

**EXPLOITING COMPARATIVE ADVANTAGE: A PARADIGM FOR VALUE
ADDED RESEARCH IN ACCOUNTING INFORMATION SYSTEMS**

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Following the lead of recent papers by Demski (2007), Fellingham (2007) and Hopwood (2007) which questioned the direction and value added of non-AIS accounting research, we discuss the state of research in Accounting Information Systems. AIS researchers face a significant hurdle in undertaking value added research given that the financial and human resources that industry devotes to research and development of AIS technology dwarfs the capabilities of academic researchers. In these circumstances, we put forward a paradigm for AIS research based on the principle of **comparative advantage**, which is the powerful economic force that ensures that trade can take place even between parties where one has an absolute superiority over the other. It is our contention that if AIS academics are to succeed in creating value added research then they have to identify what they can do that the AIS industry, despite all its financial and human resource advantages, cannot or will not do. And what economic theory indicates is that such opportunities to add value always exist—if only academics are willing to seek them out. We illustrate our paradigm by analyzing three potential sources of comparative advantage for AIS researchers and discussing illustrative examples of research in each of these areas.

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1. Introduction: Value Added AIS Research

The starting proposition for this paper is that accounting is both a field of knowledge and a profession, and hence, research in accounting inherently has to have a large component that is applied. Granting this position, what implications does it pose for research into Accounting Information systems (AIS), since AIS is even broader than the profession of accounting itself? It is hard to describe AIS in terms that would give it an identity independent of its underlying industry, in this case, that vast driver of modern economies, high tech in the form of IT, electronic communications, ERP systems, tagging technologies, and so on. It is against this background that AIS research has to find its niche and to establish its role.

In non-AIS research in accounting (which we shall denote as NAIS research) the relationship between the focus of research and the priorities of the profession is widely perceived to be fragile to say the least. While it may be an exaggeration to say that practitioners don't care about NAIS research, the recent spate of articles by senior NAIS researchers decrying in rather alarming terms the state of the field and its lack of relevance is unprecedented.¹ The concerns these authors raise about the prevailing direction of NAIS research is what prompts us to approach the question of under-researched areas in AIS by starting with the relationship between its research and practice arms.

If the worth of AIS research is to be assessed based on some measure of **value added**, as opposed to being valued for its own sake, then—given the professional basis of accounting—value can arguably only be evaluated by the extent to which it helps shape the way in which accounting is undertaken by those for whom it is a living.

That is not to say that AIS researchers aren't free to pursue whatever research that interests them, and indeed, the joy in academic research arises from the fact that it can be an unconstrained exercise in academic curiosity. But we take the fact that the direction of AIS research is brought up for discussion at all as an indication that what is being sought is a systematic examination of how AIS research can create value for the profession at large.

The essential difference between accounting, including AIS, and a field like medicine or physics is that accounting is an entirely human creation, while the hard sciences are

¹ Demski (2007), Fellingham (2007) and Hopwood (2007).

attempting to discover the unchanging rules of nature. As a consequence, even the most seemingly impenetrable research in the latter almost always affects practice because the truth that is being revealed is the basis for the workings of any application—consider the increasingly important field of quantum cryptography which is based on those most cryptic of scientific results, the Heisenberg Uncertainty Principle and “quantum entanglement”. This makes it possible to sustain a discovery driven approach to research, in which academics can, if they choose, focus exclusively on basic research while leaving it to others to find its practical application, secure in the knowledge that such an application does in fact exist.

But that is not the case in accounting since its “truths” are not fundamental or lasting—not even to the extent that discoveries about consumer behavior are in economics or asset pricing is in finance—since the reality of accounting can be changed by simply issuing a new standard, or by passing a law, such as the Sarbanes-Oxley Act.² In such circumstances those who define reality are creating (or destroying) far more value added than those who simply study what that reality happened to be at some point in the past.

This fact has particular relevance to the question of whether accounting research should be positive—meaning that it should focus, for example, on analyzing data to determine how markets reacted to past accounting events, such as earnings, while refraining from drawing a conclusion about the applicability of those findings—or whether it should be normative and explicitly aiming to devise “better” accounting practices. The latter is what medical research does whenever a new drug is developed or what an engineer does when he creates a new software application. But there is a long tradition in NAIS research of avoiding making what can be perceived as value judgments, leaving it to others to draw practical lessons, if any, from the work.

One merit of being in a profession which molds its own reality is that there are obvious candidates for those who can fulfill the role of translating theory into practice. Thus, when archival capital market research is criticized as lacking in relevance, the defense that is offered is that it is up to standard setters to apply the lessons that such NAIS research offers into the information theory perspective on accounting. There is little evidence in support of this defense, with Erick Lie’s recent work on stock option expensing being an exception that

² Consider the lasting relevance of the research into GAAP, not to mention the value of the vast investments practitioners have expended in acquiring specialized knowledge about these standards, once, as is expected to happen in the next few years, the United States joins the rest of the world in adopting IFRS.

perhaps demonstrates the rule. But at least the argument can be made that in the case of NAIS research a strategy of academics focusing only on positive rather than normative research may succeed, especially with a senior financial accounting researcher usually holding a seat at the FASB.

