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## Information Systems Research: Making an Impact in a Publish-or-Perish World

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# Information Systems Research: Making an Impact in a Publish-or-Perish World

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### Abstract:

This paper reports on the panel discussion that took place at the European Conference on Information Systems (ECIS) in Guimarães, Portugal, on 9 June, 2017. The discussion focused on three central questions: 1) "What does research impact mean for you?", 2) "What is your approach to making an impact with your research?", and 3) "What advice would you give to PhD students and early-career scholars?". While the five panelists (Samir Chatterjee, Alan R. Dennis, Shirley Gregor, Magnus Mähring, and Peter Mertens) partly differed in their views on what impactful research is and how to conduct it, they seemed to largely agree that assessing impact requires a multidimensional view, that impactful IS research requires a clear link to real-world problems ("grand challenges"), and that young scholars need to avoid the trap of confusing research gaps with research relevance. With the panel discussion and this report, we hope to initiate a discussion on the essential topic of research impact in the IS discipline and to contribute to the development of a more uniform, yet more diverse, understanding and appreciation of different approaches to making an impact with IS research.

**Keywords:** Information Systems Research, Research Impact, Publish or Perish, Panel Report.

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## 1 Research Impact and the Central Dilemma

Arguably, all researchers seek to make a “difference” with their research (i.e., conduct research that has an impact). Research impact refers to “the demonstrable contribution that research makes to the economy, society, culture, national security, public policy or services, health, the environment, or quality of life, beyond contributions to academia” (Australian Research Council, 2015).

At the same time, however, in order to advance their careers, researchers increasingly experience pressure to publish in top-tier journals (e.g., the AIS Senior Scholars’ basket of eight in the case of the IS discipline)—also known as “publish or perish”. Young scholars in particular experience this pressure: these individuals—such as PhD students and assistant professors—are subject to constant evaluation and need to “fight” for their *raison d’être* in academic world. For example, Dennis, Valacich, Fuller, and Schneider (2006) conducted a field survey on promotion and tenure requirements at American and Canadian universities and found that the great majority of the surveyed universities required academics to have published at least three papers in so-called elite journals for tenure:

*What constitutes excellence in information systems research for promotion and tenure? ...An informal survey of senior Information Systems faculty members at 49 leading U.S. and Canadian universities found 86% to expect three or more articles in elite journals. (p. 1)*

In today’s publishing world, to publish papers in top-tier journals, researchers must navigate through a review process that may take years for a single paper. Further, many top journals eventually accept and publish far less than 10 percent of the paper submissions that start this treacherous and lengthy journey. To make things worse, while one could expect that publishing in top-tier journals should go hand in hand with making a research impact, it is unfortunately not necessarily the case. For example, one may argue that, while top-tier publications tend to address specific research gaps and answer specific research questions related to these gaps, research impact concerns solving real-life problems (outside of academia). Relatedly, a published research study may have a very short half-life and/or never receive any citations. And, even if it does, it may not have been all that influential to the author(s) who referenced it (Straub & Anderson, 2010).

Further, the rising pressure that universities and researchers face from funding agencies to conduct impactful research and to “give back” to key stakeholders (e.g., students, society, businesses, and tax payers in general) further intensifies the dilemma. For example, based on the insights gained from an international “impact of research” study, the chair of the AACSB board of directors has concluded that “in the future, business schools will have to be more strategic about their research investments and more explicit about assessing the return—not because AACSB will require it, but because stakeholders are starting to demand it” (AACSB, 2012, p. 4).

In this context, Samir Chatterjee—IS professor at Claremont Graduate University and co-author of this panel report—started a much-noticed discussion about the “most influential information systems papers” in July, 2016. In an email distributed via the AISWorld mailing list, he voiced his concerns that, in the IS community, “we periodically tend to talk about elite journals by name [but] very seldom do we take the time to actually discuss what papers/content published in these journals actually have an influence” (i.e., papers that not only receive high citations but also have a real-world impact). Put differently, he sees “our field as obsessed about journal prestige” but that it pays “very little attention...to what work has actually benefitted mankind”. A controversial debate involving IS scholars from all over the world unfolded: some scholars expressed their agreement and sometimes even expanded the voiced criticism on current IS publication practices (e.g., narrow focus of top IS journals on behavioral research and general management topics; strong focus on “bean counting” when it comes to hiring, tenure, and promotion processes; publishing for the sake of “mutual admiration”), whereas others largely rejected the voiced criticism, such as by noting that knowledge itself is a “beautiful asset” that may or may not have immediate practical value.

