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THE IS CORE XII: AUTHORITY, DOGMA AND POSITIVE SCIENCE IN INFORMATION SYSTEMS RESEARCH

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ABSTRACT

Three related papers recently argued for the adoption of specific ‘organizing principles’ for academic research in Information Systems. These principles, centered on nomological networks of IT artifacts, are offered as prescriptions which, it is argued, resolve an ‘identity crisis’ in IS research. The present paper concludes that, rather than resolving an identity crisis, the prescriptions are likely to confound any search for identity by biasing future IS research into directions that do not move the field forward. We show how a positive science of Information Systems can retain the benefits sought without the recommended prescriptions.

Keywords: IT artifacts, nomological networks, scientific method, IS core

I. POSITIVISM IN INFORMATION SYSTEMS

Within the academic discipline of Information Systems, three recent articles by Benbasat and Zmud [2003], Orlikowski and Iacono,[2001]; and Weber [2003], argue for a scientific system – or as Weber alternately called it, an ‘organizing principle’ – which could define the research approaches, methodologies and, some might suggest, even the topics included in the IS discipline. All three put forth arguments for the need for better governance over IS research – arguments that address academic politics, the future health of IS research, and the methodological demands of the discipline. Both the Benbasat and Zmud and the Orlikowski and Iacono articles make detailed recommendations concerning the guidelines and principles to which research papers should conform, and suggest allowable and proscribed topics. Weber provides more general and circumspect arguments, as befits a journal editor.

The issues raised in these three papers have been raised at one time or another in all of the social sciences. More importantly, the pursuit of answers in other disciplines yielded answers and critiques of alternatives that would be foolish to ignore in our own field. It is my purpose here to review how each of these suggestions stands up to methodological trends in other 21st century sciences, many of which are further along in defining the dictates of their research than we are. By looking at their ‘scientific systems’ I would like to suggest the manner in which Information Systems research can put itself on an equal footing with other academic disciplines both in the natural sciences and in the social sciences.

WHAT IS IN THIS PAPER

In this paper, I address a relatively extensive set of issues germane to Benbasat and Zmud's [2003] system of IS research – some broached in their paper, and others reflecting assertions on which the Benbasat and Zmud system is predicated.

In Section II, I question whether the IS field is truly suffering an 'identity crisis', or whether the supposed 'identity crisis' is merely a 'straw man' thrown up to subvert the field towards the ends of a subset of IS researchers. By chronicling the evolution of technology, business implementation, and strategy since the 1950s[1] I show how rapid change and inherent fluidity in information technology can be misrepresented as an 'identity crisis'.

In Section III, I suspend judgment in order to explore Benbasat and Zmud's prescription for their supposed 'identity crisis': imposing on the field a taxonomy of 'IT artifacts' which dictate 'acceptable' research. I will show that the Benbasat and Zmud system will tend to censure relevant, high quality research, while rewarding antiquated dogma. I argue that the harm to IS research of such a system could be significant and possibly irreparable. To succeed, IS research needs to avoid dogma, and grow more fluid and multi-disciplinary.

Sections IV and V investigate specific assumptions on which the Benbasat and Zmud system is predicated. In section IV, I show that the two cornerstones of the Benbasat and Zmud system – errors of exclusion; and errors of inclusion – in fact derive from a classical fallacy. Fallacious reasoning undermines Benbasat and Zmud's claim to scientific validity, and would make IS research less credible, in turn damaging the influence of IS in business schools.

Section V looks specifically at the origins and application of nomological networks – the central machinery invoked by Benbasat and Zmud to implement their dogma. Deductive-nomology, and nomological networks lost philosophical credibility throughout the 20th century, and now are relegated to mainly descriptive endeavors like document indexing.[Bringsjord, 1999] The current state of nomology may be adequate for Benbasat and Zmud's system, since they only want to use nomological networks as a vehicle for indexing their 'IT artifacts.' But I will present evidence that even in this domain, the Benbasat and Zmud system departs significantly from the reality of academic research publication.

Section VI details what I believe can provide an effective alternative to the Benbasat and Zmud system for Information Systems research – a positive science[2] of Information Systems. Most scholarly disciplines embraced positive research methodologies over the past century – in particular the business research disciplines starting with Milton Friedman's championing of positive methods in economics. Friedman (1953) describes a *positive* science as one that constructs theories through inferences gleaned from 'experiments', (the perspective adopted in this paper) in contrast to a *descriptive* science which he likens to an 'analytical filing system.' The Chicago School was founded on the presumption that *positive* inferential science predicates either *descriptive* or *normative* science. Led by Friedman, it became a major influence on economics, law, sociology and other social sciences over the last half century I also discuss the consequences, supported by historical examples, of adopting one or another approach in research.

The arguments of Section VI are reinforced in Section VII which summarizes my conclusions about the applicability to Information Systems research of one of the most successful of the 20th century's scientific systems – 'positive' science. A closer inspection of 'positive' science is, in

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[1] See El-Sawy [2003] for an alternative chronology.

[2] Positive science is also referred to as 'positivism' although the latter term is most often associated with the system of Auguste Comte that stresses attention to actual practice over consideration of what is ideal. Friedman's adaptation of Keynes' positive science more directly addresses the concerns of 21st century business research.

particular, warranted by Benbasat and Zmud's appeal to deductive-nomological explanations, a construct that grew out of the 20th century positivist movement in the natural sciences.

II. IS THERE REALLY AN IDENTITY CRISIS?

Before inquiring into the merits of Benbasat and Zmud's prescription for the IS research 'identity crisis' it is useful to assess whether indeed this 'identity crisis' exists, or whether 'the crisis' is merely a Straw Man thrown up in support of an alternative agenda. I would first like to explore the retrospective study which engendered Benbasat and Zmud's prescriptive advice for IS research. In the process, I will take a closer look at the theory that underlies the prescriptions, and the consequences of Benbasat and Zmud's proposed system. A positive methodology of Information Systems could effectively circumvent specific biases and problems identified in IS research by Weber and could prevent the stagnation inherent in authoritarian prescriptions.

There may, in fact, be more reasonable explanations than an 'identity crisis' for the failure of a few stable 'core' topics to coalesce in IS research. Business schools, like other professional schools, tend to grow around moneyed constituencies that can fund research, endow chairs, hire students, and otherwise provide support. Within business schools, for example, the accountants have the accounting firms; finance banks and brokerages; and marketing and strategy have consulting firms. Information Systems perpetually faces a moving target with elusive constituencies. This assertion is not a justification to avoid seeking an 'identity.' Rather I suggest that IS faces a more difficult task in defining useful and meaningful bounds for its identity than other business disciplines. Any 'identity' we fabricate will be inherently fluid and usually less precise than we might like.

Table 1 is my attempt to track shifts in the types of IS research, information systems, and their constituent groups over the past four decades, and into the next.

Readers can take issue with the details of these shifts in focus and 'IT artifacts,' but the regularity of five year shifts is discernable. Shifts in the technology engendered at least three major shifts in management principles since the early 1960s.

- The first occurred after the introduction of IBM's 360 on April 7, 1964, the product of the greatest capital investment in history up to that time (IBM spent $5 billion and hired 60,000 new employees). Once they adopted S/360s, the number of people involved in central bookkeeping operations of large firms like Ford and Dupont dwindled from several thousand to several hundreds, and accountants like Harold Geneen rose to dominate corporate strategy.