Unfortunately, this particular road for basic research to influence practice by “osmosis” is much harder to follow for AIS research. AIS practice is defined by a far larger set of players than those who define NAIS practice, and if the influence of researchers on the latter is limited, then it is even more remote in the case of the former. While accountants have a large part to play (though certainly not the exclusive part) in defining accounting practice, in AIS the underlying technologies are entirely outside their control.

All this makes it all the more important that AIS research explicitly address the way in which it can create value and systematically develop a pathway for at least making feasible a possible impact on practice. If AIS practitioners and not academics are the only parties that move the profession forward then it is hard to see what value added AIS research can possibly provide.

In the next section of the paper we examine the particular hurdles that AIS researchers face when attempting to undertake value adding research in the face of the physical and human resource advantages of the AIS industry. Section 3 then introduces the principle of comparative advantage, which we argue offers a way for AIS researchers to overcome those hurdles and we discuss three examples of ways in which that principle can be exploited by academics. Section 4 analyzes those three sources of comparative advantage in greater depth, with illustrative examples of AIS research in each. Section 5 examines the development of XBRL as a case study in the application of the paradigm laid out in this paper and section 6 discusses how AIS researchers can implement the principle of comparative advantage in their research. Section 7 offers some caveats and conclusions.

2. Hurdles to Professional Impact in AIS

It is one thing, however, to aim to do applied research and another for that research to actually affect practice. The problem facing AIS even more than NAIS research is the high hurdles that it must overcome before research can possibly have a real world impact.

The global technology industry is so vast that it is hard for it to be materially changed by anything that happens in AIS research, and that remains true even if the focus is restricted only to IT that directly impact the accounting function, such as ERP systems, monitoring software such as Approva and CaseWare or bookkeeping software like Peachtree Accounting. Indeed, that is arguably a major contrast with NAIS research, since the potential is at least there for that research to affect the profession of accounting—noting that in its totality, the turnover of all accounting firms is smaller than the revenues of one major IT firm, such as Oracle or IBM—while that possibility is much more remote in the AIS realm.

Moreover, that mismatch in scale between AIS research and AIS practice holds across multiple dimensions. For example, even a single piece of accounting software is the result of hundreds of person-years of work and a financial expenditure on research and development that dwarfs the total grants available for academic AIS research.

These facts too stand in contrast to NAIS research. While the accounting industry also undertakes in-house R&D, the difference in the ratio of AIS research to IT industry research is orders of magnitude smaller than in the accounting realm, by any metric of research input or output. It also has to be kept in mind that while academic research in NAIS areas such as management and financial accounting, audit and tax is effectively a monopoly of accounting departments, research impacting AIS is undertaken by many departments both inside and outside the business school, such as MSIS, computer science, electrical engineering and so on, which dilutes the potential role of AIS researchers. (That is not to say that all these departments are doing AIS research, but that their work on tagging, ERP systems, databases, data display systems and so on have all have impacted AIS practice).

AIS is also a minority within the accounting research community, a reflection perhaps of the relatively lower penetration of technology on accounting practice in general, compared to many other modern industries. It is impossible, for instance, to describe the airline industry without reference to the aircraft that it flies, since the range and passenger capacity of those planes affect both the routing and the cost structure of airline firms. Similarly, investment banks such as Goldman Sachs identify IT as one of the key sources of their competitive advantage, which they maintain by investing in their own dedicated staff of programmers to create specialized trading systems for the firm. It is hard to argue that in accounting there

exists such a symbiotic relationship between the nature of the industry and its technological infrastructure.

That creates an opening for AIS research to define its value added more narrowly in reference to the accounting industry alone, rather than IT in general—for example, by working with an audit firm, the IIA, AICPA or the IMA on technology issues, as many AIS researchers already do. But while this tactic may reduce the hurdles that AIS research faces in translating its results into practice, sustaining such a narrow perspective is increasingly difficult given the tendency for AIS technology development to be outsourced rather than being developed internally by accounting firms or professional bodies. Thus AIS researchers cannot restrict themselves to those constituencies alone, as a NAIS researcher is able to do, happy in the knowledge that there is no other party that is interested in researching and bringing to market new financial or managerial accounting standards, as opposed to AIS products.

Another constraint particular to the AIS domain is that the lead in innovation in many AIS areas is being undertaken at the instigation or for the benefit of internal auditors who, for reasons of independence and cost, are turning to outside vendors for AIS solutions as opposed to their external auditor. This is particularly the case when internal auditors find that the solutions they need are being offered by the vendors of their general IT infrastructure (the recent purchase of the continuous monitoring software maker VIRSA by ERP giant SAP is a manifestation of this phenomenon). The bottom line is that in contrast to NAIS research, AIS research inevitably has to be seen in relation to a broader base than of the accounting industry alone.

3. The Principle of Comparative Advantage

The point of this discussion is to better understand the forces that define what value added AIS research means in this environment. The inability to justify doing only basic research combined with the difference in scale between AIS research and the IT industry, realistically places a unique burden on AIS if it is to contribute to its profession, even more so than with NAIS research.