We can (at least partly) trace the polarity of the ongoing discussions on the impact of IS research to very different understandings in the IS community of what research impact actually is and how one should measure it. This polarity implies that we, the members of the IS community, still need to engage in a broader conversation of what constitutes impactful IS research in order to develop a more uniform understanding of research impact in the IS discipline. Here, being quite generic, common definitions of research impact seem to provide only limited guidance (see also above). A related issue concerns the question of whether research impact necessarily requires contributions beyond academia or if sole contributions to academia can constitute a “sufficient” impact as well.

AACSB (2017) provides some guidance about multiple types of impact including, but not limited to, academic, teaching/instructional, practice/community, and research center impact. AACSB proposes three pages of ways to assess impact that include research-influenced changes to business practices, professional standards, or public policy; media citations (e.g., number, distribution, and effect); broadcast forums; professional development activities; researcher-practitioner meetings; faculty/student consulting projects; research income from various external sources such as industry and community/governmental agencies to support individual, research center, and collaborative research activities; community enhancement outcomes resulting from the engagement of faculty and students in community issues; tools/methods that students and faculty develop for companies; memberships on boards of directors of corporate and non-profit organizations; memberships on professional standards setting bodies or policy-making bodies; and invitations by governmental or other agencies/organizations for center representatives to serve on policymaking. Many countries, including the UK and Australia, also have multi-dimensional programs to assess research impact. For example, the United Kingdom's dual-funding system awards funding based on both the academic quality of research and its perceived wider societal impacts (Morgan Jones, Manville, & Chataway, 2017). To measure such impacts, the system considers a wide range of indicators including demonstrable academic impact and demonstrable impact to society and the economy.

Further, the controversial debates illustrate that today's academic scholars—especially those just starting their careers—face numerous, difficult decisions about how to proceed in this “perilous” world. To start with, they need to decide on a topic of study that will not soon prove to be a fad but that actually has the potential to make an impact (whatever that exactly means). But then, what are the most promising pathways to eventually make a research impact? Here, because of the complexity of research topics and the tools being used to explore them, one may need to join a team. So, how does one select collaborators? And, as research teams tend to steadily grow in size, how can one ensure that others see and appreciate one's contribution on these teams? Further, how can one ensure one's publications become visible in a sea of publications? Or, since it has become harder for others to recognize any one paper, should one even bother with publishing papers?

## 2 Panel Organization

The panel discussion took place at the European Conference on Information Systems (ECIS) in Guimarães, Portugal, on 9 June, 2017, and focused on three central questions:

- Q1:** What does research impact mean for you?
- Q2:** What is your approach to making an impact with your research?
- Q3:** What advice would you give to PhD students and early-career scholars?

Five panelists (Samir Chatterjee, Alan R. Dennis, Shirley Gregor, Magnus Mähring, and Peter Mertens) participated in the panel discussion. The panelists have received recognition for their impactful research around the globe and represent the perspectives of different countries and regions (Australia, Germany, Sweden, USA) with partly unique research cultures and, thus, also with potentially different understandings of research impact and different approaches to achieving such impact. Martin Wiener and Carol Saunders served as moderators.

The panel focused on providing researchers, especially those just starting their academic careers, with some guidance for living in a publish-or-perish world. It sought to initiate a discussion on the essential topic of research impact in the IS discipline. In particular, the panel sought to contribute to the development of a more uniform but at the same time more diverse understanding, and appreciation, of different approaches to making an impact with IS research.

In Section 3, the five panelists summarize their view on IS research impact according to the above questions (Q1-3).

## 3 Panelists' Views on IS Research Impact

### 3.1 Impact Comes from a Passion to Solve Real-world Problems (Samir Chatterjee)

**Q1:** I did not expect the amount of feedback, debate—and perhaps backlash—to my simple question on AISWorld when I asked: “Can we list few major IS papers that have had an influence?”. From the ensuing debate that unfolded, I realized that this discussion had two sides: one side that said IS does not do impactful

research that anybody (out there) cares about and another side that claimed the “status quo” is beautiful. I do not personally agree with either side and certainly not to the notion that the status quo is ok. I truly believe that some IS scholars do important work that can or does have an impact but that there seems to be a disconnect between the work’s value and whether or not the so-called “basket journals” publish it, which has become the *de facto* indicator of IS research quality.

I also realized that people view influence or impact to mean different things. Based on several interesting email inputs to my query, I concluded that impact has multiple dimensions. To measure influence, we must take a multi-dimensional view, and a paper or research project must meet more than one of these dimensions to have impact. Indeed, researchers and others often understand impact more from a body of work rather than a single publication (e.g., see Table 1).

