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Dennis F. Galletta
University of Pittsburgh, galletta@pitt.edu

Niels Bjørn-Andersen
Copenhagen Business School

Dorothy E. Leidner
Baylor University

M. Lynne Markus
Bentley University

Ephraim R. McLean
Georgia State University

See next page for additional authors

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If Practice Makes Perfect, Where do we Stand?

Authors
Dennis F. Galletta, Niels Bjørn-Andersen, Dorothy E. Leidner, M. Lynne Markus, Ephraim R. McLean, Detmar Straub, and James Wetherbe

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Dennis F. Galletta
Katz Graduate School of Business
University of Pittsburgh
galletta@pitt.edu

Niels Bjørn-Andersen
Copenhagen Business School

Dorothy E. Leidner
Baylor University

M. Lynne Markus
Bentley University

Ephraim R. McLean
Georgia State University

Detmar Straub
Temple University

James Wetherbe
Texas Tech University

Abstract:

Practitioners have played an important role in the information system (IS) field's development since its beginnings. In the 1970s, IS researchers' integration with practitioners was high with Society for Information Management members receiving copies of the *MIS Quarterly*, practitioners funding the ICIS Doctoral Consortium, and submissions receiving at least one practitioner review. Today, however, the integration between practitioners and researchers appears more distant. Given that almost 50 years have passed since the field's development, we believe that we need to reflect on the past, present, and future relationship between IS research and IS practice. Has the distance between academics and practitioners become too great? Is our relevance too low to expect practitioners to join AIS and attend our conferences? How might we increase the integration? At a panel at ICIS 2018, several panelists provided position statements about those issues.

Keywords: Research, Practice, Integration, Relevance, Relevance vs. Rigor.

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1 Introduction

This paper recounts the major points and key insights from the Senior Scholar panel at ICIS 2018. The original idea for the panel emerged from the observation that practitioners had stopped attending ICIS and our other academic conferences outside of their invitations to participate in a CIO forum or present a keynote presentation. From this observation came other questions. For example, why do practitioners not read our papers? Is that a problem? We invited Senior Scholar community members to provide comments on these issues. In the end, six members of the community responded positively to the challenges that Dennis Galletta originally formulated.

We can conceptualize the debate around the two antipodal ideas that the "sky is falling" versus the "sky is *not* falling", an aphorism attributed to a fantasy character called Chicken Little. The first set of contributors generally believe that the field does have a problem in relating to practitioners (i.e., that the sky is indeed falling). However, other contributors contend that the field does not have such a problem, which leads to a set of views that span the gap between these two extreme ends of the debate continuum. Of course, the Senior Scholars who contribute both to the position statements and to the later discussion have nuanced views that a short paper cannot fully reflect. Nevertheless, they would seem to have expressed their ideas sufficiently to stimulate thinking and further discussion.

As we started discussing the issues, it became clear that we could not really address this issue without taking a historical perspective on how the relationship between IS academics and practitioners has developed over the last 50 years. We have doubtlessly become "more academic", and we now more resemble fields such as economics and management science. While some scholars embrace this development, a growing number of business school academics (see, e.g., several papers in *Harvard Business Review* (*HBR)*) and IS academics in particular question it.

We structure this paper to reflect the fact that we have seven position statements that could each stand on its own. In their position statements, the authors analyze what they see as the root problems and possible venues to address the problems. The position statements differ dramatically in tone and depict diverse levels of urgency. Accordingly, readers should understand that we could not possibly consolidate and integrate the very diverse views into one brief, coherent strategy. However, we believe that the value will *not* come from our agreeing on a compromise formulation but from readers' contrasting the diverse views in order to form their own opinion and individual responses to the challenges. We present the statements in the following order:

1) Where do we stand compared to other business fields (Dennis F. Galletta)
2) A bit of history (Ephraim R. McLean)
3) Societal impact of our research (Niels Bjorn-Andersen)
4) Business research, prestige vs. impact (James C. Wetherbe)
5) If You want to be loved, be lovable (M. Lynne Markus)
6) The sky might not really be falling (Detmar Straub)
7) The sky is an illusion (Dorothy E. Leidner)
8) Some promising initiatives (Dennis F. Galletta)

After the position statements, we briefly summarize some of the questions and answers raised at the panel and the panelists' brief responses. We conclude the paper by discussing some promising initiatives and summarizing the issues, problems, and solutions.

2 Where do We Stand Relative to Other Business Fields: Dennis Galletta

The IS academic field began in the late 1960s and early 1970s with heavy involvement from practitioners as Ephraim McLean explains in his position statement. The Society of Information Management constituted a primary force in the field's development by funding the *MIS Quarterly* (*MISQ*) and the ICIS Doctoral Consortium. In those early days, all submissions to *MIS Quarterly* required one or two practitioners as reviewers, which suggests both practitioners' importance to our research and our research's importance to practitioners at that time.

Fast forward to 2018 and the picture differs dramatically. Statistics from other associations put the IS field at the bottom of all business fields on two measures: practitioner members of the field's top association and practitioners who attend the field's top international conference. While these measures do not represent the only measures of practitioner-researcher integration and I do not claim them as best measures, the current situation is quite striking. Our field is likely not inherently less interesting for business professionals than other fields. In an informal study I conducted (Galletta, 2016), I sampled three papers in the top journals of each of seven business fields and found that the relevance of those papers to practice did not markedly differ among the fields. That is, while some claim that our papers are unreadable and unsuitable for practitioners, those papers do not seem to have an obvious shortfall of relevance among our "cousin" business fields.

While we appear to exhibit similar relevance in our research as other fields do, we are notably far less integrated with practice on the two measures: AIS membership and ICIS attendance. Table 1 provides statistics from another informal study regarding several fields for which I could obtain data. I acknowledge that many other conferences in both business and non-business fields and in the information systems field itself may exist. Therefore, I do not mean the list to represent an exhaustive analysis but simply to support the choice to run our panel. I leave it to other researchers to examine this issue more thoroughly.

Table 1. Practitioner Membership Rates and Practitioner Attendance for Most Business Fields plus ACM

Society	Age (years)	Overall Membership	Practitioner Members	Practitioner Attendees	Source
AIS	23	4,500	0.1%	0.3%	Hallway conversation (M. Nelson)
Academy of Management (AOM)	82	18,247	7.1%	N/A	Website
American Accounting Association (AAA)	102	7,500+	4%	5%	Phone call (P. Stein)
Association of Computing Machinery (ACM)	71	100,000	70%	N/A	Phone call (B. Shriver)
American Economic Association (AEA)	132	20,450	N/A	40%	Phone call (L. Hardin)
American Finance Association (AFA)	79	8,000	7.5%	7.5%	E-mail (M. Liu)
American Marketing Association (AMA)	81	30,000+	50%	3% to 6%	Email (M. Weingarten)
INFORMS	82	11,892	18.9%	13%	Phone call (S. Renn)
Society for HR Management (SHRM)	23	300,000	Very high *	Very high *	Website **
Strategic Management Society (SMS)	37	3,000	About 10%	About 10%	Faculty (J. Prescott)

N/A = Not available
* SHRM offers a practitioner certification, which leads to a "very high" practitioner presence.
** Source: https://www.shrm.org/about-shrm/pages/2017-state-of-the-society-for-human-resource-management-.aspx

The table shows a dramatic difference in rates of the two indicators of integration with practice. Table 2 goes a step further in showing how many practitioner members we would have if we would multiply the rates of membership and attendees for each field by our membership and attendee numbers for AIS and ICIS, respectively.

Table 2. Where AIS and ICIS would be with Membership and Attendee Rates of the Other Fields

Society	Practitioner members	Practitioner attendees	AIS would have	ICIS would have
AIS (Current statistics) (Note: based on 4,500 members and 1,700 attendees)	.1%	0.3%	1 to 5	1 to 5
Academy of Management (AOM)	7.1%	N/A	319	
American Accounting Association (AAA)	4%	5%	300	85
Association of Computing Machinery (ACM)	70%	N/A	3,150	
American Economic Association (AEA)	N/A	40%		680
American Finance Association (AFA)	7.5%	7.5%	337	127
American Marketing Association (AMA)	50%	3% to 6%	2,250	51 to 102
INFORMS	18.9%	13%	850	221
Society for HR Management (SHRM)	Very high *	Very high *		
Strategic Management Society (SMS)	About 10%	About 10%	450	170

N/A = not available.
* = SHRM offers a practitioner certification, which leads to "very high" practitioner presence.

3 A Bit of History: Ephraim R. McLean

Eph McLean, as one of three founding Associate Editors of the MIS Quarterly, describes its early history. *He reviews the clear and logical factors that led to the demise of our close association with the Society for Management Information Systems (SMIS), later renamed to the Society for Information Management (SIM). He describes that our field has had to fight hard to earn the same respect afforded to finance, economics, management science, and other fields. Deans and P&T committees likely will need to revisit their key performance indicators before we can expect to expand the impact of our research on practice.*

At an international conference several years ago, a questioner from the floor challenged Detmar Straub (the editor-in-chief for *MIS Quarterly* at the time) with the question: "Why don't practitioners read the *Quarterly?*" He responded with something that I have heard him repeat on several subsequent occasions: "I understand your concern, but the journal is written *by* IS scholars *for* IS scholars, not IS practitioners. Get over it." This quote perhaps best captures the present editorial policy of *MIS Quarterly*, but it did not always have such a policy.

