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ICIS 2008 Panel Report: IS Has Outgrown the Need for Reference Discipline Theories, or Has It?

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Abstract:

We view the current belief in reference discipline theories and their value in MIS research as exhibiting dominance in our field. This article is based on the panel discussion at the 2008 International Conference on Information Systems (ICIS) held in Paris, France. It examines why this dominance has the potential to harm the future prospects of our intellectual endeavors. In counterpoint, we present the argument that the use of reference disciplines should continue. Aside from the fact that the belief in the value of reference discipline is continued, there are benefits derived from anchoring research initiatives in reference discipline theories which should be acknowledged. Under certain situations such use should be encouraged and broadened. Additionally, we will present arguments for viable alternatives to relying on reference disciplines in theory building. The alternatives are aimed at building and expanding indigenous IS theory.

Keywords: reference discipline theory, theory building, IS research, IS community

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I. INTRODUCTION

The idea of the power of reference disciplines in building IS theory has come to play a powerful, if not dominant, role in the IS research domain. Reference discipline theories have, for some time, enjoyed a privileged position in our theory exercises and constitute a hegemony over our research practice and thinking. Consider for example the AISWorld theory page (http://www.fsc.yorku.ca/york/istheory/wiki/index.php/Main_Page). This is an extraordinary gallery of theory choices, listing dozens of theories with bibliographies showing how they have been used in IS research. These theories range from economics (transaction cost, agency), to psychology (reasoned action), to computer science (complexity), to communications (diffusion of technology), to sociology (structuration). No other field seems to apply such a supermarket approach to theory.

Although our ability to assimilate theories from outside the IS domain has been impressive and useful, has such accumulation of reference discipline theory served the field of IS well in all respects? If so, is it still doing so? Are we too dependent on received theory? What have been the costs as well as the benefits of our heavy reliance on outside theory for the evolution of the IS research? By what means have reference theories come to enjoy such a privileged and dominant status in IS research that they have come to constitute hegemony?

We can determine that entire segments of the IS research community are primarily based on reference discipline theories. For example, much of the diffusion of technology research builds upon Rogers' diffusion theory; much of the decision making literature builds on social cognition and cognitive psychology; much of the group support or organizational impact literature builds on structuration theory; and much of the economics of information literature is based upon a few theories including transaction cost and agency theories. Although theories derived from other disciplines have provided a starting point and a good vocabulary for a significant amount of IS research, has the field perhaps paid a high price in terms of: (1) not adequately developing its own *indigenous theory about IT* and (2) becoming fragmented with researchers often adhering to their original traditions rather than constructing stronger and bolder scholarship focused uniquely on IS phenomena [Hirschheim and Klein 2003]?

The purpose of this article is to debate the *positive* and *negative* effects of the reliance on reference disciplines (and our use of associated terminology of referencing outside theory) on the IS research community. Toward that end, we engage in a debate, or critique, of reference discipline use in IS research. We view the current state of reference discipline theory dominance in the IS field as constituting a hegemony. This discussion examines why this has potential harm. In counterpoint, we will present the argument that reference discipline theory use should continue. In some cases, reference discipline theory has provided benefits such as a starting point for research questions and stable methodology for investigating IS phenomena. This discussion examines how such benefits can and should continue to be cultivated. These positions represent deliberately conceived end points on a spectrum to highlight the discussion.

II. ORGANIZATION OF THE PANEL

The panel was organized and presentation moderated by Fred Niederman. Presenters were selected from among those who have successfully published in major journals and participated extensively in the editing and review processes. The presenters were (in order of their presentations): Dr. Kalle Lyytinen, Dr. Varun Grover, Dr. Carol Saunders, and Dr. Shirley Gregor. Brief background statements for each are presented at the end of the article.

Presenters agreed to take specific positions regarding the overall state of reference discipline theory in the IS domain. The presenters each took a deliberate position that may not have been in concert with their own personal views in order to foster the debate. Kalle Lyytinen presented the case for overuse of and problems from reference discipline theory. Varun Grover presented the case that significant benefits have been derived from use of reference discipline theory. Carol Saunders was charged with describing how future use of reference discipline theory might be extended and improved. Shirley Gregor focused on additional issues with reference discipline theory and alternatives available to the IS community. The discussion below presents the arguments delivered during the session, augmented by additional details and references. The conclusion incorporates ideas and positions that were emergent in the discussions initiated by audience participants and panel member responses. The paper closes with an initial synthesis of ideas regarding the topic.

III. DISCUSSION OF REFERENCE DISCIPLINE THEORY

KALLE LYYTINEN

“Finlandization” of the IS Discipline: The Fate of Reference Discipline Doctrine in the Post Positivist World

The label of Finlandization was launched by hawks in the mid to late 1970s across the Western world as a “coming” new model of how the foreign policy debate in the public sphere would be organized in the West. The claim was that Finlandization would lead to “self” silencing and “disciplining” of thoughts which were outside the “official” doctrine, legitimized by a reference to an established theory of how foreign relationships should be organized. This had already happened in a tiny Western democracy called Finland, and other European countries were to follow. In the end, the notion of Finlandization was not about Finland, but about how the Western world could identify and react to a threat from the Bear in the East to make small, and later other, countries in the West subservient to its interests. In line with this interpretation, the Finlandization of the IS discipline would signify a situation where only ‘certain’ theoretical views will be accepted due to our self-disciplining. We would, in the end, conform to views espoused by our external, powerful reference doctrines.

Another form of Finlandization can be narrated through a well known joke about the Finnish mindset and sense of identity. A common genre of jokes ask, “What different nationalities would do when they face an elephant in the jungle?” A Brit would naturally praise an elephant as the gentleman of the animal kingdom, while a French man would ponder the love life of elephants, and so on. But what would a Finn say? The joke tells us that a Finn would ponder: What does the elephant think of me? Here, Finlandization is about the situation where the IS discipline is worried about what others will think of it, so that it never has time and capability to build its original theories and appreciate the wonders it sees in its environment.

In many ways, recent arguments within our field and about its fate resemble these two images of Finlandization. The arguments around the reference discipline concept and its role are at the heart of some of these arguments. Luckily, the stakes in this sort of Finlandization are small compared to those faced at the height of the Cold War. Moreover, in the academic world the elephants are rare, and they do not really care about us anyway. These two images of Finlandization, however, teach us much about how we are driven by the illusion of possibly seeing an Elephant, and how we might need to self-discipline ourselves in case we were to see it. We approach our theory and why we revere reference theories – collections of explanations and models adopted from other fields of inquiry- under this illusion. Therefore, in this presentation I will argue that the faithful promotion of the concept of a reference discipline has led to a deepening Finlandization of our discipline, and that this impairs the quality of our responses to the intellectual and institutional challenges we face today.

IS Discipline: Bleak Performance in Theory?