I am a design scientist who strives to solve real-world problems. Research impact to me has to do a lot with being relevant and useful. I always ask: so what? What is novel? How has/will this change the world? We live in a world today where different stakeholders, such as students, practitioners, venture capitalists, funding agency, citizens, and government policy agencies, consume our research. Our research must provide value and benefit to these stakeholders in some meaningful way.

**Q2:** My approach to making an impact with my research has two important ingredients: 1) one must follow one’s heart or passion, and 2) one must address a big societal problem. The first one concerns what excites a researcher. I have repeatedly seen young IS scholars say: “I am doing this because someone told me to do so” or even very sad statements such as “this methodology will get me into a basket journal”. I do not believe that research is “one” paper. Rather, I believe it is a stream that will result in papers, books, patents, or even startups. One has to be immersed in the domain for several years to make an impact. One cannot flourish in such an environment if one has no excitement about the things one does.

**Table 1. IS Influence Metrics (IS Influence Project, n.d.)**

<b>Academic metrics</b>	
Number of Citations	1 (1-100); 2 (100 – 1000); 3 (1000+)
Number of Years since publication	
Perceived quality of the journal/conference	1 (low); 2 (medium); 3 (high)
External grants funding the research	NSF or NIH or DARPA or EU (or other private)
Other disciplines using the idea in the research	Yes (1); No (0)
<b>Industry/practice metrics</b>	
Patents issued or filed	Yes (1); No (0)
Actual intervention in field or site (action or design research)	Yes (1); No (0)
Commercialization of idea into product/service	Yes (1); No (0)
Startups created based on the idea	Yes (1); No (0)
<b>Society metrics (qualitative or subjective data)</b>	
Benefit of research to scientific community	1 (low); 2 (medium); 3 (high)
Benefit of research to society at large	1 (low); 2 (medium); 3 (high)
Media coverage (radio, TV, print, movie)	Yes (1); No (0)
100-word explanation of why this paper is worthy of consideration	

The design science research (DSR) paradigm has given the IS community a tremendous opportunity to make a difference because it focuses on problems and not methods. In the US, the National Academy of Engineering (2008) has revealed the grand engineering challenges for the 21st century. These challenges concern areas that have the greatest need to make an impact. Instead of focusing on problems, many IS scholars have become engrossed in method-focused research in which they perform structural equation modeling with any data set. It is like “you have a hammer; everything you see is a nail”. The fact that our so-called elite journals have a fetish with methods has exacerbated this phenomenon. I strongly believe in good, simple design. There is beauty in simplicity. I also believe that we should test a lot of our prototypes or applications in the field. As they say, fail often but sooner to derive the best design. DSR also involves being patient. The DSR process (Peffer, Tuunanen, Rothenberger, & Chatterjee, 2007) can lead to many discoveries that the researcher can often ignore if in a hurry.

The IS discipline also has a theory fetish. I see nothing wrong in good theory work. However, we first need to tackle important, big problems with solutions and artifacts that can demonstrate utility and efficacy. When we deal with wicked problems, we need to gain traction. Theory will eventually emerge. But to put a lot of stress on designing and solving a problem and also coming up with theory is counterproductive. Further, I believe that designers' basic skill sets complement those of theoretical researchers.

**Q3:** I believe that any PhD student or early-career scholar must find relevant problems to work on. They must know and seek the right audience for their work and, in turn, find the right journals and not a basket to publish in. When I first started to work in the healthcare informatics discipline, I wrote a thought piece on how persuasive technology can change human attitudes and behaviors and how such technology can help to address the growing chronic disease conditions in the US and other developed countries. I could have tried to submit that paper in an IS elite journal, but I chose to publish it in *Journal of the American Medical Informatics Association* (JAMIA) (Chatterjee & Price, 2009). That journal's audience better suited the paper. Not only did the community receive the paper well and cite it often, I also got a call from National Institutes of Health (NIH) program managers to talk to them about the potential of persuasive technology. Since then, NIH has included behavior-change technology in many of their calls for funding opportunity—an impact I could not have achieved from an IS journal.

Further, I recommend that researchers develop a stream of research. When researchers have just begun their career, they can perhaps have one or two different streams. But they need to publish in those streams and develop a portfolio of papers, which, in turn, establishes a track record. Throughout my life, I have been fortunate to get extramural funding for my research. I think young scholars should also sign up for reviewing grant proposals because they would learn so much on grant writing and also the process behind how grant decisions are made. Finally, researchers should pick and choose the right collaborators or team for their work. Doing work with industry partners matters. It helps guide one towards achieving an impact.

