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This Paper Must be Rejected for Eleven Good Reasons: A Jekyll and Hyde Approach to Spark an Epistemological, Methodological, Ontological and Axiological Discourse

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Working Paper

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Abstract

This is a short, seemingly superficial, highly recursive, unstructured and confusingly non-scientific essay on epistemology, methodology and related topics that violates many firmly entrenched rules and numerous others that are not so deeply engrained. It therefore has to be rejected if the standard and well-established reviewing processes are applied. It will be shown in an inconclusive and inconsistent way why this inconclusive and inconsistent paper suffers from many shortcomings that will prevent its publication unless editors and reviewers accept the fact that many shortcomings are shortcomings only in the eye of the beholder. At least this paper contains relatively few convoluted statements such as the previous sentence, but instead comprises a carefully drafted list of pending problems and proposed solutions that can be used as starting points for further important discussion, as well as quite a few provocative innuendos. Down the rabbit hole ...

Keywords: Rejection, Review Process, Methodology, Epistemology, Tolerance

--- In order to understand recursion, you must first understand recursion ---

Anonymous

(In Lieu of an Introduction) Reading Manual

This reading manual should be skipped and is presented only for the utterly confused. Admittedly, the most confusing thing about this paper is that actually there is no paper, but only a fictitious dialogue between an author and a reviewer that generates a list of reasons why this paper should be rejected. The entire conversation takes place on a meta-level. The resulting list is supposed to be as exhaustive and nonoverlapping as possible, but I acknowledge that this might not be the case at all times. In this paper, I violate many well-established rules of academic publishing so that any reviewer looking for structural or content-related shortcomings will not need to spend much time finding reasons for rejection. Perhaps shockingly, I am not the least bit apologetic. Any confusion that may arise from the differentiation between content and meta-content in this paper is fully intentional, except for any unintentional mistakes that might be hidden below the surface. Most likely, this paper will be easily comprehensible only by those who have had similar thoughts before (cf. Wittgenstein, 1922). I therefore conclude that it will mostly appeal to those who do not really need it. It is dedicated to all the reviewers who spent their valuable time providing me with helpful and constructive feedback. Particular thanks go to Douglas Hofstadter for his inspiration about how to burn bridges between disciplines and Paul Feyerabend for highlighting the need to challenge existing thought systems. The dialogues and the examples provided are mostly based on feedback I have received in recent years and were modified only slightly for dramatic effect. This should become clearer when reading the paper. Occasionally it won't.

WARNING. The following pages contain disturbing material that may be inappropriate for aspiring researchers. Supervisor discretion is advised.

--- A serious and good philosophical work could be written consisting entirely of jokes ---

Ludwig 'the mad dog' Wittgenstein (1889-1951) as quoted by Dribble (2004, p. 87)

Dear Reviewer,

Thank you for your thoughtful feedback, which was very helpful for sharpening my ideas and improving the paper. In this document, I respond to all your comments in a detailed way. The rest of the paper is structured in three parts in which I will address issues related to (1) formalism and structure, (2) content and theory, and (3) methodology and validity. In order to avoid monotony, I have also included a short enigmatic interlude. In the end, not surprisingly, you will find some conclusions and, more surprisingly, a table in which you just need to tick the respective rows to reject this paper. The table can be conveniently used as a template for further rejections and bypasses other, more elaborate, procedures. The logic of my paper follows Epimenides' paradox. If you agree with the general assessment, you have to reject it and vice versa. Kind of.

Formalism and Structure: Giza or Pisa?

--- If you obey all the rules, you miss all the fun ---

(Katharine Hepburn, 1907-2003)

Reason #1: <No Particular Reason Needed>

Dear Author, after having read the first couple of paragraphs, I have to say that I am shocked by this paper. Apparently you do not understand anything about scientific research. If this absurd paper goes on like that, I would not even be surprised to find a YouTube reference. I do not find any academic value at all in this strange publication.

Dear Reviewer, I am sorry to hear that the value of this paper is unclear. I naively thought that it could serve as a starting point for discussing eleven issues that could potentially benefit the academic community. Furthermore, I believe that we can all benefit by keeping the debate regarding the "tyranny of peer review" process alive (Chua et al., 2018). Building on previous research, I did my best to provide a couple of practical examples and several reasons why the current problems exist. I believe that these topics (and many more) deserve far more attention than they are currently accorded.

Please refrain from making derogatory remarks. Please do not call my approach "shocking" only because I use an approach with which you are obviously not familiar. Ideally, you should not assess any paper that has an epistemological or methodological approach you do not like and/or do not understand in the first place. Santini (2005) fabricated a couple of hilarious reviews illustrating how easily seminal papers, for example from Dijkstra, Codd, Turing, Shannon, Hoare, Rivest, Shamir, and Adelman, could have been rejected. His wording sounds strangely familiar. Be polite: Courtesy provides a strong foundation on which to build. After all, researchers have to do what researchers have to do (or at least what their supervisors want them to do).

Reason #2: Writing Style

Dear Author, I was struck by your weird writing style. You use first person instead of third person, which is not scientific. This a normative way of doing things and it is not acceptable in scientific research. Furthermore, you refer to your personal taste without justifying your statements.

