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Communications of the Association for Information Systems

CAIS 

Higher Value of Research by Promoting Value for Researchers

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Abstract:

In this contribution to the debate started by Nik Hassan's article a personal account of the "crisis of value" is given. It comprises an analysis of what might be seen as a crisis and some of its possible causes. Finally, it provides some idealistic suggestions to address the discomfort that Nik Hassan probably shares with many others.

Keywords: information systems (IS) research, IS theory, intellectual ideals, anxiety discourse, disciplinarity

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I. INTRODUCTION

In his initial contribution to this debate, Nik Hassan expresses concerns about the value of Information Systems (IS) research [Hassan 2014]. He considers various facets of value: the epistemological value of research results, the practical value of our research and the relative value of IS compared to other disciplines. A closer look at his contribution shows that he is not only concerned about the value of IS research. He is concerned about the social construction of success and value. He is concerned about the significance of the problems that are addressed by research in IS, and is even concerned whether we are doing the right thing. I agree that there are grounds to be concerned about the state of the IS discipline, its current patterns of research, its evaluation and its dissemination; there may even be reasons to be frustrated. However, to clarify my position right at the start, I think there is also reason to be glad and grateful to work in this field. My contribution to the debate is not intended as a direct response to Nik Hassan's paper. Instead, I take his considerations as an inspiration to give a personal account of the current situation and some of its possible causes. Only in concluding the account will I present a few suggestions that address some of Hassan's concerns.

II. SYMPTOMS

Nik Hassan has already pointed out various symptoms of the crisis. I will add a few more to the list; or look at them from a slightly different angle. While the identification of these symptoms reflects my own perception and assessment, I would assume that there are others who share a similar view.

Opportunistic research: There seems to be a widespread assumption that the primary purpose of research is to get published. While publishing new ideas is certainly important, it is a problem if it suppresses an intrinsic motivation to conduct research. External incentives may create unwanted effects, such as encouraging the selection of research topics on the grounds of their suitability for certain publication outlets, and furthermore, may contribute to research being perceived as a burden.

Emphasis on performance indicators: Publication records and citation indices may be perceived as a superficial and inappropriate instrument with which to assess academic performance. At the same time they promote a feeling of helplessness as it seems that they cannot be ignored when building a career, nor is it obvious how they could be effectively replaced by other approaches.

Feeling of inferiority: In the IS literature, there is a plethora of statements – Hassan cited only a few – that indicate a lack of self-esteem permeating the discipline. On the one hand, they relate to the lack of recognition in neighboring disciplines, and on the other hand, they relate to doubts about the epistemological value of research results, e.g. [Lyytinen and King 2004]. The persistent concerns about the gap between theory and practice are one facet of this discussion. I do not think these concerns are actually about a linguistic gap. There is undoubtedly a huge gap between theoretical physics and its praxis, but in contrast to physics, IS does not enjoy an outstanding reputation among the general public.

Tense handling of research methods: It is an iron law of PhD programmes that good research has to make explicit use of a research method. The clear dominance of behaviorist methods has been somewhat qualified by the modest growth of hermeneutic or "Design Science" approaches. Nevertheless, it seems mandatory to opt for a certain *given* method. While this may be appreciated as a helpful guideline by some, others may regard it as akin to paternalism that jeopardizes their perception of academic freedom. It is hard to understand why one would have to choose a particular concept of truth for a particular investigation. Anyone who has thought deeply about the subject knows that the correspondence theory of truth works fine in many cases, while in other cases the coherence or the consensus theory may be more appropriate. At the same time, the methodological discourse has often focused on misleading issues. The "rigor vs. relevance" debate, for example, has always struck me as a strange discussion that verges on the bizarre. Why would research have to follow a superficial notion of "rigor"?

Lack of excitement: Even though the subject of our research is characterized by a multitude of extremely inspiring questions and opportunities (see below), research often seems to focus on the creation – or more likely the testing – of constructions with little epistemological value. Who gets excited by the Technology Acceptance Model (TAM) and its multiple variants? While these symptoms of the current crisis can be regarded as more or less critical features of a social system by which we are affected, but for which we are not responsible, they are accompanied by an

apparent paradox: While a considerable number of IS researchers seem to be unhappy with or even suffering under the current situation, most of them contribute willingly to the reproduction of an unloved system; activity that can hardly be considered as creating value.