- With the introduction of the Apple II on April 1, 1980, and subsequently the IBM PC, businesses found a tool to break the stranglehold of the mainframe data processing department. Empowerment, speed of decision making and islands of technology were obsessions of 1980s corporate culture. Salesmen like Lee Iacocca became the celebrity managers of the day. The securities industry was revolutionized when traders like Michael Milken divested power to their sales forces through desktop computers.

- Strategy shifts at Microsoft and Netscape in the summer of 1995 heralded a third managerial shift. This shift introduced the virtual office, salesmen-less sales through web retailing, and increasing delegation of decision making to machines, sensors and actuators. People who actually understood the behavior of these machines, people like Bill Gates, Michael Dell, and Larry Ellison became the celebrity CEOs of the day.

Table 1: The Elusive Identity of IS

Period	Information Systems	IS Constituencies	IS Research Topics (Artifacts)
Early 1960s	Unit record processing on punched cards	Data Processing Shops	FORTRAN, Algorithms, Punched Card Workflow
Late 1960s	Centralized IBM mainframe with dedicated staff for I/O	IBM Programmers	COBOL, Data Operations, Data Structures
Early 1970s	Database management and CICS shift mainframe I/O to the users	Database and network administrators	Hierarchical Database Models / Management
Late 1970s	Office automation and workflow around departmental groups dominate business; various types of ‘decision support’ systems (plain, group, executive) are promoted	Systems integrators, Decision Support, Outsourcing	MIS, Office Automation Models, Business Systems Planning, Flowcharting and HIPO
Early 1980s	Personal computers introduced	Microcomputer systems developers and vendors, Interface designers, PC programmers	DSS, Relational DBMS, DB Normalization
Late 1980s	Client server takes off, empowering workers and distributing computing power around the firm	Corporate knowledge base designers, network administrators	CASE Tools, GDSS, Systems Design and Analysis, Reengineering, Critical Success Factors
Early 1990s	GUI interfaces take off with Windows 3.1, Client-server for organizational knowledge sharing and decision making	PC Interface and Interaction programmers	Knowledge Engineering, Executive Information Systems, Islands of Automation, Outsourcing
Late 1990s	Web takes off, distributing power and information around the globe, ERP, CRM	E-commerce companies, IT Consulting	Internet, e-Commerce, Economics of IT, Charts vs. Tables, Value Chain Integration
Early 2000s	Embedded systems take of, Wireless networked devices overtake the PC	Consumer electronics designers, Video Game interface designers	Virtual Organizations, IT Enabled Business Models, Financial Models of Technology
Late 2000s (predicted)	‘On Demand’ Computing, Personal robotics take off	Robotics interface designers, HR, Organizational Structure and Design	Future Managerial Problems in Technology
Early 2010s (predicted)	Mesh webs of smart devices permeate all human activity	IT Policy makers, Government	Future Managerial Problems in Technology

The regularity of these shifts exposes a risk in relying on retrospective studies to define the field. Only a small percentage of past topics or conclusions are likely to be relevant in the current environment. Results grow obsolete and irrelevant very quickly, and scholars and businesses that rely on these results are likely to make incorrect decisions. We can’t expect a ‘central core’ composed of stale research to chase a moving target effectively; particularly if it moves at an accelerating rate if Moore’s Law and similar predictors of progress in technology continue. The mathematics is straightforward: if the lag between research and publishing is three or even five years, and if IS shifts every five years, then less than 20% of a 'central core' based on 10 year retrospective citation studies will actually be relevant to current information systems. And very little if any of the new technology in the field will ever appear in the derived 'core' defined in this way. Very old research would tend to be highly ranked, simply because it was around longer.

Exacerbating the problem with an artifact-centric approach to research is that artifacts themselves take time to create, gather, aggregate, filter and write up, creating an even longer time lag between an artifact's emergence , and its resultant investigation in IS publications. Engineers in the hardware disciplines (mechanical, civil, electrical, chemical) certainly don't wait for artifacts (or the scholars gathering them) to improve performance through innovation. It seems odd to expect their counterparts in the business and management of IS to do the same.

The word 'artifact,' which is so central to Benbasat and Zmud's and Orlikowski and Iacono's systems, conjures up notions of antiquity. An artifact is "an object produced or shaped by human craft, especially a tool, a weapon, or an ornament of archaeological or historical interest." [Dictionary, 2000] This definition seems at odds with a field whose subject matter is cutting-edge technology that is constantly changing and improving. Yet closer readings of both Orlikowski and Iacono and of Benbasat and Zmud suggest that this is honestly what they intended. It fits well with their general tenets of IS research as a centrally governed descriptive taxonomy incorporating little new technology and giving extra credibility to old research. The designation 'IT artifact' is very apt indeed.

Management author Peter Drucker conveys the risk of letting historical artifacts tyrannize new ideas in his impressions of IBM founder Thomas Watson Sr. "… if there had been a *Harvard Business Review* during the 1930s, it would have run stories about him, and he would've been considered a nut or a crank." [Maney, 2003] Drucker was commenting on what the business community thought of Watson's 'data processing' idea – a term that Watson coined, and which seemed far fetched to his business associates. If legitimacy had been up to the journals of his time, neither the IS industry, nor an IS research discipline would exist today.

Accounting, finance, and other business school disciplines traditionally serviced much more stable constituencies than IS, although current industry upheavals may confront them with their very own 'identity crises' in the near future. If indeed this change happens, we can perhaps derive some comfort from knowing that the pervasion of information systems throughout bookkeeping, securities trading, supply chains and product markets is largely responsible for the ensuing chaos.

So is the 'identity crisis' real, or are we merely ignoring our constituencies? From a static viewpoint, the rapid change in industry may indeed look like an 'identity crisis.' But it is a bit difficult for me to believe, in a large field such as IS, with a diversity of talented and intelligent researchers, that we are not blessed with a surfeit of ideas and perspectives to guide us. We may not all agree on what the 'identity' of IS research should be, but, then, isn't this the sign of a healthy, inquisitive discipline? The 'identity crisis' to which Benbasat and Zmud allude may well be the receding mirage of past research well-done and at one time important, but which no longer finds many interested constituents.

III. PICKING WINNERS

Predicated on their claim that IS research does indeed have an 'identity crisis,' Benbasat and Zmud prescribe a solution. It is a relatively draconian solution, involving a central governing authority that determines – *a priori* – which research topics may or may not be considered IS research. They make arguments (which I will explore shortly) to justify the correctness of picking 'core' topics *a priori*, rather than allowing ideas to stand or fail based on their merits. In other sciences, core topics are usually determined *ex post facto* through a system of checks and balances involving experiments and other evidence.

Whatever the intellectual merits of Benbasat and Zmud's prescription, there are pragmatic concerns. The pragmatic issue that came almost immediately to the minds of those I know who read Benbasat and Zmud was that of the politics of the field – who exactly is this proposed governing authority that will be picking winners (and losers), and on what criteria will they choose?

