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Thinking about Social Theory and Philosophy for Information Systems

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The phrase 'social theory and philosophy for information systems' invites an examination of following terms: social, theory, philosophy, information, systems, information systems, philosophy for information systems, and social theory for information systems. I shall refrain (no doubt to the relief of the reader) from providing the definition and scholarly treatment that each and every one of these terms deserves. Instead, I will pursue just a few issues where my intention is to suggest an imagination that is helpful to thinking about philosophy, social theory and information systems. Much as C. Wright Mills (1959) sought to instill in his readers a sense of what he called 'the sociological imagination', I will attempt to suggest to the reader a means of thinking about philosophy, social theory and information systems that, in a way, is more important than whatever the content of such thinking might be. The content of such thinking can and should change from philosophy to philosophy, from social theory to social theory and from information system to information system. However, once a person captures a particular imagination for raising and addressing questions about philosophy, social theory and information systems, the content of such thinking becomes nothing more than an ephemeral instantiation of a longer-lasting and more significant form of knowledge. It is this form of knowledge that motivates this chapter. I will draw on my own experiences in becoming a scholar to help evoke such an imagination and also to help explain what the terms 'philosophy', 'social theory' and 'information systems' have come to mean for me. I will also relate some actions that I have attempted in order to share these meanings with other information systems researchers.

PHILOSOPHY

In my roles as author, reviewer, editor and colleague, I have often observed the situation in which information systems scholars appear unwilling to acknowledge and accept philosophy as seriously as they do their own information systems research literature. The attitude has sometimes appeared to be one of outright resistance. Such an attitude is ironic in two ways. One would suppose that people who hold the degree of doctor of philosophy would be familiar with philosophy and receptive to its perspectives. Second, if the acceptance of philosophy can be compared to the acceptance of a technology of knowledge, one would then also suppose that scholars who bemoan resistance to technological innovations should themselves not be guilty of the same.

The outcome of taking a philosophical attitude is not so much an accretion to one's knowledge as it is a change in meaning, to oneself, of one's own knowledge and even knowledge in general. In my development as a scholar, it was unfortunately only after I completed all of my formal course work that I encountered three insights from philosophy that compelled me to learn more from it. The insights were Hume's problem of induction (see the appendix to this chapter), Gödel's proof, and the discrediting of logical positivism by the very school of thought that had advanced it in the first place. Though better classified under social theory than philosophy, a provocative first-person account of a social scientist as an expert witness (Wolfgang, 1974) also provided me with an insight no less philosophical than the first three. Altogether, the four insights awakened me from my slumber as a researcher of the empirical world. On awakening, I apprehended another empirical world—one consisting of my own research institutions, my colleagues and myself—which was just as demanding of research and explanation. I have come to believe that unless I am able to understand and explain the latter world, I cannot properly understand and explain the former world.

Hume demonstrated that induction, as a method of justification, is invalid.[1] My encounter with Hume's problem of induction moved me to suspend, if only momentarily, my scientific thinking about the empirical world and to inquire into scientific thinking itself. At the time of the encounter, I believed that science was inductive. My inquiry led me to see how the logic of justification in the empirical sciences—positivist as well as interpretive—must be deductive. This, in turn, enabled me to grasp what it means for a theory to be described by these synonymous terms: falsifiable, disconfirmable, disprovable and testable. I prefer the term 'empirically testable'. These terms all refer to why a theory can be disproved but never proved. A colleague and I recently wrote a paper (Lee and Baskerville, 2003) that, among other things, employs Hume's problem of induction in a critique of how information systems researchers have misapplied the valid notion of statistical generalizability in criticisms of case studies and how it may be properly applied instead. Interestingly, a reviewer of the paper doubted the validity of Hume's problem of induction. I found this astonishing, because doubting Hume's problem of induction is no more sensible than doubting the Central Limit Theorem. The reviewer accepted Hume's problem of induction only when my co-author and I laid out a step-by-step proof of it in the third and final version of our paper.[2]

I learned about Gödel's proof from a colleague who remarked that it can serve to explode logical positivism. Gödel's proof demonstrates that 'within any rigidly logical mathematical system there are propositions (or questions) that cannot be proved or disproved on the basis of the axioms within that system and that, therefore, it is uncertain that the basic axioms of arithmetic will not give rise to contradictions' where, furthermore, this proof 'ended nearly a century of attempts to establish axioms that would provide a rigorous basis for all mathematics.'[3] This had the effect of demolishing all pronouncements about logical positivism as the route to objective knowledge. Like Hume's problem of induction, Gödel's proof directed me to examine my own manner of scientific reasoning. Eventually I still concluded that scientific theory can be objective; however, I came to regard an objective theory not as one that exists independently of human beings and their contaminating influences, but as a social object that forms over a period of time from a process of social construction in which many generations or cohorts of researchers participate and whose properties and behaviours can be observed and explained through such empirical disciplines as the history and sociology of science.[4]

From Schön's classic, *The Reflective Practitioner*, I learned that positivism had 'fallen into disrepute in its original home, the philosophy of science' (1983, p. 48). How then, I wondered, could it be that social scientists, including many information systems researchers, still espouse and practice positivism as their approach to science? Schön quotes Bernstein (1976, p. 207):

> There is not a single major thesis advanced by either nineteenth-century Positivists or the Vienna Circle that has not been devastatingly criticized when measured by the Positivists' own standards for philosophical argument. The original formulations of the analytic-synthetic dichotomy and the verifiability criterion of meaning have been abandoned. It has been effectively shown that the Positivists' understanding of the natural sciences and the formal disciplines is grossly oversimplified. Whatever one's final judgment about the current disputes in the post-empiricist philosophy and history of science... there is rational agreement about the inadequacy of the original Positivist understanding of science, knowledge and meaning.