IS, as claimed by many, has a dismal and less than average quality tradition of building and sustaining insightful, original, and rigorous theoretical argument [Benbasat and Zmud 2003; Weber 2007]. The questions for us to ask are: How did we get into this, and what can we do to improve our theorizing? What blocks us from improving our track record? The first task then, is to look at where we are and at what prevents us from moving ahead.

If one meanders around our journal pages and tries to make sense of the intellectual landscape they portray, one can readily observe that most theoretical arguments in the IS field are squeezed into the “middle.” They are clean and rigorous, but boring, with few surprises and little passion or excitement! Much of the theory is drawn upon and legitimized by “reference theory” established in other disciplines. This is, in fact, the official doctrine that leads to our Finlandization. What will they think of us if we do not rely on their smart theory and related methods? We need to self-discipline ourselves to promote the doctrines of the external and powerful academic lines and silence views which are against this model and the resulting theoretical views. This has been a way to channel institutionally knowledge transfer and to legitimize “received” modes of theorizing. As a result, the IS discipline is not so much a free market of ideas as John King and I called for [Lyytinen and King 2005] recently, but rather, a catholic church due to our self-disciplining and the Finlandization effect.

As an example, Lynne Markus’ piece on power and politics in 1983 developed a fresh and new theoretical angle of how to examine and explain causes for system rejection using IT based explanations. Though it has received over 300 citations in the past 25 years, with many of them in our top journals, only a small portion of these engage in theory level arguments with the paper. Most of them misuse, use symbolically or just misinterpret the article [Hansen et al. 2006]. As a result, we have seen very few original theory development efforts around this initiative over the past 25 years, though the article is widely lauded as an exemplary piece of novel theoretical thinking.



As a counter example, consider the Technology Acceptance Model (TAM) theory adopted from social psychology and marketing with a full package of instrumentation. This has become one of the most widely applied "theories" in our field. Most citations seen in the recent high *MIS Quarterly* ISI index (5.83) come mainly from a few papers in TAM. This shows that this reference theory has currency (and the article in *MIS Quarterly* has over 1300 citations!). If you examine those citing TAM articles, you can quickly observe (with some exceptions) that these pieces merely inductively reiterate the same canon, thus "corroborating" again that system usefulness and ease of use can lead to higher intention to adopt. These articles show something general about human behavior in relation to technologies, but they tell little about how IT is adopted, used, or how it changes. Personally, I do not see that much in the way of new theoretical ideas has come from TAM in the past 10 years [Journal of the Association for Information Systems (JAIS) 2007 special issue], although the original work in the late 1980's was theoretically innovative and relevant in that environment (how to explain adoption of individual tools like spreadsheets). TAM and some similar theories are now even used at the wrong levels of analysis (organizations), as well as in, and for, the wrong contexts (e.g., can all technology adoptions like ERP or wireless infrastructures be explained with such a simple model?). The reason for the popularity of TAM is that one can now so easily play this reference theory card, and it must be right because it is so widely cited.

TAM is not alone in this process of Finlandization. Similar patterns can be observed in studies of trust or past studies on stages of innovation (remember the Nolan stage model?), cognitive style, and so on. The next fad is probably waiting around the corner!

Why are Reference Theories So Successful?

The reason for the success of many of these reference theories seems to be that they are so simple and general that they can be applied within any, or multiple, contexts (thus allowing for repeated results). They come with a well-packaged instrumentation and analysis route that, in most cases, is sure to guarantee "good" results (the correlations are significant, betas often high, and their R squares are acceptable!). Most importantly, the work is equipped with and backed by some theory (TRA / TBP) which gives all the credibility needed to publish. It is, after all, theoretical! At the same time, however, it relieves us from doing careful theoretical due diligence regarding what sort of theory and theorizing would give new and original insights into this phenomenon! But the reference theory works like magic in the same way Finlandization claimed to work. It makes the research process a simple and straightforward mantra of repeating the same narrative in different contexts and gives results that are statistically significant. It often leads to self-disciplining and silences other voices which would lead us to ask what narrative would best account for the data and the phenomena in our context, thus failing to engage us in the true meaning of the word in the painful process of theorizing.

One interesting part of this reference theory transfer story is that, often, some other equally "relevant" theories get blocked. In Latour's words, "they are not successfully enrolled and are cut off from the network [Latour 1987]." In most cases they are brushed aside because they are not viewed as true contributions for the IS field. Instead, they are now seen as mechanical "iterations" of applying the theory from some other field. Examples of this are numerous based both upon my personal experience working with my own submissions and from what I have seen as an editor in the review reports in top IS journals. For example, we see this with many pieces of theoretical work on organizational learning, exploration, exploitation, radical innovation, absorptive capacity, structuration theory, or actor network theory, just to name of few.

So, is the selection of "proper" reference theory just a random walk or is it a question of serendipity? Do reviewers apply some meta-theoretical properties, like "the ease of use" or "simplicity" of the theory, that influence the choice, overpowering the ultimate theory "usefulness" (i.e. theory utility or insight)? There are no empirical studies of how and why specific reference theories become chosen and what explains this adaptive process, so I cannot be certain. As an editor, I most often see reviewers go for simplicity and clarity rather than new insight or usefulness potential.

Typical in the success stories of lucky "reference theories" is that a simple and general theory (narrative) in sociology, psychology or economics addressing some potential aspect of IT use or adoption is transferred with a ready instrumentation and then further developed into a complete research routine. Very little or modest genuine theoretical thinking is involved after the initial adoption and adjustments. The theory, as it comes from psychology and/or sociology, is typically adopted without further ado, criticism, refinement, or analysis. In many cases it is typical that critical voices or debates about that theory in the reference discipline are neglected or sidestepped (e.g., TRA/TPB have been widely debated and criticized in psychology). This makes playing the theory "card" easier in the IS context as it pushes aside concerns of possible weaknesses that may decrease its legitimacy. This helps close down alternatives for the adopted viewpoint and furthers Finlandization. Often the reference theory soon becomes a form of orthodoxy. Anything which is different, modified, refined or beyond what the original theory says is deemed heresy and therefore not acceptable, leading to further straight-jacketing of thinking. Some examples in this regard are common accusations of misuses of structuration theory in our field given that it has no idea of technology. In this

sense, it should be very difficult to apply without adjustment! This, of course, differs from an accusation of whether the adopting scholar poorly understands or misinterprets original thinking (which also happens often!). Yet, allegations against unorthodox “use” are often addressed in this easy guile.

Overall, the idea of reference theory has made Finlandized IS folks look like a murder of crows looking for glittering pieces of theory in their territory that they eagerly bring to their disciplinary nest to be admired and honed.