The criteria for picking winners are succinctly defined in Benbasat and Zmud in terms of the 'IT artifacts' specified first in Orlikowski and Iacono, plus another concept, the 'nomological net.' Since 'IT artifacts' and 'nomological nets' are claimed to be central to our 'identity crisis' and prescriptions for its resolution, I'd like to delve a bit further into the meanings of these terms.

Orlikowski and Iacono begin with the precept that the only acceptable subject matter in IS research should be 'information technology artifacts': "bundles of material and cultural properties packaged in a socially recognizable form such as hardware and software." They offer scant justification for this idiosyncratic perspective, which seems odd given that hardware, at least, is usually presented in discussion, research, and the press in terms of its physical, mechanical and electrical characteristics. This is not to say that their unique perspective of 'cultural properties packaged in a socially recognizable form' is not valid; just that it needs to be better articulated if it is to be useful for prescribing the dictates of a field of research.

By its nature, the greatest impact of Benbasat and Zmud's prescriptions will appear in the journals. Other effects, it can be argued, will be knock-on effects given the central role of scholarly journals in research institutions. The winners in Benbasat and Zmud's governance system will, of course, be those researchers whose careers focus on 'core' topics – topics sanctioned by the central authority. Their papers will be accepted to IS journals, they will be tenured into IS positions, and they will come to dominate editorial boards. The losers face banishment as gypsy scholars in lesser institutions with heavy teaching loads and miserable research funding.

Because of the central role journals play in Benbasat and Zmud's system, it is useful to look at the historical roles, as well as prediction of future roles that journals will play in a research discipline and its evolution. Among the earliest research journals were the Proceedings of meetings of the Royal Society in the 17th century. In that time, the act of publishing scientific inquiry was controversial, and widely ridiculed. Publishing was expensive and scientists typically just circulated their papers, letters, or work-in-process among a small group of peers. It was not at all unusual for a new discovery to be announced as an anagram, reserving priority for the discoverer, but indecipherable for anyone not in on the secret. Both Newton and Leibniz used this approach. This method did not work well at all. Merton, a sociologist, found that in cases of simultaneous discovery in the seventeenth century, 92% ended in dispute. This number dropped to 72% in the 18th century, 59% by the latter half of the 19th century, and 33% by the first half of the 20th century [Andrade,1954]. Steady acceptance of the modern scientific paper was largely to credit for the decline in contested research.

The Royal Society was steadfast in its unpopular belief that science could only move forward through a transparent and open exchange of ideas backed by experimental evidence. Many of the experiments were ones that we would *not* recognize as scientific today – nor were the questions they answered. For example, when the Duke of Buckingham was admitted as a Fellow of the Royal Society on June 5th 1661, he presented to the Society a vial of powdered 'unicorn horn.' It was a well-accepted 'fact' that a circle of unicorn's horn would act as an invisible cage for any spider. Chief experimenter Robert Hooke emptied the Duke's vial into a circle, and dropped a spider in the center, which promptly walked through the circle and off the table. In its day, this was cutting edge research. But colleagues might rightly question our sanity were we to investigate 'unicorn artifacts' today.

The invention of the research journal was so successful that both the number of journals and of papers proliferated over the past few decades. As a result, the topics covered in any single journal tended to narrow, and readership and citation declined. Odlyzko [1997, 1998, 1999, 2000] provides compelling evidence that journals will evolve into something akin to Internet forums over the coming decade, by extending the interactivity of current Internet preprints. This change will likely open them to a greater range of ideas, some more developed than others (very similar to the ideas that permeate a forum). Forums, like markets, tend to thrive or fail based on their ability to attract talent. Highly restrictive and tightly monitored forums are least likely to thrive.

The most ambitious effort following Odlyzko's suggestions is probably that of Harold Varmus, director of the US National Institutes of Health, for a global website centralizing biomedical literature. Varmus plans for *PLoS Biology* and *BioMed Central* to provide author-funded, full access to the entire biomedical research literature for anyone with a computer and an Internet connection. It would technically be simple for many journals (including CAIS) to follow this formula once the editorial decision is made [Harmon, 2002]. These trends, which Odlyzko shows are irreversible for economic reasons, will favor greater transparency and exchange of ideas, and will limit any attempts to impose autocratic control over research topics.

IV. APPEAL TO AUTHORITY

Let's now review the merits and weaknesses of underlying theory invoked in Benbasat and Zmud's prescriptions for IS research. Benbasat and Zmud intend for their system to be regulative – that is to prescribe and regulate the conduct and publication of IS research in the future. They say:

> *"Our specific concerns herein involves two troubling trends regarding the current conduct of IS research: errors of exclusion of constructs reflecting the core properties of the IS discipline, i.e., the IT artifact and its immediate nomological net, and errors of inclusion of constructs that lie outside this scope." (p. 186)*

Their so-called 'troubling trends' regard the failure of IS academics to restrict their research to officially sanctioned topics: "the IT artifacts and their immediate nomological net" in the vernacular of Benbasat and Zmud. The choice of these officially sanctioned topics is not clearly identified within the text of the paper, but allusions are made to one specific authority: Howard Aldrich, a University of North Carolina sociology professor. Ironically, by their own standards of inclusion and exclusion of expertise in addressing research questions, Benbasat and Zmud should have selected someone else, as Aldrich is neither an expert in Information Systems, nor in the philosophy of science. Nevertheless, in an attempt to identify the legitimacy of particular topics in IS research, the following assertions are made:

1. Aldrich is (or at least is claimed to be) an authority on legitimacy of research disciplines.
2. Aldrich claims that two types of scientific legitimacy exist: "cognitive legitimacy" and "sociopolitical legitimacy" [3]; and
3. Therefore, "cognitive legitimacy" and "sociopolitical legitimacy" as defined by Aldrich provide an exhaustive set of assessment criteria for the legitimacy of any IS research.

This line of argument represents a classical fallacy – the Appeal to Authority. The conclusion that, "cognitive legitimacy" and "sociopolitical legitimacy" as defined by Aldrich should provide an exhaustive set of assessment criteria for the legitimacy of IS research is fallacious. Even if it

[3] Aldrich's definitions of these terms are quoted out of context, so it is not clear that Aldrich truly argued that these terms provide an exhaustive basis for scientific legitimacy, or were even intended to be applied in arguments of scientific legitimacy to specific fields, like IS.

were not, we would want to question why – on topics of scientific legitimacy – we should defer to scholars in sociology over those in philosophy of science which specifically addresses the domain and methodologies of academic disciplines. These indeed are troubling trends, but not in the sense that Benbasat and Zmud intended. What is 'troubling' is weakly argued theory invoking dubious constructs.

V. FINDING NOMO

'Nomological nets' – a concept central to Benbasat and Zmud's prescriptions for the IS 'identity crisis' – are constructs that appear in psychometrics, micro-level citations studies, information science, as well as in artificial intelligence, and epistemology. The nomological network is an idea that was developed by Cronbach and Meehl [1955] as part of the American Psychological Association's efforts to underpin psychological testing with philosophical theory, where the main focus was to determine the construct validity of psychological testing metrics. Their strategy was to invoke the formalism of philosophical theory to link the theoretical realm with the observable one. This network would include the theoretical framework for what you are trying to measure, an empirical framework for how you are going to measure it, and specification of the linkages among and between these two frameworks. Unfortunately, their 'nomological network' never did provide a practical and usable methodology for assessing construct validity. Nomological networks were abandoned in psychometrics after development of the multitrait-multimethod matrix. By the 1980s, even Cronbach was recommending that the field abandon nomological networks for a contextualist approach that clearly defines the boundaries of test score use [Cronbach, 1988].