The critical issue that has to be tackled is the difference in scale in AIS between academia and practice. However narrowly or broadly one defines the industry that develops the

technology that determines AIS practice, the bottom line is that the financial and human resources that it devotes to research and development puts the capacity of AIS researchers in the shade. But does that mean that there is no possibility for academic AIS researchers to have a meaningful impact? That seems to be the only common sense conclusion, and yet, that is the same as asking how a poor country can trade with a rich, developed one like the USA. Such trade is possible because of the economic principle of **comparative advantage**: *“What matters is not the absolute cost of production but the opportunity cost... even if one country has an absolute advantage in every type of output, the disadvantaged country can benefit from specializing in and exporting the product(s) with the largest opportunity cost for the other country.”*³

It is our contention that if AIS academics are to succeed in creating value added research then they have to identify what they can do that the AIS industry, despite all its financial and human resource advantages, cannot or will not do. And what economic theory indicates is that such opportunities to add value always exist—if only academics are willing to seek them out.

It is the intense competition and resources of the industry that create an opening for academic AIS researchers. Competition and rapid technological change force firms to focus on short term profits and quick payback on their own R&D. Academics have the freedom to ask questions which do not have immediate answers or generate immediately profitable applications—in other words, as the theory of comparative advantage indicates, academics have a lower opportunity cost when focusing on developments in AIS that do not lead to short term products or profits. Similarly, academics have a lower opportunity cost to invest in skills which focus more on the discovery stage of knowledge than on its practical application and commoditization. Of course, the danger in that distinction is the creation of worldviews that fail to overlap, analogous to the silos across functional areas inside firms—and the history of academic/industry relations, and not just in accounting, demonstrate that such specialization can lead to not just a lack of communication, but to feelings of intellectual superiority of each party for the abilities and perspectives of the other.

That is an attitude that AIS researchers can hardly afford to indulge in bearing in mind that some of their counterparts in industry are themselves academically trained researchers highly qualified in science and technology and far better funded—in other words, that they have

³ http://en.wikipedia.org/wiki/Comparative_advantage.

potentially an absolute advantage over them, which again should induce a focus on seeking a relative comparative advantage if there are to be any gains from trade.

What are the possible sources of comparative advantage for AIS researchers? Our aim in this paper is not to exhaustively list all feasible areas in which academics have an edge, but rather, to illustrate the utility of the principle in helping define a paradigm for AIS research. Thus, we focus here on only three obvious possibilities—the low hanging fruit of comparative advantage in AIS—drawn from instances where AIS academics generally enjoy a lower relative opportunity cost over AIS professionals:

1. **Knowledge Transfer:** *Bringing into AIS developments in related areas that might otherwise escape the attention of more narrowly focused practitioners, exploiting the lower opportunity cost for academics to scan and learn about related academic fields.* Even the most generous of IT firms, such as Google, allow their employees to spend only 20% of their time on whatever interests them as opposed to working on projects that are assigned to them. Even given their teaching and service obligations, academics have far more freedom to spend time on learning about what is going on outside accounting itself if they feel that it is important to do so, and they have the training and expertise to translate that outside knowledge into something that is accounting specific. True, they lack a profit motive to motivate such effort, but on the whole few would dispute that academics are better suited for this kind of work, which is closest to discovery driven research, than practitioners involved in their day to day work in accounting.
2. **Skill Application:** *Applying skills from outside the AIS domain that can provide insights into issues that are broadly important for AIS businesses, exploiting the lower opportunity costs for academics to study and acquire skills that are not necessary to undertake a specific AIS project but, which when applied to AIS problems, provide valuable insights.* For example, few AIS professionals know how to carry out an experiment with live subjects, but doing so may be the only way to predict how people will react to an AIS product that has yet to be introduced to the marketplace. The doctoral programs that produce accounting researchers usually aim to give them both a broader and a deeper base of knowledge than the masters programs that educate AIS practitioners. In particular, academics

tend to be better—either inherently or because of their training—at learning new academic skills, which gives them the edge at this source of comparative advantage.

3. **Long Term Normative Research:** *Helping identify potential trends that will shape AIS practice beyond the planning horizon of most profit oriented AIS firms, exploiting the lower opportunity costs for academics to speculate on ideas that will not be products for many years to come—if ever.* It may have been a businessman, the legendary Gordon Moore of Intel who said “*you gotta keep reaching. If everything you do works, you're probably not trying hard enough*”, but no one has ever confused the accounting profession with Silicon Valley, not even in the case of AIS.⁴ It is not “OK to fail” in accounting, possibly a quite understandable attitude for a profession where the perception of credibility is its very *raison d'être*. That leaves open the opportunity for academics to assume the role of the pioneers opening up unexplored country because they are not accountable to bosses or shareholders if their predictions fail to pan out.

Let us now examine each of these three areas of comparative advantage in depth to better understand how they can establish a foundation for research in AIS.