In March, 1977, the Society for Management Information Systems (SMIS)—the Society for Information Management's (SIM) original name—and the University of Minnesota founded and funded the *MIS Quarterly*. (In full disclosure, I was one of the three founding associate editors for the journal's theory and research section.)

The *MIS Quarterly* originally had two sections with equal importance and quality and, hopefully, size: application (which focused on IS practitioners) and theory and research (which focused on IS academic researchers). Two practitioners and one academic would review all papers submitted to the application section, and one practitioner and two academics would review papers submitted to the theory and research section. SMIS justified its financial support of the journal by sending copies to all of its members as a "benefit of SMIS membership."

What happened to this noble idea? Three things led to its demise. First, IS practitioners do not *write about* information systems, they *do* information systems. It soon became evident that it was hard to convince practitioners to write papers for the *MIS Quarterly* (or any other scholarly IS journal for that matter). As a result, the application section became smaller and smaller. Second, it was, if anything, even harder to find qualified practitioner reviewers for the theory and research section let alone the application section. Third, it became clear to academic authors that to write for the application section represented an academic kiss of death. Only a paper in the theory and research section counted in their promotion and tenure (P&T) journey—thus, the sad end to the application section.

To compensate for the discontinued application section, the editor-in-chief at the time, Warren McFarlan, decided, with SIM's encouragement, to provided expanded abstracts for all the papers in the journal (now all just theory and research) that a professional editor created for a practitioner audience.

However, due to the initiative's cost, SIM began to feel some budget pressures, so it asked its members if they found value in this "SIM membership benefit" (i.e., the free subscription to the journal). Sadly, they answered that they did not, and SIM stopped paying to send the journal to its members. Correspondingly, it also stopped paying for the expanded abstracts, which ceased to appear.

Finally, a decade or so later, SIM decided to end all association with the *MIS Quarterly* and passed full title of the journal over to the University of Minnesota. *Requiescat in pace*.

However, the question about the relevance that our research has for practice remains today. Indeed, it leads us to the question: "Whose fault is it; who caused it to happen?" In thinking about this question, I recall the quote from Pogo, a popular political cartoon character of the 70s and 80s: "We have met the enemy, and he is us". If we are upset that practitioners do not read our journals, it is because we do not write for them.

As the IS field has evolved since the mid-1970s when the *MIS Quarterly* began, the leading scholars in our field have worked diligently to have their published work receive the same recognition and respect that has been accorded to finance, economics, management science, and other business-school fields. We look to include our best journals in the *Financial Times* or the UT Dallas lists, and our faculty P&T committees look at publications in these journals as a key input in their decisions.

In making promotion and tenure decisions, faculty P&T committees consider three dimensions: research, teaching, and service. However, these dimensions do not receive equal attention. Most doctoral-granting universities use something similar to these weightings: 70 percent for research, 20 percent for teaching, and 10 percent for service. Further, these committees measure research almost exclusively by counting the number of publications in scholarly journals, which do not focus on professional audiences.

However, our research *does* reach practitioners—through our teaching and different types of service. What we teach in our classes to undergraduates, graduates, doctoral students, and executives and the services and consulting we provide to professional, governmental, and community organizations can have a profound impact on practice. So, if we want P&T committees to recognize the influence that our research has on practice, why not weigh the three dimensions equally? College deans and university review committees may not find such a scheme acceptable, but if enough of us undertook this more balanced evaluation (especially those who serve on P&T committees), questions about our research's impact may go away. It is up to us.

4 Societal Impact of our Research: Niels Bjorn-Andersen

> *Niels Bjorn-Andersen states that he is not worried about the lack of practitioners at our conferences. He says he is worried that we do not attend practitioner conferences or publish in industry/trade magazines. He says that we are becoming a "tax on students and the working class" and their patience and pocketbooks are wearing thin. We are in danger that students and taxpayers will stop funding us. He reviews some ongoing initiatives and suggests alternatives for measuring the societal value of our research.*

I am not worried that practitioners do not attend our conferences or read our papers. I am worried that business school academics do not attend practitioner conferences, do not publish in industry/trade magazines, and only marginally or even accidentally contribute to solving industrial/societal challenges. We appear to be happy to be a "tax on students and the working class"—we actually seem to enjoy it. The question is whether students (paying high tuitions) and workers (paying high taxes) will continue to fund our party.

4.1 Introduction

In the mid-50s, the Rockefeller Foundation and Ford Foundation published reports that required business schools to become more "scientific". Since then, we have pursued that goal with higher and higher levels of tenacity. B schools and IS researchers have become "real" scientists, which unfortunately means that researchers waste half of their time writing peer-reviewed papers that nobody reads. We do so because such papers constitute **the** key performance indicator (KPI) that P&T committees use to assess our

research. We have reduced scientific value to a number of publications or, just as bad, to citations as expressed in the h-index. As a result, we have increasingly become inconsequential to business and society.

4.2 Challenges to Peer-reviewed Scientific Publications Being the Only KPI

As the number of publications becomes more and more dominant as the sole KPI, the challenges become larger:

- Most researchers do *not* primarily focus on finding new knowledge—they focus on publishing papers in the best journals, and the more the better (almost at any cost!).
- H-index encourage dysfunctional behavior (i.e., publish anything as long as it counts).
- We publish (almost) the same research results in many different places—both in conferences and later (often much later when the issues are no longer relevant) in journals. The more the merrier. According to Scopus for 2016, the person with the highest number of publications with his name as an author had 7,000 peer-reviewed papers.
- Accordingly, 27 percent of papers in natural science, 32 percent in social science, and 82 percent in human science never garner any citations (Briggs, 2017; Ciccota, 2017).
- Most researchers either do something valuable for business/society or they write papers that they use exclusively for their career (performance management). Unfortunately, the two areas do not seem to overlap.
- Some researchers manipulate citations counts (self-citations: “I cite you and you cite me”, etc.).
- Researchers have increasingly started to use contemporary IT (BI, AI, machine learning, big data etc.) to identify the most effective templates and substantially support or even carry out (a substantial part of) the writing tasks.
- Universities are “changing from educational institutions to becoming paper writing factories producing what the rankings reward” (Belkin, 2017).
- Most universities feel pressured to play the ranking game. In fact, “Chinese universities are actively gaming the system” (Belkin, 2017).

Sadly, universities seem to be moving away from the vision formulated by Daniel Coit Gilman, first president of Johns Hopkins University. In his inauguration speech in 1876, he said: “A university means a wish for less misery among the poor, less ignorance in schools, less bigotry in the temple, less suffering in the hospital, less fraud in business, less folly in politics” (Benson, 2017). However, a huge majority of researchers do not seem to care as long as they can increase their publication counts and the h-index. Unfortunately, we even train our PhD students to do the same, which means that this practice will continue for many years.

Accordingly, we should not find the growing critique all that surprising. Titles have appeared that clearly indicate the paper’s main message. Bennis and O’Toole (2007) describe “how business schools have lost their way”. Eckhardt and Wetherbe (2014) state that is it time to start “making business school research more relevant”. Washburn-Moses (2018) report that “we make tenure decisions unfairly”. Finally, Shapiro and Kirkman (2018) provide a wake-up call that echoes Ekhardt and Wetherbe’s (2014) statement: “It’s time to make business school research more relevant”.

4.3 Funding for our Research

Most research funding comes from student tuition in countries such as the United Kingdom (UK) and the United States (US), while taxpayers serve as the major source of funding in many European countries either through national channels or through European Union (EU) channels. Universities also receive support from industry, military, and private research funds, but these sources typically have measures to ensure that they achieve the intended effects of their funding. Student tuition and taxpayer funding, which constitute the dominant sources of funding for universities globally, do not come with such measures.

As long as taxpayers and students continue to fund us, we do not need to change. However, many signs indicate that the situation will not continue. For instance, students have increasingly begun to protest tuition’s economically crippling effect. Universities often use two-thirds of students’ tuition to subsidize/fund research. Governments in a growing number of countries have cut down on university

funding and demanded that universities need to fund themselves (i.e., charge students or get funding from industry). Politicians in a number of OECD countries, where taxpayers (partly) fund research, have demanded that authors assess the "impact" of their publicly funded research (and they do not mean the number of papers and/or citations!).

This situation is like the "emperor's new clothes". We spend a huge amount of effort producing papers that become feathers in our caps. However, the real world is not asking for feathers. Rather, the real world seeks innovation and solutions to real-world problems that we seem to ignore. If we do not change to become more relevant, more and more outside academia will join the critique. We will lose our privileged status of exclusively evaluating the feathers of each other. Politicians and other representatives of society will no longer suppress their critiques for fear of appearing too unsophisticated to appreciate what we offer. They will demand that we drop the feather game or they will continue to cut our funding.