Cronbach and Meehl did their work at the Minnesota Center for Philosophy of Science, where they were strongly influenced by Herbert Feigle and Michael Scriven, proponents of Carl Gustav Hempel's philosophy; particularly ideas presented in Hempel's, [1950; 1952] treatises on language. It should be noted that, in the 1950's Hempel's philosophy did not yet encountered the criticism that would reveal its flaws. Thus Cronbach and Meehl were probably justified in trusting that they devised a consistent formal system for psychological construct validity, even if it proved useless in practice

Cronbach and Meehl's nomological networks are central to both Benbasat and Zmud's system, as well as being of central concern in citation behavior. Thus it is worthwhile to explore the origins of Hempel's nomological artifacts in philosophy in the quest to better understand the epistemology of so-called 'nomological nets.'

Carl Gustav Hempel started his career in the philosophical camp of logical positivism, but then moved on to his own deterministic version at odds with that camp. Hempel's deductive-nomological explanation of a 'fact' is a deduction of a statement (called the explanandum) that describes the 'fact' we want to explain; the premises (called the explanans) are scientific laws and suitable initial conditions. For an explanation to be acceptable, the explans must be 'true' [Hempel and Oppenheim, 1948].

According to the deductive-nomological model, the explanation of a fact is thus reduced to a logical relationship between statements: the explanandum is a consequence of the explanans. Pragmatic aspects of explanation are not taken into consideration; this approach can lead to some very strange conclusions, as I will illustrate later. Another feature is that an explanation requires scientific laws; facts are explained when they are subsumed under laws.

The language of Hempel's 'deductive-nomological model of scientific explanation' has been merged into the language of citation analysis where it provides convenient formalism, similar to what was attempted in psychometrics. From the standpoint of citation behavior, where a broad range of words found in literary citations need to be represented in a universal notation, the explananda and explanans from such 'deductive-nomological explanations' provide a convenient basis for the nodes on a network of perceived relationships between these concepts.

When psychometrics and citation analysis adopted the language of Hempel, they neglected other artifacts from logical positivism: specifically fundamental assertions that scientific laws are not genuine statements, because they are not completely verifiable; they are rules employed to make predictions. The only criterion for justifying scientific laws is the reliability of forecasts; causal laws express nothing but the possibility to make a prediction.

Hempel's idiosyncratic interpretation of logical positivism suffered repeated criticism over its life. The logical positivist Peter Railton believes that Carl Hempel belongs to a class of philosophers who are reluctantly bound to the determinism of the 19th century. Railton [1978] asserts "… the aim of probabilistic explanation is not to demonstrate that the explanandum fact was nomically expectable, but to give an account of the chance mechanism(s) responsible for it." Railton rejects the requirement for all explanations to adhere to the deductive-nomological schema. Hempel was eventually compelled to provide a predictive extension to his system, in the form of his Inductive Statistical model of explanation. Unfortunately, he later discovered that his approach suffered from serious flaws, leading Coffa [1974, p. 141] in his article *Hempel's Ambiguity* to declare this reason enough that "Hempel's views on inductive explanation ought not to be accepted."

In short, several decades ago, Hempel's philosophy seemed to be able to provide a convenient language for expressing certain ideas in psychometrics and citation analysis. But Hempel's system of deductive-nomological explanations eventually proved useless as a tool for scientific investigation. Its inconsistencies and flaws relegated it to little more than a footnote, an intellectual *cul-de-sac*, in 20th century thought.

WEIRD SCIENCE

Hempel's deductive-nomological approach can be used to draw some seriously odd conclusions. Here is one example. Hempel [1973] criticizes logical positivism's distinction between observational and theoretical terms, providing an example in Hempel [1988] concerning the inferential function of scientific theories. He argues that it is impossible to derive observational statements from a scientific theory. As an example, he gives Newton's theory of gravitation. This theory, he argues, cannot determine the position of planets, even if the initial conditions are known, because Newton's theory deals with the gravitational force, and thus the theory cannot forecast the influences exerted by other kinds of force. In other words, Newton's theory requires an explicit assumption – a *provisoe*, according to Hempel – which assures that the planets are subjected only to the gravitational force. Without such hypothesis it is impossible to apply the theory to the study of planetary motion. But this assumption does not belong to the theory. Therefore the position of planets is not determined by the theory, but it is implied by the theory *plus* appropriate assumptions. Accordingly, observational statements are not entailed by the theory, and there are no deductive links between observational statements. Hence it is impossible that an observational statement is a logical consequence of a theory (unless the statement is logically true). The peculiar implication is that the empirical content of a theory does not exist. According to such interpretation, scientific theories are rules of inference, and thus are prescriptions according to which observational statements are derived. Hempel's analysis shows that these alleged rules of inference are void.

Bottom line: Hempel concludes that Isaac Newton's theory of gravitation tells us nothing about why or how the Earth orbits about the Sun, despite the fact that Newton's theory of gravitation was used successfully to predict eclipses, trajectories, and to put a man on the moon. Nomological networks and deductive-nomological explanations could, if taken to the extreme, be used to show that the Earth sits at the center of the Universe, that unicorns are real, and all sorts of other pseudoscientific artifacts. Is this the sort of theory we want underpinning IS research?

ISI: THE MINISTRY OF SCIENTIFIC INFORMATION

The main defender of citation analysis is also its primary beneficiary. ISI[4] publishes the Social Science Citation Index, among numerous other databases. For this reason alone, questions over the legitimacy of citation analysis can and do elicit heated debate.[5] ISI always acknowledged the limits of citation analysis, but the lure of generating reams of output by blindly parsing databases proved irresistible. Since ISI was sold by its founder, Eugene Garfield, in the early 1990s, it reacted to this demand by producing software packages to help users probe its database, while charging for access as well.

ISI fields experts to defend the validity of citations wherever debate over their validity arises, much as the tobacco companies send lobbyists into every debate over smoking. The money involved is significant; the cost of buying certain ISI data rose almost fourfold between 1995 and 2003.

ISI's role in widening the use of citation analysis is self-serving. It is also accused of underestimating the risks and biases inherent in these studies. Benbasat and Zmud and Orlikowski and Iacono base their conclusions on citation analyses, in the tradition of co-citation studies from the 1980s by Swanson and Culnan [1987] with the aim of describing 'core' research areas in IS. Both studies were based on research methods used in seminal work by White and Griffith [1981]. White and Griffith originally set the format for citation analysis research as a way of identifying 'coherent groups akin to schools' in a discipline. Importantly, neither prescriptive, normative, nor regulative claims were made for the approach. Results were simply intended to provide descriptive summaries of recent history and current 'schools' in a discipline. Cozzens [1989] articulates a widely held assumption that citations are merely another form of internal evidence to be considered in conjunction with external evidence for the author's proposed addition to the corpus of knowledge about the nature of reality.