Dear Reviewer, thank you for bringing up this important topic. This is a nice warm-up exercise and our first encounter in this paper with a Swiss army knife argument for rejection, since it works equally well in either direction in a variety of situations. In a nutshell, several plausible arguments exist for the use of first person and I am not aware of any *really* compelling arguments for using third person. For example, in the style guides for CAIS (Communications of the Association for Information Systems) and JAIS (Journal of the Association for Information Systems), LeBrocq (2017) encouraged authors to use first person and active voice because it "clearer and more succinct" (p. 2) and recommended avoiding "awkward phrases such as 'the researchers' and 'the authors'" (p. 2). Sigel (2009) provided several examples and went so far as to write that the "passive voice bogs down the narrative and also indicates that the author has not thoroughly thought through his or her discussion" (p. 479). In light of all this well-justified evidence, and in the absence of plausible counterarguments, how is it possible that my submissions are regularly labeled as being nonscientific, which seems to be the preferred term for many reviewers?

Furthermore, in your review you point out that using first person and active voice is "normative", by which I assume you mean "subjective". This is not at all the case, as using first person should not be confused with subjectivity. Merely expressing that "the paper states" instead of "I state" or, even worse, "it is stated" does not make the research more objective in any way. Anthropomorphism (i.e., the attribution of human characteristics to nonhuman entities) is even something that should be avoided according to APA style. Interestingly, this whole discussion around writing style also seems to be a cultural one. German academic writing, for example, often fosters the proliferation of a complex and convoluted style aimed at signaling intellectual superiority. The heyday of this style was during the time of the Frankfurt school and its critical theory which was developed in the 1930s. Alas, the overly convoluted and unnecessarily complex style of the Frankfurt school does not reflect scholarly superiority. This assessment has nothing to do with the relevance of the content (i.e., critical theory), but simply its presentation. My humble opinion on this issue is shared by Popper, not exactly an easy read himself, who criticized Adorno for his use of language which he labeled "obscure" and "trivial". Interestingly, Adorno later criticized Heidegger for exactly the same reasons (Delacampagne, 2001, p. 175). Adorno was surely a highly intelligent and gifted man, which enabled him to employ a linguistic complexity that made his findings largely incomprehensible for those who most needed them and resulted in alienation from the common people. Compare this, for example, with the style of Albert Einstein: complicated concepts expressed with simple language, rather than the other way around. Using first person vs. third person or active voice vs. passive voice belongs to the more negligible problems that exist in academia, but they nicely illustrates how easy it is to label a paper as "non-scientific" without any need to provide compelling evidence. This might also work the other way around (i.e., labeling passive voice or third person as non-scientific), but so far I have not experienced that.

Reason #3: Rules and Principles

Dear Author, I noticed that you purposefully wrote this paper without a proper structure. This greatly reduces the value of this already mediocre publication. The scientific community has established guidelines about how a paper should be presented. I advise you to study the IMRAD (Introduction, *Methods, Results and Discussion) structure and carefully follow the guidelines to make your paper potentially acceptable in a D-journal. Treiblmaier* (2018) *got it all wrong. We need more rules and regulations! Your neglect of commonly accepted rules is even more shocking since I have a feeling that you do have a rudimentary understanding of appropriate scientific structure and that you are doing this on purpose.*

Dear Reviewer, a common doctrine in strategic management holds that the strategy should be developed first and the organization should subsequently be adapted accordingly ("structure follows strategy") (Chandler, 2013). Interestingly, in academic research this logic much too often is upside down. Detailed guidelines and checklists such as IMRAD or PRISMA (Preferred Reporting Items for Systematic Reviews and Meta-Analyses) exist about how to structure publications. Lowry (2018), for example, provided a publication template and concluded that "an academic article will generally have the following structure, from which you should almost never depart" (p. 2). Undoubtedly, formats such as this are useful as initial checklists for aspiring researchers, but at the same time they force authors into an intellectual corset from which they might never be able to escape. Even worse, as reviewers they will force their thinking upon others, similar to academic journals that force the IMRAD checklist upon reviewers and by default turn down submissions that do not follow this structure. I remember once having a paper rejected for lack of theory. Alas, it was a purely methodological paper. The reviewer did not even bother to look at the content.

A short glance back into history might help you to recognize that authors in former times used different styles to impart their knowledge. For example, many seminal works were structured as dialogues. These works include Plato's strange "Parmenides" and the famous allegorical dialogue on the foundation of logic from Lewis Carroll, the mathematician most famous for authoring *Alice in Wonderland* (Carroll, 1995). Arguments are *developed* and *not presented* by Galileo Galilei, Imre Lakatos, Friedrich Nietzsche, and Douglas Hofstadter, to name a few. In theory, it should not be the case that "the guidelines become more important than the study" (Holtkamp et al., 2019, p. 6280) or that the paper is "written according to a 'formula'" (Avison et al., 2017, p. 271). In practice, it is. The structure is given and we have to fit the content in, leading to a strange Taylorism pertaining to the way in which academic accounts of research are represented.

Many reviewers seem to be genuinely concerned to ensure that researchers do not get lost in the largely uncharted territory of academic creativity. Only the chosen few are allowed to enter this realm, and access is especially forbidden for the lower ranks. Creativity has become the law of the outlaws and is often confined to lower-tier journals. Again, rules, guidelines, and principles are not intrinsically bad. On the contrary, they provide guidance and structure and help to make research incremental and comparable. It is simply their mindless application that causes substantial damage. I therefore suggest a simple two-step approach: Learn the rules as thoroughly as possible, but if there is a *compelling* reason not to follow them, then don't. In summary, the fundamental question is whether the shared (?) understanding, something that has evolved over decades, provides the strong foundation of a pyramid or, instead, represents a lopsided tower threatening to topple. The warning signs are already there (Hirschheim, 2019).

