The current issue and full text archive of this journal is available at
www.emeraldinsight.com/0955-534X.htm

Producing and socializing relevant management knowledge: re-turn to pragmatism

Jacqueline Fendt
ESCP-EAP European School of Management, Paris, France
Renata Kaminska-Labbé
CERAM Business School, Nice-Sophia Antipolis, France, and
Wladimir M. Sachs
ESC Rennes School of Business, Rennes, France

Received November 2006
Revised March 2007
Accepted June 2007

Abstract

Purpose – Management practice is progressing at unprecedented pace and often academia is lagging behind, if not totally irrelevant, both in management research and in education. This paper strives to show how principles of pragmatism and action research are likely to increase the relevance of management research and education.

Design/methodology/approach – A reflection based on a broad review of ontological and epistemological issues leads to a call for philosophical re-foundation of management academia.

Findings – Pragmatism defines truth seeking as reducing doubt, and therefore necessarily includes the notion of a client for the research effort. Action research is a practical embodiment of this approach and deserves a more prominent role.

Research limitations/implications – The research limitations and implications are inherent in the chosen methodology/approach: a viewpoint that hopefully stimulates others.

Practical implications – This paper makes concrete suggestions as to how to bring research and education closer to the client to permit cross-fertilization and improve problem-solving processes.

Originality/value – The paper offers a meta-synthesis of ontological and epistemological approaches to the theory-praxis gap. It outlines the imminent pertinence of pragmatism as a philosophy and as a practice of management science and relates pragmatism to theory of action, purporting pragmatist paradigms of management knowledge socialization.

Keywords Management technique, General management, Action research, Innovation, Pragmatism

Paper type Viewpoint

Introduction

Innovation of management practices is happening everywhere and at a breathtaking pace. Everywhere – except in academia? A century after John Dewey deplored fundamental tensions in the "proper relation" between theory and practice (Dewey *et al.*, 1904), much management knowledge production and socialization (research and education) still takes place in splendid isolation from praxis. This although both academics and practitioners are evidently aware that the output of theory often fails to have impact on what practitioners do: there is a lively debate ongoing on this shortfall that we sample in Table I,

Wladimir M. Sachs sadly passed away in October 2007.

European Business Review
Vol. 20 No. 6, 2008
pp. 471-491
© Emerald Group Publishing Limited
0955-534X
DOI 10.1108/09555340810913502

المنارة للاستشارات
www.mana

Authors	Observed phenomena, dilemmas, critique
Ackoff (1978), Beyer and Trice (1982), Daniel (1998), Fendt (2005), Gordon and Howell (1959), Hambrick (1994), Hambrick(2005), Huff (2000), Porter and McKibben (1988), Priem and Rosenstein (2000)	Business world generally ignores, rarely applies, research
Beyer (1982), Beyer and Trice (1982), Keleman and Bansal (2002), Miner (1984), Whitley (1988)	Difficulty to render useful, to "utilize", social sciences research
Cheng and McKinley (1983), Koontz (1961, 1980), Pfeffer (1993)	Multitude of research approaches is a sign of immaturity of the field of management research
Schön (1982)	Science = "high ground of theory", practitioners operate in the "swamp of practice"
Beyer and Trice (1982)	Distinction between adoption of a research idea by decision-makers and its actual implementation
Kilduff and Kelemen (2001), Pelz (1978)	Distinction between conceptual use of research by practitioners (enlightenment on the subject) and instrumental use (act on results directly)
Shrivastava and Mitroff (1984)	Practitioners and academics have different frames of references regarding knowledge
Burrell (1989), Huczynski (1996), Miller *et al.* (1997), Whitley (1988)	Managers widely read craftsman-type, first-hand experience literature that does not meet scientific standards (alternately dubbed "Heathrow-literature", airport books, literature on principles, normative literature, guru literature, etc.)
Abrahamson (1991), Abrahamson and Fairchild (1999), Huczynski (1996), Jackson (2001), Micklethwait and Woolridge (1996)	Management gurus, fashions and fads have more impact on management action than research
Archer (1995), Argyris and Schön (1991)	There is a dilemma between applying scientific rigor and being relevant
Astley and Zammuto (1992)	Practitioners and academics use different language games
Rynes *et al.* (2001), Starkey and Madan (2001), Weick (2001)	Relevance gap, great divide (between academia and praxis)
Boland *et al.* (2001)	Knowledge representations vs knowledge transfer
Clegg and Ross-Smith (2003)	Management knowledge from B-schools is rarely surprising and remarkably formulaic
Donaldson (2005), Gapper (2005), Ghoshal (2005), Hambrick (2005), Mintzberg (2005), Nord (2005), Pfeffer (2005b)	Bad management theories are destroying good management practices (especially regarding corporate governance, ethics, taking responsibility)
Burgoyne and Turnbull James (2006), Gibbons *et al.* (1994), MacLean and MacIntosh (2002)	Mode 1 (academic predominance) vs mode 2 (priority of practical concerns) knowledge production
Byrne (1986), Pascale (1990a,b)	Body of management knowledge is occupied by unopposed management fads
Donaldson (2002), Hambrick (1994), Leavitt (1989), Mintzberg (2004), Mintzberg and Gosling (2002), Pfeffer and Fong (2002)	Lack of coherence of management education and lack of effectiveness for performance

Table I.
Sample literature on the theory-praxis gap

(*continued*)

www.mana

Authors	Observed phenomena, dilemmas, critique
Czarniawska (2003)	Body of knowledge that offers insight and reflection on organizational practice is ignored ("forbidden"); modernist ideas of control and masculine ideas of mastery are preferred over reflection
Suddaby (2006)	(regarding application of Grounded Theory Method) "[researchers] tend toward purist idealism; repeat, reinforce many myths, rigid rules . . . ", "fundamental drift toward positivism"
Gosling and Mintzberg (2006)	Management taught as if immaterial, abstract formulae, case histories, flow diagrams. Only useful if tested in practice

Table I.

and all kinds of solutions are being proposed to deal with what came to be called the "relevance gap", some of which we recall in Table II.

The synthesis in Table II suggests that many academics propose to fill the gap by adopting practitioners' agendas, i.e. by radically challenging dominant management research paradigms. This is confirmed in Table III, which samples such proposals. The purpose of this paper is to advance this discussion by revisiting pragmatism, a philosophy of science that addresses the relationship between theorizing and practice, but which is insufficiently present in academic curricula and research, particularly in Europe, less so in the USA (Child, 1995; Koza and Thoenig, 1995). Given the dominant research paradigms and the sociology of academia, many scholars engaging in practice face a dilemma, which is that they ". . . live in two worlds. The first demands and rewards speculations about how to improve performance. The second demands and rewards adherence to rigorous standards of scholarship" (March and Sutton, 1997, p. 698). We argue that pragmatism allows dissolving this dilemma by focusing on asking the "right" questions and providing empirical answers to those questions.