A more insidious problem arises from the very nature of 'schools' within a discipline. Comparisons between fields tend to be meaningless. For example, mathematics researchers rarely cite more than one or two references, whereas a typical paper in molecular biology includes dozens. Similarly, mathematics papers tend towards being sole authored, while papers in biology, medicine and physics may be by dozens of authors. Similar though less extreme differences can be found in the IS 'schools.' In such environments, cohort group and self-citation will bias the 'core' towards fields in which papers tend to be coauthored. In addition, citation analysis tends to favor:

1. areas of research that are relatively unchanging over time;
2. small coalitions publishing in narrow areas; and
3. methodology papers.

THE DIFFUSE 'CORE' OF IS

Both Benbasat and Zmud and Orlikowski and Iacono make implicit Appeals to Authority by limiting the journals that they include in their scope, arguing that these are the only 'authoritative' IS journals. Orlikowski and Iacono distill their taxonomy from a retrospective study of a decade of publications in only a single journal: *ISR*. Not surprisingly, they discovered 'core' areas in IS that are quite similar to those identified in Swanson and Culnan's study a decade and one-half earlier – vivid proof of conservative bias in the methodology.

The inherent risk in such a narrow study is that you will miss most of the work that appeared in IS research. IS researchers publish in around 120 pure IS journals and around 200 related journals [Peffers and Tang, 2003] . Only a tiny fraction of their output can physically appear in ISR which

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[4] Originally the Institute for Scientific Information, it is now owned by the Thompson Corporation of Toronto

[5] The different issues in the debate are nicely summarized in the news feature "Citation analysis: The counting house" [Nature,2002].

only publishes around 25 articles a year. Bibliometrics researcher Per Seglen [1997] found that about 15% of the articles in a typical journal account for half of the citations gained by that publication. This percentage means that a typical paper in a journal with a high impact factor may not, in fact, be cited much, and may not even be considered significant, important or 'core' research at all. He concludes that the reputation of 'major' journals depends on the impact of only a few papers, and that influential papers in a research discipline tend to be scattered across a broad range of journals. This conclusion is consistent with the argument above that the top journals run the risk of censoring many truly innovative works[6]. This fact alone clearly indicates that parsing only one or two journals for the 'core' of a discipline is a flawed, and is almost certain to exclude the actual 'core' topics in the discipline. Seglen also discovered that journals that predominately publish review articles tend to be cited most, and that the number of citations is further biased by the topical areas in which the research is conducted – that in some topics, small cliques of researchers will gratuitously co-cite each other, driving up their statistics in the ISI databases, vastly overweighting the perceived significance of their research.

Seglen's findings may understate the situation in IS research. The average citation count at any of the leading IS journals tends to be quite low, perhaps less than one citation per paper per year. At the same time, a survey by Peffers and Tang [2003] identified 326 journals recognized by IS researchers as publication outlets, reflecting an exceptionally high dispersion of IS research across journals. Perhaps these figures should not surprise us. Throughout the 1990s, IS enabled corporate reengineering was the major force breaking down 'stovepipes' and internal fiefdoms in organizations. Consequent innovations in IS are therefore likely to appear in a wide range of outlets. These same organizational trends are also widening the range of interests in other business disciplines (e.g., e-commerce amongst Marketing faculty, electronic markets and microstructure in Finance). IS set this trend, and it is only natural for IS research to embrace a broadening of the field and its potential research outlets. Furthermore, Prusak and Davenport [2003] conducted research that suggests that a diffuse core may be the rule rather than the exception in business research of all sorts. Thus the diffuseness of an IS core should not surprise us.

The likelihood that citation analysis restricted to one or two journals will miss the significant intellectual trends in the field is, thus, high in IS, and will increase with the number of journals in which IS researchers publish. Restriction of our 'core' search to just one or two journals, though easier to perform, is likely to produce meaningless results. It is likely to miss the most significant intellectual movements in the IS discipline.

The consequences are clear. If these systemic biases in citation analysis are allowed to define the bounds of IS research, the topic matter of academic IS will consistently lag that of industry and vendors, and IS research will indeed grow irrelevant. If business school administrators and accreditation boards now consider IS as expendable, then defining the boundaries of the field in terms of past subject matter can only exacerbate our problems. And expect the *Harvard Business Review* to publish more articles to the theme of Carr's 'IT Doesn't Matter.'[Carr, 2003]

VI. TRENDS IN 21ST CENTURY SCIENCE

An extensive literature in the philosophy of science describes the manner in which various possible perspectives of science can be used to conduct research inquiries. In the social sciences, these approaches broadly fall into three categories:

1. *positive* science, provides a body of systematized knowledge concerning what is;

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[6] Some argue that 'journals publish what they have published'. That is, as journals age, they develop specializations. As a result, authors send papers to them that are likely to be accepted because they are of the same type as appear in the journal.

2. *normative* (also called *prescriptive* or *regulative*) science provides a body of systematized knowledge discussing criteria of what ought to be; and
3. *descriptive (*also called *interpretive)* science concerns the way in which the objects of scientific study are expressed in the language.

In his influential essay *The Methodology of Positive Economics*, Nobelist Milton Friedman [1953] cites the influence of logician John Neville Keynes in first defining these categories in a meaningful way in the social sciences. In the natural sciences, concepts of *positive* and *descriptive* science had been debated from antiquity. Friedman's essay likens *descriptive* science to an 'analytical filing system' of science: tautologies that are important for the internal consistency of any intellectual discipline, but which necessarily are predicated on the antecedent of a *positive* science that constructs theories through inferences gleaned from 'experiments.'

Descriptive science is language shorn of inference. Viewed solely as a language, theory contains no substantive content; it is a set of tautologies. Its function is to serve as a filing system for organizing empirical material – a taxonomy. The criteria by which theory can be judged are those appropriate to a filing system. Are the categories clearly and precisely defined? Are they exhaustive? Do we know where to file each item, or is there considerable ambiguity? Is the index system so designed that we can quickly find an item we want? Are the items we want to consider filed together? Does the filing system avoid elaborate cross-reference? The answers to these considerations depend on both formal logical, and situation-specific factual considerations. Ultimately, some questions in a deductive science will be unanswerable –as guaranteed by Gödel's Incompleteness Theorem which proved the impossibility of a comprehensive self-contained logic. Such questions would not be able to adapt by borrowing building blocks from other fields because such moves would place the research on a taboo, in-between boundary that doesn't fit nicely into the existing taxonomy.

In contrast, a positive science tests the legitimacy of a single hypothesis; or perhaps a population of *competing hypotheses* that carry varying degrees of strength based on their past predictive performance, by comparing its predictions with experience. In theory, there are no unanswerable questions in positive science (though obtaining the answers may, in many cases, be prohibitively costly); by the same token, there are no immutable 'truths' or indisputable 'facts' either. The hypothesis is rejected if its predictions are contradicted either frequently or more often than are predictions from an alternative hypothesis. The predictions need not be about unobserved events; data may be artificially censored to use the theory to 'predict' events already observed. A hypothesis is accepted if its predictions are not contradicted, and the hypothesis may become generally accepted if it survives many opportunities for contradiction. Factual evidence can never 'prove' a hypothesis; it can only fail to disprove it.