Given this development in philosophy—and in the technology of knowledge in particular—one may view the persistence of social scientists in adhering to traditional positivism to be an extreme case of resistance to technological innovations.

Effective, devastating accounts of positivism are nothing new or novel, despite the fact that the majority of information systems researchers have not learned about them. Popper (1965), who rejected logical positivism and explicitly described his own position as anti-positivist, brought Hume's problem of induction to bear on and demolish logical positivism's verifiability criterion of meaning. It was in reaction to this deficiency that Popper formulated his demarcation criterion for distinguishing science from non-science, where the criterion pertains to what Popper called falsifiability.

Wolfgang's first-hand account as a social scientist who gave expert witness testimony in court (1974) caused me to pause and reflect as much as did my encounters with Hume's problem of induction, Gödel's proof and the discrediting of logical positivism. Testifying in an American court regarding the death penalty, Wolfgang offered statistical evidence that black men receive the death penalty more often than white men, all other factors being equal (such as the severity of the crime committed). In the cross-examination, he was asked if the random sample of counties in his statistical analysis included the county where the crime was committed. When Wolfgang replied in the negative, the judge ruled his testimony irrelevant.

An insight that I derived from Wolfgang was that, even though this result was absurd from a scientific viewpoint, it was entirely rational from a legal viewpoint. This insight led me to the philosophy of law. Then, with the philosophy of law providing a vantage point that placed science and the philosophy of science in relief, I came to see science as just one form of knowledge and expertise that was not necessarily any better or worse than, but simply different from, other forms of knowledge and expertise. In this way of observing science, I could find no justification that scientific thinking, as just one form of knowing, must be regarded as the ideal after which all other forms of knowledge and expertise must necessarily model themselves. Regarding Wolfgang's experience as an expert witness, I now explain to my students that the logic of scientific reasoning is neither right nor wrong and the logic of the legal rules of evidence is neither right nor wrong; the two logics are simply different, just as the languages and cultures of two ethnic groups are neither right nor wrong, but simply different from each other (Lee, 1982).

Hume's problem of induction, Gödel's proof, the discrediting of logical positivism and Wolfgang's insight all eventually taught me that, in order to become a scholar, I needed to shift the focus of my study, if only occasionally, away from the objects typically examined by the natural and social sciences and instead towards scholarly knowledge itself as the object of inquiry. I have come to regard philosophy as being this kind of study.

Any discussion of philosophy that stands beside a discussion of social theory also needs to consider these terms: *ontology*, *epistemology*, *methodology* and *method*. These terms have usually confused me despite whatever dictionary or textbook definitions I have consulted. Part of the problem is that the '-ology' suffix of the first three indicates that they refer to fields of study, whereas common usage of the terms indicates that they refer to the subject matters of study. It is in the same way, it has been said, that the term 'history' can refer to both a subject matter (the past) as well as the scholarly field that studies it (the academic discipline of history). Learning from history, I have chosen to ignore the -ology suffix of the three terms and regard them instead as referring to subject matters.

A scholarly school of thought's *ontology* comprises its members' foundational beliefs about the empirical or 'real' world that they are researching. Some information systems researchers who subscribe to logical positivism proceed from the belief that the physical and natural world studied by the natural sciences constitutes the only true reality, with the important exception of quantifiably measurable constructs (such as IQ) harboured in human individuals. In contrast, some information systems researchers who subscribe to social constructionism believe that certain entities—such as the shared beliefs held by a long-established group of people, their social structure and their culture—also form part of the real world, even though these entities are invisible, intangible and, in a real sense, subjective. Furthermore, social constructionists believe that these human-made (30 years ago, the adjective would have been 'man-made') entities are social objects and, in being objects, are as real for human beings as any aspects of the physical and natural world. One's beliefs about what comprises the real world have an effect on what one seeks to observe, what one subsequently observes, how one explains what one observes, and the reasoning process by which one performs each of these. Researchers usually accept their school of thought's ontology as 'given', do not question it and need not even be aware of it. In fact, to the extent that researchers are not aware of their ontology or even this term's definition, we might better refer to it as their ontology-in-use.

Epistemology is sometimes defined as the theory or science of knowledge. I find this definition unsatisfactory because it begs the question of what a theory is. This question, in turn, is complicated by the fact that what a theory is depends on, among other things, an epistemology.[5] I now conceptualize an epistemology as a broad and high-level outline of the reasoning process by which a school of thought performs its empirical and logical work. For example, unlike hardcore positivist researchers, social constructionists believe that scientific investigations of socially constructed realities, such as the culture of a given organization, call for reasoning processes different from those used in scientific investigations of rocks, circuit boards and animals. Also worth noting is that the same ontology can lead to more than one epistemology. A positivist ontology, for example, can lead to the highly mathematical reasoning process seen in economics as well as to the qualitative reasoning process that framed Darwin's development of his theory of evolution. And for the same reason that we might prefer the term ontology-in-use to ontology, we might prefer the term epistemology-in-use to epistemology.