The Way Ahead: Absolve the Idea of Reference Theory

One reason for this situation is the assumption that we can always truly benefit from adopting reference theories and teaching this process can act as an effective guideline for good theory-based research in our field. In fact, “reference discipline” itself is a terrible, misguided term that should be banned as it gives IS scholars wrong expectations about what makes a theory good. For me (and people in many other disciplines) any theory is a good theory if it helps us understand, reveal or explain the specific phenomenon under study, no matter where it comes from. We need more imagination than imitation! The idea of reference narrows the idea of what theory can do to “pre-selected” glimpses and explanations. In addition, it moves our focus from improving our process of theorizing to the fetish adoration of the outcomes. Yet in the end, in good scholarship the process counts more than the outcomes. As a result we treat theory in our field in fetish ways. They are like “things” available in theory supermarkets (just wait for the ultimate theory portal!) which can be selected, applied and vindicated just by consuming them mechanistically. Their main purpose is often to reinforce obvious and simple ideas which are rarely interesting. As such, they are not very insightful in accounting for IT related phenomena. By assuming the mindset of a reference theory we are becoming intellectually lazy and in turn (and this is more dangerous!), we are finding this to be acceptable.

Treating theories as commodities to be imported and capitalized upon has resulted in a situation where our theoretical arsenal has become bleaker and increasingly one sided. The notion of reference disciplines has blocked the advancement of multifaceted theorizing and improvement of our skills in the pursuit of theoretical rigor and boldness. Imagine if people in physics, biology or sociology had believed in the power and model of reference disciplines! We would still claim that the world is flat, the sun circles around the earth, that species were created by God or some mystic intellectual design, or that society works like a human body! Consequently, in IS we have too many simple and general theories, too little accuracy, refinement and finesse in our theoretical vocabularies, and too little theory variation. Foremost, we have too little tolerance and humility in the face of bolder and original theoretical thinking in our writing and reviewing practices. In our research and review endeavors we are usually simple inductionists constantly seeking to confirm our simple models of the world, though the whole idea has been dead since Hume! In contrast, we should seek to become bold falsificationists or brave de-constructivists in our theorizing.

Overall, I claim that the Finlandization of the IS discipline due to the implementation of the notion of reference disciplines has been at least as detrimental to the IS community as the hawks in the 1970's claimed the process of Finlandization to be for Finland and for Western societies at large. Surprisingly, the canon of reference disciplines and its consequence of Finlandization has lasted longer in our field than in Western societies at large. Finlandization as an idea died to the fresh air of perestroika, and the fall of the Wall.

Within Finland the process never truly happened in the ways claimed by the Western hawks looking at the society from a distance and not understanding its perilous history. Likewise, the IS discipline acts as if it were completely Finlandized. However, only if you look at it from a distance and do not know about its history. IS has in reality, in many situations acted like Finland; it is small, sometimes witty, but always resilient in its own ways to resist external pressure and come up with new solutions. But if we continue with the assumption that comes with the notion of reference discipline and allow it to reign it will easily turn into a true Finlandization. We gradually start to believe in its logic that wisdom comes from outside and we should constantly worry what others think of us (I see it often in review reports). If continued, this tendency will increasingly spoil the minds of many IS scholars regarding what a theory is and how to engage with it. It will also make us increasingly lazy which prevents us from being bold and brave. Over time it will come with a huge opportunity cost. Too many IS researchers will work on topics with ideas they should not be using, and they will repeat the use of ideas that are not up to the job. What we need more is those from within to reach out to new and genuine theorizing that is needed to grasp the increasingly complex aspects of IT in human enterprise. We need to stop believing that wisdom comes from without and above, and thus stop the increased Finlandization of our discipline. The fate of all this is in our hands if we are smart and do not think so much what others think of us but rather, exercise our wits and will smartly.



Don't Throw Out the Baby with the Bathwater: Reference Disciplines Matter!

I am going to be making the case *for* reference disciplines. While I will argue that we could have done better in our use of reference discipline theories, abandoning them will not be fruitful for our field.

Let's say that we have a complex problem and we have an accessible help desk right next door. The help desk has dealt with, well, not exactly the same problem but a number of problems that are somewhat similar in nature. Are we going to refuse help because it might constrain the way we do things? Instead, are we going to reinvent the wheel and develop our own solutions? I think that if we do it is somewhat absurd. If we've got ready potential help on framing and understanding our problems, then the choice of how we use it is entirely up to us. So, whether you like the term reference discipline or not, it is at best a "reference" for our discipline rather than the discipline itself. I also believe that reference disciplines can help us develop high quality IS theory if we use them better.

Reference Disciplines Help Us Develop Good Theory

What is the objective of theory? It is largely to explain and predict. This is so whether we are looking at transaction costs economics and the phenomena of outsourcing, resource-based theories and how technology facilitates competitive advantage, or behavioral theories and their impact on technology usage. These theories are helping us understand and predict IS-related phenomena, *and that is a good thing*. I would argue that in a field as diverse and young as IS we need organizing lenses. We need ways to put things together, to enhance sense-making, and if we get it from outside, then I think that's entirely appropriate. If we enhance our ability to explain, predict and understand our phenomena, then we are creating good theory.

Reference disciplines are usually more mature than the IS discipline itself. They've had a longer history and often have more formal theoretical constructs at higher levels of abstraction. We should draw from them and create mid-level theories that are engaged with IS related phenomena. In other words, if we can contextualize these theories and enhance our ability to explain and predict within *our* context, I would say that's a good thing.

Additionally, reference discipline theories give us a level of consistency in our discourse. We can talk to each other and use consistent terminology which facilitates the cumulative tradition within the IS discipline. I would argue that's also a good thing.

At the end of the day, have we made progress as a discipline? To critics who question the substance in IS, I would say that I don't think IS is at the end of the intellectual food chain. There is evidence in recent papers that we have made progress in actually giving back to the reference disciplines [Baskerville and Myers 2002; Grover et al. 2006]. We have been developing an intellectual engine that we churn. And again, that's a good thing.

So when we look at reference disciplines I would say the choice we make regarding how we use them to develop theories is ours. If we use reference discipline theories as organizing lenses to help understand our phenomena, draw from formal theories to create IS-specific mid-level theories, achieve consistency in our discourse, and contribute back to the reference disciplines themselves through superior ability to explain and predict, then we are creating good theory.

Reference Disciplines Help Us Benchmark Quality

If you refer to the first ICIS, it was at Peter Keen's talk [Keen 1980] where he coined the term reference disciplines. Over the 25 years since that time, what we have done as a discipline is assess a variety of different theoretical perspectives drawn from other disciplines. We have adopted them. We have adapted them. We have assimilated them. And, as a consequence of that, we have improved the quality of our work. That's not only on the theoretical front, that's on the methodological front as well. This positive outcome for our field is largely due to our use of reference disciplines.