Friedman's insights were not particularly novel; he was merely bringing into the social sciences insights that permeated the perspective and conduct of research in the natural sciences after the turn of the 20th century. These insights arguably motivated establishment of the great European research societies – The Royal Society in London, and the Académie des Sciences in Paris. Both were founded in the 17th century to promote experimentation and to overturn the Aristotelian *descriptive* science dictated by the medieval church. Physicist Max Tegmark [2003] comments that the intellectual tension between these two scientific paradigms

> *"… arguably goes as far back as Plato and Aristotle. According to the Aristotelian paradigm, physical reality is fundamental and mathematical language is merely a useful approximation. According to the Platonic paradigm, the mathematical structure is the true reality and observers perceive it imperfectly. As children, long before we had even heard of mathematics, we were all indoctrinated with the Aristotelian paradigm. The Platonic view is an acquired taste."*

Modern scientists are Platonists by necessity. The objects of scientific study are now so far outside direct sensory experience that it is seldom possible to build useful theories from human observations. Human senses simply are not built to deal with subatomic particles at one extreme; and galaxies at the other. Mathematical models are needed to describe and predict reliably across the full range of phenomena that 21st century science studies. In the social sciences, these mathematical tools are often statistical[7] [Stigler, 2002]

Delving a bit further into the distinctions between these research approaches, consider that until late in the Renaissance, three Greek philosophers – Socrates, Plato, and Aristotle – defined much of the received wisdom of the world. The first of the three, Socrates, refused to write down his ideas, insisting that to do so dulled the memory. The growth in complexity of science all but relegated the Socratic approach to polite discourse after hours and over a beer.

Socrates' pupil, Plato, was a champion of reasoning through problems in a constantly shifting world, dividing the world into ideas or objectives which were permanent; and into phenomena, which were measurements or perceptions of those ideas and objectives. According to Plato, you were limited in the information about the world of phenomena that could be gained through measurement and the senses. Only reasoning through a problem or situation goes straight to the idea. Plato's star pupil, Aristotle, carried on this tradition, though he took fundamental issue with Plato's philosophy. The son of a physician, young Aristotle followed his father in making detailed taxonomies of the anatomies of animals and their behavior in the wild. These taxonomies engendered a "bookkeeping" mentality, and a dedication to rigid rules of logic that distinguished him from the more inquisitive Plato. Aristotle's rule-laden taxonomies segued with the divine authority of the medieval church, and reigned well into the modern era.

The success of mathematics in predicting previously unknown phenomena – in relativity, quantum mechanics, and other fields –pushed the natural sciences into the Platonic camp. Plato's ideas and demeanor seem most attuned to the world of the 21st century. This conclusion perhaps reflects the psychological attitude needed by scientists to address the increasingly complex phenomena they are called upon to study. The Platonic view embraces argument and discovery centered on idealized models (theories and hypotheses), allowing rapid advancement and innovation is science. It avoids the evasion and weasel words of Socratic Method. It eschews the mindless checklists and structure of Aristotle. It seeks a holistic picture of a discipline that embraces open-ended inference, innovation and change.

THE TYRANNY OF TAXONOMIES: A HISTORICAL PARABLE

The logical positivists were obsessed with the indeterminacy and uncertainty revealed in Einstein's theory of relativity (which showed that locations and motion were relative, not absolute) and Gödel's Incompleteness Theorem (in response to Bertrand Russell's paradox), and quantum phenomena (first noted in Einstein's 1905 paper on the photoelectric effect). Their response was to construct new theories of inference which would encompass this new science. In contrast, Hempel's determinism can be seen as a nostalgic attempt to recapture the certainty and determinism of Aristotle.

A historical parable illustrates the dangers inherent in a central authority dictating the scientific investigation. In the thirteenth century, Thomas Aquinas fused the philosophies of Aristotle and Augustine relating to scientific inquiry. In Aristotle's teachings, one of the significant 'facts' about nature was that all things were combinations of the four elements: air, fire, earth and water; another 'fact' was that the Earth was stationary, and the Sun revolved around the Earth.

In this environment, investigation by experiment was considered impious, and could lead to dangerous transgressions. Galileo Galilei – known to his academic peers as 'the wrangler' – ran afoul of the Inquisition and was placed under house arrest for the remainder of his life. His transgression was to refute Aristotle in his *Dialogue on the Great World Systems, Ptolemaic and*

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[7] The word statistics derives from 'a person skilled in statecraft' which was coined for its summary descriptions of populations required to run complex nation-states.

Copernican [Galileo,1632], a book that was quickly added to the *Index of Banned Books* maintained by the Inquisition. Galileo's authorship earned him excommunication by the Church, for his impious conclusion that the Earth orbits the Sun. Not until 1992 did Pope John Paul II vindicate Galileo and officially concede that the Earth was not stationary – that it revolved around the Sun. The wheels of justice and truth turn slowly in a tyranny of taxonomies.

An interesting 'citations' angle to the Galileo's tragedy is that the *Index of Banned Book* established the concept of 'authorship' in the modern world. Before the *Index* – issued by Pope Paul III as a part of the reformation –establishment of ownership of intellectual property, or of authorship was not clear. Medieval texts often went unsigned, and if a name was attributed, it was often the scribe who had compiled the book, or a benefactor who had employed the scribe. The *Index* was the original citation list. Until the Inquisition began keeping the *Index*, knowledge was thought to be immutable, and authorship unimportant. The Inquisition elevated authorship to a position of importance, by identifying banned works and those responsible for them.

We are now full circle. Citations are ultimately the stuff of nomological nets, following the precepts of Hempel's notation where the *explananda* and *explanans* from 'deductive-nomological explanations' provide the 'IT artifacts' that comprise the nomological network. The parable of Galileo should make clear the dangers of such an approach presented as a *normative-regulative* theory of Information Systems research. Such systems are likely to be narrow, rigid, and incapable of incorporating experimentation or inference into its framework. This inherent conservatism will surely lead to pitfalls that parallel those in medieval Aristotelianism:

1. An intellectual despotism that declares significant 'facts' about Information Systems as being irrefutable; 'artifacts' as being immutable; with discourse frustrated by a weary, bickering tyranny of catalogers;

2. Editorial policies at major journals which adjudicate that any investigations into 'facts' cannot be considered a part of IS research, and thus cannot be publishable in IS journals;

3. Proscriptions on publishing experimental research, which even if they do not prohibit it completely, force the interpretations of that research to be shown to be consistent with 'facts' laid down in the nomological nets of IT artifacts; and

4. Academic discourse that favors the creation of artifacts – neologisms, explananda and explanans – over inferences about underlying behavior.

POSITIVE AND DESCRIPTIVE SCIENCE

Can the dangers and shortcomings in the system presented in Benbasat and Zmud be satisfactorily resolved? I think the answer is 'yes' if we are allowed to jettison the baggage of 'artifacts' from Hempel's deductive-nomological explanations. In their place we should opt for mainstream logical positivism, rather than the Hempel's flawed philosophical *cul-de-sac*. It is to this end that I want to dedicate the last part of this paper.