Less high level than epistemology is *methodology*. It refers to a more specific manner in which to do empirical and logical work. The same epistemology can have several methodologies. A social constructionist epistemology, for example, would recognize Van Maanen's ethnography (1979) and the grounded theory of Strauss and Corbin (1998) to be methodologies.[6] Furthermore, the device of differentiating first-order concepts and second-order concepts and the device of uncovering facts through informants' lies are *methods* that fall under Van Maanen's ethnographic methodology. Likewise, we may regard the procedures of open, selective and axial coding to be methods in Strauss and Corbin's grounded-theory methodology.

Some information systems researchers regard published writings about ontology, epistemology, methodology and methods as if they constituted a body of law to be looked up, learned, applied and obeyed, where any researchers who disobey are to be treated as deviants. I hold a different view. I regard explicit descriptions of ontology, epistemology, methodology and methods to be human-made entities that, as Kaplan would say (1964), are *post hoc* logical reconstructions of actual logics-in-use. It is always possible for a reconstruction to be wrong (and, to the extent that a map is never a territory, all reconstructions are necessarily imperfect). Hence the pronouncement of any ontology, epistemology, methodology and method need not be received as sacrosanct but can be judged, disputed, rejected and replaced. For example, even though most scholars would share the opinion that positivist and interpretive ontologies are contrary and conflicting, an instance of actual research that integrates the two should not be summarily dismissed for having broken 'the law', but can be usefully regarded as the instance that refutes and overthrows the shared opinion. After all, based on the philosophical imagination that can emerge from Hume's problem of induction, Gödel's proof, the discrediting of logical positivism and Wolfgang's account as an expert witness, one can and must regard all scholarly knowledge itself as a social construction. It is not immutable but under our power as a community of scholars to question, amend, correct and improve.

SOCIAL THEORY

'Theory' is difficult to define explicitly, but seasoned information systems researchers seem to recognize theory readily when they see it and are quick to voice criticism when they do not see it. (For example, referees of papers submitted to journals and audience members at research seminars are often quick to sniff: 'This paper has no theory!') My understanding of what constitutes a theory follows less from any explicit definition and more from examples and tacit knowledge that I carry from situation to situation.

The clearest and most basic example of theory arguably comes from the natural sciences and those sciences that emulate them.

In the natural sciences, a theory is typically operationalized as a collection of independent and dependent variables that are related to one another by the rules of mathematics or formal logic and that, furthermore, are related to an empirical referent by the rules of experimental design. Popper (1965) has commented that the propositions making up a scientific theory need to satisfy four conditions: they must exhibit internal logical consistency, they must be empirically testable, they must survive attempts at empirical testing, and they must be at least as explanatory or predictive as any rival theory. This operationalization can readily suit those social sciences that model themselves on the natural sciences, but is at best an incomplete operationalization for researchers who focus on the social dimension in social science.

Schutz (1962b) provides an ingenious device—involving the distinction between first-level constructs and second-level constructs—to account for the social dimension of social theory. According to Schutz, a social science theory and a natural science theory are no different in their logical form. Of course, there remain major differences between them, one of which pertains to some empirical work that a social scientist, but not a natural scientist, needs to perform prior to formulating a theory. In the given organizational or other social setting that a social scientist is observing, the social scientist's empirical work consists of interpreting the meanings that the observed human individuals create and share, and that they attach to one another, to their organizational setting and to their history. Being part and parcel of the real world that the social scientist encounters, these subjective meanings are objective reality. As such, they require data collection or observation by the social scientist no less than does any other aspect of objective reality that he or she encounters. Schutz conceptualizes these subjective meanings as first-level constructs—meanings constructed by the human subjects in the social setting that the social scientist seeks to explain. It is only on the basis of these first-level constructs that the observing social scientist may properly found the constructs (hence, second-level constructs) comprising his or her scientific theory.[7] Because subjective meanings or first-level constructs exist in the empirical subject matter of social science but not natural science, it is appropriate to describe the subsequent second-level constructs or theory as being *social* theory.

An interesting consequence that follows from Schutz's view of social theory is that natural science methodology can be seen as a limiting case or subset of social science methodology. In the language of mathematics, we can say that natural science methodology represents the limiting case of social science methodology where the first-level constructs (the meanings that the subject matter has of itself, its setting and its history) 'go to zero'. Equivalently stated, a social science theory (second-level constructs) not only must satisfy all the same logical and empirical requirements that a natural science theory satisfies (e.g. Popper's four conditions), but must also account for the world of subjective meaning (first-level constructs). In the sense that natural science deals only with the former but social science must deal with both the former and the latter, natural science methodology may be regarded as a subset of social science methodology.

A social theory, whether positivist or interpretive, need not be stated in terms of independent and dependent variables. It may be stated in the form of propositions not mentioning any variables, as long as the propositions are logically consistent, are empirically testable, survive attempts at empirical testing, and are at least as explanatory or predictive as the propositions comprising any rival theory. Process theory is a genre of theory fitting this description, whereas variance theory is the genre that makes use of variables (Mohr, 1982; Markus and Robey, 1988).