4. Exploiting Comparative Advantage in AIS Research

4.1 Knowledge Transfer

One illustration of this approach is the research on using Benford's Law to identify possible instances of fraud, which has resulted in easy to apply statistical tests now being built into many AIS software packages.⁵ Similarly, one can cite the use of regression techniques and random sampling in auditing as instances where researchers played an important role in taking methodologies developed in other fields and validating their use in accounting, with subsequent incorporation into AIS software.

If this is the source of comparative advantage that AIS researchers wish to focus on, the question is what the best approach is to succeed at it. One option is to leave matters to serendipity, allowing individual researchers to focus on what most interests them and see if there is a follow up by practitioners who come to recognize the utility of that work. A problem here is that a researcher focusing on his particular area of specialization may be

⁴ <http://www-sul.stanford.edu/depts/hasrg/histsci/silicongensis/moore-ntb.html>.

⁵ For instance, Nigrini and Miller (2007).

more interested in extending theory in the original domain than on simply transferring that knowledge to AIS. Thus, much of NAIS research on the market impact of accounting reports has ended up veering into finance instead, as the focus shifts away from the specific issue of the content of the accounting statement and to the more general question of how investors react to financial information. This may be very valuable research in its own right, but the value added in accounting itself is diminished as a result of this drift.

A strategy that is more likely to result in successful creation of value added in AIS itself is for researchers who have specialized knowledge of related areas of study with potential for application in AIS to think explicitly about comparative advantage: to ask themselves whether these are areas that AIS practitioners are likely not to focus on for whatever reason (lack of obvious connections to AIS or immediate profit potential, specialized nature of the area, such as pure mathematics or statistics, or the need for a considerable process of modification before application to AIS) and then to go about the hard work of not only converting the external methodology to make it suitable for AIS, but to also popularize their work in such a way that it grabs the attention of practitioners.

There are a number of scholarly studies that clearly demonstrate the potential of knowledge transfer for AIS. One such example concerns the Dempster-Shafer theory of belief functions⁶ that was developed in statistics in the seventies, was extensively applied in artificial intelligence in the eighties, and then was “transferred” to auditing in the early nineties⁷ to become the foundation of the most sophisticated method for evaluating audit risk and aggregating audit evidence. It is noteworthy that the pioneering scholarly publication on this topic was followed soon thereafter by an application paper directed at practitioners⁸.

Another area that has richly benefited from knowledge transfer is bankruptcy prediction, where the first major breakthrough occurred in the sixties when the classical statistical technique of discriminant analysis was utilized by Altman in the development of the Z score⁹. While subsequently various other statistical methods (such as Logit) were successfully

⁶ Shafer, 1976.

⁷ Srivastava and Shafer, 1992.

⁸ Srivastava, 1993.

⁹ Altman, 1968.

applied in this area, it also benefited from transferring powerful artificial intelligence techniques such as inductive rule learning¹⁰ and neural networks¹¹.

The most famous application of knowledge transfer in business academics was the use of the heat equation from physics to develop an option pricing model by Black and Scholes.¹² This was directed research: Black and Scholes weren't trying to contribute to physics, but instead made use of established theory in a new application, demonstrating a breadth of knowledge that is unlikely to have been present in the finance profession of their day.

AIS researchers have a way to go if they are to emulate these success stories, but we would posit that the value added they provide will be higher if they explicitly aim to exploit their comparative advantage in knowledge acquisition, transfer, application and communication, along the lines that these examples have so successfully demonstrated.

4.2 Skill Application

The second approach towards achieving comparative advantage over practitioners is perhaps the one that is most commonly cited when accounting researchers are called upon to defend their relevance: that academics have analytical skills to look at business in a different way from practitioners. The widespread complaints about the relevance of NAIS research also indicate, however, that this approach is also the one that poses the greatest danger of creating parallel rather than overlapping worldviews by researchers and practitioners.

A good example of this approach from NAIS research is the success of Bob Kaplan at Harvard who brought not one, but two innovations into management accounting practice: Activity Based Costing and the Balanced Scorecard. In each case the innovation was developed outside academic accounting, in fact by engineers working inside firms that the author happened to consult with. What Kaplan and his colleagues did was not just to recognize an important innovation, but to systematically take the two critical steps that were needed to take a good idea and make it a general technique with wide applicability: First, they utilized their scholarly training in conceptualizing and generalizing the innovation proposed by the practitioners to develop a rigorous theoretical basis for what had been one off projects inside a single firm, and second, they then popularized the techniques by publishing

¹⁰ Messier and Hansen, 1988.

¹¹ Wilson and Sharda, 1994.

¹² Black and Scholes, 1973.

in journals that aimed at practitioners and not just academics (Harvard Business Review in Kaplan's case).

In AIS research there are several promising examples of how researchers can examine issues that are generally relevant to AIS practice using skills that most AIS practitioners are unlikely to possess. A good illustration is the work of Arnold and Sutton on the use of decision aids by decision makers.¹³ As Sutton explains, their work looks at two related questions: *“Why have intelligent decision aids had such limited success in the audit domain? And, how might intelligent decision aids be more effectively designed and used in the audit domain?”*¹⁴ These are clearly extremely important questions for the development and deployment of AIS systems, especially for firms that wish to add “intelligence” to such systems and transform them from data management tools to higher value added decision support software.