We start by discussing the ontological and epistemological reasons for the theory-praxis gap, to then argue the imminent practicality and pertinence of pragmatism as a philosophy and as a practice of management science. We then relate pragmatism to theory of action and emphasize the necessity for a close collaboration and cross-fertilization between researchers and their clients. We conclude with a call for action research, and action learning, both pragmatist paradigms of management knowledge acquisition and socialization that encourage active involvement of academics in solving pertinent management problems and thus producing and socializing relevant management knowledge. Relevant, that is, to practitioners in their problem-solving process, and to the advancement of management science in both research and education.

Ontology, epistemology and the theory-praxis gap

Judging by published papers most management research is aimed at advancing theory, seen as producing outcomes such as "if the environment becomes more complex, then companies with decentralized decision making perform better." It is then assumed that this statement translates into a guideline for managers in the form ". . . in complex environments, to improve performance, decentralize decision making." We happen to

www.mana

Authors	Proposed solutions
Schön (1982)	Use one's "repertoire of design knowledge" (experience, and body of knowledge acquired during training) valid for classes of situations and apply to the unique situation at hand
Blau (1994)	"5 pathways of transformation", e.g. expand developmental periphery to include outside organizations
Hambrick (1994)	Present and communicate management research better; "open up the incestuous, closed loop of the academy's conferences" (p. 13)
March and Sutton (1997)	Create research that answers to two different "reputation systems" (academia and praxis)
Pettigrew (1997)	Double hurdle, double handles: management research must meet scholarly quality standards and practical relevance
Huff (2000)	Restructuring of the knowledge production process
Duderstadt (2000)	Difference university-business school; "ongoing relationship with … practice; closely coupled to needs of society. (…) tightly linked to practice; respond more rapidly to changes in society
Lorange (2002)	Rejects Cohen and March's (1973) "anarchy" metaphor for business research and teaching; proposes (citing a private correspondence with the LBS Dean G. S. Bain): "… a balance between academic pursuits and providint relevance for practitioners"
McKelvey (2002, 2003a,b)	Distinguish science of objects, of subjects; latter more
Clegg and Ross-Smith (2003)	appropriate frame for discipline of management. Introduce "phronesis" (pragmatic, variable, context-bound)
van Aken (2004), Van Strien (1997)	As do professionals, in design sciences: use the problem solving cycle, or "regulative cycle". Define the problem out of its "messy" context, as in Schön's (1982) "naming and framing". Then plan an action, apply it and evaluate it
van Aken (2004, 2005)	Distinguish "design sciences" and "explanatory sciences"; distinguish descriptive research = organization theory, and prescriptive research = management theory Use prescription-driven research: it is solution-focused, acts from a player's perspective, has a intervention-outcome logic, has a research question seeking an alternative solution to a class of problems, has tested and grounded technological rules as research output, is of heuristic nature and is justified by saturated evidence
Suddaby (2006)	Avoid misreadings (and probable subsequent miswriting and misteaching) of seminal texts; acknowledge context-relation and human limitations
Samuelson (2006)	A new definition of "rigor": analytical and conceptual skills, connections between social and environmental, challenge key assumptions; voice values
This paper	Management professors should adopt a stance rooted in pragmatism, i.e. confront themselves and their certitudes with reality, reality being the principal source of doubt which is in turn the driving force for inquiry
This paper	Accept the complexity and speed of cutting-edge management in a globalizing world and invite these multiple realities into the classroom

(*continued*)

Table II.
Examples of proposals to bridge the theory-praxis gap

المنارة للاستشارات

www.mana

Authors	Proposed solutions
This paper	… and vice versa bring the classroom to them, rather than covering anachronistic canons of seemingly essential knowledge skills for managers
This paper	Rather than avoid subjectivity in research and teaching, management faculty should seek and acknowledge researcher interference (action research, action learning) as a source of organizational change and innovation as well as of management development

Table II.

believe that this particular statement is essentially correct, and useful, and there is ample academic literature in its support. But that is not the point.

The point is that in aspiring to reduce to such statements all knowledge required for management, management research is following in the footsteps of physics and other natural sciences. A theory in such sciences is a collection of statements in the form "if a then b." The assumption is that a manager versed in relevant theory would know that doing x under conditions α will lead with reasonable certainty to conditions β, or as van Aken (2004) would put it, that descriptive knowledge readily translates into prescriptive rules, very much like knowledge of physics translates into design guidelines for an engineer. As described by Simon (1969) natural scientists construct theories in hierarchically organized abstractions whose complexity, specificity and applicability increases as one move to lower levels. For each layer we do have to know "only that part of the system that is crucial to the abstraction" with which we are dealing (p. 16). "It is fortunate that this is so, for if it were not, the top-down strategy that built the natural sciences over the last three centuries would have been infeasible. [. . .] This skyhook-skyscraper construction of science from the roof to the yet unconstructed foundations was possible because the behavior of the system at each level depended on only a very approximate, simplified, abstracted characterization of the system at the level next beneath. This is lucky . . . " (pp. 16-7).

Simon attributes to luck (sic!) that the ontology of natural sciences – the nature of reality under study – is such that the highly structured, formalistic epistemology – the manner of acquiring truth about reality – is fruitful. We contend that social sciences in general and management science in particular are not this lucky. Most management practitioners know that this is so. Why? First, because organizations are complex social systems (Thompson, 1967) composed of a multitude of unique, idiosyncratic agents, endowed with intentionality, who interact in a non-linear way. Second, organizations are open systems and therefore managerial problems are open-ended and constantly expanding (Churchman, 1994). Third, management is inherently interdisciplinary and requires difficult ethical judgments (idem). All this complexity leads to unpredictability and complicates theory's job of predicting the outcomes (King and Cleland, 1978; Lorange and Vancil, 1977; Mintzberg *et al.*, 1976; Steiner, 1979; Wooldridge and Floyd, 1990).

