greatest thrust to a field. And significant thrust is required to break free from the gravity-like pull of a paradigm. In summary, if the AIS field can: a) avoid paradigm blinders, b) refuse methodological biases, and c) accept rigor/relevance tradeoffs as a way to maximize research goals, then it stands to benefit greatly by examining new methods. And thus, this methods issue holds the promise of propelling the field to new heights.

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