Reason #4: Rigor and Relevance

Dear Author, your paper is not rigorous and does not qualify as a sound academic publication. This also makes it hard to replicate.

Alternatively: Dear Author, your paper is not relevant and does not qualify as a sound academic publication. Your findings are severely limited by your reliance on personal experience.

Dear reviewer, with all due respect, rigor and relevance are continuous rather than dichotomous properties and it isn't only beauty that lies in the eye of the beholder. This idea might be easier to grasp for relevance, which I will come back to, but is also true for the methodologies commonly used in IS research, which are not always as rigorous as they first seem (Treiblmaier, 2019). Although a certain lack of rigor might be easier to attribute to qualitative methods, it is equally true for quantitative procedures. In terms of relevance, it might not even be possible to decide objectively whether a specific topic is relevant. Any academic community is shaped by inherent belief systems regarding what is worthy of investigation. This assessment can only be done from the outside, for example by industry. This is not a new discussion in information systems research, and already more than a decade ago Rosemann and Vessey (2008) proposed principles and criteria to ensure that IS research findings are applicable to practitioners. A more recent suggestion to apply intervention effect rates as a path to research relevance came from Siponen and Baskerville (2018), who argue that the endless cycle of proposing theories and empirically validating them does not foster our understanding regarding the practical value of these theories.

All in all, the objective assessment of relevance is an illusion. What is relevant for you might not be relevant for me because this assessment depends on the conditions in which we currently find ourselves. One particular kind of medicine might be life-saving for those suffering from a particular ailment, yet regarded as unimportant by those who are not. On a very general level, this notion even extends to the discussion of whether information systems is a science (McBride, 2018).

Author's comment: I've found a remarkable proof of this fact, but there is not enough space in the margin of this paper to provide it.

It is not the finding that ear lobe asymmetry is an indicator for leadership potential, as published in Harvard Business Review (Senior et al., 2011), or the revelation that female hurricanes are deadlier than male ones as published in the Proceedings of the National Academy of Sciences of the United States of America (Jung et al., 2014) that makes me skeptical. Rather, it is the use of relatively small sample sizes in combination with traditional statistics that leaves a displeasing aftertaste. Not surprisingly, especially the latter story generated a great deal of attention, was heavily criticized, and is now more or less debunked (Samenow, 2014). Potential flaws in research design aside, it is the concept of hypothesis and theory testing based on *p*-values that creates a false aura of rigor. Depending on the underlying probability of a true effect, a *p* value of 0.01 has a false-alarm probability of at least 11% and a *p* value of 0.05 of 29% (Nuzzo, 2014). Enough said. I do not argue that all submitted articles should be published, irrespective of content and method. As far as content is concerned, it might make sense to spread the word, for example, that endless replication studies or only minor model modifications might not be worthy of publication. When it comes to methodology, more detailed submission guidelines might also help because it is bitter receiving a desk rejection saying "I do not like SEM", especially when it comes from the editor of a journal that frequently publishes articles in which that particular method is applied. A certain level of stochasticity seems to reside not only in games of dice.

What's in a Paper? – When Worlds Collide

--- It is the theory that decides what we can observe ---

(Albert Einstein, 1879-1955)

Reason #5: Lack of theory

Dear Author, for publications I require a strong theoretical contribution. Anchoring this research on previous theories could be a solution to provide an interesting and novel contribution to theory or a challenge to existing theory. In its current form there is no theory and therefore your article makes no contribution.

Dear Reviewer, the discussion about whether theory has become a "fetish" (Avison & Malaurent, 2014) in IS research is a long one, with countless arguments for and against it, and I will not reiterate it here. As far as I am concerned, the much more important question is how useful our notion of theory is in the first place to achieve what it is that we want to achieve (whatever that might be). It might be useful to start with a definition: In the positivistic received view, scientific theories are "axiomatic calculi where theoretical terms were given a partial semantic interpretation via correspondence rules connecting them to observation statements" (Suppe, 2000, p. 102). Suppe claimed that this view died on March 26, 1969, and further elaborated: "Today much of science is atheoretical, as it was then. … Today, models are the main vehicle of scientific knowledge" (Suppe, 2000, p. 109). This perspective has not received much attention in the IS community. An alternative definition states that "a theory may be viewed as a system of constructs and variables in which the constructs are related to each other by propositions and the variables are related to each other by hypotheses" (Bacharach, 1989, p. 498). The main idea of hypotheses is that they can be tested and rejected. If we take a look at the 100+ theories being used in IS research (Larsen & Eargle, 2015), how many of those are actually testable? How many have been conclusively rejected over the past 5 decades? Isn't that the purpose of scholarly research if we follow the strict theory development and testing paradigm?

The reality, however, is quite different. The vast majority of theories, (more specifically, what we label as theory) do not lend themselves to straightforward testing. Several include normative components; others are qualitative descriptions of certain types of phenomena; and many are simply too complex to be tested or depend on countless contingencies. Not surprisingly, many of those so-called theories that can be easily tested (we might as well call them models) have become fairly popular because minor model modifications and their application in different geographical regions or with different populations create endless possibilities to produce articles that satisfy editors and reviewers scanning every submission for the occurrence of the term "theory". Similarly, another popular notion is that of "theoretical lens" about which Niederman and March (2019, p. 17) conclude: "We found no evidence that any scholars have defined the term theoretical lens in any formal way." In conclusion, the best solution might be to allow for both kinds of research. Hooray for theory articles, hooray for nontheory articles. Indeed, there is one predicament that should be avoided at all costs: desk rejecting papers for not using theory (not using the label "theory"?) without giving the authors a fair warning. If you insist that we incorporate theory, please make this clear prior to submission.