In the original paper the researchers developed a theory using their skills in psychology and cognitive science allied with their knowledge of AIS technology and how it interacts with its users. Subsequently, a large literature was inspired by Arnold and Sutton's work, focusing not just on extensions to their theory, but also testing it using surveys, experiments and field based research. This is classically academic work utilizing skills that are largely absent in practice. At most businesses may undertake surveys, but they often fail to incorporate rigorous scientific methods designed to achieve objectivity and avoid selection bias.

However, while practitioners may be generally interested in a topic of this sort, they are likely to find any one paper difficult to follow and overly narrow and fail to understand that research advances in incremental steps. The very lack of familiarity of AIS practitioners with the academic methodology used in skill application research—which is what gives researchers their comparative advantage in the first place—can also serve as a barrier to transferring the results into practice. For example, while academics see the utility of experiments as a way of testing the impact of AIS technology that has yet to be widely implemented in practice, practitioners, unaware of the constraints and tradeoffs faced in experimental research, may question the value of such stylized analyses conducted with non-professional subjects. Equally, as the academic literature develops a critical mass it is all too

¹³ Arnold and Sutton, 1998.

¹⁴ <http://www.uhu.es/ijdar/documentos/DARC06/Sutton.pdf>

easy for research to be motivated by a desire to respond to what prior papers have said rather than the real world problem that motivated the original work to begin with.

Thus this strategy is also not guaranteed to lead to researchers attaining a value added with their work, as the history of NAIS research indicates—for example, the work in auditing on how to organize workflows to facilitate the effectiveness of the audit team that has now veered off into more general research into the psychological theory of human cognition, with auditing simply being a special case rather than the main focus.

A key safeguard again is to keep the search for comparative advantage at the forefront of the research project, by focusing less on the specialized analytic skills of the researchers and more on what insights they are meant to provide for AIS practice—in particular, on those insights that AIS practitioners cannot obtain on their own. Otherwise, researchers are all too likely to fall back on defining their comparative advantage only against each other and end up losing their real-world reality check.

4.3 Long Term Normative Research

The third approach towards comparative advantage is the one with the highest risk/highest return. It requires researchers to correctly identify the future of the most dynamic areas in both the economy and in accounting, and the one most susceptible to discontinuous change. But it also plays to the greatest strength of the academics, their freedom not to have to provide a payback within months that is often demanded of their counterparts in practice.

The commission that examined the 9-11 attacks famously concluded that the reason that the government did not stop them was a “failure of imagination” by policymakers that anything like them could ever occur. In fact, novelists had long imagined attacks by hijacked aircraft precisely like what took place in New York and Washington. In the absence of accounting novelists, it is up to academics to fulfill a similar role for the AIS profession: to imagine what is possible, not just what is immediately feasible, which is precisely what a strategy of long term normative research attempts to do.

McCarthy (1982) has long advocated what he calls “design science” for AIS research: *“Unlike positive research that examines the current state of practice to understand it better, design science strives to identify the means to improve upon practice. Thus, researchers using this methodology often build new systems*

to evaluate the feasibility of their prescriptions and to gain deeper insight into the problems investigated.”¹⁵

That is similar to what this third strategy entails, though McCarthy is specifically focused on his REA data model of business events¹⁶, which is probably among the best examples of long term normative research in AIS. It took more than 10 years since the pioneering publication of REA in the late seventies and early eighties until its vision was justified on practice in the development of modern ERP systems in the early nineties.

A particular attraction of this strategy in AIS is that it takes a possible weakness and makes it a source of strength: the relatively lower level of IT penetration in accounting practice compared to other comparable industries, such as finance, and the greater reliance on technology bought from outside vendors rather than being internally developed. This gives academics less competition in forecasting the future role of technology in the practice of accounting, which is essentially what this third strategy of comparative advantage implies. What we propose has the more general aim of extrapolating from current implementations of IT in related industries to the relatively less advanced accounting domain. This reduces the risk of simply indulging in unfounded speculation but it also means that the reasons for the particular resistance to change within accounting to IT need to be taken into account.

To make concrete what a long term normative research strategy might look like in the AIS domain, we now discuss some examples of our own work. We hasten to add that we focus on these examples not because they are the only or even the best application of this strategy, but because we are most familiar with how these projects evolved as a result of changes in the AIS industry and our conscious application of seeking value added through achieving comparative advantage.

The first example is the work that we, along with many other AIS researchers, have undertaken in the area of continuous auditing, which aims to help bring about a new, IT-enabled form of assurance. Given the extent to which businesses are run in real time with ubiquitous technology, the still low technological use in auditing seems to be a mismatch that cannot and should not persist. With that directed goal in mind, we have worked with internal auditors at various major firms on joint projects that help understand what CA-enabled auditing will look like and how auditing methodologies will have to be reengineered

¹⁵ David et al, (2002).

¹⁶ McCarthy, 1982.

to take advantage of existing technologies such as ERP systems, as well as emerging AIS products such as Approva or ACL. The issue of barriers to change in this area is also explicitly addressed. Hence, we both build our CA systems on the firm's existing ERP capabilities and attempt to learn from the difficulties firms faced when implementing those ERP systems about what similar problems might arise with making CA an operational reality.