4.4 The Quest for Measuring the Relevance of Research

In several countries, governments have now institutionalized processes to assess research. The Research Assessment Framework (REF) in the UK represents a notable example. In the 2021 impact assessment, which has already begun, the weight attached to societal impact has been increased to comprise 25 percent of the total weight in the REF. The Australian Government has also introduced societal impact/relevance to assess research.

The Danish Government has appointed a committee of university presidents from the four largest universities, two universities from Norway and Sweden, and two experts to advise the government on what to do. The report is due in June, 2019.

We also see that attendance in the Advancing and Evaluating the Societal Impact of Science (AESIS) conference has grown. An increasing number of researchers now focus their research on how to assess what we do by employing "socio-metrics". It is perhaps a symptom of a growing problem.

4.5 Which Societal Goals to Pursue

If we as researchers in B schools in general and in IS in particular should be measured on "societal value", one can use two basic dimensions to do so: relevance and impact. Space does not allow for a long discussion. However, there are at least three options for pursuit of relevance.

First, the REF in the UK suggests five relevance areas that I find cover almost all research: 1) economic factors such as economic growth, employment, and commercial benefits; 2) health and welfare; 3) public policy, law, or service; 4) culture, art, and entertainment; and 5) quality of life and work. Second, the UN has formulated 17 sustainable development goals (SDGs) to transform our world. Third, the Lund Declaration of 2015 specifies that "Europe must speed up solutions to tackle grand challenges through alignment, **research**, global cooperation and **achieving impact**" (emphasis added) (ERA Portal Austria, 2015). The detailed description here stresses "innovation" as the key relevance criterion.

However, we do not seem to care. I do not remember colleagues citing any of the above relevance criteria as key in their research. B-school researchers seem quite content to study different ways of placing the deck chairs or even removing stains on the deck chairs on the Titanic despite the more relevant issue of identifying possible icebergs.

4.6 Solution to Address the Calamity

Again, space does not allow for a long discussion here. However, I point to some initiatives in this area

- Research Evaluation Frameworks (UK 2014/2021). The massive Australian and UK effort to assess research has begun to put more emphasis on impact assessment. The Higher Education Funding Council for England (HEFCE) has increased the weight of "impact" from 20 percent in 2014 to 25 percent 2021 (though, in reality, the increase reflects more than five percent since publications have less weighting as a result).
- Social Impact Assessment Methods Productive Indicators (SIAMPI) identify indicators to assess societal impact, EU funded, with studies in Netherlands, France, Spain and UK) (2009–2011). They are now the basis for impact assessments in the Netherlands.
- IMPACT-EV (EU commission 2014–2017)

- STAR metrics (US NSF 2010–2015)
- ERC Impact Framework (European Research Council)
- Open Science (EU commission, OECD, etc.)
- Researchfish (which > 100 research funding bodies now use, and it seems that industry has begun to strongly back this initiative)

Personally, I do not find any of these initiatives ideal since almost all of them come with considerable costs. Some have estimated that the REF assessment in the UK, which occurs every seven years, costs as much as one-third of the total amount available for research in one year. Furthermore, most of the initiatives rely exclusively on scientific peer-reviewed journal papers (which is exactly what I think we need to get away from), although some attempt to apply much wider measures (e.g., altmetric measurements). Add to this that nobody questions the basic problem that P&T committees predominantly measure researchers with respect to how many journal papers they publish in the best journals regardless of whether the research is innovative or creates societal value. Finally, none of the initiatives include self-assessment as a tool. They all apply the perspective of the funding agency and see researchers as objects.

I propose an alternative to these efforts based on self-assessment. Specifically, I suggest to measure societal value or contribution of a researcher on the following five dimensions:

1) The extent to which researchers actively disseminate their research through public presentations, public media, exhibitions, and so on, to non-academics
2) The extent to which "relevant" non-academic stakeholders pick up the research results (theories, methodologies, tools and conclusions)
3) The extent to which researchers take an active role in offering research based advice in networks, committees, and so on, outside academia
4) The extent to which researchers work in partnership (collaboration, consulting, action research, etc.) with non-academics in order to solve societal challenges, and
5) The extent to which researchers obtain research funds from relevant external stakeholders.

For each of these five dimensions, there is a "definition-graded" five-point Likert scale. For instance, for the first dimension, researchers themselves should choose whether they 1) only write for academic audiences; 2) have identified publications' societal beneficiaries, but nobody outside academia are likely to have read the publications; 3) can identify individuals (e.g., former students) and/or organizations who have read some of the publications but where recognition is limited and results are not likely to have direct effect; 4) have obtained recognition of the theories, methodologies, tools, and conclusions in the relevant sector of society or organizations; or 5) has obtained widespread use of published research in relevant organizations and/or major global associations, regions, global NGO's, other supra-national bodies, and so on.

This self-assessment will not take more than 10 minutes to fill in, and it could then be discussed with a department head at an annual development session, where individual researchers could receive feedback on their performance.

Such a methodology is much cheaper and much more effective than any of the impact-assessment efforts that I mention above. Best of all, it will actually make (a substantial number of) researchers focus on identifying and avoiding icebergs such as the one that prevented the Titanic from completing its maiden voyage.

4.7 Conclusion

Researchers conducting research and publishing peer-reviewed papers without **any** concern for societal value (e.g., as expressed in the five relevance criteria in the UK REF framework and/or in the United Nations' 17 sustainability development goals) are, in my opinion, self-serving, narcissistic, and fundamentally unethical. Since we know that one gets what one measures, society should dramatically reduce performance measures that encourage an almost exclusive focus on peer-reviewed papers, which far too often are not read by anybody. Instead we should develop measures encouraging researchers to pursue societal value.

5 Business Research; Prestige vs. Impact: James C. Wetherbe

James Wetherbe reports that typical tenure and promotion metrics of publications and citation counts are disconnected from what practitioners value. Medical schools integrate research into practice using "translational research", which takes lab research to practitioners (Eckhardt & Wetherbe, 2014). Translational faculty are tenured professors who practice medicine and also work with practicing physicians on clinical initiatives and coauthored papers. He provides five specific recommended initiatives with a caveat that change in universities occurs glacially slowly but has optimism that applying strong scientific capabilities found in business schools will help speed up change.

We appear to be at a critical crossroads in business school research and publication. Outsiders have begun to increasingly scrutinize universities' practice of spending money on research and giving faculty time off from revenue-generating teaching to conduct it. Consider that the Bauerlein, Gad-el-Hak, Grody, McKelvey, and Trimble (2010) in the *Chronicle of Higher Education* have pointed out that most of the research published in scholarly journals never even receives any citations and that two recent papers in *BizEd* (Bizoux, 2018; Glick, Tsui, & Davis, 2018) convincingly argue the current business research model used in B schools is unsustainable. Thus, it has become increasingly clear that schools must rethink their performance metrics and incentives to encourage faculty to produce research with practical value.

Unfortunately, scholars and business practitioners alike have observed that academic research that schools produce often seems to have little (if anything) to do with improving the success of its ultimate customer: practicing entrepreneurs. For example, Pfeffer and Fong (2002) write: "There is little evidence that business school research is influential on management practice, calling into question the professional relevance of management scholarship" (p. 78). And even Bauerein et al. (2010) lament that too much academic research is "redundant, inconsequential and outright poor".

5.1 Metrics and the Disconnect

Why are research and practice so disconnected in business scholarship? The problem is that promotion and tenure committees reward research faculty based on two metrics that have nothing to do with what business needs: 1) the number of scientific papers they write that appear in prestigious journals, which other academics exclusively read and control, and 2) their citation count: the number of times other researchers cite their work.

Neither of these metrics recognizes that research should benefit business practitioners. So professors spend most of their time researching sometimes obscure topics that they think other professors—not business leaders—will have an interest in. P&T committees also strongly enforce the metrics. For example, these committees often fire professors during the tenure-evaluation period if they do not perform well on these two dimensions.

5.2 Translational Research

While many business professors view putting research into practice as incompatible with research universities, they need only to consider medical schools to see that this view has become outdated. Medical schools understand that research driven solely by biologists, chemists, and other research faculty who never treat patients does not serve patients well (Eckhardt & Wetherbe, 2014).

Medical schools integrate research with practice through what the medical community refers to as "translational research". Translational research takes scientific research conducted in the lab and makes it useful to medical practitioners and the general public. Fully integrated translational research faculty are tenured professors who both practice medicine *and* use the latest scientific techniques to answer questions about medical techniques from practicing physicians.

In addition, they often coauthor research papers with basic scientists and collaborate on clinical initiatives with clinical faculty. The work of translational medical scientists means the knowledge-production engines of medical schools advance basic science, applied science, and the practice of medicine.

Why should business research and business professors differ?

5.3 Initiatives to Increase Impact of Research

Below, I present five initiatives that focus on increasing impact and improving the economics of business research.

First, create research centers or institutes that corporations fund and that focus on solving business problems (Baker & Wetherbe, 2012), such as the MIS Research Center at the University of Minnesota, which 30 corporations in the Twin Cities sponsor to investigate innovative uses of information technology (Wetherbe, 2001). Wetherbe (2016) provides a case study that illustrates how to achieve funding of a corporate funded research center.