As a sidenote: Similar to the previous section, the *p* value plays a major role because it is one of the major vehicles for hypothesis and theory testing, especially for those with a rigor fetish. I have briefly elaborated on that in the previous section, but I just want to add that Cohen (1994)—famous for his work on statistical power and effect size—noted that he resisted the temptation to call the testing procedure "statistical hypothesis inference testing", indicating that this would have yielded a better acronym than NHST (null hypothesis statistical testing). Admittedly, commonly accepted empirical procedures for theory testing as a whole do not even involve *p* values, which is mostly due to the fact that they are unlikely to yield significant outcomes when the complexity of the model increases. An established workaround is the application of fit statistics, which are heuristics rather than statistical tests: So much for rigor. And I have not even touched upon relevance in this section.

Reason #6: Lack of Explanation

Dear Author, this paper is nothing but a long rant. Academic papers should have a clear purpose and strive to find the underlying rationale instead of only complaining. Even if the problems you raise really exist, you do not provide any useful explanation about why this should be the case.

Dear Reviewer, I apologize for not having discussed the root cause of the problem more thoroughly. Many papers have been published that criticize the review process, but, in general, there seems to be some dispute about what has caused the current situation. I label the current situation as the "toxic square". Chua et al. (2018) provide two corners: (1) the typical reviewer tends to be junior faculty or a PhD student and (2) appropriate incentive mechanisms are lacking. I add two more corners. Corner 3 represents the inherent drive of editors to keep the acceptance rate low in order to signal superiority. This potentially yields very weird practices including the routine rejection of every paper in the first round, the cancelation of special issues for lack of sufficient submissions to reject, and, finally, the purposeful invitation of submissions from junior faculty that can be easily rejected. I have witnessed all of them. Corner 4 is the amazing ability of our tools (i.e., the methods, which at times might even include logical reasoning or ignorance) to reject any paper. I repeat: *any* paper. Anyone disagreeing just needs to read the brilliant paper from Santini (2005) in which he illustrated how seminal papers can be rejected with arguments that sound surprisingly logical. If this can be done with the work of geniuses, how much easier can it applied to us mere mortals? Interestingly, at times challenging game theoretic situations arise in which the rejection of someone else's paper increases one's own likelihood of getting published (e.g., when authors of a special issue also act as reviewers). In this case, the square turns into a pentagon that incorporates the extrinsic motivation of the reviewer. And this does not even take into account that in a substantial number of cases the so-called double-blind peer review process is a myth because the reviewers know the authors' identities or can make a reasonably accurate guess (Yankauer, 1991).

A more in-depth analysis of the current situation might yield several root causes. One interesting route that deserves further investigation is the so-called "Methodenstreit" (methodological dispute), which produced conflicting positions favoring either a natural or humanities / social sciences approach. This dispute left its footprint in several fields and in the 1960s yielded the "Positivismusstreit" (positivism dispute) and, in a very gross simplification, led to the emergence of quantitative and qualitative schools, each of which contain many fine-grained differentiations.

Quant- itative research relies on numbers and sophisticated methods. Empirical measurement as well as statistical and mathematical techniques are applied to attain	Qual- itative research includes a wide range of approaches that do not rely on numerical measurements. In general the number of cases is low. The final goal is

inference, which designates a process
whereby a conclusion is drawn
without certainty, but
with some degree
of probability
(Lavrakas,
2008).

Figure 1. The Inference Trapdoor
(aka Epistemological Bird of Prey (*Aquila chrysaetos refutans*))

Figure 1 juxtaposes quantitative and qualitative research. I label it a trapdoor because, similar to eponymous cryptographic procedures, it is a one-way street. You can read it top-down starting from the left or right, but, if you start from the bottom (i.e., inference) and want to make your way up, it is not clear whether you will end up on the left (quantitative) or right (qualitative) side. Either way is fine. According to (King et al., 1994, p. 4) the "differences between the quantitative and qualitative tradition are only stylistic and are methodologically and substantively unimportant". Still, both traditions have their own tools and mindsets that suffice to reject each other's work. Needless to say, within the respective traditions several subgroups exist that operate in similar ways. Finally, the dominating power structures, in the case of IS research best exemplified by the standards of MIS Quarterly, actually determine what the truth is (Introna & Whittaker, 2004) and thus (implicitly) determine the dominant paradigm.

Reason #7: Missing Implications and Practical Advice

Dear Author, this paper is at times way too philosophical. While there might be some value for it in behavioral research (I am a design scientist at heart), epistemological discussions should be left for the philosophers. This has absolutely no value for industry. We are a practical discipline, not a philosophers' club.