The term 'social' in social theory requires additional comment. For some researchers, any theory about human individuals is social theory. For other researchers, social theory is not so much about human individuals as it is about shared, socially constructed institutions that endure even when the individuals who are momentarily present are replaced by new ones. Consider the conceptualization of an organization as a collection of people. Such a conceptualization would mean that when all the people in the organization change, the result would be a new organization—but this need not be the outcome at all. This suggests an alternative conceptualization: the organization is that which stays the same even when all the people change. The things that stay the same, or at least change at a much slower pace than the turnover of people, would be social objects that include the organization's culture, its social structure, its standard operating procedures, many of its business processes, its folklore and its norms for behaviour. In this alternative conceptualization, the unit of analysis in social theory would not be individuals portrayed as decision makers, but would be the social objects that enable, constrain and otherwise shape the behaviours and thinking of all the different generations of individuals who enter, pass through and leave the organization. Just as the data populating a database can be seen as a fleeting instantiation of the enduring database schema, the individuals populating an organization can be seen as a fleeting instantiation of the organization's enduring culture and social structure. In this analogy, social theory would more properly be about extra-individual entities such as culture and social structure than directly about individuals themselves.

In any case, what social theory is about therefore depends on the ontology of the school of thought that is doing the theorizing. The ontology positing that individuals are agents of social structures, where the social structures shape what the individuals think and how they act, would result in a genre of social theory quite different from the ontology positing that individuals determine their own fates through the decisions they make and the actions they take. To recognize further the significance of the term 'social' that this discussion suggests, one may argue that the former ontology would be better suited to developing a social theory while the latter ontology would be better suited to developing a theory of the individual.

INFORMATION SYSTEMS

The terms 'information', 'systems' and 'information systems' have fallen into such careless use that they seemingly no longer denote anything different from one another. In the same way, 'information' has come to be used interchangeably with 'data' and 'knowledge', while 'systems' has almost always come to denote computer systems. And 'information systems' can mean the same as 'information technology', where both terms sometimes simply designate 'the computer'. Such usage trivializes and obscures the rich ideas that these terms originally signified.[8]

Systems theory is a well-developed body of knowledge and offers ideas that can advance current information systems research and practice. Some of its basic concepts are that systems are composed of subsystems, that the subsystems interact with and transform one another, and that the properties of the system as a whole result not only from the properties of its respective subsystems, but also from the interactions across them. Emery and Trist (1960) offer an explanation that still rings true today. Elsewhere (Lee, 2003) I have fashioned an application of their explanation to organizations and information technology, the gist of which follows.

A conventional (and incomplete) view of information systems focuses on information requirements—which describe the information that an organization requires from an information technology so that it can function and achieve its goals—as well as how to design, implement, install or otherwise procure information technology so that it can deliver the information required. This view dominates the assorted waterfall models of systems design and, one can argue, still permeates some of the recent and sophisticated notions of information systems development. This view is incomplete because it is blind to systems other than the technical system as well as to the mutually transformational interactions that unfold between technical systems and other systems.

In addition to the information technology comprising the technical system, there is also the organization comprising the social system. Just as there are information requirements that the social system poses to the technical system, there are organization requirements that the technical system poses to the social system. The hardware and software of an ERP system, for example, can pose the requirement that the organization must reengineer its manufacturing processes to fit the processes that the ERP software was programmed to manage.

Once the technical system is designed and implemented so as to provide the information required by the social system, the technical system itself would be changed, where the change would then trigger new and different organization requirements for the social system to satisfy. Then, once the social system is designed and implemented so as to deliver the organization required by the technical system, the social system itself would be changed, where the change would then trigger new and different information requirements for the technical system to satisfy. These mutually and iteratively transformational interactions can be expected to continue without end. Hence whatever results from them is not determinate but emergent.

An information system can be defined as this emergent result. An information system is not the information technology alone, but the system that emerges from the mutually transformational interactions between the information technology and the organization. In an early sociotechnical study in the information systems discipline, Bostrom and Heinen (1977, p. 18) express, at least in my paraphrasing of them, that an information system is that which results from the intervention of an information technology into an already existing social system, as much as an information system is that which results from of an intervention of a social system into an already existing information technology. In my reformulation of this, I emphasize that an information system is the result of an information technology enabling an organization, as much as an information system is the result of an organization enabling an information technology (Lee, 2003).

In sociotechnical systems in general, the social system does not have to be an organization. It can be an ethnic group, a virtual team, a neighbourhood and so on. Likewise, the technical system does not have to be a collection of hardware, software, networks and data structures, but can be technology in other forms, such as the division of labour across different work roles that would help in processing a firm's raw materials into the products or services that it sells. Both systems theory in general and sociotechnical systems in particular predate the computer era and have accumulated a large body of insights that can be mined for application to information systems.

This discussion of information systems presumes a scenario in which the information technology is indeed designed and implemented for the purpose of satisfying the organization's requirements and the organization is indeed designed and implemented for the purpose of satisfying the information technology's requirements. Not all situations, of course, fit this ideal. In the situation where an information technology (the technical system) is scrupulously designed, implemented, installed or otherwise procured, but no accompanying preparations are made in the organization (the social system), the information technology's requirements of the organization will nonetheless manifest themselves by evoking undesigned and therefore, most likely, undesirable changes in the organization. Such results can include human resistance to the information technology (Markus, 1983; Orlikowski, 1989) and even the failure of the information technology and hence the information system overall. The point is that changes in either the social system or the technical system will be accompanied by changes, whether designed or not, in the other system. The emergent result is more likely to achieve the intended goals if, first, the continually evolving requirements of both the social system and the technical system are regularly monitored and taken into account and, second, the required changes materialize by design before undesirable changes materialize by default.