Second, encourage and reward faculty for engaging in meaningful business consulting. Eckhardt (2018) and Eckhardt and Wetherbe (2018, 2016) provide illustrative examples.

Third, encourage and reward faculty to serve on boards of directors to identify and fund business problems. For example, Best Buy faced a crossroads in 1999 when Internet commerce began to grow and its board pondered how to adjust its strategy in this new world of "click vs. brick" retailing. As a Texas Tech faculty member and Best Buy board member, I offered to establish a research program at the school in which a cross-functional team of marketing and information technology researchers could answer important questions about retailing in the future. This program led to a US$500,000 Best Buy research grant and a 12-year research program to investigate Internet buyers' behaviors.

Fourth, create and support translational business faculty appointments for professors who have training in scientific research techniques and also want to participate in business practice. Concurrently, create/support translational business journals, treat them as prestigious, and reward professors who publish in them. To speed up publication and share papers in a timely manner, online social media-based journals such as HBR.org and EIX.org need to become the norm rather than the exception.

Fifth, when evaluating faculty performance, include business consulting activity and corporate-funded research and its impact on businesses.

5.4 Conclusion

The old adage goes that it is easier to relocate a cemetery than to get faculty to change. Survival can serve as a strong motivator. Getting business professors to change their research agenda requires faculty leadership and deans who embrace fundamental institutional change. We can influence the Association to Advance Collegiate Schools of Business (AACSB) to embrace impact and translational research.

While such change is never easy, business schools have a strong scientific capability to build on. They only need to apply that capability to issues that organizations that employ their graduates find much more relevant.

6 "If You Want to be Loved, Be Lovable" (Ovid): M. Lynne Markus

M. Lynne Markus provides a provocative title "If you want to be loved, be lovable" (a quote from Ovid). She divides our field's issues into design and implementation and encourages us to test a hypothesis that our problem constitutes one of design rather than implementation. She discusses implications of two practice-relevant research models—translational research (Eckhardt & Wetherbe, 2014) and policy research (Majchrzak & Markus, 2013)—and where we should go from here.

Since the earliest days of our field, IS academics have questioned our relationship with practitioners. Now more than ever, it is timely for us to do so when the academy as an institution is under attack on economic, social, moral, and political grounds and when regulatory bodies around the world have challenged us to demonstrate that our teaching and research have impacts (not just impact factors) commensurate with societal investments in them.

Historically, much of our discussion on the topic of the university–practice relationship has had a distinctly whiny tone: we *are* relevant, so why do practitioners not appreciate us for what we do? Today, it seems, we are more willing to accept Ovid's advice—to question our appeal to practitioners and to try to improve it. But, if we have a problem here that we can solve through our own actions, we need to be sure that we really understand the problem we need to solve. Does our problem concern *how* we are doing what we

are doing now (an implementation problem) or does it concern *what* we are actually doing (a design problem)?

If an implementation problem, solutions might include welcoming practitioners to our conferences, reinstating "applications" sections in our journals, creating research centers funded by corporations, investing in research impact assessments, establishing certificate and executive doctoral programs, and building better relationships with our undergraduate and graduate students. Defining our challenge as an implementation problem represents an attractive proposition given that none of these solutions requires fundamental change in our traditional research and teaching practices.

For the sake of provocation, however, I propose that we should test the alternative hypothesis that we face a design problem, which means that we should evaluate solutions that demand considerable change in what we are doing now, particularly in the area of research. Is IS research relevant enough to practice? And, if not, how can we make it more so? Do other fields offer models that we could and should emulate?

One cannot do justice to these questions in a short position paper. We must concede two obvious points at once. First, IS scholars already do much research that focuses on practice and has impactful outcomes. Examples include the IT management literature and the design science movement. Second, even if we decided that IS research should become more relevant to practice, that does not mean that *all* IS research should be. We should think of our collective body of knowledge as a *portfolio* of research outputs that span the gamut from basic to highly applied.

I propose that we thoroughly explore the implications of two practice-relevant research models that other fields have offered: 1) the medical school model (as Eckhardt and Wetherbe (2014) have also suggested) and 2) policy (Majchrzak & Markus, 2013) and evaluation research (Pawson & Tilley, 1997).

The first model has various interesting possibilities. For example, one possibility concerns evidence-based practice, which the management field has adopted (Rousseau, 2006) and researchers have introduced to the IS field (Wainwright, Oates, Edwards, & Childs, 2018). Another possibility concerns translational research, which differs from basic research, clinical research, and population research (Rubio et al., 2010). Large-N primary studies (experimental and analytical), meta-analyses, and systematic literature reviews characterize the medical school research model. The unit of analysis is usually micro, often the individual. The best primary studies report unexpected adverse outcomes and effect sizes for the hypothesized outcomes of interest. An example research question relevant to IS is: how much does adopting enterprise architecture improve an organization's performance?

I believe that much research done in our field today under labels such as analytics and big data, economics, meta-analysis, and design science would fit the hallmarks of research in the medical school model. It would be an interesting exercise to assess how much and how well IS research covers the research subtypes found in the medical literature.

The second model, policy and evaluation research, opens up opportunities for practice-relevant research to many qualitative (in addition to quantitative) social science-oriented IS scholars, who have traditionally stood aloof from outcome-oriented research. A focus on social or causal processes or mechanisms rather than causal effect sizes distinguishes policy and evaluation research studies. These studies often target macro units of analysis (organizations, networks, programs, projects, systems) and, as a result, usually involve small "Ns". They display a marked concern with unintended behaviors and outcomes in addition to exploring how interventions work (when they do). Examples of IS-relevant research questions in the policy/evaluation model include: how, where, and why do (or did) enterprise architecture program(s) achieve expected organizational outcomes? How and why do (or did) implementation strategies affect the outcomes achieved from enterprise architecture programs?

Here, I think, we would find many fewer IS research studies that fit the policy/evaluation model than studies that fit the medical school model. We doubtlessly have many case studies that provide evidence bearing on policy/evaluation questions but, I believe, many fewer qualitative studies that researchers have *expressly designed to answer them*. Instead, many qualitative researchers seem to design their studies primarily to illustrate and extend grand social theories (Avgerou, 2013) rather than to shed light directly on practitioners' concerns.

A move to conduct more IS research in the policy/evaluation tradition would face several challenges in addition to contemporary IS qualitative research practices. First, business organizations (especially) will not always willingly expose themselves to investigations designed to reveal their missteps and their "best practices". Second, publishing IS policy/evaluation studies would require reviewers and editors to

embrace an expanded view of theory (Majchrzak, Markus, & Wareham, 2016). Theory in the policy/evaluation tradition is critically important, but theory is not a "three-letter acronym (bigname, date)". Instead, theory can richly articulate why a problem exists or persists and cover both actors' understandings and sociotechnical conditions. Or theory can pose a clear argument about how an intervention should address a problem. Rather than "comprehensive models", policy/evaluation research needs to compare rival theories that would enable researchers to distinguish between bad interventions and good interventions that have been poorly implemented. In addition, rethinking our view of theory would force reviewers and editors to confront and possibly modify how we understand *causality* (Markus & Rowe, 2018). In particular, we would need to accept causal explanations with only contingently general claims.

In short, I challenge IS researchers to engage the hypothesis that we are not loved by practitioners *because we are not lovable*; that is, because we are not doing enough of the kind of research that practitioners would love. I propose further that we particularly lack a strong body of IS policy/evaluation research. Although we face numerous obstacles in growing this part of our field's research portfolio, I believe the impacts on practice would make the effort worthwhile.

7 Old Wine in New Bottles: The Great Rigor vs. Relevance Debate: Detmar Straub

Detmar Straub begins his statement of position with a quote from a famous mistaken barnyard fowl: "The sky is falling! The sky is falling!". His position is that Chicken Little might be overstating the issues given 19 ways in which we do influence practice (an augmented list from Straub and Ang (2011)). He states that, not only is the sky perhaps not falling at all, but it is difficult to study the phenomenon of whether the sky is actually falling due to data-collection problems.

7.1 The Past: Reconsidered

"The sky is falling! The sky is falling!"

—Chicken Little

The IS field has debated the serious question of whether it has sufficiently connected to and addressed practitioner concerns likely since its beginnings in the post-World War II period. Researchers have made various arguments about whether the sacrifices that need to be made to make research rigorous result in findings that have an impact on the world of praxis. While this issue resurfaces every few years, one cannot doubt that the IS researchers have serious disagreements about the truth or falsehood of the claim that we are or are not relevant.

I reduce the complexities of this great debate to whether Chicken Little is right and the sky is actually falling or if he is being histrionic and greatly overestimating the problem (and causing undue panic). I believe as I always have (Straub & Ang, 2011) that Chicken Little is very possibly overstating the case. With that said, note that I assuredly do not mean that the field *definitely* has a major impact on the real world. Rather, I make the point that we do not really know because no one has actually ever studied the matter from the standpoint of whether we transfer our knowledge effectively to praxis. I sense that we are relevant, but others clearly hold the opposite opinion, which we can see in my colleagues' contributions in this paper.