Let's say we throw out reference disciplines and argue that, for our field, they have outlived their usefulness and we should not use them anymore. I think we lose something there. We lose benchmarking. How do we then benchmark our research? You might say, "Well, maybe we're mature as a discipline. We don't need external benchmarks. Maybe we can benchmark internally as mature disciplines do." Having seen the editorial review process extensively from the editorial side, I don't think we're quite there yet. I think we still have a lot of disagreements on what we consider good research. We are not at the point where we can just throw out reference disciplines and perform complete internal benchmarking.

If we throw out reference disciplines, then we'll probably face the issue of a smorgasbord of theory. We'll have a lot of private theories coming up. Indigenous to our field but private little theories. Then we'll have a fundamental problem regarding how we discriminate between them and recognize good theory. As has been said, "Unlike pornography, we may not even recognize (good theory) when we see it" [Maanen et al. 2007]. We will end up with gate-keeping problems on theories and we will have a proliferation of many "mini" theories. Because let's face it - construction of good, high level theory is tough. And yes maybe we should engage in such tough work. But let's do so carefully. Do we want to expose half baked theories to the world in our top journals - journals that are beginning to gain prominence on impact factors and display widely recognized quality? Do we risk being too tentative and too ad hoc on our theories as we learn how to recognize good theory? Isn't that too risky or perhaps even too arrogant for a young field?

If we throw out reference disciplines, I think we lose integration and filtering lenses. For example, if you look at strategic systems in the 1980's they were proliferating in practice. The IS community's reaction to them was to create 2X2s and little frameworks; ways of theorizing and thinking strategically. But I would argue that it wasn't until we brought in resource based theories and transaction cost economics to integrate ideas, integrating logic of competition, markets, and resources that we started making progress on strategic thinking. So my argument is: Do we want a field that is fragmented, not by reference discipline but by the framework of the month club? Do we want to go back to that cycle of frameworks that are easy to generate and difficult to test and apply?

So I would argue that if we don't draw reference discipline theories we lose benchmarking, create a risky gate keeping problem of theory recognition, foster the framework of the month club, and lose some potentially great integrating and filtering lenses. As a low paradigm field we're not quite at the point where we can take this chance.

Reference Disciplines Help Develop New Theory

Some people say that developing new theory is very tough when you deal with reference disciplines. I would argue for the contrary. I would say that if we take theories from reference disciplines and we just apply them to our field, then the intellectual contribution belongs to the reference discipline. If we morph those theories and embed them in our phenomena we have opportunities for studying these deep interactions between the social and technical systems. What other field has a vested interest in these interactions? This represents fertile ground for new theory. Similarly, if we look at a phenomenon from many different theoretical lenses, identifying contradictions from different reference discipline points of view, then we can ask the question, "Why are there these contradictions?" Again, we have fertile ground for developing new theory. It is in these higher planes that transcend theory that new theoretical ideas can emerge. Likewise, if there is debate about the efficacy of a reference discipline theory in the reference discipline itself we should not ignore it. Instead, we should bring the debate into our discipline and try to resolve it with respect to our (IS) phenomena. Again, there is ground for new theory.

So, I would say new theory emerges from such tensions. We should embrace tensions rather than run away from them. Reference disciplines offer us a vehicle to do this. If we engage with reference disciplines, embed them into our phenomena, examine the same phenomena through multiple lenses, and engage in reference discipline debates, we create fertile ground for developing new theory specific to our field. The chance of success is far better than if we did not have reference discipline theories in the first place.

Reference Disciplines Help Bolster Our Credibility

This is an old argument. While a lot of people have made this argument, let's look at the reality. IS is a young field. It is institutionally young, and often folks within IS in institutions are put up against other people in more mature disciplines for resources and tenure. I think it really helps to have a common vocabulary in which we can communicate and say, "You know what, we're doing the same things." My argument would be that continuing to take risks within the context of reference disciplines buys us the leverage to take risks outside this context. Too conservative? Perhaps. But let's push the envelope of reference discipline theory as it is embedded in our context, while also engaging in indigenous theory. Let's hedge. If we do it right, these reference discipline theories could be a part of our solution and not the problem.

And as a final note let's bring back the elephant. If IS encountered an elephant in a room, what I would contend is that IS should not react by being concerned about the love life of the elephant. IS should not even care about what the elephant thinks of it. IS should say, "Look at me. I can climb you, and I can reach bigger and better heights!"

Alternative Ways to Leverage Reference Discipline Theories

I'm taking the position that reference disciplines can be good. If they are good, we must ask ourselves: How can we use them to our advantage? There are different ways of acquiring knowledge. One way is to explore existing reference discipline theories. Another way is to exploit them. I'm all for exploiting if you can find something good to exploit. I will provide two examples of reference discipline theories that IS researchers have exploited by taking a broader theory and focusing on computer use and technology. Reference theories can also be expanded and I will briefly describe how. I also will discuss two techniques used to build new theories from reference discipline theories. I will conclude with institutions within the IS community that are available for potential theory builders.

Examples of Exploitation of Reference Discipline Theories

Two examples of reference discipline theories that have gained considerable momentum within the IS community are Technology Acceptance Model (TAM) and Adaptive Structuration Theory (AST). These two theories have been frequently cited and widely used as the basis of IS research. TAM is one of the most, if not the most, cited theories claimed by the IS research community. The two seminal TAM articles by Fred Davis and his colleagues in 1989 in *MIS Quarterly* and *Management Science* have 4638 and 2799 citations, respectively, in Google Scholar¹. TAM is based on Fishbein and Azjen's [1975; Azjen and Fishbein et al 1967] Theory of Reasoned Action and limits the scope of this broad psychology theory to computer usage. In addition to the IS research community, TAM also is of value in terms of helping our students reflect upon computer usage and resistance to change. My experience, especially with undergraduates, is that they seem to react more favorably to TAM than any other theory covered during my management of technology course. TAM is very useful for them. They tend to state unthinkingly that nobody ever likes change. But that is not true, and TAM at least gives them a different way of thinking about change related to system use. TAM is a theory that the students can grasp. I recognize that many perceive that TAM, even with its many expansions, is too simplistic, and many pointed out problems with TAM. Yet it offers an alternative view of system use and it deals with an issue that is highly salient to the IS community.

Another theory that is frequently over-simplified (or misapplied) is Adaptive Structuration Theory (AST). DeSanctis and Poole's [1994] *Organization Science* article introducing AST has been cited 1096 times.² Both AST's and Wanda Orlikowski's ideas about the duality of technology were derived from Giddens' Structuration Theory. Structuration Theory, a sociological theory, has been very useful in helping IS researchers think about change processes. It suggests how structures can constrain organizational processes, but then how they themselves also can be changed through the repetition of processes. I think this is a great example of how the IS field has taken a theory from a reference discipline that totally ignores technology and modified it to incorporate technology. We have used AST as a basis for thinking about what happens when we bring technology into an organization. The reference discipline theory did not pay any attention to technology at all, but we IS researchers have adapted the theory to do so.