The second example of long term normative research that we have undertaken is in the area of enhanced business reporting, which aims at reengineering the financial reporting model by and for technology-enabled firms.

In the wake of the Enron and WorldCom scandals, there were many calls to rethink the approach to business reporting and several initiatives by various professional bodies to do so. In particular, the Enhanced Business Reporting Consortium (EBRC) created by the AICPA, defined itself as a *consortium of stakeholders collaborating to improve the quality, integrity, and transparency of information used for decision-making in a cost effective, time efficient manner*.¹⁷ To help guide the EBRC, its Public Company Taskforce created a set of sample reports that illustrate the kinds of enhanced disclosures that it feels are necessary and useful for complex organizations in today's information economy.¹⁸ For the purposes of this paper, what is most notable about these sample reports is that they were not especially "radical". The taskforce's self-imposed mandate was developing a structure for voluntary disclosures that "enhanced" the coverage of the statutory annual income statement and balance sheet, as opposed to questioning the underpinnings of those reports themselves, such as the continued relevance of GAAP.¹⁹ As the chair of the Public Company Task Force wrote during the process that created the sample reports: *"Formats that follow outlines that are already in general use in the business information supply chain are likely to gain faster acceptance than those that are new... We will explore potential enhancements to the existing financial reporting format but will not consider wholesale re-structuring of the financial statements."*

The incremental approach of the EBR process is obviously justified in terms of change management and political reality. It is also a vivid demonstration of the principle of

¹⁷ <http://www.ebr360.org/>.

¹⁸ <http://www.ebr360.org/ContentPage.aspx?ContentPageId=27>.

¹⁹ For this reason the consortium changed its name to "Enhanced" Business Reporting from "New" Business Reporting.

comparative advantage as applied to this long term project aimed at changing the accounting reporting model: practitioners, such as those at the EBRC, are inevitably constrained by the need to win acceptance from numerous stakeholders with conflicting interests, especially if they are to obtain consensus on a product that can be implemented in the near to short term. The need to maintain comparability with the existing reporting model and to create a workable change path is another limitation on the scale and scope of innovation that can be proposed—the equivalent of the backward compatibility constraint in the IT industry.

In other words, practitioners face a high opportunity cost when proposing change, but any change they put forward is more likely to gain acceptance. Researchers face the exact opposite problem, but they retain their comparative advantage as long as their aim is to envision the long term future of the profession and the disruptive innovations that developments outside the field will likely impose on it, rather than taking it upon themselves to limit their imagination only to what is possible in the here and now.

Thus, our academic work on enhanced business reporting asks a question that no AIS practitioner would be allowed to indulge in on “company time”: *What would a reporting system look like if it was designed from scratch for 21st century firms using 21st century tools for a 21st century audience?* This is a very important question to ask because it brings technology to the forefront in a profession that has often consigned it to the backburner. But at the same time, we are entirely realistic as to the immediacy of the applicability of this research: “...*this paper puts forward one set of ideas and visions with that hope that this will begin a process that will create a shared effort on reengineering business reporting from a technology perspective. Our intention here is to lay out some of the larger forces that will shape any new business reporting model and to describe a general framework to organize the thinking of this nascent movement. What this paper really is intended to do is to invite the participation of all those who feel that we can and must do better in the way in which we measure, manage and communicate firm performance in the 21st century.*”²⁰

Our justification for this line of research is that it is serving exactly the role that academics should play in a profession, by making use of their freedom to imagine without a demand for an immediate ROI, and the freedom too, to fail in a profession where such risk taking is not, and perhaps, cannot be tolerated. The value added that this initiative is meant to provide is not a new reporting system that firms will implement as is—that is hopefully the work

²⁰ Alles and Vasarhelyi, 2007.

product of the EBR Consortium. Rather, our aim is to generate out-of-the-box thinking for reporting methodologies that will provide a guidepost for the continuing long term evolution of reporting practice: to plan ahead now to second generation products so that there is no loss of forward momentum once the practitioner-led first generation advances are implemented.

5. Leveraging Comparative Advantage: The XBRL Example

The best likelihood of achieving breakthrough value adding AIS research comes when researchers are able to simultaneously exploit all available sources of comparative advantage—knowledge transfer and skill application in the service of a normative long term goal—as part of a comprehensive and complementary strategy rather than relying only on one of them. That gives the best chance of overcoming the hurdles of the lack of resources and funding that often result in AIS researchers being at an absolute disadvantage relative to their counterparts in practice.