John Neville Keynes [1917] points out that positive science deals with 'what is' not with 'what ought to be.' The Orlikowski and Iacono paper makes a similar claim in spirit (p.130) regarding their own arguments, so they are likely not to object to positive research couched in this manner. The task of positive research is to provide a system of generalizations that can be used to make correct predictions about the consequences of any change in circumstances. Its performance is to be judged by the precision, scope, and conformity with experience of the predictions it yields. From this standpoint, a positive science is 'objective' in the same sense as any of the natural sciences such as physics and chemistry. However, there are complications; IS deals with the interrelationship of people and machines and the investigator is also a part of the subject matter being investigated. These complications raise specific difficulties in ensuring objectivity at the same time that they provides the researcher with a class of data not available to the natural scientist.

The interaction between experimenter and object of the experiment is sometimes used to disparage the social sciences for their inability to conduct so-called 'controlled experiments.' But similar problems exist in astronomy, where just as in the social sciences, it is impossible to replicate an experiment completely. Other issues arise in physics, where the process of observing the position of subatomic particles affects the precision of the measurement of momentum (the Heisenberg Uncertainty Principle). No experiment can be completely controlled, and every experience is at least partly controlled in the sense that some disturbing influences are relatively constant during its course. 'Control' is a matter of degree.

In contrast to the deductive nomology of Hempel, positive sciences don't recognize absolute and immutable 'truths,' 'facts' or 'artifacts.' Rather hypotheses are built from assumptions, axioms, or postulates that, for the sake of convenience, are considered true within a particular context. As an example of the character of these assumptions consider Isaac Newton's take on the falling apple. An apple, or any other body with mass, in a vacuum at sea level, falls a distance *d* that is a function of time *t* and the gravitational acceleration *g* where $d= gt^2/2$. If we drop a metal ball from a second story flat, we can expect this formula to describe the acceleration of the ball accurately. In contrast, if we drop a feather, expect a very poor prediction of the acceleration of the feather, and an indication that the model needs to be extended to include air pressure. Or assume that the experiment is conducted 10 miles above sea level where the gravitational acceleration is different. Newton, in pondering how his hypothesis broke down at the extremes of the ranges of his parameters, developed two new constructs: mass and the gravitational constant. These ideas were coined by Newton because entirely new concepts were required to explain observations in an expanding array of experiments. It is this sort of inference into underlying behavior that I think best justifies the creation of new concepts.

Newton himself had a difficult time with both the mass and the gravitational constant concepts, as was indicated by the numerous revisions that he made to his *Principia Mathematica.* Within the context of Newtonian physics, both were immutable quantities; but later investigations by Einstein showed them both to be mutable at high speeds or around large masses. Logical positivism was to a large extent a reaction to the insights provided by Einstein's theory of relativity and subsequent 20th century developments that revolutionized physics and logic. Positivism endeavored to create scientific systems that could embrace the new discoveries in physics at the turn of the 20th century in a way that the deterministic systems could not.

Descriptive science and normative science, on the other hand, cannot be independent of positive science. Descriptive science – the process of gathering observations – is a necessary part of data collection that precedes positive research. Normative-regulative science – the policy decisions concerning 'what should be' – can only intelligently be founded on 'what is' – the conclusions of positive research. Most arguments over policy center on the consequences – economic, welfare, and political –resulting from disparate predictions of the expected outcome, rather than to fundamental differences in basic values. Questions in industry about whether a system will truly lower expenses, provide competitive advantage, or allow better decision making will almost never be about objectives (lower costs, competitive advantage and good decisions are usually seen as desirable outcomes), rather they are about the predictions of how a particular system will operate in the future in the environment in which it is implemented. .

The ultimate objective of a positive science is the development of a theory that yields valid and meaningful predictions about phenomena not yet observed. Such a science does not deduct its theories from first 'truths' or immutable 'facts.' Rather it is a complex inferential 'language' designed to promote systematic and organized methods of reasoning. Positive science eschews arbitrary 'facts' for a series of hypotheses that form a body of theory which is effectively able to tell us about a reality that is not yet fully comprehended.

The validity of a hypothesis in this sense is not itself a sufficient criterion for choosing among alternative hypotheses. Observations are necessarily finite; potential hypotheses infinite. There may be support for multiple hypotheses at any time, and the dialectic surrounding the predictions of these various hypotheses provides a healthy environment for inquiry. Such academic environments are never orderly, nor are they easily managed under a single governing authority. But they promise greater innovation and progress than the restrictions of the 'filing system' of deductive science. Deductive or interpretive science is necessary, but should be seen in the light of its role in helping to construct, tes,t and select hypotheses.

Friedman puts an interesting twist on Occam's Razor in pursuit of this goal. He considers a theory simpler if it requires less initial knowledge to make a prediction; it is more fruitful the more precise the resulting predictions, the wider the area within which the theory yields predictions, and the more additional lines for further research it suggests. Logical completeness and consistency are relevant, but play a subsidiary role; their function is to assure that the hypothesis says what it is intended to say and does so alike for all users of the theory. The users themselves provide the checks and balances for a research discipline (whether they are in industry, academe, or anywhere else).

Positive research in Information Systems will be more than the sum of its tautologies if it is able to predict, and not just describe the consequences of actions. Empirical evidence is crucial to two different aspects of positive IS research:

1. in constructing hypotheses and

2. in testing their validity.

The description of a hypothesis can only be considered complete if its tautological implications are not contradicted in advance by experience that has already been observed. Additional observations may then be used to deduce facts capable of being observed but not previously known. These predictions can then direct additional observations, the gathering of new evidence , in a perpetual chain of questions and answers. Such free-ranging inquiry, unbeholden to a central authority, is the hallmark of modern, positive science.

CIVIC DUTIES: THE NATURE AND ROLE OF EXPERIMENTS

'Good citizenship' in research starts with sound experimental design motivated by testable hypotheses. It would be fruitless to belabor the extensive literature on experimental design here. here. But if the IS discipline is to push forward the bounds of knowledge, and is to be useful to anyone, then links between theory and observations need to be articulated clearly.

Good citizenship considers the other researchers who will build on your work. If the researcher is a theorist, then good citizenship means that this researcher also assures that the hypotheses generated by their research are testable – that potential experiments, metrics, and criteria for validation or falsification are clearly articulated. On the other hand, if the research is experimental, descriptive, or interpretive then there must be a clear statement of the hypothesis being addressed, the problem being solved, its importance, and its constituencies. These obligations are owed the academic community at large.

Vague allusions, weasel words, and poorly articulated concepts, no matter how entertaining or colorful, do a disservice to the research community. They confound understanding by forcing endless debate over definitions of terms, predictive implications, and claims to authorship. They slow discovery by making it difficult to design experiments or to test the hypotheses put forward. They penalize those who follow up one's research, by giving short shrift to measurement, validation and experiments.

These standards of 'good citizenship' exist for a reason – where they are not practiced, research makes little progress at great expense, the field lacks an 'identity' because it lacks testable hypotheses, and researchers become more interested in the 'prize' of another line item on their vita, rather than in the general advancement of knowledge.

Experiments are demanded as an integral part of positive research. Positive research can be seen as a cascade of decisions, each decision exacting constraints on the next decision in the cascade (Figure 1).