### 3.2 Be a Change Agent (Alan R. Dennis)

**Q1:** To me, the secret to creating research with impact is to be a change agent—the larger the better. As researchers, we often devote the most attention to changing the way that we conduct our research and the topics that our discipline discusses. Indeed, one can make a desirable impact if one can do a good job of bringing greater rigor and relevance to the way we conduct the research in our discipline and in other academic disciplines. However, we may have a broader vision for our research.

To enact this vision, we need to consider which stakeholders we wish to serve. In the business community, we may choose to guide practitioners, help them successfully conquer the challenges they face, and change their practice for the better. Or we may choose to serve stakeholder groups outside of the business community. For example, we may choose to shape and change the public debate by focusing our research on relevant problems that society faces (e.g., fake news and its disruption of democracy) or by developing technology that can help large global populations.

The problem is that doing so typically does not produce papers in top-tier journals. Thus, one has to pick and choose what one wants to do at different stages in one's career.

**Q2:** My approach to making an impact with my research has its foundation in my industry experience. I have co-founded five software start-ups that have gone on to develop commercial software packages. I have now begun on start-up number six.

The problem is that practitioners do not care about the same things that academics do. As the founding publisher of *MIS Quarterly Executive* over 15 years ago, I have talked with many practitioners about what types of papers we should publish. They wanted two types: software reviews that would tell them which package to buy and primers on emerging technologies so they would know whether to buy them. Academics find neither of these types of papers particularly attractive.

Technology users do not really care about the same topics that interest academics either. My research on fake news has received popular press coverage in BuzzFeed, Vanity Fair, and so on. However, my paper with Taylor Wells (Wells & Dennis, 2016), which examines the use of emails versus voicemails for romance, has received the most popular press coverage. Not my most important research from an academic perspective, but it received over 300 media mentions in print, online, on radio, and on TV.

To summarize, with reference to Gandhi who once said: "Be the change that you wish to see in the world", I would say: if you want to change practice, go work for a company. If you want to change what regular users do, go work for the popular press.

I think that we should also remember that, as academic researchers, our primary mission lies not in focusing on near-term problems and opportunities; rather, our greatest contribution to society comes by focusing 20 years into the future on wicked problems and large opportunities that companies have no interest in. Do you know who invented the windows and mouse interface and in what year? Think about it for a minute.

Doug Engelbart invented it when he was a researcher at the Stanford Research Institute (SRI) in 1967 (<http://www.dougenelbart.org/firsts/mouse.html>). Engelbart told me that he showed it to a host of computer companies and none showed interest in it. They encouraged him to focus his research on more relevant topics such as a better command line interface. Imagine how the world would be different today if Doug had followed the advice of those companies and focused on “relevant” research instead of his “irrelevant” research.

**Q3:** I have four pieces of advice—especially for junior scholars and doctoral students—to make an impact. First, be unique. Do not follow the crowd. Today, everyone is chasing analytics, which is well and good. If you want to make an impact, do not bother. Find a different topic.

Second, do not look for “research gaps” to fill. Filling gaps is for dentists. Much has been written on why filling gaps leads to the comfortable mediocrity of incremental research. Instead, strive to be interesting. Davis (1971) provides some useful guidelines about what makes research interesting (see Figure 1).



Figure 1. A Checklist for Interesting Research (from Davis, 1971)

Third, do not strive to publish papers; strive to receive citations. At first, one can find it exhilarating to publish work, especially in top-ranked journals. But the real secret to creating impact lies in being cited, which arises from being useful to other researchers and being novel, interesting, and controversial.

Finally, one also has to make oneself visible to others in the discipline by networking at conferences. It is not enough to just present papers at conferences. One should also attend them and discuss not only one’s own research with other researchers but also offer helpful comments about *their* work.

### 3.3 Impact Comes from Tackling Important Problems (Shirley Gregor)

**Q1:** My view on research impact concurs with policy that the Australian Government has begun to introduce to encourage academics to produce work that has impact outside of academia (as the paper’s opening paragraph defines such impact).

In my view, information systems (IS) by its very nature should produce research that has impact. It is one of the sciences of the artificial (Simon, 1996), a branch of the applied sciences that produces knowledge not just to describe the world but also to seek how to change and improve it for human and societal benefit (Gregor, 2006; Niiniluoto, 1993).

IS research groups around the world provide good examples of what impact means and how to achieve it. These groups include (among many others): Jay Nunamaker’s group at Arizona State University, the CISR

group with Peter Weill and Jeanne Ross at MIT's Sloan School of Management, Niels Bjørn-Anderson and colleagues at the Copenhagen Business School, and Jörg Becker's team at the University of Münster. These groups and others like them have several things in common: they explicitly focus on doing "useful" research; they receive substantial funding from industry for joint projects, which indicates that industry finds their work valuable; and they produce books and reports that are widely read outside academia. The Queen of Denmark even knighted Niels Bjørn-Anderson in 2003 for his contributions to society. Jay Nunamaker and his colleagues have reflected on their experiences in carrying out research with "high-value real-world impact" over many years in systematic programs of research in a paper that pertains to the question at hand, and I recommend it for further reading (Nunamaker, Twyman, & Briggs, 2017).