An example of hitting this sweet spot is the development of XBRL, which *“is a language for the electronic communication of business and financial data which is revolutionizing business reporting around the world.”*²¹ When XBRL was first proposed it embodied all three sources of comparative advantage. First, it represented a knowledge transfer into accounting of the already well established XML tagging technology. The key breakthrough was not in reinventing the wheel with tagging, but rather, the imagination to see the value added that tagging would provide to accounting reporting. Second, with skills with XML in short supply in accounting—precisely because there had been no need for practitioners to have acquired those skills before—the advocates of XML had to take the lead in demonstrating what needed to be done if accounting information was indeed to be tagged. In particular, they had to prototype the taxonomies that underlie XBRL and show AIS practitioners what it would entail to create a widely accepted and comprehensive XBRL taxonomy. Most important of all, the XBRL pioneers were driven by a vision of accounting transformed by being freed from the printed page where it had resided since the days of Pacioli. It required a clear-eyed idealism and endless patience to put up with the skepticism and ignorance of those who could not see any need for change, while being perfectly aware that even in the best case scenario it would take years to make XBRL a reality.

²¹ <http://www.xbrl.org>.

The objective of XBRL is not an evolutionary change in accounting reporting, but a discontinuous jump into a technology driven future. This is clearly a normative rather than a positive project, and the prospects are good that XBRL will be progressively transformed from theory into practice.

Thus XBRL is a superb case study for AIS researchers for how they can radically transform AIS practice and create a remarkable value added by exploiting their comparative advantages in knowledge, skills and patience to push long term initiatives, even in the face of seemingly insurmountable obstacles. Indeed, one might even think that XBRL has the potential to be the Black-Scholes of accounting—the long desired prototypical example of how research can transform practice by creating something that is beyond the ability of practitioners to emulate.

Unfortunately there is one flaw in this success story: XBRL was not the work of AIS researchers, but that of a practitioner, Charles Hoffman. Developing XBRL was not part of his job as a CPA, and much of what he did in launching XBRL—creating a community of like minded individuals and developing the prototypes—was done on his own time. In effect, he had to act as an academic while working as a practitioner, a very difficult balancing act that only a few visionaries can pull off—which is what gives academics their opportunity to have a comparative advantage.

It is worth noting how Hoffman described his achievement: *“People call me ‘the father of XBRL,’ but I like to refer to myself as the guy who did the obvious. I was in the right place at the right time, with the right set of skills”*.²² What Hoffman did was to act less like a traditional accountant and more like a classical entrepreneur: he saw an opening in the market and developed a product to fill it. There is a lesson there for AIS researchers, to follow the lead of not just Charlie Hoffman, but also of the AIS industry, which is seeing increasing startup activity supported by hedge fund and venture capital funding. If AIS researchers are to follow their industry’s trends, then they too need to be more entrepreneurial, mindful of market openings and willing to take risks.

²² <http://www.xbrl.org/Announcements/Hoffman-14Dec2006.htm>.

AIS academics may have missed the boat as far as XBRL is concerned, but the point of this example is for AIS researchers to learn how they can ensure that next time it is they who will be in the right place at the right time, with the right set of skills.

6. Implementing the Principle of Comparative Advantage

Basing AIS research on the principle of comparative advantage affects more than the kind of questions that researchers look at, it also requires a fundamental rethink of how research is conceived, carried out and communicated. Of course, pursuing such a strategy only makes sense if researchers accept the notion that the value added from their work comes from its relationship with the AIS industry, as opposed to more traditional academic metrics of success, such as publishing in the same journals as NAIS research.

Implementing the comparative advantage paradigm begins by researchers systematically and realistically assessing their strengths and weaknesses relative to the relevant sector of the AIS industry. The aim is to identify where their relatively lower opportunity cost enables them to define a source of comparative advantage, be it in knowledge transfer, skill application, long term thinking or some combination of these. This industry scanning and gap analysis must not be confined to the launch phase of the research project, however, for AIS, like IT in general, is characterized by rapid change, and hence opportunity costs and comparative advantages are continually shifting. The surest way for research to lose its ability to create value added is to fail to recognize that industry has caught up and taken the lead in an area that was once only of interest to academics.

A process of transference of ideas from lab to industry is commonplace in engineering, chemistry, medicine and other hard sciences, but it is a dynamic almost entirely absent from the mindset of most accounting academics. That hands-off attitude may make some sense in NAIS research where the accounting profession itself is conservative towards change, but it is self-defeating in the case of AIS.

Hence, for example, our work on continuous assurance has had to be recalibrated over time, from the early days when it was largely an academic-led exercise to help define what the concept meant and to sell it to practitioners, to today when an entire new industry has arisen in the continuous auditing space and which has taken the lead in product development. It would obviously make little sense for us to do the same research in 2007, when Approva,

Oversight Systems and ACL are spending large sums of money developing and marketing CA software, as we did in 1999 when the CICA/AICPA issued its pioneering “red book” to help introduce the concept of CA to the accounting profession. In 2002 we were examining where demand would arise for CA, a subject that fortunately became moot with the passage of Section 404 of the Sarbanes/Oxley Act.²³ As late as 2006 we did collaborative research with Siemens Internal Audit on prototype CA software, but the next phase of this ongoing work will be making use of Approva as the technological infrastructure supporting CA.²⁴

As our comparative advantage in defining what CA software might look like has definitively ended with the rise of the CA vendors, our research has increasingly shifted towards developing a rigorous methodology for taking manual audit procedures and “CA-enabling” them so that they can be run using the industrial-strength software. While we remain committed to a normative, long term focus for our research, the skills that we are calling upon to establish a comparative advantage have also shifted from knowledge of IT superior to most practitioners at the time, to a conceptual understanding of the audit process that is still considered value adding by our industry collaborators.