The immediately preceding discussion presumes that an information system is simply an instance of a sociotechnical system in general. In other words, does the established body of sociotechnical systems theory necessarily apply in the case of an information system? The answer depends, in part, on what the term 'information' means. The *Oxford English Dictionary* offers these and other definitions:[9]

> The action of informing. . .; communication of the knowledge or 'news' of some fact or occurrence; the action of telling or fact of being told of something.
>
> Knowledge communicated concerning some particular fact, subject, or event; that of which one is apprised or told; intelligence, news. spec. contrasted with data.
>
> As a mathematically defined quantity. . .; now esp. one which represents the degree of choice exercised in the selection or formation of one particular symbol, sequence, message, etc., out of a number of possible ones, and which is defined logarithmically in terms of the statistical probabilities of occurrence of the symbol or the elements of the message. The latter sense (introduced by Shannon. . ., though foreshadowed earlier) is that used in information theory, where information is usually regarded as synonymous with entropy.

Because the second definition of information mentions 'data', its definition would also be helpful. An *OED* definition of datum is:[10]

> pl. The quantities, characters, or symbols on which operations are performed by computers and other automatic equipment, and which may be stored or transmitted in the form of electrical signals, records on magnetic tape or punched cards, etc.
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> **1970** A. Chandor *et al. Dict. Computers*. . . Data is sometimes contrasted with information, which is said to result from the processing of data.

These definitions indicate that information itself is a rich phenomenon that deserves its own separate focus no less than either information technology or organizations. These definitions also suggest that information cannot be neatly categorized under either the 'social system' heading or the 'technology system' heading. Perhaps a third type of system, the 'knowledge system', needs to take an equal place next to the social system and the technical system (where this would raise the non-trivial issue of how to define 'knowledge') [see Chapters 6 and 7 for some contrasting views—Eds]. In this suggested framework, an information system would be the emergent result of the mutually and iteratively transformational interactions among the social system, the technical system and the knowledge system. As for the design, behaviour and properties of a knowledge system and how it interacts with a social system and a technical system, one could take advantage of numerous existing bodies of knowledge, which include information theory, hermeneutics, phenomenology and the sociology of knowledge, and the history, sociology and philosophy of science, where science is regarded as being about only one form of knowledge. Information systems scholars whose investigations implicitly examine the interactions among the three systems include Liebenau and Backhouse (1990), Mingers (1995), Lee (1994a, 1994b) and Ngwenyama and Lee (1997).

One might suppose that people who call themselves information systems researchers are already well familiar with the systems approach and that it distinguishes their research from those of other scholars also interested in information technology. Unfortunately this is not the case. A large segment of information systems research consists of behavioural studies of how people and organizations do and do not use, adopt or diffuse information technology, where the studies do not account for the mutually and iteratively transformational interactions between the social system and the technological system. Indeed, in most of these studies the term 'system' or 'information system' appears to be interchangeable with 'information technology'. Arguably, many of these studies are not information systems research at all, but organizational research. Similarly, there are studies that focus on information technology, see the system only as the technology and do not account for interactive effects between the technological and the social. Information systems researchers include some who are systems theorists, such as Checkland and Holwell (1998), but the information systems research community overall has not come to realize the significance of this body of work.

ILLUSTRATIONS

No ideas on ontology, epistemology, methodology and methods are sacrosanct and immutable. They can and should always be further developed. I have applied my conceptions of 'philosophy', 'social theory' and 'information systems' in attempts to move the information systems field forward. I am pleased with some outcomes but not others.

Believing that logical positivism's flawed, inductive theory of knowledge could be replaced, I offered my own account of positivism in which its theory of knowledge is not inductive but deductive, involving hypothetico-deductive logic (Lee, 1989). This account was the result of my ruminations about philosophy and social theory and formed the core of my demonstration that even the study of a single case, such as the field study of an information system in a single organizational setting, can satisfy all the same criteria of rigour that are satisfied in natural science research. To my knowledge, this substitution of a deductive theory of knowledge for an inductive one went unnoticed by the community of information systems researchers, positivist and otherwise, even though this particular article has been highly cited.

Accepting Schutz's ideas about both the distinction and the relationship between first-level constructs (which as we have seen refer to the understandings that observed human subjects have of themselves, their setting and their history) and second-level constructs (which refer to the theory that researchers develop in order to explain what they are observing and that they craft to satisfy their criteria of rigour), I rendered Schutz's idea into an account (Lee, 1991) of how interpretive research and positivist research are not opposed and mutually exclusive, but compatible and mutually supportive. I designed this framework to move forward not only traditional positivism and traditional interpretivism, but also the state of social relations within the community of scholars, often suffering from a warring-camps mentality among positivist and interpretive researchers. Scholars already well entrenched in a positivist or interpretive research tradition have reacted, if not with polite silence, then with animated protestations that I am going against time-honoured definitions of the ontological, epistemological and methodological dimensions of positivism and interpretivism. Of course, given my belief that ideas on ontology, epistemology and methodology are always in need of further development, I only saw progress in liberating myself (and hopefully others) of the older ideas. Interestingly, some younger (i.e. not yet entrenched) scholars have seen no problem in my effort to integrate positivist and interpretive approaches to organizational research and have even wondered what my 1991 article says that is new or different!