But rather than agree with Chicken Little that the sky is falling, I would rephrase my position as: "If the sky is falling, what is the evidence for it?" and "Might the sky not be falling if we were to rigorously and scientifically examine the evidence for and against the 'sky is falling' proposition?". What would it take to put forward a set of hypotheses and gather data to test these contradictory knowledge claims?

7.2 What would be Scientific "Evidence" that the Sky is Not Falling?

For starters, I would argue that opinion pieces in the *Wall Street Journal,* many (perhaps even most) papers in the *Harvard Business Review,* and governmental studies that worldwide groups with vested interests in the results undertake are not scientific (as I conceptualize it here) and should not be confused with "evidence" as in the evidence-based management movement. In the previous contribution, M. Lynne Markus cites papers in the management (Rousseau, 2006) and IS fields (Wainwright et al., 2018) that

attempt to define what evidence might be. While often interesting, inspirational, and even cutting edge, newspaper articles and governmental white papers are seldom scientific[1].

I hold the straightforward (but perhaps not universally held) view that science concerns building enduring theory of some sort. Whether that theory is descriptive, predictive, explanatory, closed form analytical solutions, or design theory (Gregor, 2006) is not an issue as I see it. Nor is the form of Aristotelian empiricism to show that the theory has merit as a real issue. The world needs scholars to engage in more than unsupported opinions to influence executives, governmental policy makers, and managers in other sectors. If we can do no better than newspaper editorials, even from venues such as the *New York Times* or the *Wall Street Journal*, then we are truly in the wrong business. Governmental and business white papers also have limitations in that most of the people who create these documents neither have training in nor practice science as I describe it above. Thus, treating such sources of information as knowledge claims is problematic.

7.3 What are the Many Ways Whereby We can Influence Practice?

I need to repeat here at least the partial list of the ways in which we might both be relevant, which we could make even more so by transferring this knowledge to praxis. Eph Mclean refers to some such ways in his contribution in Section 3, but I present more below that I pull partially from an *MIS Quarterly* editorial that I wrote with Soon Ang (Straub & Ang, 2011) in which we question the assumption that rigor and relevance are inevitably trade-offs. I have also added other items for recency.

1) Textbooks and other books that reflect the best theoretical and practical thinking in the business fields
2) Higher-education courses and degree programs
3) Non-credit continuing education programs for edification
4) Short courses or seminars (e.g., for continuing education certificate units)
5) Public speaking engagements by academics
6) Newspaper articles and media appearances
7) Brochures that describe in lay terms research centers' ongoing research
8) White papers that popularize IS scientific findings
9) Teaching students the principles of IT consultancy throughout the curriculum
10) Corporate training by academics
11) Certificate programs
12) Collaborative research between academics and practitioners
13) Sponsored conferences based on research findings and made available to both academic and practitioner audiences
14) Faculty internships in organizations
15) Findings presented to university advisory groups
16) White papers and policy briefings that researchers distribute to targeted lists and/or that appear in specialized journals such as the proposed *IS Policy Letters* (Lucas, Agarwal, Clemons, El Sawy, & Weber, 2013)
17) Executive doctoral programs
18) Academic-practitioner journals (e.g., *MISQ Executive*, *Academy of Management Executive*)
19) Scholarly journals (a very limited part of overall knowledge transfer to praxis)

If we reconsidered some of these mechanisms, we can see that executive doctoral programs (#17) have really come into their own lately with a worldwide interest in starting and nurturing such programs. As for white papers that popularize scientific findings (#8), I need to mention that many research centers already write such papers. One notable example includes the Temple University Fox School's series by the

[1] The points of view expressed in this panel, including my own, are much closer to being opinions than "evidence". That is not to say these ideas might not still be useful to suggest ways in which we ought to study the generic topic, but I doubt if any of my distinguished colleagues would argue that we are engaging in a scientific discussion here. If they indeed would, then mea culpa, but I do not see our opinions, however articulately expressed, as "science".

Institute for Business IT (IBIT), which writes MIS faculty research in such an accessible way that practitioners can readily find valuable lessons for managing their organizations.

Klein and Rowe (2008) discuss an interesting combination of higher education and degree programs (#2) and collaborative research between academics and practitioners (#12). They propose that PhD students should write their dissertations through internships and collaboration with practitioners. While we might always desire that PhD students should do so, they would actually only do so *where appropriate*. It certainly does not apply in all cases.

I feel that the effect we have on our business students, many of whom either take basic IS courses or major in the field, represents the greatest impact that we have on practitioners (#2 and, to some extent, #1). This effect occurs at all levels, but the undergraduates obviously represent the most numerous group. Let me mention a simple example in my own case and show how it translates into difficulties of actually studying the true impacts.

I have regularly taught both an undergraduate and graduate course in "Global Systems Sourcing and Integration" since the late 1990s. This course teaches the major intellectual lesson that outsourcing for cost reasons alone (after subtracting transaction costs) represents a short-sighted management practice; the initial consideration in any sourcing decision should be strategic (i.e., does outsourcing allow the organization to better focus on its core competencies?). Any well-informed educator in our field would obviously recognize that theories such as the resource-based view of the firm, agency theory, transaction cost theory, relational theory, institutional theory, and so on lie behind these arguments. Moreover, such an educator would also know that an active IS outsourcing research stream that speaks to this critical managerial decision exists, which excellent meta-analyses such as the one that Lacity, Khan, Van, and Willcocks (2010) provide illustrates. When students learn the thinking that lies behind this past literature, educators can also introduce them to the lively IS outsourcing empirical stream, which future managers enthused by the concept of evidence-based practice in particular may find helpful.

How would I know whether my course on global IT sourcing has had an impact? I could, of course, look to the deeply flawed student evaluations. But an even more interesting and perhaps valid way to test this might be to ask students two to five years afterwards whether they remembered a course concept and to report if it affected their thinking and decision making on sourcing decisions and relationships. Of course, this kind of retrospective data would suffer from all the standard measurement problems, but a study of this sort would at least be in the right vicinity. It would not be asking practitioners if they have read the latest issue of *Information Systems Research*—a trivial question because we know in advance that they have not.

7.4 Difficulties in Studying the Phenomenon and Studying the Wrong Phenomenon

I would be the last person to suggest that it would be easy to study the impact this or any course (let alone a whole degree program) had on practice. Should one gather information from students immediately after, six months later, or five years afterwards? Each approach naturally has pros and cons. Whereas students may not remember the details of the theories and empirical evidence a course introduced them to, they may remember the ideas themselves. How can a researcher effectively separate them out?

Can we study the transfer of knowledge to praxis at all? I think we can, but we should avoid at all costs studying the wrong thing and then claiming that it has a major bearing on whether we influence practice since doing so would constitute the classic case of the "dog barking up the wrong tree". Please note that scholarly journals as a mechanism for knowledge transfer fall at the bottom of the list (#19). It is almost completely beside the point that practitioners read or do not read (the latter being almost certainly the case) scholarly journals. Executive doctoral programs may be increasing this impact mechanism slightly, but overall they do not represent a good way to measure our effect on praxis. Why? The whole point of a scholarly paper is that one must learn its rigor. Scientists write papers for other scientists who can review and verify their scientific worth. They cannot do so if they subjugate the science to ensure that wider audiences find its meaning accessible, which explains why the other 18 mechanisms play such a decisive role in engaging practitioners in the value propositions of our work. Certainly, we can popularize our research for executive, managers, non-profit officers, and government officials (Lucas et al., 2013), but doing so is a far cry from thinking that practitioners can ever truly read and use every paper in the IS scientific opus; admittedly, some papers can be useful for students, but the vast majority cannot in my humble opinion.

So, the bottom line: do we impact practice? I see many reasons to believe that we may. Of course, if a teacher is plain incompetent or if curriculum redesigns or outreach programs do not really consider practitioners' underlying needs, then the final answer may end up being "no". But in that we have never really studied the question of knowledge transfer to praxis, I have to remain agnostic on this issue at this point[2]. We have the means, I trust. How effectively do we deploy them? It remains unclear at this point in time.

The younger scholars in our field might find this issue worthwhile. And if they can determine how to study it well and produce interesting results, they will advance our knowledge of how we, as a scientific community, are doing and help to explain whether the sky is falling or not. But, in the meantime, it might be helpful if Chicken Little does not start a panic in our community by yelling so loudly that the sky is falling when we really do not know whether it is or not (scientifically speaking).

8 The Future Starts With the Present: Dorothy E. Leidner

Dorothy Leidner reviews the AIS's mission and finds that we have made great strides in academic leadership but that we have not become the premier professional association for practice. Practitioners seem to prefer to meet academics who are affiliated with elite schools, and academics seem to prefer to contact those practitioners in high places in highly visible firms. She provides a solution and roadmap for focusing more on our students as future practitioners.

The specific question at the center of this panel discussion is why so few practitioners (if any) join AIS or attend AIS conferences. If low practitioner attendance represents a problem, what steps might we take to address it? Generally speaking, this question falls in the scope of discussions on the relevance and impact of research and questions of whether we should at least partially judge our research based on its contribution to practice. In this position statement, I touch on both the specific question and the more general discussion.