III. CAUSES

Historical background: To understand the current situation, it is essential to take into account two highly relevant aspects. First, the early representatives of the IS field often worked in environments where they did not enjoy much respect: Weber even speaks of "hostile actions ... undertaken from ... colleagues from other disciplines against the IS discipline" [Weber 1997, p. 13]. That situation created a considerable pressure to acquire legitimacy. Second, as with any other emerging discipline, early IS research suffered from its immaturity with respect to terminology, results and academic practice. In a critical essay on the state of the then evolving discipline, Dearden complains about a "mishmash of fuzzy thinking and incomprehensible jargon" [Dearden 1972, p. 90]. Against this background, it is understandable that early IS researchers made an effort to build a more solid, scientific foundation. Both reasons make favoring a behaviorist approach an obvious choice, since that approach is based on the successful model of research established in Natural Science. Furthermore, building a reputation relies upon developing established publication channels that adopt advanced review processes. In this respect, the strategy chosen in the early days of IS was successful, which might explain why some are reluctant to challenge it. It is intuitive to regard it as an asset that took a great deal of effort to develop and therefore warrants protecting.

Insufficient success in attracting the best: It seems that we failed to establish our discipline in society (I am afraid that this applies in any country where IS programmes are run). Although the pivotal relevance of IT and information systems is undisputed, even those people who are aware of IS do not generally perceive it as a driving force behind future developments. Moreover, we have evidently failed to communicate to pupils and their parents that studying IS offers fascinating topics and presents a wide range of attractive career opportunities. Therefore school teachers are unlikely to advise their brightest students to study IS at university. As a consequence, IS suffers from a lack of outstanding undergraduates and brilliant PhD students. Therefore, it is not surprising that more than a few of our PhD students lack self-confidence and enthusiasm. Many inevitably tend to follow the guidelines and do not dare to develop new ways of thinking.

"Lock-in syndrome": It takes time and considerable effort to become part of an academic community and to learn the specific code of action. Those of us who were socialized successfully, for example by getting tenured, are likely to take the realities of academia for granted. It is our world. It may look strange from the outside, but we are familiar with it. While this kind of identification may be perceived as rewarding, it limits our ability to even think of alternative ways of organizing research: "Human beings have a knack of getting trapped in webs of their own creation" [Morgan 1986, p. 199]. Furthermore, those who are particularly successful in the current system may not have a strong motivation to question it.

IV. WHAT CAN BE DONE?

I do not know. However, in my state of perplexity I am still idealistic enough to at least think of some, perhaps naïve suggestions to overcome the discomfort that Nik Hassan probably shares with many others. They are not intended as advice. They rather serve to bolster my own spirit and confidence.

Be outraged: In a recently published pamphlet, Hessel [2011] warned young people against accepting a world of which they disapprove. Instead, he invited them to be outraged and to strive for change. I like this attitude, because I think that being outraged is an expression of being involved and committed. It may seem that such a demand is inappropriate for academia. Indeed, I am not thinking of leaving the path of critical discourse, but rather of pursuing it with passion and persistence. However, while critiques of research contributions seem to be accepted and (hopefully) appreciated by many, I still detect a remarkable reluctance when it comes to criticizing the "system". Nik Hassan's contribution is an example of this attitude: Even though he is obviously unhappy with the state of our discipline, he emphasizes that criticism is not his intention. While this attitude may have its roots in a civilized sense of politeness, I do not think it is appropriate. Critique and the willingness to fight for what we regard as being right is not only a key element of our concept of Science, it is also a prerequisite for overcoming the adversity that besets our work and our lives. Of course, the demand for (passionate) critique is accompanied by the need to justify our claims. How could there be hope for a better world, if the young do not get outraged when they are given a piece of advice like this: "After we have had the satisfaction of publishing in Management Information Systems Quarterly (MISQ) or Information Systems Research (ISR), and at least after making tenure and Full Professor, it is time to start thinking about researching big ideas that make a difference" (Manju Ahuja quoted by Hassan [2014]).

Relax: Even though we are involved in the factual manifestations of academia, no matter whether we benefit from or suffer under them, we should not take them (or ourselves as researchers) too seriously. I think it helps to tell



ourselves once in a while that there are other things in the world deserving of our attention. Especially if we dislike certain aspects of our discipline, it is a good idea to be detached and look at the situation from the outside to develop a more analytical view of incentive schemes and patterns of (opportunistic) action. Is it not, for example, interesting that a discipline that recommends its practitioners re-design actual patterns of action and adopt agile approaches over more restrictive methods is extremely reluctant to develop a relaxed attitude toward research methods, and moreover, seems unwilling or unable to rethink the constructions it is based upon? The suggestion to relax may be perceived as presumptuous, particularly when made by somebody who has been tenured for a long time and therefore does not have to struggle with career considerations. It is certainly not meant as such. I think that especially at the beginning of a career path it makes sense to reflect critically upon the subject, orientation and incentive schemes of a discipline: What value does a career have, if it is driven by criteria of which you are not convinced, or of which you even disapprove?