Dear Reviewer, in this matter I can only provide a couple of personal recommendations. Initially, for aspiring IS PhD students, I suggest they dive deeply into methods, rules, and principles. Read textbooks on statistics, econometrics, and modeling. Put a book on grounded theory under your pillow and indulge in case studies. Develop a passion for design science and requirements engineering. When you are done with all of that it is time to take a deep breath. Afterwards read *Marienthal: The Sociography of an Unemployed Community* by Jahoda, Lazarsfeld, and Zeisel (1971), originally published in 1933. In this work, the authors report on the effect of long-term unemployment and use a wild mix of quantitative and qualitative data as well as what might be called action research. This ingenious masterpiece would not stand any chance against a picky researcher who has done his/her homework and mindlessly applies paradigms, principles, rules, etc. It violates basically every rule that can be found in structured reviewing manuals and yet it is perceived as a hallmark of modern sociology. The Marienthal study may not be rigorous in a modern sense, but who would doubt its relevance? It frees your mind from the assumption that ingenious research has to follow strict rules all the time. The reason I pick this particular study is that Paul Lazarsfeld later became president of the American Sociological Association and was proficient in statistics (latent structure analysis). His credentials therefore place him above suspicion of any restriction to only one single type of epistemology. He was simply flexible enough to choose the methodological approach he deemed to be most relevant for the work at hand.

Figure 2 summarizes the practical advice you were asking for in a layered structure, which should appeal to all rule aficionados out there. Starting from the bottom, I consider the default mindset of "accept", rather than "reject", to be the only appropriate one. This might be somewhat difficult to apply for journals that strive to have acceptance rates in the single digits. As I have outlined previously, courtesy should be a matter of course. As a rule of thumb, review a paper if and only if you fully understand it. If you do not understand parts of it (e.g., the methodology) refrain from making decisive statements, and especially from making rejections. Acknowledge your limitations. Apply all rules and regulations as long as they make sense. Most importantly, if you really have to reject a paper, I suggest following what I call a CJC approach: (1) criticize what is wrong, (2) justify your decision (e.g., by providing references), and (3) suggest a correct approach. More often than not, it is only the criticizing that takes place. Finally, I label the layer to end all layers as academic nirvana. At this stage, you have freed yourself from any guidelines and are open to evaluate any publication as it is, independent of default mindsets as well as epistemological and methodological foundations. Only the chosen few will ever reach it.

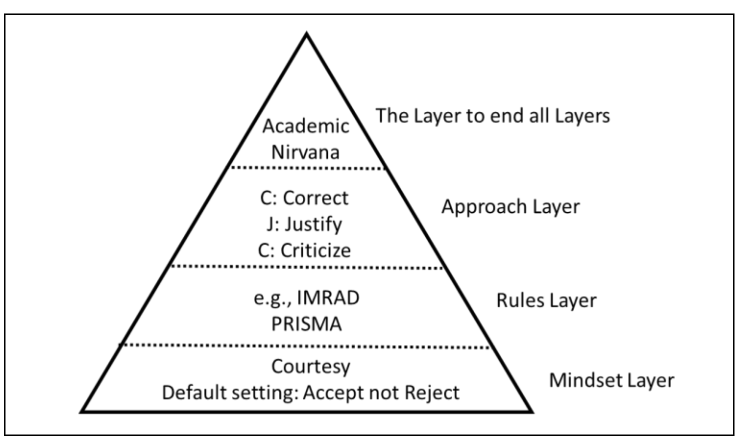

Figure 2. Toward Academic Nirvana

I am not a huge fan of endless enumerations and suggest that we take a break now:

--- The Interlude: Chinese Whispers or the Mutilation of Ideas ---

An elongated room. Just a tiny blue light at the end of the tunnel which is slowly getting closer. The reviewer (R) enters wearing a purple robe that bears the letters PG-PhD. The author (A), who sits comfortably in an ejection seat in front of a table covered with a white tablecloth (cf. Figure 4), stops reading Hofstadter's GEB (twentieth anniversary edition 1999)*, inserts a bookmark on page 152, and looks up to the reviewer.*

R (*makes a note: "reason for rejecting #12: Introducing a dialogue in an academic paper"*): This form of dialogue is stupid. You just give me one more reason to reject this paper. We have well-established procedures about how a paper has to be structured. You are turning a nice asynchronous communication into a fake synchronous one.

A: I know. But let's consider this format for a moment. If we want to *develop* an argument rather than *present* it, a dialogue might be superior to the traditional form of presentation. Stepwise. The human brain is not very good at absorbing and memorizing abstract information anyway.

R: So what's your point in this plaintive rant? You say it's all bad?

A: No, it's all good (Stafford, 2018). We have a foundation, but it might not be as solid as we believe (Siponen & Tsohou, 2018). We still need to investigate many topics critically, and that includes our intellectual foundations. After all, that is what we are getting paid for and not for the sheer number of publications we produce. It is only openness and relativism that will help us, which does not mean that there should be no rigor at all.

R: But only the number of rigorous publications in top-tier journals promises salvation, pronounced as tenure.

A: I know. Publish-or-perish. Kock (1999) documented the consequences.

R: Still we need an objective measure for performance. Thank goodness we have learned how to develop and test theories rigorously, and how to evaluate them. As Popper has said …

A: Popper is heavily misunderstood. It is all out of context (Treiblmaier, 2019).

R: What do you mean?

A (*kicks away a black swan*): Testing hypotheses and theories does not work the way many researchers believe. For example, a systematic bias exists that is caused by false positives and the overestimation of the effect sizes of true positives (Camerer et al., 2018).

Black Swan (*is afraid of being falsified and flies away*): Kraaaaal!

A: Anyway. My theory, which I outline here …

R: To me it sounds more like a framework.

A: Model?

R: Proposition!

A: Hypothesis?

R: Stop it!

A: ἓν οἶδα, ὅτι οὐδὲν οἶδα.