**Q2:** Research that makes an impact should start with an important problem. A good quote comes from Medawar (1979, p. 20):

*Any scientist of any age who wants to make important discoveries must study important problems. Dull or piffling problems yield dull or piffling answers.... The problem must be such that it matters what the answer is—whether to science generally or to mankind.*

In some areas of academia, the view that "true" scientists work on basic or foundational research that then leads on to applied science and application to real-world problems still pervades. This view at its extreme corresponds to the "linear" view of research that flowed from the policy work of Vannevar Bush in the US in the mid-1900s, who held that "applied research invariably drives out pure [research]" if one mixes the two (Bush, 1944, p. 83). Others have shown through historical examples that the linear model does not represent how science works in important ways. For example, Stokes (1997) points to the groundbreaking work of Louis Pasteur on the germ theory of disease, which partly originated from his solving a problem with fermentation of beets in an industrial process to produce alcohol (an important problem).

In IS research, we have explicitly recognized research approaches to investigate both important real-world problems and make justifiable contributions to knowledge. These methods include design science research and action design research, which researchers who want to make impact should adopt. I have personally applied these approaches with supply chain management in the beef industry (Gregor & Jones, 1999) for which I received the honor of being made an Officer of the Order of the Australia in 2005. As another example, I have used them in work on the adoption of e-government in Bangladesh (Gregor, Imran, & Turner, 2014).

**Q3:** I advise such scholars to conduct research that focuses on real-world problems whenever possible. Often, this approach may mean working with industry partners who can give insights into which problems are important. For researchers who have not yet been able to build up relationships with partners, which can take years to establish, one may find benefit in working in teams with more experienced colleagues (see Nunamaker et al., 2017).

Industry-based projects can be time-consuming and produce a fewer number of publications compared to behavioral research, so it is wise to have a portfolio approach with a range of work of different types. "Design-oriented behavioral research" also represents a type of work that can produce relevant work with impact while also aligning more with traditional approaches (Maedche, Parsons, & Gregor, 2017). This work tackles a real-world problem but does it via experiments that contrast typically different features or capabilities of a system. Lukyanenko, Parsons, and Wiersma (2014) provide a good example: they conducted experiments showing how different forms of class-based modeling influenced the quality of data that users generated as in citizen science. The work of these researchers has been judged important enough for a *Nature* publication and has clear implications for practice. Such work may be more easily publishable in good journals compared with design science approaches because it can be presented in a form that matches traditional journal expectations and can also provide results that can be readily translated to practice.

### 3.4 Every Which Way You Can (Magnus Mähring)

**Q1:** First of all, I think we need to acknowledge that the path from a specific piece of research that an individual (or team) conducts to a "real-world" impact is often not direct and that it does not have to (and often should not) be. I think we need to accept that not all research is—or needs to be—*immediately* useful and that striving for such immediate usefulness might often diminish work's quality and value. However, I think one should develop a sense of—and an approach to—ways in which the research one works on might or will *ultimately* be useful. Having a well-developed sense of how to share and spread one's research effectively makes one's work as an academic more meaningful in a deeper sense than counting acceptance

decisions or citations. Relatedly, one should not focus on *switching* from “research” to “practice” but on *bridging* the two—on clearing paths from rigorously developing theory to improving practice.

To me, to perform this bridging, one needs to nurture different channels and ways to interact with and contribute to practice. For example, one can repackage and extend a research paper and try to publish it in boundary-spanning journals, such as *California Management Review* or *MIS Quarterly Executive*, or a practitioner outlet such as a professional magazine. To do so, one needs to translate one’s research into a form that emphasizes actionability and reduces ambiguity and caveats. For example, for four years, I regularly wrote opinion pieces for the Swedish *CIO Magazine* that I always based on a research topic. Sometimes I wrote about my own research, sometimes more generally about a topic I was interested in, and sometimes about a new research idea I had. Whatever the case, I always had to boil the topic down to its practical essence: what it really concerned and why CIOs, other IT professionals, and managers in general should care about it. I was lucky to have a subscribing audience, but, essentially, one needs only a blogging account to do the same, and it is a great practice.