DISCUSSION

Two puzzles facing the community of information systems researchers are the persistence of traditional, inductive positivism (positivism that is unaware of Hume's problem of induction and the accompanying difference between inductive and deductive theories of knowledge) and the lack of a systems approach among information systems researchers. To examine the puzzles, we can take an approach informed by philosophy, social theory and information systems. The resolution of these puzzles would exceed the scope of this chapter and is likely to require a multi-year, multi-site research programme.

We may frame the two puzzles as requiring us to develop scholarly knowledge about scholarly knowledge itself. As mentioned earlier, philosophy can be regarded as being this kind of study. For a long-term research programme, this suggests that lessons from earlier investigations in the philosophy of science would be relevant. And because it can be difficult to separate the philosophy of science from the history of science and the sociology of science, a research programme for resolving the two puzzles might similarly consider the perspectives of philosophy, history and sociology as reinforcing one another.

A social theory perspective, which can be historical and sociological, can provide the key to resolving the two puzzles. Rosabeth Moss Kanter (1977, p. 291) offers a method that is useful in interpretive social theory. When a researcher observes what appears to be someone engaging in irrational behaviour, then either (1) that person is actually behaving in a way that they themselves would consider irrational or (2) the researcher has not yet grasped the bigger picture in which the person's behaviour is rational. The method is to begin with the assumption that no person behaves in a way that they themselves would consider irrational and then to seek additional facts and to build or refine a theory that would allow the researcher to see how the behaviour is rational. Of course, it is still possible that the person *is* behaving in an irrational way; however, this would be accepted not as an opening observation, but only as a conclusion carefully drawn from a thorough empirically and theoretically based investigation.

This method can be helpful to a resolution of the two puzzles. First, there is what initially appears to be irrational behaviour in the majority of information systems researchers who abide by positivism. Abiding by traditional, inductive positivism would initially appear to be irrational because it has 'fallen into disrepute in its original home, the philosophy of science' (Schön, 1983, quoted above). Second, there is what initially appears to be irrational behaviour in the majority of information systems researchers who apparently do not take a systems approach. Therefore, either the majority of information systems researchers are behaving irrationally or there is a bigger picture in which the rationale of their behaviour would become evident.

For a possible example of such a bigger picture, consider the speculation in which positivist information systems researchers not only lack knowledge of the discrediting of logical positivism in the philosophy of science, but also face sanctions against acquiring and using such knowledge. There are some information systems doctoral students who have not read *The Structure of Scientific Revolutions* by Thomas Kuhn (1996) or *The Sciences of the Artificial* by Herbert Simon (1981). Some have read little or none of the philosophy that explains the foundations of different traditions of scientific research, including their own. Furthermore, some mention that their professors advise them against pursuing research that departs from any positivist/quantitative approach; doctoral students, who exist in dependency relationships with their professors, are in no position to disagree. Continuing the socialization are faculty recruitment committees, editorial boards of journals, programme committees of conferences, and tenure and promotion committees—where, in the United States and Canada, the pro-positivist members of these decision-making bodies largely outnumber those who are familiar with, much less accept the validity of, research approaches beyond positivism.

In light of this bigger picture, an information systems researcher's embrace of positivism clearly emerges as rational behaviour. Such a picture, however, is only illustrative of what the situation might be. To determine what the bigger picture actually is, a long-term research programme would be required and might include an ethnography of information systems researchers. Furthermore, whereas this discussion has only focused on how one might possibly explain the apparently irrational behaviour of information systems researchers who persist in abiding by logical positivism, one could similarly explain the apparently irrational behaviour of information systems researchers who do not take a systems approach.

A systems approach could also help in resolving the two puzzles. Taking a systems approach, a long-term research programme that investigates information systems researchers themselves could examine their technical system (the system of processes by which they transform research inputs, including existing theory and research methods, into research outputs, such as published articles), their social system (the system of roles, rules and other elements that help form the information systems research community), their knowledge system (the system of espoused theories about how to do research, theories-in-use about how to do research, shared beliefs and historical knowledge that, in interacting together, form what information systems researchers know), as well as how the three systems interact, transform one another and support the emergence of an information system among information systems researchers. The analysis of such a system could identify how the interactions among its three subsystems allow the emergence of a situation where key information does not materialize, is not accepted or is suppressed. In particular, this key information refers to information about the discrediting of positivism by the philosophy of science and information about the systems approach.

CONCLUSION

Taking an approach informed by philosophy, social theory and information systems to the study of information systems research and information systems researchers can lead to findings that would help the information systems research community do better information systems research. Such an approach would require a philosophical imagination, a social theory imagination and a systems imagination. Better information systems research will emerge if information systems researchers capture the three imaginations.

A GUIDE TO THE LITERATURE

Regarding philosophy and social theory for information systems, a good starting point to the literature is the work of Thomas S. Kuhn. His monograph *The Structure of Scientific Revolutions* (1996) is seminal, but also readily and widely misunderstood, as indicated in the anthology *Criticism and the Growth of Knowledge* (Lakatos and Musgrave, 1970). Kuhn's own anthology *The Essential Tension* (1997) is helpful for underscoring the strongly historical and sociological dimension to his philosophy of science. If one comes to understand or describe Kuhn's concept of 'paradigm' without any reference to the sociological or without any acknowledgement that Kuhn is presenting a social theory of science, then one has missed the significance of the role that the scientific *community* plays in Kuhn's depiction of what science is and what scientists do.