Expanding Reference Discipline Theories.

TAM and AST both narrow the focus of a broader theory. It is also possible to exploit a reference discipline theory by expanding it. Typically theories are initially designed to answer what, why and how questions. That is, in answering the 'what' question, they describe the key constructs, variables and concepts that describe a phenomenon. In answering the 'how' question, theory developers describe the relationships among the selected constructs and variables. Sometimes, when answering the 'how' questions, causality is mapped out. In answering the "why" questions, theory builders establish the psychological, economic and/or social dynamics to explain their selection of constructs and relationships.

We can exploit reference discipline theories by adding value to them or by putting our distinctive mark on them. We can do so by extending the boundaries of the rudimentary reference discipline theories to include IS topics. In particular, we can add value by answering who, when and where questions. For example, we can expand the theory to describe who is covered by the theory. Is it the systems developers? Users? Potential users? Do they act differently than other people because of the IS? Are there some characteristics of our particular discipline that allow us to build on the theory? Are there distinguishing temporal aspects? For example, time is so critical in terms of the use of information. When is the information provided, and how does the time of its delivery affect performance? What is the starting point for a system-related phenomenon? What is the ending point? Where is the system being used? Does the technology create something unique about the setting that we need to consider when

¹ Google scholar: <http://scholar.google.com/scholar?hl=en&lr=&q=Fred+DAvis&btnG=Search> downloaded February 10, 2009

² <http://scholar.google.com/scholar?hl=en&lr=&q=adaptive+structuration+theory&btnG=Search> downloaded February 10, 2009

understanding a phenomenon? Where in the organization (i.e., at what level of analysis) should we focus our attention when expanding a reference discipline theory?

Building New Theory from Reference Discipline Theory

The way to clearly establish our identity as a discipline is to develop distinctly new IS theories. However, even then we can build upon reference disciplines. I will describe two tools for doing so: metatriangulation and building from paradoxes.

Metatriangulation is a three-phased, qualitative meta-analysis process that may be used to explore variations in the assumptions of alternative paradigms, gain insights into these multiple paradigms, and address emerging themes and the resulting theories. It helps theorists recognize, cultivate, and accommodate diverse paradigmatic insights. Ideally these insights can be used to create new theory.

My colleagues and I used metatriangulation to try to understand power theories in the IS literature from different viewpoints. We describe our application of metatriangulation in Saunders et al. [2003]. Our original intent was to consider a variety of ways of looking at power. We wanted to reflect upon the underlying paradigms of the articles that we reviewed and then come up with one grand theory of power for IT. Upon examining the underlying paradigms we realized that it was immensely difficult to synthesize the varied perspectives. In spite of significant effort and time, we couldn't come up with one grand theory because the assumptions underlying the paradigms conflicted so much that we couldn't take the various views of power and synthesize them. As a result, we came away with a richer understanding of power, and that's what we tried to convey in our paper [Jasperson et al. 2002]. But it wasn't one grand theory. It was one small step.

Perhaps the approach we used can serve as a reasonable model for others attempting indigenous IS theory building. Maybe it is unrealistic for most of us to start off thinking that we're going to create a grand theory. Maybe we should start by theorizing and providing the basic steps in understanding a phenomenon. Then others in the community can build upon these steps until we have a rudimentary IS theory that is clearly recognized as our own.

Varun raised the issue of looking for "the tension" in IS phenomena. There are some great thoughts about theory building in a 1989 issue [i.e., #4] and a 1999 issue [i.e., #4] of *Academy of Management Review*. For example, the 1999 issue had an article on metatriangulation by Lewis and Grimes [1999]. In the 1989 issue, Poole and Van de Ven describe how to build upon paradoxes by looking for tensions and using them to develop a more encompassing theory. They map out four ways to use paradoxes in theory development: (1) accept the paradox and use it constructively; (2) clarify the levels of analysis; (3) consider the role of time in the theory; and (4) resolve the paradox through synthesis. Understanding the reason for these tensions can help in extending the boundaries of a reference discipline theory, or, in the case of resolution through synthesis, developing new theories.

I would like to shift now and talk about theory building from the perspective of an editor. Building good theory is clearly a challenge. That is probably why we don't see as many theories as we would like. However, getting the theory through the review process may be even more of a challenge. My advice to authors is that they initially accept that theory building is a painstaking process that requires attention to detail and blue sky thinking at the same time. The different constructs must fit together so that they make sense and all loose ends must be tied. Theory builders need to go through iteration afterwards to perfect the theory. One approach would be to start with theorizing about constructs that relate to the IS discipline and to build simple models that can be refined over time. This is consistent with what we've seen with TAM and AST research when they are been applied to the IS discipline.

It is almost a given that theory builders will be frustrated with at least some, if not all, reviewers of their paper. There are always loose ends, and reviewers are more than happy to point them out. As a result reviewers frequently say, "You didn't think of this. You didn't think of that." They may even try to steer theory builders in directions they don't want to go. However, I think the most serious problem in the review process is that many reviewers expect theory builders to test their theory when they first seek to publish it. I believe that it is hard enough to come up with good theory, and that we shouldn't expect authors to introduce and empirically test their theory in the same article. Let's publish papers that have the theory and then allow members of our community to test the theory if they find value in it.

Institutional Support

Fortunately, there are institutions in our discipline to support theory building. One is the *Journal of the Association of Information Systems* (JAIS) workshops where editors work with authors in developing their theories. Proposals must be submitted in advance, and the selected papers are discussed in a workshop after ICIS. Another institution is *MIS Quarterly's* Review section. Lynne Markus is spearheading efforts to encourage theorizing and theory building.



Probably the greatest institutional support is our journals. It sounds obvious, but if we want to be recognized for our theories, we should publish them in IS journals. I attended a session at the Academy of Management this year that was focused on the need for more theories in IS. One presenter even started the session with a quote from one of my editorials (an auspicious start, right?). This presenter described a new IS theory that he had developed and proudly said it was under review at *Organization Science*. I had to wonder why the presenter hadn't sent his IS theory to an IS journal rather than an interdisciplinary journal. Perhaps it is a commentary on our field. That is, we draw so heavily from reference disciplines that interdisciplinary journals are natural homes for our theory creations. Note that seminal TAM and AST articles were published in interdisciplinary journals. While it may make sense to publish IS theory in interdisciplinary journals, I would argue that we won't really be recognized for our own theories unless we give them a home in IS journals. *JAIS* and *MIS Quarterly* are sending clear signals that they are seeking more IS theory. I urge authors to send their theory creations to these and other distinctly IS journals.