**Q2:** To me, having an impact starts with really caring about practitioners, the challenges they face, and how one’s research ultimately connects with these challenges. It also entails engaging with practitioners, translating one’s research, and seeking many different ways to do so. For many academics, executive education constitutes a great way to do all these things. But connecting with local chapters of CIO executive clubs, attending events, and making oneself visible and accessible to local press can also be useful. As done does so, I believe that one should use a dialogical approach and to not use research findings as truths hurled at recipients. Rather, engaging in dialogue means listening, pursuing conversations with an open mind, and using one’s research findings as suggestions that fuel the conversation. Instead of making claims, talk about your findings in an explorative way, offer them as suggestions, and inquire honestly about how it resonates with recipients. Edgar Schein’s process consultation advice to “remain helpful” and the idea that impact comes from other people’s willingly changing their minds serve as great guidance for these dialogues.

One should also not overlook teaching—not only executive teaching—as a way to have impact. In teaching situations, I have found it useful to focus on sharing insights and not specific research results and to look behind the “easy wins” in terms of happy participants and towards helping participants become more capable, reflective, and ethical managers and professionals. A particularly impactful teaching approach is to use live projects in which students work in teams to deliver value to an external client. In guiding these consultative processes, one can help students excel and challenge them to put theory to practical use in many ways.

**Q3:** First of all, I advise early-career scholars to not always seriously take or slavishly follow senior scholars’ advice. Senior scholars do not always remember what it is like to start out an academic career. More specifically, for many tenure-track faculty, publish or perish is a reality. Tenure-track faculty need to be mindful of their current institutional context: it represents their current reality and, in most cases, will probably not materially change over the rather few years they have on their tenure clock. However, I would suggest that they also ensure that their practice is a movable feast (i.e., that their priorities and choices help shape a practice that is viable and attractive elsewhere). They might want to or have to move to another institution. However, most of all, they are less vulnerable and more likely to enjoy their craft if they believe that they have the option of moving to another context. Not caring about journal lists and baskets is a risky and limiting strategy even if one happens to work in an institution that espouses such an approach. Only caring about journals lists and baskets might cause one to obscure the higher purpose of one’s work over time. Celebrate publication “wins”: few things are as rewarding as a conditional accept decision, but also remember to remain helpful—to connect with and serve practitioners. I deeply believe that remaining helpful makes scholars better but also that it makes them more resilient and less vulnerable to potentially shifting academic fortunes. So, in essence, I advocate “publish and *parish*”: in addition to publishing, early-career researchers should also ensure that they develop a sense of who their constituents are, that they care about their community (i.e., the “parish” they have chosen), and that they make this parish an integral part of your role and your work as an academic.

### 3.5 Moving Away from One-dimensional Assessments (Peter Mertens)

**Q1:** In my opinion, the question of what impactful research appears to relate closely to the distinction between demand pull and technology push. Specifically, when I have to prioritize research activities, my principal proposition is that demand pull is more important than technology push. Demand has driven many major innovations and inventions in our discipline. Just to give two examples:

1. Konrad Zuse developed and produced the first German computer (also one of the first worldwide). Interestingly, Konrad Zuse was neither an electrical engineer nor a mathematician but a civil engineer who suffered from the number and variety of calculations he had to perform when planning a building.
2. Two Harvard Business School students invented the spreadsheet because they felt it was too boring to recalculate the new subtotals of rows and columns after modifying only a few values in the cells of their statistical tables.

On the other hand, technology push often leads to gimmicks with a rather short lifespan and questionable societal value such as Pokémon Go or geo-fencing.

Further, when it comes to demand pull, I think that IS researchers should focus on solving existing and severe problems in their own country (see examples in Figure 2). For example, consider an example from my country, Germany: more than other industrialized countries, Germany's population has an almost disastrous age structure. In a few years, it will take two jobholders to pay for one pensioner. Additionally, jobholders need to provide for children and refugees who lack formal education and command of the German language. In this context, the so-called polarization theory suggests that the German population will be split into two categories: highly qualified and specialized workers who are underrepresented in the workforce and people who are hard to employ but overrepresented in the workforce. Against this backdrop, I am particularly interested in two kinds of assistance systems using artificial intelligence (AI):

1. Systems that help (further) increase the productivity of highly qualified employees in banks, manufacturing companies, government agencies, and so on.
2. Systems that help integrate poorly qualified persons into the value-creation process by providing real-time instructions; that is, the system provides workers with relevant information about what to do at the very moment when they need it. The instructions would depend on their knowledge and language.