R: Now again what is that supposed to mean? It sounds all Greek to me.

A: Exactly. It *is* Greek. It is attributed to Socrates, who never wrote down anything himself. It literally means "I know that I know nothing", but this could also have been a problem of translation and back-translation. In this translated version it would be a paradox. Another version is οἶδα οὐκ εἰδώς, which means that I am not capable of knowing (for sure?), which has a completely different epistemological meaning. No one can tell for sure.

R: So it is unreflective adoption that bothers you?

A: Now you get it. We need to acknowledge the inherent contradictions of our domain. It is like Russell's barber is using Occam's razor.

R (*looks confused*): This statement sounds so … true.

Epimenides (*passes by with a blue light torch in his hand and looks disappointed*): Darn, you just ruined a pretty good meta-joke.

A (*nods*): Do you know why I mentioned axiology etc. in the title?

R: To show off?

A: Partly yes. But more importantly, I agree with others, (e.g., Niederman, 2018) in that I believe that we have many skeletons in the cellar. Hopefully we can get rid of them. Many of the answers are already out there.

R: Any examples for that?

A: Many. Take, for example, a seemingly simple concept such as significance testing, which is the prerequisite of most theory testing.

R: This is surely important, right? Many consider it to be the foundation of our discipline. You first develop theories, and then you test them.

A: Sure. But this procedure contains countless limitations. You might even call them flaws.

R: Huh?

A: It can all be traced back to the creation of a hybrid system that forced Fisher's *p* value into Neyman and Pearson's rigorous rule-based system. This was not done by the researchers themselves, who were bitter rivals, but by less knowledgeable authors of statistics manuals (Nuzzo, 2014).

R: So you are saying that…?

A: Erroneously significant results are far more likely than you might believe. So much for the rigor of theory testing (and also theory creation). Ioannidis (2005, p. 696) wrote that "It can be proven that most claimed research findings are false" and he provided some compelling reasons why this is the case.

R (*scratches his symmetric ear lobes*): Bummer. I understand. That means that our rigorous neighboring disciplines such as economics, or fields such as production planning, are clearly superior because they are built on solid mathematics.

A: Not necessarily. We are talking about assumptions. Deterministic modeling is based on assumptions, and many contingencies are left out of the equation. Sometimes even deterministic systems are not predictable, which is known as deterministic chaos (Radzicki, 1990).

R: Holy cow. Then the only rigor that is left is pure mathematics.

A: Wait a second. For the most part you are right. At the beginning of the 20th century the mathematician David Hilbert wanted to build a rock-solid mathematical foundation. Whitehead and Russell wrote *Principia Mathematica*. But Gödel destroyed their hopes with the two incompleteness theorems.

R: That must have been frustrating …

A: It sure was. And don't forget that every logical system is based on the logic of the underlying axiomatic system. One drop of water plus 1 drop of water equals 1 drop of water: 1+1 =1 (Curry, 2012).

R: Finally, the YouTube source. I could see that coming. Now you are overdoing it.

A: I apologize, but sometimes the importance of the statement trumps the reputation of the source. Besides it is an MIT video posted on YouTube. Obviously, there is no need for us to change the axiomatic foundation of the underlying mathematics, but it shows that it would be interesting to investigate our foundation more closely. For example, if your personal axiom is that a paper without theory is worthless, you can at least warn potential contributors prior to submission.

R: But this is not specific to IS research.

A: Definitely. It is not the tools I am talking about. It is the mindset—the inherent belief system.

R: Finally I understand. I need a glass of red wine now. But why do I always have to be the stupid one in this dialogue?

A: You are not. You are asking the right questions. You are the smart one. *Takes the purple robe from the reviewer, swings it around, presses the button and disappears in a constructivist manner by entering the multiverse* (Tegmark, 2014)*, leaving the baffled positivist reviewer behind. Epimenides, who has accidentally set the tablecloth on fire with his torch, has already left to get a shave.*

--- End of Interlude ---

The Medium is the Message is the Method is a Monster

--- There is always a well-known solution to every human problem — neat, plausible, and wrong ---

(H.L. Mencken, 1880-1956)

Reason #8: Imperfections

Dear Author, (~~after a long search~~) I (~~finally~~) found a flaw in your paper that definitely prohibits publication …

Dear Reviewer, your quest for perfection much too often causes more harm than it does good. Undoubtedly, major shortcomings need to be corrected, and, in case this is not possible, submissions have to be rejected. However, the pressure for rejection has led to a practice in which the desperate search for shortcomings often becomes the reviewer's first and foremost goal. Trust me, you will always find a flaw, be it a real one or rather a matter of opinion. The perception of perfectionism in general has changed quite a bit over the years, and the ancient Greeks' ideas on this matter differed substantially from those that followed with Christianity. In the middle ages, God was the ultimate reference and thus the only entity that could reach perfection (Bloomfield, 1957). In modern times, this view has obviously changed and in some fields imperfection is not only acknowledged but also appreciated. For example, in the arts minor imperfections are incorporated either on purpose (Navajo rug, see Figure 3 on the left) or constitute an art form in themselves. Kintsugi (middle) is the Japanese art of repairing broken or cracked pottery using gold or silver. Similarly, Wabi-sabi (right) is a Japanese aesthetic world view that cherishes imperfection. It is only in research and science that we strive for ultimate perfection. Or do we?