Physics, when studying mutual attraction of objects, assumes that all their qualities other than mass are irrelevant; shape, aesthetic appeal, usefulness or color do not influence the dynamics of motion. Similarly, biologists studying the eating patterns of mice do not individuate among subjects in the same experimental group and classical

المنارة للاستشارات

www.mana

Table III.
Research paradigms and the theory-praxis gap

Authors	Research paradigms
Dewey (1929)	Knowledge and action should not be separated
Gordon and Howell (1959), Pierson (1959), Schlossmann *et al.* (1987)	The purpose of management research is prescription of rational decision-making by rational, long-term planning top managers
Peirce (1960)	The purpose of management science is to create shared understanding
Berger and Luckmann (1966), Searle (1995)	Management research must be constructionist as organizations are ontologically social constructions
Simon (1969)	Distinction between explanatory and design sciences
Starbuck (1976), Starbuck and Nystrom (1981)	In order to understand a system one should try to change it
Hayek (1989)	The pretense of knowledge: theorizing based on partialization of analysis, exclusion of human intentionality (and thereby morality), use of deduction
Emory (1985), Nagel (1979), Seth and Zinkhan (1991)	The purpose of management science is to understand, describe, explain, and perhaps predict
Cheng and McKinley (1983), Thomas and Tymon (1982)	Relevant management theory must fulfil five key user needs: descriptive relevance (external validity), goal relevance, operational validity, non-obviousness, timeliness
Montgomery *et al.* (1989)	The purpose of management research might ultimately be, but need not be, application
Cross (1993), Evbuomwan *et al.* (1996), Hubka and Eder (1996)	The purpose of management design theory is to contribute to the knowledge of the designer
Archer (1995), Sayer (1992)	Reality paradigm: there exists a material world independent from observers, objects have particular causal powers; management science must link intervention with outcome
Child (199), Koza and Thoenig (1995)	Europeans are more concerned with academic reputation, US scholars more with performance
Elsbach *et al.* (1999), Schulz and Hatch (1996)	Paradigm wars
Lorange (2002, p. 17)	B-schools to be driven by market . . . directly address its needs; keep up with the thinking of its leading clients; acknowledge "economies of scale" as value creator (. . .)
van Aken (2004, 2005)	The purpose of management science is more than understanding: it includes prescription, i.e. the developing and testing of alternative solutions: "field-tested and grounded technological rules"
	Management research is "design science"
	Management research paradigm is the combination of research question, methodologies . . . and the nature of the pursued research products (p. 224)
Ghoshal (2005)	Away from "pretense of knowledge" toward acceptance of human intentionality and choice, and thereby of responsibility
Pfeffer (2005a)	Management and organizational science must be concerned with more than science and theory

www.mana

economists choose not to open their "black boxes" and consider that differences in performance of corporations stem exclusively from industry and market structures. In this perspective strategic management is limited to industry selection according to its "attractiveness", rational choice of strategy based on the analysis of competitors' strategies and acquiring the missing resources necessary for competing in a given market (Porter, 1980). However, since Penrose's (1959) *The Theory of the Growth of the Firm* and emergence of the resource-based view of firms, conceptualized as systems of tangible and intangible resources (Amit and Schoemaker, 1993; Dierickx and Cool, 1989; Montgomery *et al.*, 1989; Wernerfelt, 1984, etc.), performance is regarded as the result of the firm's inimitable idiosyncratic resource system and its capability to combine resources in order to build and leverage competencies (Hamel and Prahalad, 1994; Sanchez *et al.*, 1996).

Recognition of the idiosyncratic nature of organizations poses a problem of predictability and generalization (McKelvey, 1997). How can then theory built on observation of other companies, even a great many of them, be applicable to a specific individual company? It is possible only when what the theory addresses is truly generic and isolated from influence by factors that may be unique to every corporation. There are not many such theories.

Indeed, the vast majority of theoretical articles published by management journals offer very tentative conclusions that were tested in limited empirical contexts. They frequently contain disclaimers of validity of proposed theoretical statements, that morph from the assertive "if a then b" into a much fuzzier "if something like a then it could be that b," and call for further research to affirm or disprove theoretical notions advanced by the authors. These are usually calls in the wild, because other researchers show little inclination or may lack the opportunity to pursue this particular agenda, and anyway few articles offer theses that are sufficiently operational to be applied elsewhere. Moreover, most top-rated journals call for articles that contain theoretical novelty. They ask authors to be erudite, and most are, and display a good grasp of prior research, but they concoct a "new theory," which frequently is *ad hoc* and incorporates elements from several possibly but not necessarily compatible sources. The result is that management theory resembles a synapse neural network or a mycelium of branching, threadlike hyphae and not at all a hierarchical structured tree of knowledge.

Yet dominant management theory is implicitly based on ontological assumptions that are similar to those underlying natural sciences, as illustrated by scholars (McKelvey, 2002, 2003a,b) who calls for reinvention of organization science through methods that better justify beliefs, resting on more plausible truth findings without ignoring the complexity of intricate, multi-causal reality of managers. His proposal is multi-disciplinary, based on a merger of complexity, post-modernists ontology and agent-based modelling, and rests on the belief that enriching the theoretical side of the theory-praxis divide will make the gap go away. While specifics of McKelvey's proposals are novel, there is a distinguished tradition of this kind of optimism. Simon himself thought in 1960 that duplicating "the problem-solving and information-handling capabilities of the brain is not far off; it would be surprising if it were not accomplished within the next decade" (Simon, 1960, cited in Weizenbaum, 1976, p. 245) Forrester, the inventor of systems dynamics displayed similar optimism in a 1970 Congressional testimony, in which he stated that "the human mind is not adapted to interpreting how social systems behave" and that until the advent of his and

المنارة للاستشارات

www.mana

similar methodologies "there has been no way to estimate the behavior of social systems except by contemplation, discussion, argument and guesswork" (Forrester, 1970, cited in Weizenbaum, 1976, p. 246), dismissing thus as inferior "the ways in which Plato, Spinoza, Hume, Mill, Gandhi, and so many others have thought about social systems" (Weizenbaum, 1976, p. 246). Skinner (1974) decries "the disastrous results of common sense in the management of human behavior" and proposes as remedy scientific analysis. While we do not object to developing "more scientific" theories, we are sceptical that this would necessarily lead to the narrowing of the gap. For one, most managers we know would not abdicate their decision-making responsibilities to scientific models. There are human functions for which computers and scientific theories "*ought* not be substituted. It has nothing to do with what computers can or cannot be made to do. Respect, understanding, and love are not technical problems" (Weizenbaum, 1976, p. 270).

Most management scientists do not explicitly address esoteric ontological assumptions and epistemological strategies discussed so far. Rather, such assumptions are socialized into many of us, in a business academic environment that increasingly moved away from practice (Mintzberg, 2004; Schlossmann *et al.*, 1987; Whitley, 1988) and thus implicitly follow the lead of the likes of Simon, Forrester, Skinner and McKelvey. Thus, we are an army that patiently strive to construct the building blocks of the grand theory that, one day, will improve management practice. Without engaging further here into the lively nomothesis vs idiography debate we wish to state that so far this is wrong, or at least does not often work. But do we know what the right ontological assumptions are? Yes, but only in the vaguest of terms; social reality has hitherto proven elusive to that kind of characterization, and fortunately so, as some would argue, on humanistic grounds (Weizenbaum, 1976), because we are part of that reality, and acquiring excessive self-knowledge and self-control would take the spice out of life, transform beyond recognition the essence of the human condition. Fortunately, the epistemology suggested by pragmatism needs far fewer ontological assumptions than the dominant paradigms. Indeed, it focuses on interfacing science and practice, and defines the notion of truth in a manner that renders it imminently practical, regardless of the nature of reality at hand.