AIS's mission, worth repeating, is to serve "society through the advancement of knowledge and the promotion of excellence in the practice and study of information systems". The AIS states that it represents the "premier professional association for individuals and organizations who lead the research, teaching, practice, and study of information systems worldwide". The association has made excellent strides in becoming the premier professional association for individuals who lead the research, teaching, and study of IS worldwide. Clearly, it has not made commensurate strides in becoming a premier professional association for individuals and organizations that lead the practice of IS worldwide. Why? And should AIS do more on this front, or is the mission unrealistic? I presume that AIS has not attracted practitioner membership primarily because other organizations, notably the Society of Information Management, already serve this need. Although SIM bills itself as society of "senior-level" IT professionals, over the years, the organization has admitted members at two ranks below that highest level and many IT consultants. Indeed, the SIM has over 5,000 members. In other words, one does not need to wait until one has become a CIO or senior VP of IT in an organization to join SIM. It is not clear what the SIM practitioner might gain from also joining AIS.

Being an AIS member has two major benefits: knowledge (access to the AIS eLibrary) and community (meeting with colleagues once or several times a year at conferences). Only if practitioners found value in the eLibrary or in attending academic presentations and meeting academics would we entice practitioners to join AIS. This is then where the specific question of why so few practitioners join AIS (and whether, or not, this is a problem) converges with the question of whether our research should be relevant, and not relevant in a cursory few sentences appended to a discussion but relevant in changing the way IS is practiced in organizations. More practically, it raises the issue of whether practitioners have anything to gain from networking with academics at academic conferences.

I believe that 1) not all academic research needs to be relevant in the sense of changing the way that organizations practice IS and 2) practitioners have as much interest in attending our conferences as we have in attending theirs. As the previous position statements note, either student tuition or taxes largely fund our research. Thus, I do not know why we should target our research toward industry. If our field

[2] On the issue of whether we are engaging in the topics that interest practices, there is actually some empirical evidence that we are. See Straub and Ang (2011) on this. One of the best examples of empirical work that shows this is Baskerville and Myers (2009).

produces some research that has high value to practice, then we need not concern ourselves that not all research produces such relevance. Nor should we feel concerned that most research does not produce such relevance—if all of our research was relevant to practice, there would be much too much relevance for practice to even absorb. We should at least consider the possibility that practitioners do not need more relevance from our research—they have neither the time nor the absorptive capacity to process it. Some researchers might have a particular affinity for practice-based research and frequently grace the pages of *HBR* and other outlets of interest to practice. Others might have no affinity for such research. Both provide equal value to the field. As long as we continue to produce some research that has relevance to practice, I think we need not concern ourselves that our research lacks "enough" relevance. I go so far as to believe that even research that should change the way that organizations practice IS will not do so unless the "right" outlet publishes it (e.g., *HBR*, *SMR*, or major news outlets) or the "right" people (individuals with affiliations to the top 10 or 20 business schools) teaching in company-specific executive-education programs present it. In this sense, one may confine relevance to a certain few with elite affiliations. It would run counter to progress in the field to encourage all researchers to focus on relevance. Our field benefits when we have avenues that enable researchers from all backgrounds, affiliations, and budgets to flourish.

Some governments have begun to redefine relevance, as Niels Bjorn-Andersen notes in Section 4, based on research's societal impact. On the surface, this redefinition may appear to represent a means to assuage taxpayers' and politicians' concerns about using taxes to support research that does not appear to immediately benefit society, though we should have great concern about it. Researchers have long enjoyed a system wherein other researchers, through rigorous review processes and through reading and use, determine research's relevance. However, we did not always do so. Back when the Catholic Church constituted a major force behind the legitimacy of governments in Western Europe, it considered research that ran counter to the its doctrine as irrelevant (at best) and resulted in the researcher's excommunication (at worst). Galileo represents perhaps the most famous example of a scientist who faced excommunication for research findings that contradicted the Catholic Church's religious doctrine. We should all have concern at the notion that a government agency has the power to determine impact. Such agencies will value research that supports the political thinking at the time—whether it results from religious doctrine or secular morality. These agencies will count research whose findings run contrary to the dominant political thought as irrelevant. Researchers risk no longer receiving funding—at which point we will have gone full circle. To best ensure that research advances knowledge, scientists themselves and not funding agencies need to evaluate impact.

In terms of increasing practitioner-researcher interaction via practitioner attendance at our major conference(s), I opine that practitioners have as much interest in our conferences as we have in theirs. I do not mean to suggest that neither has any interest in the other's conferences but that people are busy, time is valuable, and budgets have limits. When individuals need to make a decision, they will naturally tend to attend the event where they meet other individuals in similar positions and who face similar challenges. Hence, academics will naturally tend to go to academic conferences and practitioners to industry conferences. That said, small, ground-up initiatives to bring practitioners to ICIS (the CIO Forum, for example) and initiatives to bring academics to industry conferences provide a valuable mechanism for practice to interact with research, such as the Academic Track at SIM Connect Live that Mary Sumner and Michelle Kaarst-Brown organized. The 2018 SIM Connect Live (the SIM major annual conference) included, for the first time, an academic track wherein invited academics presented their research to practitioners. For each presentation, a practitioner served as discussant. The track ran for two days. On the first day, authors of outstanding *MIS Quarterly Executive* papers (nine in total) gave 15-minute presentations about their research. One hurdle concerned cost—in the same way that practitioners might not see the value of an US$800 registration to ICIS, academics did not see the value of a US$1000 registration for SIM Connect Live. The presenters' registration was eventually covered at no cost to them. It is these types of innovative initiatives that will provide opportunities to bridge the academic-practice divide, but we must recognize that few individuals on both the practice and academic side will have the financial and time resources to benefit from them.

Ultimately, though, if we truly desire closer ties to practice, I think we need to focus more on our relationship with current students than on our relationship with practitioners, which explains what I mean by "the future starts with the present". Our present students will become future practitioners. If these students do not find our research relevant now or in the future when they begin working, then we have no reason to expect that practitioners will find value in our work. One way to strengthen the tie between research and students involves encouraging more undergraduate and master's students to participate in

research as researchers and not subjects. In countries where undergraduate and masters' students conduct theses, academic institutions have a better connection with practice. When students learn to appreciate the value of research as students, then they will also be more likely to value research once they become practitioners. In the United States, few business undergraduates and MBA students conduct theses, which sends the message that academic research lacks relevance for students and/or lacks relevance for their work once they become practitioners. We should also incorporate our research into our (both undergraduate and graduate) classes so that students learn about and appreciate our research. Instead of viewing students as consumers of our knowledge, we might need to start thinking of them as co-creators and look for ways to involve them in our projects. No doubt other ways to strengthen this relationship exists. I think this issue deserves as much, if not more, attention than how to strengthen the current ties with practice. If we start by strengthening the connection between our students and our research now, then, ten years from now, this issue of our research relevance to practice and practitioner involvement in AIS might have resolved itself.

9 Some Promising Initiatives: Dennis F. Galletta

Compared to other fields, some evidence shows that our field has the lowest amount of integration with practice (as measured by the proportion of practitioner members in AIS and at ICIS). The panelists raise many issues on both sides of the debate: some raise our level of concern and some provide some assurance that we have either imagined the problem or that we should not consider the surprising measures in the first two tables in this paper as harmful. At the same time, I believe that we have much research that we have "left on the table" and not communicated to the practitioners in our field. I also fear that regulators might oversimplify our current situation and misinterpret our low amount of integration as a lack of relevance.

As many of the panelists note, many in our field have connections with practice. For many years, ICIS has held the CIO Forum, which brings high-level practitioners to the conference (although most do not register and do not attend our paper sessions). Also, a high-level practitioner from business or government usually provides a formal keynote speech at ICIS. Our journals also have reached across to practitioners. *MIS Quarterly Executive* constitutes one journal that explicitly focuses on practice.

While we do have these excellent resources, some researchers still have a nagging feeling that, while relevant for practice, much of our research never reaches practitioners. Our feeling might be overly pessimistic as Detmar Straub and Dorothy Leidner note in presenting many mechanisms for reaching practitioners. Also, we have recent, strong evidence of a spreading movement to try and communicate more with practitioners, such as with medical translational scientists (Eckhardt & Wetherbe, 2014). New modes of communicating with practitioners have sprung up over the past year.

Some promising endeavors have entered an advanced development phase: *MIS Quarterly* has recently begun a new collaboration with Massachusetts Institute of Technology's *Sloan Management Review (SMR),* which will turn selected papers into practitioner-focused pieces in *SMR.* Also, Science2Practice.org—a new global endeavor by *European Journal of IS,* Universitat Liechtenstein, and the Operational Research Society—focuses on bringing research to practitioners.

Separately, directly inspired by Ekhardt and Wetherbe's (2014) description of translational scientists in the medical field, at Seoul in 2017, Dennis Galletta sought and obtained approval from the Senior Scholars to create practice-focused short reports from current AIS publications under the name "InPractice". AIS went on not only to approve the proposal as a service to members but to provide a start-up editing budget. As of this writing, the AIS Council is considering combining this initiative with Science2Practice, which it has adopted as an AIS initiative.