In summary, I have outlined ways of using reference discipline theories to our advantage. We can exploit theories from reference disciplines by narrowing them to focus on IS issues or expanding them to consider the "who, where and why" aspects of the phenomenon that are particularly salient to the IS community. We can also build distinctive new IS theory using such tools as metatriangulation and methods for building from paradox. We now have options in the IS discipline, and I hope we will use them by publishing in IS journals. We have IS theories, but we can leverage institutions within our community to build more.

SHIRLEY GREGOR

A Credo for Good, Useful Theory Native to Information Systems

As an Australian, I am neither North American nor European and have the good fortune to come from the Pacific Asia region. I think in Australia we often see parallels between the sorts of things we do and what the Scandinavians and Finns do. I sometimes think it's because we're so far outside of the main spheres of influence of North America and Europe that we can go ahead and think out of the box and more for ourselves. We are perhaps not as influenced into mainstream thinking, such as the need for "reference discipline thinking." Personally, I agree with Kalle Lyytinen that we need an entirely new way of thinking about what we in IS do. To continue with Kalle's story about the elephant, I think if an Australian was confronted by an elephant in the bush, he'd say, "Gee, mate, what are you doing here? I'd better get away quick." This illustration gives a little of the flavor of what I propose; it's a *practical* way of thinking that is concerned with effectiveness and usefulness.

Table 1 presents a summary of the points that are made. In the interests of promoting discussion, my arguments are made a little more forcefully than would normally be the case in an academic paper, and Gregor [2009] provides more background to the arguments. I might be seen as being overly critical of IS research. But in fact, I think IS has already come a long way toward being open minded and accepting different ways of doing research, possibly quite a way further than some of the other disciplines that are in business schools. The aim is to be constructive and stimulate debate regarding how we can improve IS research.

Table 1. A Credo for Good Useful Theory Native to Information Systems	
1.	IS/IT is a Practical Science
2.	A Practical Science studies artifacts
3.	Pseudoscience is bad
4.	Theory builds on evidence
5.	IS/IT theorizing has new challenges

IS/IT Is a Practical Science

The first point is that IS/IT is a *practical science*, or a *science of the artificial* using Simon's terminology [Simon 1996]. What is meant by a practical science? The sciences can be placed in three interconnected categories [Strasser 1985]. One category is for the *natural sciences*, which study naturally occurring phenomena. The natural sciences advanced rapidly from the time of the Age of Enlightenment in the 16th and 17th centuries when the scientific method was proposed. Another category is for the *human sciences*, which were given more attention from the 19th century on, focus on human beings as the object of study and include disciplines such as psychology and sociology. The term "human sciences" was advanced by the philosopher Dilthey, whose writings have been influential for interpretivists [Makkreel 2009]. Since the last half of the 20th century we have had more focus on the *practical sciences* which study objects that are the result of human activity. The practical sciences have a very long

history, although it is perhaps only in the last 50-60 years they have been given more attention in terms of their epistemology. Medicine is one of the oldest fields of studies. Going back to Aristotle we find he distinguished between *epistēmē*, which is the study of objects that do not admit of change, and *technē*, which concerns the bringing into existence of something that could either exist or not exist; that is, the contingent [Parry 2007]. Each *technē* aims at some end. For example, health is the end of medicine, and medicine produces artifacts such as medical treatments, drugs and surgical interventions. What distinguishes the practical sciences and makes them different from other forms of science is that practical application, or the production of useful knowledge, is their **primary** aim. Practical applications are not **incidental** to the pursuit of knowledge, as in the applied form of other branches of science (for example, atomic power arising from physics). An agricultural scientist must study physics, chemistry and so on, but that is not the end in itself. The end is to make “certain types of extra-scientific praxis possible” [Strasser 1985 p. 57].

A Practical Sciences Studies Artifacts

The second point is that practical sciences study artifacts. The things that we study are different in important ways from naturally occurring phenomena such as rocks (geology) and the stars (astronomy) and also from human beings (psychology). We study things that people construct – the view of Herbert Simon in the *Sciences of the Artificial* [1996]. If we are going to study constructions, then we need to think about them differently from natural things and humans, as these constructions are produced by human agency; they are produced with an aim. The stars may have been produced with an aim, but that’s beyond the bounds of this discussion, as we are limiting it to artifacts produced by human agency.

The types of questions that we ask are: How do you do something? What works in accomplishing particular tasks? The artifacts concerned could be products such as a decision support system but they could also be interventions, For example, enterprise resource planning implementations or software modeling tools, as our field has a very broad range of artifacts. When we ask questions about how to do something, the answers mean careful thought about the types of reasoning used, because we need to think very, very clearly about causality and teleological reasoning, which may not be as critical in other forms of science. In astronomy, we don’t need to ask why the earth circles around the sun. We need different reasoning in the practical sciences, and we need to devote more attention to thinking about the modes of reasoning we employ.

Pseudoscience is Bad

Science is not dead, and pseudoscience is still a bad thing. To me science means something like credible, well-supported knowledge in accord with evidence. Similarly, in a *History of Science*, Gribbin [2003 p. 63] says that science is supposed to employ methods that compare “hypotheses with experiments and observation to weed out the wheat from the chaff.” It is important for us to recognize our field as a science because it gives us ways of thinking that can draw upon the considerable history of the philosophy of science.

A problem is that in some of the disciplines that we use as reference disciplines, what I would call pseudoscience is in operation. In some cases, the methods of science are aped, without them being understood terribly well and without them being applied well. A thing that bothers me when I read articles about the scientific method and epistemology in IS papers is that often these papers quote work in management, accounting, sociology and so on. The authors do not go back to the philosophy of science literature, and often what we end up with is second or third-hand thinking at best. In itself, this problem illustrates a problem with reference discipline usage. If we are using literature from another field then we need to try to understand that literature as well as we can so as not to deal with it at a superficial level, possibly ignoring counter or more recent arguments that have occurred in the discipline itself. Truex, Holmström and Keil [2006] expound this view to good effect.

What has compounded this problem is that in the twentieth century there were a few movements in thought which I think, in the longer term, will be regarded as “blips” in thinking. Over time there are many movements in the philosophy of science. Successive movements are sometimes a reaction to what has gone before, and they swing to another extreme as a counterbalance. Each movement may present some new ideas that are real and valuable, but they also have aspects which are then criticized and found wanting in subsequent waves of thought. As an ironic example, Kuhn’s views on paradigmatic change in science [Kuhn 1996] have had an enormous influence but they can be criticized on several grounds, and he spent some time himself trying to distance himself from the more radical views of science that came after him [Godfrey-Smith 2003].