Pragmatism and management research

Pragmatism (Dewey, 1929; James, 1907; Peirce, 1992; Peirce, 1998) is a philosophy of science that emphasizes the link between action and truth, arguing that the ultimate test of a belief is the willingness to act on it. One might be tempted to characterize it with the catchphrase "put your money where your mouth is", if it were not that one of the great masters himself provides us with more colorful "truth's cash value" (James, 1907, p. 200) or "the true is only the expedient" (James, 1907, p. 222). Pragmatism aims at creating useful knowledge by addressing pressing, contemporary problems and translating acquired knowledge into action. To pragmatists, scientific knowledge is useful when it helps people to better cope with the world or to create better organizations. The notion of usefulness applies across two dimensions: epistemological (is this information credible, well-founded, reliable?) and normative (does this help advance our projects?) (Wicks and Freeman, 1998). Pragmatism seeks to link action and truth, and not trade one off for the other, as many scholars seem to believe. Even van Aken (2004), a fervent proponent of relevance in research and teaching, paraphrases March and Sutton (1997) in one of his

المنارة للاستشارات

www.mana

pleas that management research be more grounded in action: "A quest for field-tested and grounded technology rules, which in the field of management will be predominantly qualitative and heuristic by nature, means trading the priestly beauty of truth for the soldiery glory of performance" and continues to regret that this "may be too high a price" (van Aken, 2004, p. 242). We in turn regret this either-or or dilemmatic view of the tensions between rigor and relevance and between theory and practice. It stems from a popular yet imprecise and populist reading of Pragmatism, as a means of cutting corners, "tolerating anything that flies."

James and Dewey considered pragmatism as a way of transcending the irresolvable philosophical and metaphysical dilemmas. Rorty (1985, p. 5) notes that both fathers of American pragmatism considered that in general, philosophy should be used as a "forum in which people can talk how to fulfil their needs, which beliefs work to get them what they want, without running into Platonic or Cartesian impasses" (cited by Wicks and Freeman, 1998). Pragmatists consider confrontation with reality through action as the principal source of doubt, which in turn feeds scientific curiosity and becomes the driving force to inquire in order to settle that doubt. Thus, action and the interrogations stemming from it are what drive the agenda of science (Peirce, 1992, 1998). Pragmatism accepts all well constructed paradigms of scientific inquiry as valid when they are appropriate, that is where the nature of studied reality is such that the paradigm leads to useful results. Put it differently, an epistemology is valid when ontology fits. This, of course, may strike some as a tautology, and it is, but only within the closed self-referential system of dominant scientific logic. Considered as a belief that drives one's scientific posture it is powerful and has far reaching consequences. There is an asymmetry in the relationship of pragmatists and scientists adhering to dominant paradigms. The former respect the latter, and are willing whenever they judge it useful to employ their methods and finding; the contrary is unfortunately not true.

Pragmatists, or at least some of them (Ackoff and Emery, 1972; Churchman, 1968, 1970, 1971, 1979), see causality as being capable of accounting for only a fraction of social reality. Many phenomena are co-produced by multiple other phenomena, each of which is necessary but insufficient. Furthermore, the producers interact with each other in producing their product: they form a system, a collection of elements whose constituents are bound together by virtue of their co-production of the product, and which can be considered a cause only collectively. Unlike in General Systems Theory (von Bertalanffy, 1968), which views systems as sets of interrelated elements and focuses on the study of the structure of their relations, the pragmatic view of system is functional, focusing on the finality of the whole.

One is tempted to conclude that this systemic view of reality can easily be accounted for through classical statistical techniques such as multivariate analysis, which after all handle "multiple causes" pretty well. True, but only to a limited extent. Indeed, another basic tenant of pragmatism, a key ontological assumption if you will, is that social reality is populated by teleological systems that are capable of pursuing different ends in the same environment and maintain and end across a range of contextual conditions (Ackoff and Emery, 1972). This is, we argue, nothing more than to reaffirm the old philosophical stance of primacy of free will over determinism: purposeful systems, such as people or organizations, are capable of behavior that transcends rules and of intent to do so. Therefore, any deterministic model of such behavior, while potentially useful and contributing to understanding, has the potential of becoming invalidated. In fact many

المنارة للاستشارات

www.mana

interventionist techniques of social sciences, for example in psychology or organizational change agency, construct with their subject an understanding of producers of their behavior only to facilitate and encourage transcending them in the hope of achieving therapeutic effects or increased performance.

To a pragmatist any explicit model of a purposeful entity that identifies several co-producers of a product – the kind produced by dominant management research – is best assumed to be incomplete, even if it accounts for most of the observed variance in dependent variables (Sachs, 1976, 1977). Indeed, if the model is complete and likely to remain so in foreseeable future, then the entity is not purposeful, since it is incapable of transcending its behavioral rules. While this may well be the case, to assume that it is not, is a more prudent research strategy. The pragmatist is not against such models, but calls for their use with caution rather than assiduousness or technocratic enthusiasm, observing that such complete robust models are rare if they exist at all, and assuming that there are co-producers which cannot be known explicitly at a particular point in time.

Pragmatic practice of management science

Pragmatism is a philosophy that invites deep commitment to practice. Three eminent management scholars, Russell Ackoff, West Churchman and Donald Schön were trained as philosophers, and told one of the authors that their personal choice to get involved with practical problems was "to live their pragmatism." Churchman was from 1954 to 1958 simultaneously the chief editor of *Philosophy of Science* and of *Management Science* (which he founded), something hard to imagine in today's compartmentalized academia. In his guest editorial, published on the 60th anniversary of *Philosophy of Science,* he complains bitterly about the gap between science and practice: "The journal seems to spend most of its pages on the puzzles and imperfections of scientific theories, especially those arising in that most confusing of all the disciplines, physics. Whether human being should study physical nature, whether it is dangerous or ethical to do so, how such studies relate to other human interests and activities; in fact, all the issues complicating the study of physics are never discussed in the journal" (Churchman, 1994, p. 132). His close associate, (Ackoff, 1979) expresses similar misgivings about operations research degenerating from a field dedicated to solving human problems to a technocratic discipline defined by its own tools and paradigms.