The concept involves inviting authors of AIS journal publications to write a much shorter version of their paper after some surgery to remove scientific jargon and fine details that only statisticians and other researchers appreciate. The shorter versions would also contain a link to the full papers for those interested in more depth.

AIS has about 4,000 student members (Nelson, 2018). If they received quarterly or perhaps eventually monthly newsletters, some would hopefully retain their memberships after graduation. If we estimate that two percent converted their student membership to practitioner memberships, over 10 years we would have 800 practitioner members.

9.1 Key issues Raised in the Panel's Question and Answer Session

In this section, we capture and summarize questions from the audience and answers from the panelists (if provided). In all cases but one, the panel moderator identified the audience member.

9.1.1 Fred Niederman, St. Louis University

Fred Niderman: This is all well and good for PhD granting institutions, but what about the considerable amount of research done by the other 75 percent of AIS members? The *MIS Quarterly Executive* essentially aims to make *MIS Quarterly* type of research accessible to people in industry, but I would argue that there is a good deal of research (HICSS is filled with it) that has no intention of operating at the level of *MISQ* precision, definition, and academic language yet would be of clear interest to folks close to practice. I think it is a typical senior scholar mistake to view the field as only the basket of eight (or subbasket of two) and try to get practitioners interested in this material rather than addressing the array of less precise but more applicable work in which they'd be interested that is already being done.

The panel did not address this point as they mainly perceived it as a comment and not a question.

9.1.2 Merrill Warkentin, Mississippi State University

Merrill Warkentin: We have to distinguish between basic science and applied science. We sometimes do basic science and the usefulness takes time to emerge. All knowledge has its own value and knowledge for its own sake is worth discovery, and therefore publication. You never know what knowledge may become valuable years later! It's the basic distinction between science and engineering. I think that we in MIS are scientists, not engineers.

Jim Wetherbe replied that he has negotiated that a percent of applied research—for example 25 percent of a US$2 million FedEx grant—could be used to fund academic research.

9.1.3 Jason Thatcher, University of Alabama

Jason Thatcher: Is this just an American problem? Master's students in other areas of the world such as Scandinavia read *MIS Quarterly.*

Niels Bjorn-Andersen: We cannot really use the basket of eight in Scandinavia for undergraduate students, and, if we do, most of our students do not read the papers. On the other hand, we can use them at the graduate level but not for executive education.

9.1.4 Rob Nickerson, San Francisco State University:

Rob Nickerson: P&T practices are constraining us. Do we blame the deans and the *Financial Times*?

Ephraim McLean replied that there might not be as many constraints as we have put on ourselves.

9.1.5 Unidentified Audience Member

Are we vulnerable to claims of malpractice or assertions of lack of ethics if we do not respond to the need for greater relevance by using measures of impact of our research to practice?

The panel could not address this point.

9.1.6 Dov Te'eni, Tel Aviv University

Dov Te'eni: There are two possibilities for a lack of integration between academics and practitioners: we either have nothing to transfer or there is no channel by which to transfer research findings. I believe it is the latter. The AIS and *MIS Quarterly/MIT* initiatives are "great" initiatives, and the Science2Practice initiative complements them as yet another channel. The AIS should promote research-practice dialog through all these channels. The ultimate vision is to have a feedback loop between research and practice and the current constraints of our communication channels impose a serious hindrance. Feedback from practice to research could build more of a common ground in our communications with practice, correct and enrich our findings, and direct research to research questions that are more relevant to practice. The full Science2Practice proposal is detailed in a recent editorial (Te'eni, Seidel, & vom Brocke, 2017).

Detmar Straub replied that we are transferring knowledge already as he notes in his position statement.

Jim Wetherbe replied that we should collect teaching evaluations two to five years after a course is offered so people have enough time to apply the concepts and can have a context to evaluate the course's usefulness.

Niels Bjorn-Andersen replied in a similar vein that some influence after graduation should be measured.

9.1.7 Cynthia Beath, University of Texas, Austin (Emeritus)

Cynthia Beath: I believe that the central lesson of our field is that applications of information technology in organizations are sociotechnical systems. Based on my extensive experience in talking to practitioners over the last 20 years (I estimate that, in my field research, I talk to more than 100 practitioners a year about their experiences in trying to get value out of information technology), I conclude that we are not getting our message across to practitioners. The place where I see a serious lack of appreciation of the sociotechnical point the most often is when I study the uptake of new technologies. The primary reason companies fail to get more benefit from new technologies is because they fail to consider the social aspects of the system. So I'd agree that "sky is falling"—we are failing in our central mission and this continues to have major negative impacts on our economies many decades after we first articulated this point.

Detmar Straub pointed out that the argument is based solely on anecdotal information and added that organizations have funded her research. That is evidence that her research impact is being felt.

9.1.8 Murray Jennex, San Diego State University

Murray Jennex: Agreeing with Fred Niederman, at master's-granting institutions, there are many endeavors along these lines. One such movement includes bringing research into the classroom and conducting research symposia for all levels (undergraduate, master's, and doctorate). Industrial representatives provide judging, so we do have more integration with practice than would appear by only looking at our research published in the top journals. This is a California State University (CSU) mandate (the symposia and research relevance that the CSU is the practice system, while the UC was the basic research system).

10 Conclusions

When we compare ourselves to other business school fields, we face competition in different ways. We can see such competition in the difficulties we face, such as losing the MIS core course from several curricula (Gill & Bhattacherjee, 2009), having fewer journals in business school journal lists than other fields (Ormans, 2016), and having more difficulties in reaching the top journals compared to other fields (Dennis, Valacich, Fuller, & Schneider, 2006). Such competition might indicate that people who represent other business school fields do not highly value our work.

Perhaps the newness of our field has caused us to exercise excessive caution to avoid any perceptions that we have relaxed scholarly standards. We have made substantial gains, such as finding that the *MIS Quarterly* has boasted an impact factor that exceeds most other business fields' journals (Management Information Systems Quarterly, 2018). However, the so-called "impact factor" measures relevance according to other academics, not practitioners. Accordingly, such a measure only considers a small (and some would say rather irrelevant) range of all potential stakeholders.

10.1 Translation Contribution Framework

If practitioners and researchers would decide to integrate more closely, we need to evaluate the value of the exchanges that would take place. It would be naïve to state that all research has an equal chance of becoming relevant to practitioners just as it would be naïve to expect that all practitioner activity has an equal chance of becoming reported in research journals. Table 3 offers a two-by-two framework for such exchanges. The two dimensions describe the 1) direction and 2) specificity of the exchanges. The coloration of the cells indicates the likely frequency with which the exchanges occur. The lighter the cells, the less likely the expected transfer occurs at present.

Practitioners → researchers: researchers already benefit from practitioners. They have already conducted many practice studies whether in strategic or operational contexts. Studies published in *MIS Quarterly Executive* overly illustrate such benefits, but many studies across a variety of journals examine practice or use a practice context. Major findings that generalize to many other contexts have the highest

visibility. On the other hand, findings that have rather limited generalizability help researchers understand issues and options for practice, and they are extremely useful for teaching.

Researchers → practitioners: practitioners can obtain information from researchers in many ways. The panelists provide many examples. Practitioners would not necessarily have to read our journals. They could engage researchers as consultants, learn from executive courses, and read our textbooks. However, these mechanisms are only brief and intermittent encounters that fail to keep practitioners continuously up to date with our findings. Practitioners could find value in reading about the major breakthroughs we might create from observing generalized practice and our conclusions from observing specific practices. Those conclusions could help them understand how they might improve practice.

Table 3. Translation Contribution Framework

		Direction of contribution	
		Practice to research	**Research to practice**
Contribution specificity	**General**	**Blockbuster theory** High-visibility generalizations; major industry trends (e.g., cloud services)	**Practice revolution** Practice adopts breakthrough ideas (e.g., systems development lifecycle model)
	Specific	**Evolutionary Ideas** Understanding issues and options; excellent cases; first adoptions of new technologies (e.g., early electronic commerce research)	**Practice refinements** Concrete solutions to specific questions or practices (e.g., website A/B testing; action research)

While practitioners may rarely learn about major scientific breakthroughs at all, they likely learn about major general breakthroughs more frequently than they learn about our specific findings. We speculate that many researchers already likely communicate major breakthroughs in theory to practitioners who participated in the relevant studies, to practitioners who employ consultants, or to the news media. We also speculate that large groups of people outside academia could find the specific findings relevant, such as operational users of new technologies, lower-level managers who manage operational users, high-level managers who acquire technologies, CIOs making strategic decisions, customers, and even members of the general public. However, we not only face difficulty in reaching such people, but they may have limited decision-making power to act on those findings.

We believe that the translational function, in addition to many of the panelists' specific suggestions, could help fill in these two "researcher -> practitioner" cells. Managers need to learn about major breakthroughs in understanding and about specific practice refinements. We believe that value exists in both major breakthroughs and practice refinements and in communicating them as they occur.

We cannot easily summarize the issues the panelists raise in this paper due to their number and variety. However, we provide a cross-section of points that the panelists raise in Sections 10.2 to 10.6.