Some of the blips in thinking that occurred in the twentieth century appear to have had an inordinate, and unfortunately, lasting effect in some of our reference disciplines as well as IS. One of these blips was logical positivism. Logical positivism was an extreme form of empiricism that is quite unsupportable on logical grounds, as demonstrated early on by Karl Popper [Popper 1986] and has been regarded as “defunct” in the philosophy of

science for some time [Passmore 1967]. Unfortunately it appears some thinking from logical positivism stayed on, I think particularly in North American universities, and still influences current practice in management, accounting, and IS amongst others. One problem with logical positivism is that it is such an extreme form of empiricism that it doesn't pay attention to concepts such as causality. As an illustration, many of our scholars in IS (including me) have been sent the text by Dubin [1978] in research methods classes, especially those in universities following University of Minnesota traditions. However, traces of logical positivism remain in Dubin's otherwise splendid book. He would not recognize causality in theoretical statements, as causality is not something that is directly observable (an extreme empiricist view). This background means we have a real conundrum. We are in a discipline that is concerned with artifacts that are brought about or "caused" by human agency, and yet we countenance research traditions that rely on out-dated and discredited views of causality.

A further problem that is, in part, related to the last problem is that of over-reliance on and misunderstanding of the place of statistical methods. Modern statistical methods have a short history and they are not in themselves a sign of good science. Sadly, it often appears that when people talk about "rigor" in our reference disciplines and in IS, they mean statistical rigor. It should be very clearly understood that statistical rigor is *not* the same as scientific rigor. This is where "pseudoscience" creeps in. For example, statistical methods do not deal at all well with causality [Pearl 2000] which is of special interest in practical sciences. What we need for scientific rigor is good instruments for measurement (if used) and sound arguments, where possible alternative explanations for whatever is argued for, especially causality, are eliminated.

Some further confusion has occurred concerning the preoccupation with differences among groups that see themselves as positivists, interpretivists and so on, and how science is practiced as a social activity. In the last half of the twentieth century this preoccupation led to the "sociology of science". In its strong forms the sociology of science is often associated with relativism, a position which again has problems with internal consistency [Godfrey-Smith 2003]. Although the study of the social aspects of science is a legitimate interest, it may have deflected attention from other important aspects to science. For example, the structure of arguments and reasoning methods. Further, it is unclear what the discussion in the sociology of science means for the study of artifacts.

What we can conclude from the above is that a number of our reference disciplines, such as management, have a rather confused relationship with the philosophy and practice of science. Many of these disciplines, like IS, are relatively new and have not developed bodies of knowledge which have the same levels of generality and acceptance as more traditional areas of science. Further, some theories that we might cheerfully adopt in IS may not be supported by much empirical research. Miner [2003] surveyed the 73 theories found in the management literature and had them rated regarding their perceived importance and scientific validity. Only 25 were rated highly in terms of scientific validity.

So we have quite a problem. If we use theory from other disciplines, then the theory may have been developed using confused views of science or pseudoscience, and there may not in fact be much supporting evidence for the theory. So why risk borrowing dodgy theories from other places?

Theory is Built on Evidence

A view that needs recognition is that theory should build on evidence. The first step in the scientific method is to generate hypotheses or conjectures by induction from evidence, possibly accompanied by imagination or guesswork.

There are many examples of how good theories have arisen from a strong empirical base. One example is Rogers' theory of the diffusion of innovations [Rogers 1995]. I've heard that Rogers believed he had examined every known study of diffusion in the world. This theory may be outdated now to some degree, but in its time it has been an incredibly influential theory, and still has much relevance. Further, Rogers' book is one of the most cited works in the social sciences.

The importance of the first step in the scientific method, that of building upon evidence, is something that is not always recognized in IS and other disciplines. In part this may be due to discomfort with using inductive methods, which have problems as pointed out by the influential philosopher of science, Karl Popper, amongst others. However, problems of naïve induction can be avoided by going beyond mere counting of instances: for example, by looking carefully at cause and effect relationships and chains of evidence and using abduction. Hempel [1966 p. 15] gives a good account:

The transition from data to theory requires creative imagination. Scientific hypotheses and theories are not derived from observed facts, but invented in order to account for them. The transition from data to theory requires creative imagination.

Note that in this account the “observed facts” are still taken into account. They are not ignored or thought of little importance.

As an author, reviewer, editor and supervisor of students, I see an opposite view in many papers and amongst reviewers. There seems to be a view that for a “safe” Ph.D or paper, you find some interesting sounding theory from a reference discipline and then apply it to IS. You more or less “put your hand into a bag,” pick out some theory from somewhere else, find a few constructs, then put them together with some type of argument and form a theoretical model, then engage in nice safe theory testing. This process is referred to as “theory building and theory testing.” Ray Paul, a former editor-in-chief of the *European Journal of Information Systems*, calls it building “castle confections upon sand” [Paul 2005 p. 209].

Proceeding along these lines as a primary means of research in IS means that as a discipline we are truly “building upon sand.” The theory testing we engage in is mostly statistical and we are likely to get correlations of .30 or more, or significant R-squared, for all sorts of reasons including common method variance if we are doing surveys. Often the R-squared that is observed is small, meaning that only an incomplete account of the phenomena is given anyway. How did we know in the first place that the reference theories used would apply in IS? In IS/IT we are studying the most complex artifacts that have ever existed, ones that involve technology as well as people. Why should management, economics or marketing theory apply?

The point is that we should be more open to using induction and abduction and spurn the spurious legitimacy bestowed by adopting theory that comes from somewhere else. We should be teaching our PhDs and requiring our researchers to show when they advance a new theoretical model that it has some grounding in studies and evidence that relate to IS/IT artifacts.

This argument does not mean that we do not ignore working deductively from theory to get new models completely. There have been a few brave attempts to try to develop grand theory by deduction in IS. The work of Ron Weber is one example with his theory of representation [Weber 1997]. However, he has used systems-type theory as a base, which is perhaps something closer to the IS domain than theory we might receive from management or marketing. Further, this new theory has been tested in experiments which perhaps provide more hard data than surveys.

There are a few more points to make. Some of our very influential work in the IS domain has been done using inductive and abductive methods. For example, Davenport and Short [1990] developed their ideas on BPR by going out and studying what people were doing, observing what was working and what was happening, and then writing it up. They didn’t write it up in one of our top journals, but their papers have been extremely well cited and have led to work in our top journals. I would see that as a good thing.

What happens is that, if you do the first steps, if you observe what people are doing in practice or you build something yourself, you are starting on the theory building process. When your subsequent paper goes to a journal, it should *not* be regarded as atheoretic if you do not have a reference discipline theory to explain what’s happening. The explanations might even come from our own area: other people found these methods worked previously, and people have adapted them. In this way we start to theorize in our own area. We do have theory that’s been built in our own area even if it’s not always recognized as such. An example is our knowledge of decision support systems and how to build them. Some might refer to this as ‘implicit theory’ [Eierman, Niederman, and Adams 1995] or more recently as ‘design theory’ [Gregor and Jones 2007].