Regarding social theory specifically, a good point for motivating many of its methodological and philosophical issues is the debate between Alfred Schutz and Ernest Nagel in the anthology *Philosophy of the Social Sciences* (Natanson, 1963), along with Nagel's *The*

Structure of Science (1961) and the three volumes of Schutz's *Collected Papers* (1962a, 1964, 1966). Two books by Richard J. Bernstein, *The Restructuring of Social and Political Theory* (1976) and *Beyond Objectivism and Relativism: Science, Hermeneutics, and Praxis* (1983), helped to bring coherence to much of my own thinking on philosophy and social theory.

Regarding the matter of what constitutes an information system, I consider two articles—one by Bostrom and Heinen (1977), 'MIS problems and failures: A socio-technical perspective, Part 1: The causes' and the other by Emery and Trist (1960), 'Socio-technical systems'—to be my favourites for capturing and conveying the spirit of what an information system is. There is, of course, a large body of systems theory that originated long before the emergence of electronic information technology and that information systems researchers have appropriated and further developed; however, as intellectually impressive and forceful as this body of literature has been, its availability seems to have had no impact on the emergence and permanence of the idea that an information system is, basically, 'the computer' or that the terms 'information systems' and 'information technology' can usually be used interchangeably.

Similarly, regarding the matter of what information is, I consider a paper by Boland (1991), 'Information systems use as a hermeneutic process', to be excellent and unique in its demonstration, in very human terms, of what information is. However, most of the information systems literature has always seemed to treat the terms 'data', 'information' and even 'knowledge' interchangeably, despite the literature's espoused definitions otherwise.

APPENDIX: HUME'S CONTRIBUTION TO THE UNDERSTANDING OF INDUCTION[11]

Induction refers to a process of reasoning and can be a synonym for generalizing. It refers to a reasoning process that begins with statements of particulars and ends in a general statement. Reasoning from data points in a sample to an estimate of a population characteristic is an instance of induction. Campbell and Stanley call attention to 'some painful problems in the science of induction' (1963, p. 17, original emphasis retained):

> The problems are painful because of a recurrent reluctance to accept Hume's truism that *induction or generalization is never fully justified logically*. Whereas the problems of *internal* validity are solvable

> within the limits of the logic of probability of statistics, the problems of external validity are not logically solvable in any neat, conclusive way. Generalization always turns out to involve extrapolation into a realm not represented in one's sample. Such extrapolation is made by *assuming* one knows the relevant laws. Thus, if one has an internally valid Design 4,[12] one has demonstrated the effect only for those specific conditions which the experimental and control group have in common, i.e., only for pretested groups of a specific age, intelligence, socioeconomic status, geographic region, historical moment, orientation of the stars, orientation of the magnetic field, barometric pressure, gamma radiation, etc. *Logically*, we cannot generalize beyond these limits; i.e., we cannot generalize at all. But we do attempt generalization by guessing at laws and checking out some of these generalizations in other equally specific but different conditions. In the course of the history of a science we learn about the 'justification' of generalizing by the cumulation of our experience in generalizing, but this is not a logical generalization deducible from the details of the original experiment. Faced by this, we do, in generalizing, make guesses as to yet unproven laws, including some not yet explored. . .

Hume, an eighteenth-century Scottish philosopher, 'is almost universally credited with discovering the problem of induction' (Rosenberg, 1993, p. 75). Wood (2000) offers a detailed explanation of Hume's problem of induction. The problem of induction is about how to establish induction itself as a valid method for empirical inquiry.

Induction can be expressed in the form of Argument 1.1 in Figure 1.1. The status of induction as a valid method of empirical inquiry is open to question, because the second statement does not logically follow from the first. Wood refers to this as Problem 1. Wood continues: 'To make Argument [1.1] valid, we need an additional premise, such as [the] Uniformity of Nature assumption or: ''The future will be like the past'',' where the result is Argument 1.2.

Argument 1.2 employs a form of the uniformity of nature assumption as the first statement in an argument that takes the form of a syllogism, which consists of a major premise, minor premise and conclusion. The major premise is the first statement in the syllogism. The second statement, 'In past experience, all *F*s have been *G*s', plays the role of the minor premise. Applying the major premise to the minor premise leads deductively to the conclusion, 'Therefore, the next *F* will be a *G* or all future *F*s will be *G*s.' Note that the conclusion in Argument 1.2 is the same as the second statement in Argument 1.1. Therefore, if Argument 1.2 were

Argument 1.1

- In past experience, all *F*s have been *G*s.
- Therefore, the next *F* will be a *G* or all future *F*s will be *G*s.

Argument 1.2

- If in past experience, all *F*s have been *G*s, then the next *F* will be a *G* or all future *F*s will be *G*s.
- In past experience, all *F*s have been *G*s.
- Therefore, the next *F* will be a *G* or all future *F*s will be *G*s.

Figure 1.1 *First attempt to justify induction. Source: Based on Wood (2000)*

valid, it would provide a proper way of establishing the validity of induction.

Whereas Argument 1.2 performs its deductive reasoning correctly, the conclusion in any syllogism can be valid only if its major premise is valid. Wood refers to the following as Problem 2: In Argument 1.2, how would we know that the major premise—the uniformity of nature proposition—itself is valid? We would therefore need to take a step back in order to establish the validity of the uniformity of nature premise.