Pragmatists see their action as driven by dialectics, a process of arriving at truth through confrontation of different points of view. Of course, we all know of individuals that practice dialectics in splendid isolation, by incorporating in their argumentation the thesis, the anti-thesis and synthesis; French high-school pupils are taught to write their dissertation in this manner. But for dialectics to work best there must be at least two actors, each with a point of view, in disagreement and caring enough to fight for their stance. The process can come to an end either by the actors developing jointly a new point of view or by there being an arbiter who does this. Truth emerges as a synthesis of the opposing views, and becomes the thesis for the next cycle of dialectical progression. Legal and democratic systems are based on this process: nobody expects a lawyer for the accused to present any truth other than that most favorable to the client. In autocratic systems pluralism of views is suppressed and truth is decreed by the most powerful actor. Smart corporations encourage pluralism and debate, although they may use autocratic means to achieve closure. For example, a multi-industry conglomerate decided to focus on its defense business and systems integration work. It divested several

المنارة للاستشارات
www.mana

billion dollars from its portfolio, and had lots of cash while clarifying further its strategy. Investment bankers constantly called with propositions of companies to acquire. The company applied a dialectical method for evaluating such propositions, after a cursory preliminary screening. A consultant (one of the authors) would produce a short memo summarizing the doubts in the minds of the top management team. Based on this, two or three of the top advisory firms would be asked to produce a "quick and dirty" assessment of the acquisition opportunity and its strategic fit. They did not know of each other. The top management team and the consultant would receive several opinions, which usually were presented as the "ultimate truth," but seldom agreed with each other. After a discussion with the top management team the consultant would propose a synthesis position. Occasionally the Chairman disagreed with the consultant, reaching and imposing a synthesis of his own.

The dominant paradigms delegate the job of determining truth to procedures that are accepted by scientists as being valid. Dialectics take the process a step further, recognizing that truth is a social construct. Contradictory processes and pluralism are better at producing truth in certain situations, among them the kind of situations that interest management. Dialectics does not reject the classical scientific way of arriving at truth; it merely postulates that it is good at doing what it does in certain circumstances, such as when dealing with physical objects or a limited class of managerial phenomena, or to put it differently, when it is hard to conceive of reasonable social actors that would question the validity of the process.

The corollary of the discussion so far is that the job of a particular researcher dealing with a particular situation is not to seek objective truth in a self-contained procedural cocoon, but to produce an input that will advance the social truth-seeking process and improve its quality. Truth is not contained in any given piece of research output, but is result of the scientific process and of the broader societal processes encompassing it. Truth is constantly refined and advanced. To produce a useful contribution the researcher must be immersed in a specific social process of truth-seeking, be a contributor to a pluralistic conversation. Frequently, this can be best achieved through classical scientific method, but at other times this is best achieved through reasoned and honest advocacy of a specific point of view. Objectivity "is a characteristic not of the data, but rather of the design of the inquiring system as a whole: does it try to be open to all those aspects it deems relevant" (Churchman, 1979, p. 147).

If truth is produced by a social process, and if there must be consensus or arbitration to reach a synthesis, then why not explicitly address the question of who the client for research is? Pragmatists consider a problem to exist only when there are at least two possible courses of action and if there is doubt as to which is preferable. Problem solving is determining the preferred course of action (Ackoff, 1962). When there is no doubt, there is no problem. If the cafeteria at work offers the choice of meat or fish, and if one is allergic to fish, there is no problem. A problem arises when one has no allergy and both dishes are equally tempting or repulsive. Doubt is a state of mind. Therefore, for there to be doubt there must be a mind, this is to say somebody harboring that doubt. We propose to call that somebody a client (Churchman, 1970, 1971). In much management research researchers are their own clients, or at best other researchers are. No wonder then that the output is likely to be irrelevant to managers.

المنارة للاستشارات

www.mana

But if researchers work on client problems, how is research different from consulting? The answer lies in considering what differentiates science from non-science and the process of reflection.

Bridging the gap: the reflective practitioner and the practicing reflector

Schön (1987) studied practitioners in many fields, such as management, architecture or medicine. He observed that most of them act in everyday situations based on habits and intuition, without taking their time to explicitly analyze their assumptions or expectations. They act on what is called "tacit knowledge" (Polanyi, 1958, 1967) or "theory in action" (Argyris, 1993; Argyris and Schön, 1991). It is only when things do not work as expected that the better practitioners take the time to reflect on what is different from previous situations or on what went wrong. It is in this process that they learn, modifying the theories that underlie their actions, rendering in the process explicit what was hereto tacit. For a while they become reflective practitioners, or what we may call *ad hoc* researchers. In times of such reflection the manager may greatly benefit from the assistance of a trained researcher, provided they can share the same language and work together. Parenthetically let us note that in modern societies many managers received at least partial training as scientists.

A researcher engaged with a client will have to act and speak like a manager. Otherwise, she or he may lose the understanding and/or the patience of the client, and takes the risk of becoming irrelevant, losing thus the fundamental insights that come from action. Managerial situations are fast-paced, idiosyncratic and stochastic; executives work under pressures of diverse stakeholders, limited time, incomplete information and other constraints, and will not tolerate for long a slow deliberate process typical of traditional researchers. However, there usually is nothing to prevent the engaged researcher from being more reflective or more scientific. An engaged researcher can simultaneously pursue two agendas: that of the client and that of research. Subject to constraints of confidentiality the researcher can contribute from his practical experience to the advancement of management science. Engaging in client-based research motivates the researcher to become multidisciplinary. Very few management problems are ones that can be treated with tools and concepts from a single managerial discipline. A problem of escalating costs may at first appear as financial, but in fact be result of wrong incentives or attitudes by the employees. A pure researcher has the luxury of pleading specialization as an excuse for not tackling such a problem. A practitioner has no such luxury.

Pragmatism and action research

Action research is one approach that illustrates the ideas advanced so far. It has the potential of improving the practical relevance of research (Evbuomwan *et al.*, 1996). While in classical methodological approaches the researcher studies organizational phenomena without intervening in the managerial process, in action research he or she simultaneously studies the phenomena and actively participates in organizational change (Baburoglu and Ravn, 1992). Action research is based on collaboration between researchers and research subjects. The primary objective is to solve practical problems while expanding scientific knowledge. It proceeds in two stages: joint diagnosis and formulation of applicable theory; and collaborative implementation of change and assessment. Prominent organization scholars such as Lewin (1948), Ackoff (1974,

المنارة للاستشارات
www.mana

1999), Argyris (1993), Churchman (1968), Emery and Trist (1972), and Schön (1987), advocated some form of action research. Many related theories and methods of pragmatic and practice-near inquiry, (e.g. participative action research (Freire, 1970) and cooperative inquiry (Heron, 1996), etc.), but also methods and theories of learning and teaching such as action learning, self-directed learning, experiential learning (Howell, 1994) have been inspired by these scholars.

Baskerville and Myers (2004) identify four key premises of action research that stem from pragmatist philosophy: the meaning of all human concepts are defined by their consequences (Peirce, 1905); truth is embodied in practical outcomes (James, 1907); inquiry is controlled because rational thought is interspersed with action (Dewey, 1938); and human action is socially contextualized and human conceptualization is a social reflection (Mead, 1913). Action research (Argyris, 1993; Argyris and Schön, 1991); Chia and Holt, 2006; Emery and Trist, 1972; Hardy and Leiba-O'Sullivan, 1998; Jarzabkowski, 2004) calls for the scholar "getting his or her hands dirty," by becoming involved in practical organizational work that actually produces change or novelty. Science, to the extent that it is useful, informs the scholar-practitioner's actions in the field, and integration of disparate theories takes place "in action." The action researcher learns from his or her involvement what is useful, and what is not, what works and what does not, and brings back into the realm of academia an enriched research agenda as well as field results that can be interpreted with the usual battery of scientific tools.