IS/IT Theorizing has New Challenges

The final point is that we have many challenges to deal with in theorizing in IS. First, we have to escape from the hegemony of not just reference discipline thinking but also from our reference disciplines’ views on science. These problems are serious. We’ve also got a problem in that we deal with phenomena that are very fast changing and very complex. How do you theorize about things that will change completely in five years? A new technology will be developed. You’ve got a new form of electronic market or form of communication. How do you theorize in that problem situation? I think this is something we have yet to deal with.

As a final point and just to be even more provocative, we shouldn’t freeze our thinking too soon. We are a new discipline. The fact that IS became influential in a few North American universities early on and developed particular modes and models of theorizing does not mean that we have to stay with those models. These models reflect views at one point in history and particularly some remnants of logical positivist thought. The world and philosophy of science have moved on and so should we. In a recent panel someone said that, “what we’re doing works” (one of our editors). In my personal view, I do not think it is working, or at least not as well as it could. I am with Kalle: we need the freedom to build good and useful theory native to our discipline and to think about our theorizing in new ways.

IV. CONCLUSION

Emergent Questions and Ideas

Several important concerns were voiced by session participants that extend the discussion of reference discipline theory and its impact on the IS field. Although these points were raised during various stages in the panel presentation, they will be addressed together in this section to preserve the flow of the major arguments and reduce the sense of 'going off on tangents'. The issues emerging during discussions pertained to: (1) the research context and definitions for the discussion including defining the IS phenomenon and who are its stakeholders; (2) power structures and the "realities" of the field, including pressures to publish and the difficulty of getting reviewers to recognize the validity of indigenous theory; and (3) specific reference discipline theories such as the garbage can model, allied research tactics, and as the use of design science.

The panel session did not debate the nature of IS phenomena and what topics are central, peripheral, or perhaps outside of this domain. Shirley Gregor, however, addressed this topic in discussing artifacts and defining the IS domain as: the study of these creations in the very broad sense to include not only specific technologies, but also interventions and approaches. Similarly, differences in perspective among varied stakeholders were not directly addressed, although comments from some participants focused on issues regarding publication standards as well as theory content in doctoral seminars. While reviewers of the original panel proposal noted potential differences among business school, information school, and engineering/computer science faculty, issues differentiating researchers at universities with varying approaches to evaluating work published in different levels of journal might be another basis for dimensionalizing faculty constituencies.

Implicit in the discussion of referent discipline theories are issues of power structures, gatekeeping, as well as referee and editorial instructions and behaviors. It is well and good to talk of the need for indigenous theory, but if these are impossible to publish, or to publish in key journals, what is the incentive or utility of creating these? Frustration with both the preponderance of papers using or misusing reference discipline theory and with the difficulty of publishing alternative theory formulations was expressed by all of the presenters. Future discussion of reference discipline theory directed to specifics of the existing IS community may address these issues more specifically.

One theme emergent from discussion with session participants involved the contrasting approaches of Herbert Simon and the sciences of the artificial with James March and the garbage can model of problem related activity. It was proposed that within the IS field we have "theory-oriented" research that starts with solutions in search of problems, following March's garbage can model. This would contrast with research focused on the IS phenomena looking for patterns that could eventually be formulated as theory. Questions were raised pertaining to the means by which the particular set of reference discipline theories have come to be used in IS research. Clearly a significant portion of the IS researchers entered the field, particularly in its earlier years, with either concentrations or minor fields of study in areas including communications, sociology, psychology, operations management, and management and have found the tools learned in these fields to be employable in the IS domain. It would be open to empirical analysis as to whether the employment of particular reference discipline theories mirrors the background studies of those using them. In many ways the IS field has considered itself to be multi-disciplinary and, for better and worse, can be categorized by an openness to many influences.

In light of the strong influence of reference discipline theories an important question is whether it is important to generate indigenous theories. Both Varun Grover and Carol Saunders suggested that the adoption and evolution of outside theories may be a mechanism for doing this. Both Kalle Lyytinen and Shirley Gregor proposed the importance of directly taking action to create and sustain such indigenous theory. The potential of "design science" to address creating indigenous theory was proposed by a session participant. As noted by Shirley Gregor, such an approach is sometimes viewed in a narrow technology building sense but it can be effectively applied broadly to a wide range of artifacts. Though they didn't come up explicitly in the discussion, a variety of approaches including grounded theory, meta-analysis, and 'think pieces' might also be well considered as mechanisms for generating indigenous theory. However, it is good to keep in mind Varun Grover's caution regarding a "theory of the month" approach and the need for effective means to evaluate and integrate such generated theories for institutionalization in the IS domain.

It is clear that this discussion touched the surface of many important topics and issues that deserve further examination and on going refinement. It is the hope of the authors that this discussion, as represented here, provides stimulation for better use of referent discipline theory, where it is to be applied, and growth and diffusion of high quality indigenous theory for the health and empowerment of the IS domain.

Some Observations Toward a Synthesis: Points of Agreement and Disagreement

It should be stressed that participants were asked to take extreme positions for the sake of highlighting key issues and prompting sharp discussion. Two points of agreement and two points of disagreement among them were observed in the session. Those supporting the use of reference discipline theory joined critics in noting the problematic nature of poor use of reference discipline theory. Both Varun Grover and Carol Saunders noted that appropriate use of reference discipline theory can be difficult and requires adjustment for application in the IS domain. Additionally, both critics of use of reference discipline theories observed specific applications where such use has been of positive value to the IS discipline. All speakers acknowledged the potential value of theory building pertaining to IS phenomena and its importance to the IS field. All of the speakers additionally acknowledged the challenge and difficulty of such indigenous theory building.

Differences, however, focused on the net effect of reference discipline theory and its effect on "mindset." Kalle Lyytinen emphasized the pernicious effect of reference discipline theory on the IS field as a filtering out of bold new ideas and a filtering in of rigorous but modestly interesting work. He warned that such a mindset can generate a kind of "self-censorship" and repetition of familiar and predictable procedures in lieu of creativity and expansion. Shirley Gregor stressed the tendency to adopt poorly conceived external theories rather than return to original thought and, in the process, to undervalue and misperceive efforts to create and diffuse new IS theory.

The discussion of the influence of reference discipline theories on the MIS research domain is broad and rich. It is not likely to be resolved in a single panel discussion. Perhaps it would be fair to conclude that the use of reference discipline theory can be improved by insisting on greater subordination to phenomena in the MIS domain and return to the origins of each theory as evolving in the reference discipline. It would also be fair to conclude with support for promotion of indigenous theory, recognizing that this can be an intrinsically difficult task and one that is difficult for reviewers to evaluate. As a final word, it was very clear in the panel that the success and long term health of the MIS research domain is of vital interest and importance to all concerned.

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Editor's Note: The following reference list contains hyperlinks to World Wide Web pages. Readers who have the ability to access the Web directly from their word processor or are reading the paper on the Web, can gain direct access to these linked references. Readers are warned, however, that:

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