Wood explains that there are two ways by which we could attempt to establish the validity of the uniformity of nature proposition, which is denoted as Theory 1 in Figure 1.2. One way is by recourse to Argument 2.1, but its mode of reasoning is induction exactly as Argument 1.1's mode of reasoning was induction; therefore, the same Problem 1 that arose for Argument 1.1 would also arise for Argument 2.1. To remedy this instance of Problem 1, we would again need an additional premise, where the result is Argument 2.2.

Argument 2.1

- In past experience, all tests have confirmed Theory 1.
- Therefore, the next test will confirm Theory 1 or all future tests will confirm Theory 1.

Argument 2.2

- If in past experience all tests have confirmed Theory 1, then the next test will confirm Theory 1 or all future tests will confirm Theory 1.
- In past experience, all tests have confirmed Theory 1.
- Therefore, the next test will confirm Theory 1 or all future tests will confirm Theory 1.

Figure 1.2 *Second attempt to justify induction. Source: Based on Wood (2000)*

As it turns out, Argument 2.2 employs the uniformity of nature proposition as its major premise, just as Argument 1.2 did. Because Argument 2.2 takes the form of a syllogism, its conclusion can be valid only if its major premise is valid. The result is that Problem 2 would recur: How would we know that the major premise in Argument 2.2 is valid? We would need to take a step back in order to establish the validity of the major premise in Argument 2.2, just as we previously took a step back in order to establish the validity of the major premise in Argument 1.2. The result is that we would find ourselves in an infinite regress taking the form of what would then be Figures 1.3, 1.4, 1.5, and so on, where the stream of reasoning would have no conclusion. Rosenberg offers a succinct description of Hume's truism (1993, p. 75):

> Hume recognized that inductive conclusions could only be derived deductively from premises (such as the uniformity of nature) that themselves required inductive warrant, or from arguments that were inductive in the first place. The deductive arguments [e.g. Arguments 1.2 and 2.2] are no more convincing than their most

> controversial premises and so generate a regress, while the inductive ones [e.g. Arguments 1.1 and 2.1] beg the question. Accordingly, claims that transcend available data, in particular predictions and general laws, remain unwarranted.

The enormous significance of Hume's truism leads Campbell and Stanley (1963) to take the positions that '*induction or generalization is never fully justified logically*' and that 'we cannot generalize at all' (emphasis in the original, cited above).

ENDNOTES

[1]David Hume's own work and an extensive secondary literature on his work are readily available, including coverage in encyclopaedias and textbooks. Rosenberg (1993) provides a good example.

[2]The proof is reproduced in the appendix to this chapter.

[3]'Gödel, Kurt', *Encyclopædia Britannica* retrieved April 14, 2003, from Encyclopædia Britannica Online, http://80-search.eb.com.proxy.library.vcu.edu/eb/article?eu=37902.

[4]In particular, I am referring to Berger and Luckmann's classic, *The Social Construction of Reality* (1967), and the corpus of Thomas Kuhn's work. The sociological influence in Kuhn's history of science is evident in the following (1970, pp. 237–8): 'Some of the principles deployed in my explanation of science are irreducibly sociological, at least at this time. In particular, confronted with the problem of theory-choice, the structure of my response runs roughly as follows: take a group of the ablest available people with the most appropriate motivation; train them in some science and in the specialties relevant to the choice at hand; imbue them with the value system, the ideology, current in their discipline (and to a great extent in other scientific fields as well); and finally, let them make the choice. If that technique does not account for scientific development as we know it, then no other will.'

[5]Hermeneutically speaking, one can argue that the terms ontology, epistemology, methodology and method can only be understood in one another's context and that, therefore, their unrelated dictionary definitions are necessarily incomplete.

[6]Mingers (2001) offers helpful distinctions within methodology itself.

[7]Giddens' idea of the 'double hermeneutic' (1984) embodies the distinction and relationship between first-level constructs and second-level constructs. The double hermeneutic is the idea that a social theory contains interpretations of everyday life, which itself can then form its own interpretations of the social theory and thereby be changed by it, which in turn would call for the social theory to render interpretations of the changed everyday life. In other words, second-level constructs contain interpretations of first-level constructs, which themselves can then form their own interpretations of the second-level constructs and thereby be changed by them, which in turn would call for the second-level constructs to be based on interpretations of the changed first-level constructs. The idea of first-level constructs and second-level constructs can also

be found in the work of Van Maanen (1979), where they appear as 'first-order concepts' and 'second-order concepts'.

[8]Mingers (1995) also laments the lack of clear definitions within the information systems field and suggests a model of the relations between information, meaning and data.

[9]Accessed on April 20, 2003 at http://80-etext.lib.virginia.edu.proxy.library.vcu.edu/etcbin/oedbin/oed2www?specfile=/web/data/oed/oed.o2w&act=text&offset=203839170&textreg=0&query=information.

[10]Accessed on April 20, 2003 at http://80-etext.lib.virginia.edu.proxy.library.vcu.edu/etcbin/oedbin/oed.link?query=datum.

[11]Reprinted by permission from Allen S. Lee and Richard L. Baskerville, 'Generalizing generalizability in information systems research', *Information Systems Research*, 14(3): 223–4. Copyright 2003, the Institute for Operations Research and the Management Sciences, 901 Elkridge Landing Road, Suite 400, Linthicum, MD 21090 USA.

[12]Campbell and Stanley describe 'Design 4' as follows (1963, p. 13)

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R O1 X O2
R O3   O4
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