Is an action researcher a manger or a scientist? The answer is both: he or she becomes a manager to a certain extent and a full participant in the process of managing an organization. She is a researcher, because she seeks to render explicit the implicit, and because she brings scientific rigor to the task of management. To a pragmatist science is not defined by the knowledge it produces (much knowledge hitherto reserved to scientists became available to the common man today), nor by the use of specific tools and techniques (commonly referred to as scientific method, although we prefer to reserve this term for a broader and more philosophical concept). Ackoff (1962) points out that that "common sense" and "scientific" inquiries are extreme poles of a continuum defined by the degree of control that goes into the inquiry. Control is to know why and to what purpose particular actions are undertaken. In other words, spelling out the assumptions and clearly stating the expectations. Control is also continually verifying that the assumptions still hold, and making sure that expectations are met. If one or the other is not true, then control is taking corrective action (Ackoff, 1967). Most everyday actions, including managerial ones, are not controlled to a very large extent. Many surprises arise, because the context (assumptions) changes. Many actions do not lead to expected outcomes, but are not recognized as failures, and therefore result in no learning or corrective measures. The role of a researcher is to bring more control to managerial action. Scientific inquiry is controlled inquiry.

Conclusions

We argued in this paper that the theory-praxis gap is alive and that it is largely due to basic philosophical flaws in management academia. We purport that there is an alternative rooted in pragmatism and embodied in approaches such as action research and action learning. To those who deplore the irrelevance of management research but

المنارة للاستشارات

www.mana

reject pragmatism on grounds of scientist orthodoxy we say: it is not pragmatism, but rather irrelevant theory – and thereby lack of pragmatism – that has led to the current divorce between academia and management practice. With the late Ghoshal (2005, p. 75) we regret that: "Many of the worst excesses of recent management practices have their roots in a set of ideas that have emerged from business school academics over the last 30 years". We contribute to the current discussion with some pragmatic proposals to improve relevance of management knowledge production and socialization both in research and education (Table II).

We would not be true to the pragmatist's obligation to be skeptical and cautious about its theses, if we did not raise the question; and what if the gap did not matter? Can we continue to ignore the lack of relevance in management research? After all, the ivory tower has its charm and while there is financing, why not enjoy? Management practice is progressing at unprecedented pace, and it becomes increasingly rigorous . . . scientific. But like the late Sumatra Ghoshal (2005) we think that academics bear a responsibility. If we do not truly seek to understand and in a time frame that permits us to prepare our students better and in more timely manner for what expects them in practice, the gap between their learning years and textbooks and the reality "out there" as they experience them, will become intolerable. They are likely to discard their learning and become "obsessed with the real world and skeptical as most of them are of all theories, managers are no exception to the intellectual slavery of the 'practical men' to which Keynes referred" (Ghoshal, 2005, p. 75).

If one sets aside skepticism about bureaucratic gobbledygook one might find a ray of hope in the definition of research by the UK Research Assessment Exercise as quoted in the "Code of practice for the assurance of academic quality and standards in higher education: Postgraduate research programmes" put out in September 2004 by the Quality Assurance Agency for Higher Education and distributed to British academics (p. 4):

> Research [. . .] is to be understood as original investigation undertaken in order to gain knowledge and understanding. It includes work of direct relevance to the needs of commerce and industry, as well as to the public and voluntary sectors; scholarship; the invention and generation of ideas, images, performances and artifacts including design, where these lead to substantially improved insights; and the use of existing knowledge in experimental development to produce new or substantially improved materials, devices, products and processes, including design and construction.

References

Abrahamson, E. (1991), "Managerial fads and fashions: the diffusion and rejection of innovations", *Academy of Management Review*, Vol. 16 No. 3, pp. 586-612.

Abrahamson, E. and Fairchild, G. (1999), "Management fashion: lifecycles, triggers, and collective learning processes", *Administrative Science Quarterly*, Vol. 44, pp. 708-40.

Ackoff, R.L. (1962), *Scientific Method: Optimizing Applied Research Decisions*, Wiley, New York, NY.

Ackoff, R.L. (1967), "Management minsinformation systems", *Management Science*, Vol. 14 No. 4, pp. 146-56.

Ackoff, R.L. (1974), *Redesigning the Future*, Wiley, New York, NY.

Ackoff, R.L. (1978), *The Art of Problem Solving; Accompanied by Ackoff's Fables*, Wiley, New York, NY.

المنارة للاستشارات

www.mana

Ackoff, R.L. (1979), "The future of operational research is past", *Journal Operational Research Society*, Vol. 30 No. 2, pp. 93-104.

Ackoff, R.L. (1999), *Re-creating the Corporation: A Design of Organizations for the 21st Century*, Oxford University Press, New York, NY.

Ackoff, R.L. and Emery, F.E. (1972), *On Purposeful Systems*, Adline Adherton, Chicago, IL.

Amit, R. and Schoemaker, P. (1993), "Strategic assets and organizational rent", *Strategic Management Journal*, Vol. 14 No. 1, pp. 33-46.

Archer, M.S. (1995), *Realist Social Theory: The Morphogenetic Approach*, Cambridge University Press, Cambridge.

Argyris, C. (1993), *Knowledge for Action: a Guide to Overcoming Barriers to Organizational Change*, Jossey-Bass, San Francisco, CA.

Argyris, C. and Schön, D.A. (1991), "Participatory action research and action science compared: a commentary", in Whyte, W.F. (Ed.), *Participatory Action Research*, Sage, London, pp. 85-96.

Astley, W.G. and Zammuto, R.F. (1992), "Organization science, managers, and language games", *Organization Science*, Vol. 3 No. 4, pp. 443-60.

Baburoglu, O.N. and Ravn, I. (1992), "Normative action research", *Organization Studies*, Vol. 13 No. 1, pp. 19-34.

Baskerville, R. and Myers, M.D. (2004), "Special issue foreword", *MIS Quarterly*, Vol. 28 No. 3, pp. 329-35.

Berger, P. and Luckmann, T. (1966), *The Social Construction of Reality. A Treatise in the Sociology of Knowledge*, Penguin, London.

Beyer, J.M. (1982), "Introduction to the special issue on the utilization of organizational research", *Administrative Science Quarterly*, Vol. 27, pp. 588-90.

Beyer, J.M. and Trice, H.M. (1982), "The utilisation process: a conceptual framework and synthesis of empirical findings", *Administrative Science Quarterly*, Vol. 27, pp. 591-622.