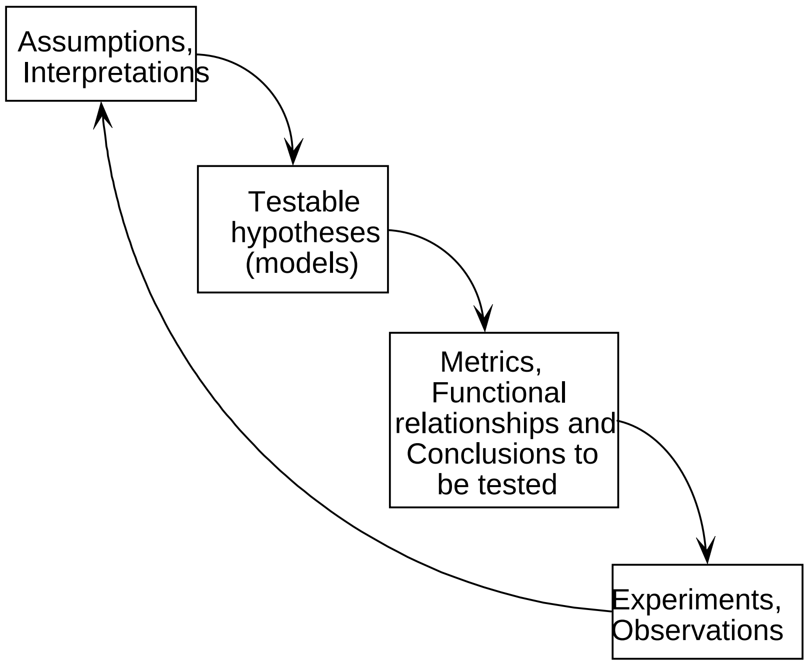

Figure 1. Research Decisions

Seldom does one research project or paper cover all four decisions. It is more likely that particular projects may address only one or two of them. But, because each decision is either motivated by, or must be validated or refuted by other decisions, every project is obligated to suggest experiments, hypotheses, or any other constructs needed to complete the cascade.

If your project involves defining a new theory, hypothesis, model, system or method (i.e., the 'testable hypothesis') then it seems reasonable to expect

1. assumptions that must be explicitly stated in terms that allow the hypotheses associated with the project to be tested, and
2. suggestions for specific tests, along with the particular metrics used to evaluate those tests; and
3. that the researcher should be able to specify some constituency interested in the hypothesis, and indicate how much validating / refuting it would be worth.

To meet these criteria, formal logical or mathematical exposition of the testable hypothesis is recommended; nebulous terms – e.g., strategy or consumer satisfaction, should be clarified by stating specifically how these quantities would be measured, and where the data would be collected.

If an experiment is being conducted, then it seems reasonable to expect that:

1. it should specifically validate or refute some stated testable hypothesis;
2. if the testable hypothesis was nof articulated prior to this research project, then it seem reasonable to expect a complete articulation of the model, its constituents, and its significance; and

3. the experiment should reach a conclusion on whether the model revision is warranted, and what revisions are needed.

The optimal strategy for a field then, at minimum, is an ongoing cycle between

1. analysis / rationalization / merging of empirical findings into a coherent corpus of knowledge and

2. empirical testing of logical implications arising from that corpus. Favoring one point of view to the point of *excluding* the other might prevent this cycle from operating.

Positive research in Information Systems would presume that all experiments are driven by the statement of testable hypotheses. Data collection, interpretation, and reporting of results are most convincing when statistical. The ultimate validation criterion is whether the testable hypothesis is able to predict future unobserved events and states; perhaps not perfectly, but with estimable error.

VII. CONCLUSION AND IMPLICATIONS FOR IS RESEARCH

Once they established their criteria for legitimacy of topics and research in IS, Benbasat and Zmud continued by attacking particular research topics as illegitimate. This attack, I think, foreshadows the general use that would be intended for their system, and thus deserves inspection. One area taken to task is research ‘associated with a customer’s product understanding’ which according to them ‘are in the domain of marketing, and are better left to scholars in marketing – scholars with more expertise in such matters.’ At least three problems result from such a parochial view. First, this view reinforces the traditional ‘stovepipe’ view of academe in an era when industry is obsessed with breaking down these same stovepipes. In fact, information technology itself is largely responsible for the integration of disparate disciplines. The current academic ‘stovepipes’ are outdated artifacts themselves – the finance, accounting, marketing, and strategy ‘stovepipes’ date only from the 1950s, when Harvard Business School revised its curriculum to reflect the ‘stovepipes’ within Proctor & Gamble, the most admired firm of the day.

Second, individuals within a particular ‘stovepipe’ may, in fact, not have all of the expertise to adequately address the topic in question. This lack of full knowledge is one reason that firms are now so keen to break down these antiquated stovepipes.

Finally, great breakthroughs in science often resulted directly from disciplinary cross-pollination For example, Mendel’s genetics resulted from the application of statistics to agriculture and relativity from the application of mathematics to physics. The innovator’s dilemma is how to break down existing stovepipes. Ideally our research discipline would recombine good ‘genes’ from other disciplines into IS research to help our own body of knowledge to adapt and grow efficiently, as opposed to the xenophobic cataloging of old genes to assure that they stay unsullied by foreign genetic material.

For the IS discipline to embrace Benbasat & Zmud’s narrow view of science would be unhealthy, and surely bound to stifle progress in the discipline, relegating it to a slough of irrelevancy. If adopted as standards for IS research, they would very likely bias the research agenda, leading to a decline in the relevance and quality of IS research, while discouraging a broad base of participation in the field.

It would be a tragedy of first proportion to watch Information Systems research decline as a discipline at the same time that information systems grow to become the single most dominant force in the economic success of firms, products, and nations. The choice confronting the Information Systems discipline is clear. The so-called ‘identity crisis’ within the discipline can be resolved in one of two ways:

1. By dictating a set of 'core' topics that are approved for IS research. Then the major question confronting the discipline is "Who has authority to dictate the 'core' topics?" Exactly the same conundrum confronted the Enlightenment academics attempting to tackle the inconsistent and sometimes silly conclusions drawn by medieval Aristotelians. Whether authority resides with senior academics in the field, or in the precedent set by prior citations in particular journals, the end result is likely to be a tyranny of obsolete or antiquated ideas; and a conservative research environment that discourages, or in the extreme proscribes, research that is innovative, iconoclastic, and relevant.

2. By institutionalizing methodologies of a positive theory which embraces inference and experimentation, and on which normative-regulative opinions may be based. Such methodologies do not constrain *a priori,* the topics which researchers may investigate. They do assure that conclusions that are drawn meet specific measures of quality, accuracy, and external validity. Positive research in Information Systems invites debate and experiment – both a requisite of any healthy academic discipline. Positive theory lacks the order and comfort of Aristotelian dogma; in return it allows disciplines to remain dynamic and open to new insights.

In my opinion, rapid and ongoing changes in Information Systems make a positive science of IS the best alternative to provide future research direction for the IS field. A positive science encourages the exploration, argument, and discovery centered on ideals and objectives that will keep the discipline relevant to industry. The choice for IS academics is clear – there are two roads down which IS research may pave its future.

One ends at the Museum of IT Artifacts, haunt of ghosts that once were demigods.

The other road opens vistas without end, in pursuit of new ideas and challenges. This road demands more effort – you can't afford to rest on your artifacts. But its rewards are commensurately greater. Travelers on this road share their delight in lively discourse in ideas, the excitement of new discoveries, and the experiments which assure that truth and knowledge are ultimate victors in any dispute.

I know which road I intend to travel.

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