10.2 Context

Our main academic society (AIS) and our top conference (ICIS) appear to have the lowest proportion of practitioners among all business fields. These limited measures of the academy's integration with practice are informative and rather objective and easy to collect, although we acknowledge that we might need to examine other measures. The lack of integration is striking because we had very tight integration in our field's early years.

10.3 Why has this Situation Arisen?

We write merely for other academics and that limits our impact on practice. People are busy. Time or money are not abundantly available for practitioners to attend our conferences just as we lack time to attend theirs. This situation might change without special interventions, however, as taxpayers, governmental agencies, and accrediting bodies show signs of wanting to see impact on business and society.

10.4 What are the Barriers to such Integration?

Practitioners can find our journal papers difficult to read. The academy not only lacks incentives to integrate with practitioners but also, in some ways, has a hostile view toward such integration. We lack useful, reliable measures to determine the extent of such a hypothetical problem.

10.5 What are Some Encouraging Signs?

Research in our top journal appears to have a similar relevance to research in other business fields. We influence students through our teaching and sometimes through consulting. We have academic/practitioner journals such as *MIS Quarterly Executive* and *Academy of Management Executive*, and we already practice some solutions that we mention in Section 10.6, such as brochures, textbook impact, collaborative research, faculty internships in organizations, and speaking engagements. Executive doctorate programs have also increased.

10.6 What are Some Solutions for Increasing our Integration with Practice?

We should employ "translational researchers" and create channels to communicate results of our research to practitioners. Some such channels (AIS *InPractice*, *MISQ*/*Sloan Management Review*, and Science2Practice.org) have already emerged but remain nascent. The CIO Forum at ICIS and the Academic Track at SIM Connect Live provide integration. We should become more practice-oriented in the problems we study. We should attend practitioner conferences and publish in industry and trade magazines. Other prescriptions might be more difficult, given that the academy changes very slowly: We should assess relevance with measures that go beyond impact factors and bibliometrics. We should work to reengineer university incentive systems for promotion and tenure.

Academics should not only welcome but seek collaboration with practitioners in research to reward value to practice/society. Research centers with practitioner funding could help promote relevance and impact. Consulting activity could help in not only building relevance but also in enhancing the classroom experience. Universities should provide incentives to encourage such practices. We should recognize the value of a portfolio of research that includes basic sciences and practical applications and not promote one type at the expense of the other. We should consider other models of research such as policy and evaluation research. Textbooks should reflect the best theoretical and practical thinking. Public speaking engagements and media releases would raise awareness of our work.

We should enhance teaching effectiveness measures. One idea is to ask former students perhaps two to five years after graduation if they remember course concepts and if they have applied any of what they have learned in their professional positions. We should engage undergraduate and master's students in our research activities. We should incorporate our research into our teaching by describing our studies and their relevance to the courses they take from us. We should formally devote a proportion of research grants from industry to academic research. We should request practitioners to participate in our courses in some way, such as presenters in our classes or as judges for student presentations.

11 Limitations and Conclusions

We recognize several limitations in this study. First, there were no practitioners involved in this panel, so we do not provide insights from the very people about whom this panel was formed! Also, the observations in the paper might exaggerate concerns raised by some in our field, as Detmar Straub and Dorothy Leidner articulate. Finally, our panelists reflect only the United States and Scandiavia. We did not have representatives from other countries with significant concentrations of IS academics in places such as Germany, Australia, New Zealand, or Asia. We would certainly have valued their contributions, and we encourage future inquiry with broad representation across the world. However, while this panel had limited representation, we believe that we uncovered some valuable themes. Further, the presentation at ICIS included attendees from those regions, and, thus, their participation broadened our observations.

The panelists have provided a diverse set of problem descriptions and solutions. After 50 years, researchers should perhaps begin to carve out best directions for the field to follow. Should we continue down the current road and become more and more scientific (like economists or operations researchers), or should we aim for greater integration with practitioners and increase the societal relevance of our research? As the three practice-related initiatives that we describe in Section 10.6 grow, perhaps merge,

and blossom, we might have a different discussion for the 60th anniversary of our field than we have currently for the 50th anniversary. Only time will tell.

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About the Authors

Dennis F. Galletta is an AIS Fellow, a LEO awardee, Fryrear Faculty Fellow and Professor at the University of Pittsburgh, where he serves as Doctoral Director for the Katz Graduate School of Business. He has published in journals such as *MIS Quarterly, Information Systems Research, Management Science, Communications of the Association for Information Systems, Journal of the Association for Information Systems,* and others. For the *MIS Quarterly,* he serves as a senior editor and won a Developmental Editor Award in 2006 and a Provost's Mentorship Award in 2016. He was program co-chair for ICIS 2005 and AMCIS 2003, chaired the first AMCIS in 1995, and co-chaired ICIS 2011. He served as AIS president, ICIS treasurer, AIS council member, and editor-in-chief of *AISWorld*. He was founding co-editor-in-chief of *AIS Transactions on HCI* from 2008-2018 and established the concept of Special Interest Groups in AIS in 2000.

Niels Bjørn-Andersen is Emeritus Professor of Business IT at the Copenhagen Business School where he served as full professor 1987-2016. He also served here as director of the Center for e-Business from 1998-2005 and as director of Center for Enterprise Systems from 2005-2011. He has more than 50 peer-reviewed journal papers, 25 books and more than 200 other publications. He has carried out collaborative research with organizations like CISCO, Heineken, IBM, Microsoft, Maersk, and SAP, and he has been the recipient of more than 20 external research grants predominantly from EU research bodies and industry. He has been awarded the AIS-LEO award, the IFIP Outstanding Services Award, the prize for outstanding services by the Danish equivalent of SIM (as the only academic ever), and has been knighted by the Queen of Denmark for his contributions to the field of Information Systems.

Dorothy E. Leidner, PhD is the Ferguson Professor of Information Systems at Baylor University. Dorothy received her PhD in Information Systems in 1992 from the University of Texas at Austin. She holds an honorary doctorate from Lund University (2018). She is a Fellow of the Association of Information Systems. She currently serves as a senior editor for *Information Systems Research* and for the *Journal of the Association of Information Systems*. She has served terms as editor-in-chief of *MIS Quarterly Executive* and *Data Base for Advances in Information Systems* as well as senior editor for *MIS Quarterly* and the *Journal of Strategic Information Systems.* She is widely published in such journals as *MIS Quarterly, Information Systems Research, Organization Science, Journal of Management Information Systems, Decision Sciences Journal,* and *Journal of Strategic Information Systems,* among others, with over 36,000 citations (scholar.google) and numerous best paper awards.

M. Lynne Markus is the John W. Poduska, Sr. Professor of Information and Process Management at Bentley University and an associated researcher at MIT's Center for Information Systems Research. She has published extensively in the areas of digital business and interorganizational governance, enterprise systems and business processes, electronic communication and knowledge reuse, and organizational change management. Her current research interests include digital innovation in the financial and health sectors, the responsible use of data and algorithms, and the changing nature of work. He was named a Fellow of the Association for Information Systems in 2004 and received the AIS LEO Award for Exceptional Lifetime Achievement in Information Systems in 2008.

Ephraim R. McLean is a Regents' Professor, the G.E. Smith Eminent Scholar's Chair in Information Systems, and the Director of the Center for Health IT, all in the Robinson College of Business at Georgia State University. He earned his BME and M.E. at Cornell University and his S.M. and Ph.D. at MIT's Sloan School of Management. He has published over 130 articles in publications such as *Harvard Business Review, Sloan Management Review, California Management Review, MIS Quarterly, Information Systems Research, European Journal of Information Systems,* and *Journal of the Association for Information Systems*. He is the co-author or co-editor of seven books, including *Strategic Planning for MIS, Management of Information Systems,* and *Information Technology for Management.* He is one of the founders of the Association for Information Systems and served as the AIS Executive Director for nine years. In 1999, he was named an AIS Fellow and in 2007 was recognized with the LEO Lifetime Achievement Award.

Detmar Straub is a Professor and the IBIT Research Fellow at Temple University's Fox School of Business. He is a Regents Professor Emeritus of the University System of Georgia and formerly the J. Mack Robinson Distinguished Professor of IS at Georgia State University. He has over 200 publications in the areas of technological innovation, information security, e-Commerce, international IT studies, and business/social science research methods. His doctoral degree is from Indiana University (1986).

James Wetherbe is the Richard Schulze Distinguished Professor of Business at Texas Tech University with over 40 years of experience in academia and industry. His experience includes professorial and administrative positions in higher education including the Universities of Minnesota, Memphis, Houston, and Texas Tech; and management positions with computing, energy, and consulting companies. He is rated as one of the top 12 consultants and lecturers on MIS by *Information Week* and ranks as one of the 20 most influential scholars in the field. He co-received the first *MIS Quarterly* Distinguished Scholar Award and has authored or co-authored of 36 books and widely published in top journals with over 14,000 citations. He has served as editor for *MIS Quarterly*, *DATA BASE*, and EIX.org. He has brought in over US$15 million in funded research during academic career. He has also served on the board of several major corporations such as Best Buy and CIBER.