Enjoy: I strongly believe that knowledge implies interest [Habermas 1972] and joyful enthusiasm. The subject of our research offers a wide range of fascinating topics. We are witnessing a huge transformation unprecedented in the history of humankind: one enabled by technological advances and ever changing language games, and that represents “a continuing evolution of how we understand our surroundings and ourselves ...” [Winograd and Flores 1986, p. 179]. Those of us in the IS field are at the centre of this transformation: a position that gives us the intriguing privilege of being able to think how to shape that transformation. Information systems are linguistic artifacts that are tightly interwoven with patterns of action. Among other things, this leads to the question of how the language we speak influences the way we design the linguistic artifacts (languages, models, code ...) that constitute information systems, and how those linguistic artifacts in turn influence the way we speak, act and think. There is a plethora of inspiring discourses and discoveries, e.g. in sociology (of knowledge), social psychology, organizational studies, linguistics, and computer science, on which we can follow up. In order to enjoy studying our research subject, it is important to demand a culture that appreciates contemplation as a value in itself [Aristotle 1999, Book 10]. It also requires us to promote rational discourse, not only as a pivotal instrument to foster recognition, but also as a medium to create identity, sense and joy. Hence, by contributing to a research culture that fosters intellectual hedonism, we do not create value only for ourselves. Ultimately, we will contribute to the value of our research; and may improve the chances of attracting the best people to the field.

Be free and ambitious: Academia should be the place in society where freedom is an undisputed value. This includes the freedom to question and criticize what we are doing and how we are doing it. It also includes the freedom not only to choose between a given set of methods, but also to create your own method [Frank 2006], or indeed to opt for epistemological anarchy [Feyerabend 1975]. Of course, the freedom to ask questions alone is not sufficient. One should also be ambitious and pursue possible answers with self-confidence and persistence: “sapere aude!” In addition, if epistemological anarchy is to be constructive its acolytes should have extensive knowledge of existing methods. Being ambitious implies striving for difference, i.e. for an independent, strong profile of the discipline. That suggests developing elaborate concepts not only of (cross-) organizational action systems but also of information systems. Not everybody likes the pathos of romanticism (and my own view of it is somewhat ambivalent, too). Nevertheless, I would like to see not only our PhD students feeling inspired by Hyperion’s cry: “Are we like tame poultry that must not leave the yard, because it is fed there? No, we are like the young eagles that the father has thrown out of the nest in order to catch their prey in the high skies.”

Surprise your peers: PhD students are often provided with guidelines on how to successfully write a dissertation (e.g. those proposed by Davis and Parker [1979]). Others are given the advice to adopt the style of papers in respected publication outlets as the model for their research. While providing such guidelines certainly makes sense, it is at the same time misleading. First, it neglects the value of academic freedom; for fostering creativity and for creating an attractive environment. Second, it contributes to the reproduction of certain patterns of research that are not necessarily the most valuable in the long run. Therefore, I would rather see contributions that seek to be different, and to surprise peers. What does it take to surprise the experts? Surprise is not restricted to research results, but may also include original perspectives on a research subject, perhaps manifested by asking surprising questions, or a refreshing way of describing research results. This certainly does not mean I advocate lowering the bar defining good research. On the contrary, if the surprise criterion had been applied, it would probably have barred the publication of more than a few of the articles found in highly ranked journals.

Help create a better world: I share the widespread belief that truth is a pivotal objective of scientific investigations. However, I do not believe that truth is enough. Research in IS also aims to improve the development and use of information systems with respect to certain goals. The respective constructions will not comprise only IT artifacts but will always account for corresponding patterns of action. In other words, we are designing *possible worlds*. While any knowledge contribution that is intended to qualify as scientific requires justification, truth alone is not sufficient in this case. A possible world is intended to differ from the factual world. Therefore, statements describing possible worlds cannot be tested against reality. Designing possible worlds should result in offerings to inspire those who

might realize them, thereby emphasizing an essential goal of Economics, namely, creating interesting (and feasible) options for action. I would not go as far as Rorty [2000] who recommends replacing truth as the central objective of scientific enterprises with a (justified) hope for a better world. However, supplementing truth with hope seems a stimulating, value-adding idea.

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