Figure 3. Imperfections with a Meaning
(Haragayato, 2016; Regents of the University of Michigan, 2016; Riimaitee, 2017)

Max Delbrück, winner of the 1969 Nobel Prize for Physiology or Medicine, introduced the phrase "principle of limited sloppiness". This does not imply that research should be done carelessly, but rather indicates that our conceptual understandings of systems are always a bit muddy and that unexpected results can often emerge (Grinnell, 2009). If this holds for the natural sciences, how much more relevant might it be for IS research? Summarizing the interplay between science and the arts, Kenny (2017, p. 8) concluded that "imperfect doesn't mean 'bad,' and it doesn't mean 'wrong.' It is simply the way things are. This is life."

The meticulous search for shortcomings might become even more dangerous when combined with a conflicting worldview or limited knowledge, such as unfamiliarity with a certain methodology. This creates endless opportunities for rejection. There is even a name for the latter—the Dunning-Kruger effect—which describes how people with limited ability in a particular subject tend to overestimate their own abilities (Kruger & Dunning, 1999) (BTW: I am aware that in this paper the authors rely on NHST, but this does not *necessarily* mean that their findings are wrong). I consider their paper, aptly titled *Unskilled and Unaware of It*, and perhaps unfairly awarded with the Ig Nobel award in 2001, to be a very important contribution that should humble us all because no one is exempted. Quite obviously, the Dunning-Kruger effect is particularly alarming for those operating heavy machinery, conducting brain surgery, or becoming president of a nuclear power nation. In such cases confidence might *trump* competence. In a regular review process, this effect can be largely avoided if reviewers follow the "approach layer", which I describe in Figure 2. By not only criticizing, but also justifying, their decision and correcting the criticized approach, they are forced to expose their own understanding of a particular subject. In summary, the question remains whether the quest for perfection leads to better research or simply to more irrelevant research. What the heck is perfection in the first place?

Reason #9: Poor Methodology

Dear Author, with all due respect, Jekyll and Hyde is not a method. I have never seen anything like it. Our community embraces a repository of well-accepted and tested methods that have been used for decades and all of a sudden you come up with something completely new.

Dear Reviewer, about 3.3 million years ago the first stone tools for facilitating tasks were used by predecessors of the genus Homo in Kenya (Harmand et al., 2015). Luckily for us, they never had to undergo a rigorous peer review process that, most likely, would have revealed that the sharpening of stones was an untested procedure that did not follow the standards and principles prevailing in the savannas of Eastern Africa. All in all, my personal experience with so-called blue ocean strategies (Straub, 2009), namely novel and potentially breakthrough ideas, is more than frustrating (cf. Niederman, 2018). I once had a paper rejected with the unbeatable argument that the mere fact that a particular method has not previously been used in IS research indicates that it has no utility. It is statements such as this that make even the most venturesome methodological explorer return to the safe haven of technology adoption research.

In 1964, Canadian philosopher, educator, and communications theorist, McLuhan, introduced the phrase "the medium is the message", indicating that the medium (e.g., television, newspaper) is more important than the actual content (McLuhan, 1964). In the case of academic research, the medium becomes the method or the theory. Much too often the perceived complexity of methods tends to obscure their usefulness and the mere presence of the term *theory* lifts the paper to the next level. This often marginalizes considerations regarding the true power of methods, as I outline in other sections of this paper. I am aware that I would have sounded far more academic if I had used, for example, "Hegelian discourse" rather than "Jekyll and Hyde". Interestingly however, the latter term has previously been used in academic papers, for example in economic experiments (Bissey et al., 2010). Labels aside, more than a quarter of a century ago, Pinsonneault and Kraemer (1993) identified five weaknesses of survey-based research in IS. A replication of this study might be advisable to see whether those issues have been addressed satisfactorily in the meantime. We do have a repository of accepted methods, but it might be a good idea to question whether that repository is large enough and sufficiently understood. As a conclusion for the review process, I suggest that if we put on our epistemological armor we should at least leave the methodological visor open.

Reason #10: Lack of Validity

Dear Author, your paper is not representative. You are just expressing your personal opinions. In order to be of any value for the academic community, I suggest that you replicate your study with a sample of 250 participants (undergraduate students do not count, irrespective or your research design). Doing it in this specific way, your findings can be validated and become much more useful.

Dear Reviewer, in this paper I present not only my personal views, but also an accumulation of shared opinions arising from talks with many other faculty members. Admittedly, the data-gathering process was shockingly subjective and subject to my own interpretation. Thank you for mentioning validity in the first place. I believe that, like most other types of validity, external validity is poorly understood. It is deceptively simple to reject or bash existing research on the basis that the results are not generalizable. All too often assessment of a paper is reduced to a couple of easy-to-remember yet dead wrong principles, such as "student samples are always bad" or "a high response rate indicates generalizability". In a short yet highly informative editorial note, Blair and Zinkhan (2006) list the three paths to academic generalization: theoretical generalization, probabilistic generalization (i.e., through samples), and empirical generalization (i.e., through replication). Most often it is only the second path that is considered, and is nevertheless also frequently misunderstood. External validity is a standard tool for rejecting empirical papers, be they qualitative, quantitative, or even design science. However, many other types of validity exist (Boudreau et al., 2001), all of which might be somehow related.