The fact that we have industry collaborators at all is an indication that the kind of AIS research we pursue is markedly different from most NAIS research where the gap between academia and practice is reflected by the almost total absence of industry in research projects. From data obtained from third parties such as Compustat rather than directly from its sources, to inferences made about the attitudes and behaviors of market participants drawn from data analysis rather than interviews or surveys, NAIS research is all too often characterized by a disconnect between researchers and the industry they are studying. Unfortunately, it would not be difficult for AIS research to follow down this same road, especially as industry takes an absolute advantage in product R&D.

For example, one of the characteristics of some current research in AIS is the application of techniques developed in NAIS research, such as experiments or even event studies, to the AIS domain. And the result is often unfortunate, since little attention is paid to the fundamental differences between AIS and NAIS practice. Thus, the bibliography of too many AIS papers includes references that are obsolete, failing to recognize that while a

²³ Alles et al, 2002.

²⁴ Alles et al, 2006.

twenty-year-old paper in auditing or managerial accounting may still be current, that is often not the case of a reference in IT that is only a few years old. For example, the assumptions made in a continuous auditing article published in 1999 or one on XBRL from 2002 are likely to be outdated, as circumstances, both technical and societal, have changed so markedly. Similarly, market studies on AIS technology adoption seem to discount the fact that with modern firms so fundamentally driven by technology and adopting it pervasively and continuously, it is very hard to assess market reactions to AIS alone in a way that can be done when a firm adopts a new financial accounting standard.

What characterizes these missteps in AIS research is a definition of value added that has more of an internal, academic focus than an outward looking one—to be blunt, the aim to gain access to journals that primarily publish NAIS research by sticking as close as possible to NAIS methodologies. As a result, there is no attempt to identify a comparative advantage for the work, which, by forcing an analysis of where the research stands relative to AIS industry, would better protect the researcher from the criticism that he or she has failed to understand the reality of AIS. To avoid the judgment, in other words, that was expressed all too often by practitioner discussants at the recent 2007 Waterloo AIS conference (which is unique for an accounting research conference in that practitioners are not just made welcome, but they are also asked to actually comment on research) that this paper “*would not interest industry in its current form*”.²⁵

While it would be foolish to argue that there is nothing for AIS researchers to learn from the methods and approaches of NAIS research, they have to be seen as tools and not an end in their own right. In our opinion, AIS research has to have an identity of its own and a demonstrated record of adding value to AIS practice before it can be taken seriously by NAIS researchers, which realistically, it never will be if NAIS researchers perceive AIS research as second-rate aping of their own work.

It is also important to note that identifying a comparative advantage is only a part of the process in creating value added. The insights of researchers must also be communicated credibly to the practitioners that can make use of them. XBRL would never have reached the stage it is now at if Hoffman had been content with simply publishing his idea, and had not dedicated his time to pushing for its acceptance by the broader AIS community. Similarly, if

²⁵ http://artsms.uwaterloo.ca/accounting/uwcisa/symposium_2007/Program.htm

they are to create value added, AIS researchers have also to be willing to do a kind of work that they may not be familiar with, which is helping transform their theories into practice to the extent possible.

We did this in continuous auditing by hosting a series of conferences around the world, with the initial aim of simply explaining to our academic colleagues (and academic minded practitioners from industry) what continuous auditing was all about. But with the advent of the CA industry it is striking that the majority of attendees at recent CA conferences are practitioners and the focus of the meetings is rapidly shifting from being entirely research oriented to more of a CA-software user one. Consolidating the value added of research through such initiatives as professional conferences and other outreach efforts is something that AIS research needs to do more often.

7. Caveats and Conclusions

In our discussion of relative opportunity costs as the basis for a comparative advantage for AIS researchers, we were deliberately disingenuous in defining that opportunity cost only relative to AIS practice. As the increasing attempts by AIS researchers to clone the methodologies of NAIS research indicate, there is another measure of opportunity cost highly salient to AIS researchers: the difficulties that they face in publishing in the top-tier accounting journals, all of which are dominated by empirical NAIS capital markets research.

It is unfortunately likely that our proposal for AIS research to create value added through the pursuit of comparative advantage relative to industry will only exacerbate the divide between the priorities and methodologies of AIS and NAIS research—and hence, the hurdles that AIS researchers will face in the journal process. Similarly, consolidating value added through such activities as hosting conferences and writing professionally-oriented papers are activities unlikely to be rewarded in promotion decisions in many research-oriented universities.

While these are critical concerns, they are ones that we cannot adequately respond to in this paper, even assuming that we had a solution to offer. Instead, we end with the caveat that achieving value added on some dimension is surely better than having it in none. Given the constraints under which AIS operates—of being, to be blunt, a junior partner in an IT industry with a large, thriving and well funded research program of its own, not to mention

being outsiders to an accounting profession historically averse toward applying technology to long-established work practices—establishing a space within which academic AIS research is credible and has a role to play should perhaps be the first imperative.

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