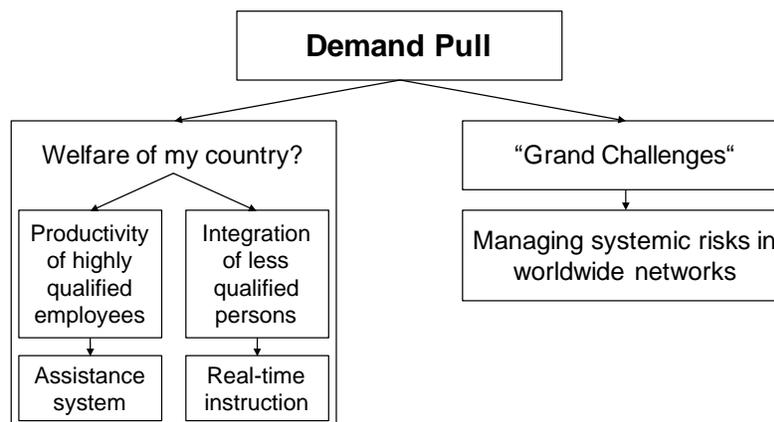


Figure 2. Demand Pull (Examples)

Moreover, to me, all major scientific communities should try to identify the “grand challenges” in their discipline. For example, we conducted a survey with about 150 scientists and practitioners from Austria, Germany, and Switzerland to identify major IS-related problems to solve in the long run (Mertens & Barbian, 2013). Among other things, the survey results indicated that we need new ways to better manage systemic risks in worldwide networks (see Figure 2 above). In response, I am currently working on setting up a major research project that involves about 40 colleagues. Hopefully, we will receive the grants we need to fund this project.

**Q2:** My approach to making an impact includes common ways of transferring academic knowledge to practice, such as participating in professional development and writing papers in practitioner journals and books targeted at specialized technologies (e.g., supply chain management with SAP systems). Also, I have motivated my doctoral students to create start-ups based on the research they conducted for their thesis. To prepare them for such entrepreneurial careers, my colleagues and I have organized dedicated courses.

My former doctoral students have founded 25 firms that currently employ about 3,000 people altogether. Over the last three years, I visited and interviewed all of them. Notably, only two of my students' firms have

undergone bankruptcy, which is—compared to the numbers published by the Federal Statistical Office of Germany—far below average.

**Q3:** Given the above, I would recommend that doctoral students and early-career scholars spend two to four years as an employee in a firm where they can learn much and gain different experiences. Put differently, I suggest that a year with Microsoft in Silicon Valley is better than a one-year research stay at the University of California. However, doing so would mean that doctoral students and early-career scholars would have less time to publish research papers in highly ranked U.S. journals. In this regard and referring to me, a colleague of mine once wrote: “Some older colleagues with good and secure pensions advise them [young and untenured researchers] not to care about impact in highly ranked journals” (Frank et al., 2015, p. 286).

Finally, if I were an authority who could define the rules, I would write a list of some 10 tasks (“decathlon”) that an IS professor has to undertake: teaching and supervising students, helping students find internships and graduates find adequate jobs, writing textbooks and developing educational materials, researching and transferring results to IS specialists and managers, cooperating with research and development (R&D) units in firms, consulting politicians and public officials, facilitating the formation of spin-offs, and so on (see Figure 3). On this basis, university departments could then decide how much weight they wanted to attribute to each criterion when it comes to hiring candidates.

1. Teaching and supervising students (including arranging internships)	6. Promotion of university location and facilitation of spin-offs
2. Writing textbooks and developing other educational materials	7. Self-administration of university
3. Research and transfer of results within academia (publications)	8. Self-administration within community (e.g., editorial board of journal)
4. Research and transfer of results into practice (e.g., patents)	9. Coaching of academic staff, doctoral students, etc.
5. Research and transfer of results into politics and society	10. Acquisition of third-party funds

Figure 3. Decathlon of the University Professor (Mertens, 2010)

## 4 Summary and Conclusion

In summary, although the five panelists differ in their views on what impactful IS research is and how one should conduct such research, they also do share some common ground. Most importantly, with regard to the question of what constitutes research impact, all panelists seem to agree that assessing impact requires a multidimensional view (i.e., that more than *one* way to make an impact exists). Here, several panelists point to the importance of considering different stakeholder groups. For example, Samir Chatterjee suggests a three-dimensional set of IS influence metrics that includes academic, industry/practice, and society metrics. Similarly, Peter Mertens’ “decathlon of the university professor” (see Figure 3 above) relates to a diverse set of stakeholders ranging from (doctoral) students, academic staff, and fellow researchers to practice partners, politicians, and society at large. In providing following comment via email prior to the panel discussion at ECIS in 2017, Shailendra Palvia (Long Island University, USA) also highlights research impact’s multidimensionality:

*One main point is that each research article must articulate its impact on academia, industry, government and society in that order. If the impact is only on academia, that counts for 1 point. If also industry, then 1 + 2 = 3 points. If also on government, then 1 + 2 + 3 = 6 points. If also on society, then 1 + 2 + 3 + 4 = 10 points. That is an extremely simplistic way of measuring impact.*

Also, the panelists seem to agree that impactful IS research requires a clear link to real-world problems with a particular focus on the “grand challenges” of our time. In this regard, the panelists stress that, in order to make an impact, one cannot “simply” describe existing challenges; rather, one needs to take on a more active role and serve as “change agents”. However, the panelists seem to disagree somewhat regarding the scope, or type, of change that researchers should aim for. For example, while Samir Chatterjee and

Shirley Gregor hold the view that impactful research needs to go beyond contributions to academia (e.g., research that helps change practice, public debate, and the world in general), Alan R. Dennis opines that one needs to be what one wants to change and that researchers can also have an impact by contributing to the academic discourse and “changing research” itself. In this context, he argues that academics and practitioners tend to have differing interests and that researchers’ primary mission and, at the same time, key privilege is to focus on the large opportunities and more long-term problems that companies may not (yet) have an interest in. Relatedly, Magnus Mähring notes that not all research is, or needs to be, immediately useful and that striving for that might lead to shortsighted “solutions” and, thus, adversely affect the quality and value of research outcomes. These quite different views on research impact also reflect a central dilemma that especially many young researchers face nowadays: it is difficult if not impossible to please the proponents of each view at the same time. Even though a panel discussion and report cannot reconcile these different views, we hope that they can contribute to the incremental development of a broader understanding and greater appreciation of different approaches to making an impact with research in the IS community.

In terms of advice for PhD students and early-career scholars, three major points emerged from the panel discussion. First, the panelists reiterated that young researchers should follow their heart and passion when selecting a research topic. While research can be lots of fun, it is also hard work, which requires a level of endurance that is hard to maintain without being really passionate about a topic. As such, young researchers should also try to avoid following the crowd. Second, early-career scholars should not always take the advice of senior scholars too seriously or follow it too slavishly. The world in which they find themselves differs markedly from the world in which the senior scholars started their careers. Third, all five panelists urge PhD students and early-career scholars to make sure that their research addresses an important (societal) problem. For example, young researchers need to avoid confusing research gaps with research relevance. For example, one reason for a gap in prior literature may also be that it simply lacks relevance. In this context, Peter Mertens points to the important distinction between technology push (e.g., big-data analytics) and demand pull (e.g., aging population) and the arguably greater importance of the latter. Of course, that does not mean to suggest that new technologies, such as big-data analytics, lack relevance; however, instead of being the starting point of a research project, one should consider them as a potential means to solve a pressing societal problem. Furthermore, the panelists recommend that young scholars engage with practitioners and other external stakeholder groups in order to better understand their challenges and problems. Such engagement may range from starting an Internet blog to repackaging a research paper for a practitioner audience—as Peter Trkman (University of Ljubljana, Slovenia) also noted in an email comment<sup>1</sup>—to actually working in practice before entering academia. (On a side note, it should be noted that four of the five panelists worked in practice before becoming a professor.)

In addition, the panelists advise PhD students and early-career scholars to diversify their research portfolio. For example, working on research projects that involve industry partners can take a lot of time. Therefore, young researchers should find a reasonable balance between projects that require significant time commitments and less time-consuming projects. The same applies to balancing the risks associated with submitting to highly ranked “basket” journals versus “less ambitious” outlets. Further, the question of submitting to academic journals versus practitioner or teaching journals is not always an “either-or” situation. Researchers may realize a “double impact” if they conduct rigorous research and target it to (top-ranked) academic journals and then translate the message for other audiences. For example, Mullarkey and Gogan (2017, p. 3) recommend:

*“Double-impact” case research...as a way to get optimal synergy out of each case study that is conducted (Gogan, 2015). Each case can be disseminated through publication as a discussion case, or, as a practitioner-oriented example case. In addition, each case can be considered for presentation at an appropriate academic conference and for publication in a scholarly journal.*

In fact, if one can translate the academic research for practitioner journals, it may indicate that the research is not only rigorous but also relevant. Finally, several panelists indicate that making a research impact typically results from a body of work and will not likely result from a single publication regardless of whether this publication appears in a basket journal or not.

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<sup>1</sup> He said: “This spring I’ve started with a blogging experiment. Practitioners are on LinkedIn, so let’s follow them. I am trying to turn my research papers into semi-humorous posts for practitioners.”

In conclusion, we hope that this report on our ECIS panel discussion provides academic scholars—especially those just starting their careers—with some food for thought and guidance on how to “survive” in today’s publish-or-perish world.

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