Blau, P.M. (1994), *The Organisation of Academic Work*, Wiley Interscience, New York, NY.

Boland, R.J., Singh, J., Salipante, P., Aram, J.D., Fay, S.Y. and Anawattanachai, P.K. (2001), "Knowledge representations and knowledge transfer", *Academy of Management Journal*, Vol. 44, pp. 393-417.

Burgoyne, J. and Turnbull James, K. (2006), "Towards best or better practice in corporate leadership development: operational issues in mode 2 and design science research", *British Journal of Management*, Vol. 17, pp. 303-16.

Burrell, G. (1989), "The absent centre:the neglect of philosophy in anglo-american management theory", *Human Systems Management*, Vol. 8, pp. 307-12.

Byrne, J.P. (1986), "'Business Fads': what is in and what is out", *Business Week*, pp. 52-6.

Cheng, J.L.C. and McKinley, W. (1983), "Toward an integration of organization research and practice: a contingency study of bureaucratic control and performance in scientific settings", *Administrative Science Quarterly*, Vol. 28 No. 1, pp. 85-100.

Chia, R. and Holt, R. (2006), "Strategy as practical coping: a Heideggerian perspective", *Organization Studies*, Vol. 27, pp. 635-55.

Child, J. (1995), "Guest editorial", *Organization Science*, Vol. 6, pp. 117-8.

Churchman, C.W. (1968), *The Systems Approach*, Dell, New York, NY.

Churchman, C.W. (1970), "Operations research as a profession", *Management Science*, Vol. 17 No. 2, pp. B37-B53.

المنارة للاستشارات

www.mana

Churchman, C.W. (1971), *The Design of Inquiring Systems: Basic Concepts of Systems and Organizations*, Basic Books, New York, NY.

Churchman, C.W. (1979), *The Systems Approach and Its Enemies*, Basic Books, New York, NY.

Churchman, C.W. (1994), "What is philosophy of science?", *Philosophy of Science*, Vol. 61, pp. 132-41.

Clegg, S.R. and Ross-Smith, A. (2003), "Revising the boundaries: management education and learning in a post-positivst world", *Academy of Management Learning and Education*, p. 2.

Cohen, M.C. and March, J.G. (1973), *Leadership and Ambiguity*, McGraw-Hill, New York, NY.

Cross, N. (1993), "Science and design methodology: a review", *Research in Engineering Design*, Vol. 5 No. 2, pp. 63-9.

Czarniawska, B. (2003), "Forbidden knowledge: organization theory in times of transition", *Management Learning*, Vol. 33, pp. 353-65.

Daniel, C.A. (1998), *MBA: The First Century*, Bucknell University Press, Lewisburg, PA.

Dewey, J. (1929), *The Quest for Certainty: A Study of the Relation of Knowledge in Action*, Minton, Balch, and Company, New York, NY.

Dewey, J. (1938), *Logic: The Theory of Inquiry*, Henry Holt & Co., New York, NY.

Dewey, J. *et al.* (1904), "The relation of theory to practice in education: discussion", paper presented at the National Society for the Scientific Study of Education Meeting, Atlanta, GA.

Dierickx, I. and Cool, K. (1989), "Asset stock accumulation and sustainability of competitive advantage", *Management Science*, Vol. 35 No. 12, pp. 1504-11.

Donaldson, L. (2002), "Damned by our own theories: contradictions between theories and management education", *Academy of Management Learning and Education*, Vol. 1 No. 1, pp. 96-106.

Donaldson, L. (2005), "For positive management theories while retaining science: reply to Ghoshal", *Academy of Management Learning and Education*, Vol. 4 No. 1, pp. 109-13.

Duderstadt, J. (2000), *A University for the 21st Century*, University of Michigan Press, Ann Arbor, MI.

Elsbach, K.D., Sutton, R.I. and Whetten, D.A. (1999), "Perspectives on developing management theory, circa 1999: moving from shrill monologues to (relatively) tame dialogues", *Academy of Management Review*, Vol. 24 No. 4, pp. 627-33.

Emery, F.E. and Trist, E.L. (1972), *Towards a Social Ecology: Contextual Appreciation of the Future in the Present*, Plenum, London.

Emory, W.C. (1985), *Business Research Methods*, Irwin, Homewood, IL.

Evbuomwan, N., Sivaloganathan, S. and Jebb, A. (1996), "A survey of design philosophies, models, methods and systems", *Journal of Engineering Manufacture*, Vol. 210, pp. 301-19.

Fendt, J. (2005), *The CEO in Post-Merger Situations: An Emerging Theory on the Management of Multiple Realities*, Eburon, Delft.

Forrester, J.W. (1970), *World Dynamics*, Wright-Allen Press, Cambridge.

Freire, P. (1970), *Pedagogy of the Oppressed*, Herder & Herder, New York, NY.

Gapper, J. (2005), "Comment on Sumantra Ghoshal's 'Bad management theories are destroying good management practices'", *Academy of Management Learning and Education*, Vol. 4, p. 1.

Ghoshal, S. (2005), "Bad management theories are destroying good management practices", *Academy of Management Learning and Education*, Vol. 4 No. 1, pp. 75-91.

المنارة للاستشارات

www.mana

Gibbons, M., Limoges, C., Nowotny, H., Schwartzman, S., Scott, P. and Trow, M. (1994), *The New Production of Knowledge: the Dynamics of Science and Research in Contemporary Societies*, Sage, London.

Gordon, R.A. and Howell, J.H. (1959), *Higher Education for Business*, Columbia University, New York, NY.

Gosling, J. and Mintzberg, H. (2006), "Management education as if both matter", *Management Learning*, Vol. 37 No. 4, pp. 419-29.

Hambrick, D.C. (1994), "What if the academy actually mattered?", *Academy of Management Review*, Vol. 19, pp. 11-16.

Hambrick, D.C. (2005), "Just how bad are our theories? A response to Ghoshal", *Academy of Management Learning and Education*, Vol. 4 No. 1, pp. 104-7.

Hamel, G. and Prahalad, C.K. (1994), *Competing for the Future*, Harvard Business School Press, Boston, MA.

Hardy, C. and Leiba-O'Sullivan, S. (1998), "The power empowerment: implications for research and practice", *Human Relations*, Vol. 51, pp. 451-83.

Hayek, F.A.V. (1989), "The pretense of knowledge (Nobel Lecture)", *American Economic Review*, December, pp. 3-7.

Heron, J. (1996), *Cooperative Inquiry: Research into the Human Condition*, Sage, London.

Howell, F. (1994), "Action learning and action research in management education and development", *The Learning Organization*, Vol. 1 No. 2, pp. 15-22.

Hubka, V. and Eder, W. (1996), *Design Science: Introduction to the Needs, Scope and Organization of Engineering Design Knowledge*, Springer, London.