For illustration purposes I suggest the following simple experiment. Choose a tablecloth, preferably one made from expensive silk that forms various pleats (cf. Figure 4). Use a waterproof pen to label the respective pleats as different types of validity: external, internal, conclusion (AKA statistical validity), and construct. Depending on your personal preferences you can split up, for example, construct validity into discriminant and convergent validity, or include more obscure and often overlooked or underestimated types such as face validity, test validity, diagnostic validity etc. I suggest placing internal and external validity on opposite sides, but depending on your belief system, the angle is up to you (Jiménez-Buedo & Miller, 2010). What does the angle represent anyway? Next, select the most precious china from your wall cupboard, fill it with red wine (any ordinary bottle of Domaine Leroy Richebourg Grand Cru 1949 will do), use the same waterproof pen to label the china as "structure" and carefully place it on the table. Finally, grab one side of the cloth (i.e., a particular type of validity) and quickly apply more weight to it. What happens to the weight of consideration afforded to the types of validity on the other side of the cloth? Moreover, what happens to the precious china (i.e., the structure) and the red wine (i.e., the contents)? This simple experiment not only allows you to better understand the intricacies of validity, but also to share the excitement of empirical research with your significant other. The conclusion is that you can *always* reject an empirical paper on the grounds of a particular type of validity. Yes, always. If it is seemingly perfect in one aspect, a different perspective will inevitably suffer. It is simply the case that, for some obscure reason, some violations seem to be more acceptable than others.

Figure 4. The Metaphorical Tablecloth (Trimming Shop, 2019)

Reason #11: Wrong Causality

Dear Author, *the validity experiment you describe in this paper was eye-opening and quite useful to better understand the surprising interdependencies of validity. My divorce lawyer is interested in further details and will follow up soon. However, this will not save you from getting rejected. Finally, I found another major issue. The causality you propose is all wrong. There is a paper from John Doe published in the* Journal of Journals *showing that reality actually works the other way round.*

Dear Reviewer, the previous publication of a result does not necessarily make it sacrosanct, no matter where it was published. This especially pertains to the complex issue of causality. An in-depth discussion needs to start with ancient Greek philosophers and must include numerous philosophical perspectives. In a recent publication, Markus and Rowe (2018) discussed this topic thoroughly from an IS perspective and suggested a three-dimensional framework that combines causal ontology, causal trajectory, and causal autonomy. In a nutshell, causality is a much too complicated subject to be addressed by a simple cross-sectional examination. When in doubt, logical reasoning should be regarded as superior to prior publications, especially when those publications do not provide any reasoning.

(In Lieu of a Conclusion) Infamous Last Words

Reviewer's decision: This paper is a kind of intellectual sucker punch. It is too hard for me to reach a decision. I have to hand this over to other reviewers who are more knowledgeable and tolerant than I am. Finally I must admit that at least occasionally I had a good laugh. The author raises a lot of issues, some of which seem to be quite provocative and might be the basis for further discussion.

Dear Reviewer, thank you for taking the time to read my paper. To sum up, in business-related courses we emphasize principles of effectiveness and efficiency. That is what we teach and preach. The academic review system celebrates the opposite: High rejection rates serve as a proof of quality, with the underlying assumption that the review process is objective despite much evidence to the contrary. In their study focusing on biological research, Neff and Olden (2006) showed that the "review process can include a strong 'lottery' component, independent of editor and referee integrity" (p. 333). On top of that, this highly complex system is not even capable of delivering the results it is supposed to (Ioannidis, 2005). More often than not, this leads to a frustrating cycle of rejection and resubmission, potentially spanning several years. Academic papers are not like red wine: Unpublished, they do not age with dignity. All too often, the review process is not a feedback mechanism, but rather a filter mechanism in which reviewers try hard to find reasons for rejection. This especially holds true for so-called top journals, where the pressure to fulfill a quota is particularly high. Table 1 summarizes the topics discussed in this paper and provides a checkbox that can conveniently be used to reject it.

Table 1. The Exemplary Rejection Table

No.	Reason	Sample statement	Checkbox
1	No particular reason needed	This paper does not provide any specific value	□
2	Writing style	The use of active voice is non-scientific The use of first person is non-scientific Alternatively: The use of passive voice is non-scientific	□
3	Rules and principles	This paper does not follow the IMRAD* structure	□
4	Rigor and relevance	This theory does not have any relevance Alternatively: This paper does not have any rigor	□
5	Lack of theory	This paper does not develop/improve/test theory	□
6	Lack of explanation	This paper does not provide reasoning for the underlying phenomena	□
7	Missing implications and practical advice	Implications for academics are missing Alternatively: Implications for practitioners are missing	□
8	Imperfections	I found a flaw on page XXX, line XXX, which does not correspond to the paper from XXX, published in the journal XXX in the year XXX	□
9	Poor methodology	For example: I do not like structural equation modeling (SEM) More specific: I do not like partial least squares for SEM Alternatively and more general: This method has not been used before	□
10	Lack of validity	The results of this paper are not generalizable**	□
11	Wrong causality	The model is misspecified	□

* Placeholder: you can include any rule set you like.
** Example shown for external validity only; further statements are available for all types of validity from the author upon request.

I want to end this paper with a huge THANK YOU to all of you who have dedicated your valuable time providing me with advice that was useful for improving and fine-tuning my publications. Keep on doing your excellent work. You know who you are. Einstein hated the peer review process (Spicer & Roulet, 2014), yet, although many may agree with this sentiment, the scarcity of Einsteins means that we certainly need a process to filter out poor papers. However, the way in which this is done has become dysfunctional and at times patently absurd. Many of these topics are intellectual dynamite for spontaneous enlightenment. Anyone got a match?—QED.

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