Huczynski, A.A. (1996), *Management Gurus: What Makes Them and How to Become One*, Thomson, London.

Huff, A.S. (2000), "Changes in organizational knowledge production", *Academy of Management Review*, Vol. 25, pp. 288-93.

Jackson, B. (2001), *Management Gurus and Management Fashions*, Routledge, London.

James, W. (1907), *Pragmatism, A New Name for Some Old Ways of Thinking. Popular Lectures on Philosophy*, Longmans, Green, and Company, New York, NY.

Jarzabkowski, P. (2004), "Strategy as practice: recursiveness. Adaptation and practices-in-use", *Organization Studies*, Vol. 24, pp. 489-520.

Keleman, M. and Bansal, P. (2002), "The conventions of management research and the relevance to management practice", *Academy of Management Journal*, Vol. 13, pp. 97-108.

Kilduff, M. and Kelemen, M. (2001), "The consolations of organization theory", *British Journal of Management*, Vol. 12, pp. S55-9.

King, W.R. and Cleland, D.I. (1978), *Strategic Planning and Policy*, Van Nostrand Reinhold, New York, NY.

Koontz, H. (1961), "The management theory jungle", *Academy of Management Journal*, Vol. 4, pp. 174-88.

Koontz, H. (1980), "The management theory jungle revisited", *Academy of Management Review*, Vol. 5, pp. 175-87.

Koza, M.P. and Thoenig, J.C. (1995), "Organisational theory at the crossroads. Some reflections on European and United States approaches to organisational research", *Organization Science*, Vol. 6 No. 1, pp. 1-8.

المنارة للاستشارات

www.mana

Leavitt, H.J. (1989), "Educating our MBAs: on teaching what we haven't taught", *California Management Review*, Vol. 31 No. 3, pp. 38-50.

Lewin, K. (1948), *Resolving Social Conflicts: Selected Papers on Group Dynamics*, Harper & Row, New York, NY.

Lorange, P. (2002), *New Vision for Management Education: Leadership Challenges*, Elsevier Science, Oxford.

Lorange, P. and Vancil, R.F. (1977), *Strategic Planning Systems*, Prentice-Hall, Englewood Cliffs, NJ.

McKelvey, B. (1997), "Quasi-natural organization science", *Organization Science*, Vol. 8 No. 4, pp. 352-80.

McKelvey, B. (2002), "Model-centered organization science epistemology", in Baum, J.A.C. (Ed.), *Companion to Organizations*, Blackwell, Oxford, pp. 752-80.

McKelvey, B. (2003a), "From fields to science: can organization studies make the transition?", in Westwood, R. and Clegg, S.R. (Eds), *Debating Organization: Point-Counterpoint in Organization Studies*, Blackwell, Oxford, pp. 47-73.

McKelvey, B. (2003b), "Postmodernism vs. truth in management theory", in Locke, E. (Ed.), *Post Modernism and Management: Pro's and Cons and the Alternative Research in the Sociology of Organizations*, Elsevier Science, Amsterdam, pp. 113-68.

MacLean, D. and MacIntosh, R. (2002), "Mode 2 management research", *European Journal of Management*, Vol. 20 No. 4, pp. 383-92.

March, J.G. and Sutton, R.I. (1997), "Organizational performance as a dependent variable", *Organization Science*, Vol. 8, pp. 698-706.

Mead, G.H. (1913), "The social self", *The Journal of Philosophy, Psychology, and Scientific Methods*, Vol. 10, pp. 374-80.

Micklethwait, J. and Woolridge, A. (1996), *The Witch Doctors: What Management Gurus are Saying, Why it Matters and How to Make Sense of It*, Heinemann, London.

Miller, D., Greenwood, R. and Hinings, B. (1997), "Creative chaos versus munificent momentum: the schism between normative and academic views of organizational change", *Journal of Management Inquiry*, Vol. 6 No. 1, pp. 71-8.

Miner, J.B. (1984), "The validity and usefulness of theories in an emerging organizational science", *Academy of Management Review*, Vol. 9 No. 2, pp. 296-306.

Mintzberg, H. (2004), *Managers Not MBAs: a Hard Look at the Soft Practice of Management and Management Development*, FT/Prentice-Hall, London.

Mintzberg, H. (2005), "How inspiring. How sad. Comment on Sumantra Ghoshal's paper", *Academy of Management Learning and Education*, Vol. 4 No. 1, p. 108.

Mintzberg, H. and Gosling, J. (2002), "Educating managers beyond borders", *Academy of Management Learning and Education*, Vol. 1 No. 1, pp. 64-76.

Mintzberg, H., Raisinghani, D. and Théorêt, A. (1976), "The structure of 'unstructured' decision processes", *Administrative Science Quarterly*, Vol. 21 No. 2, pp. 246-75.

Montgomery, C.A., Wernerfelt, B. and Balakrishnan, S. (1989), "Strategy content and the research process: a critique and commentary", *Strategic Management Journal*, Vol. 10 No. 2, pp. 189-97.

Nagel, E. (1979), *The Structure of Science*, Hackett, Indianapolis, IN.

Nord, W.R. (2005), "Treats and some treatments: responses to Ghoshal", *Academy of Management Learning and Education*, Vol. 4 No. 1, p. 92.

Pascale, R.T. (1990a), *Managing on the Edge: How Japanese Companies Use Conflict to Stay Ahead*, Simon & Schuster, New York, NY.

المنارة للاستشارات

www.mana

Pascale, R.T. (1990b), *Managing on the Edge: How the Smartest Companies Use Conflict to Stay Ahead*, Simon & Schuster, New York, NY.

Peirce, C.S. (1905), "The architectonic construction of pragmatism", in Burks, A.W. (Ed.), *Collected Papers of Charles Sanders Peirce*, Vol. V, Harvard University Press, Cambridge, pp. 3-6.

Peirce, C.S. (1960), "The rules of philosophy", in Konvitz, M. and Kennedy, G. (Eds), *The American Pragmatists*, New American Library, New York, NY.

Peirce, C.S. (1992), *The Essential Peirce: Selected Philosophical Writings*, Vol. 1, Indiana University Press, Indianapolis, IN (1867-1893).

Peirce, C.S. (1998), *The Essential Peirce: Selected Philosophical Writings*, Vol. 2, Indiana University Press, Indianapolis, IN (1893-1913).

Pelz, D.S. (1978), "Some expanded perspectives on the use of social science in public policy", in Yinger, M. and Cutler, S.J. (Eds), *Major Social Issues: A Multidisciplinary View*, Free Press, New York, NY, pp. 346-57.

Penrose, E.E.T. (1959), *The Theory of the Growth of the Firm*, Basil Blackwell, Oxford.

Pettigrew, A. (1997), "The double handles of management research", in Clarke, T. (Ed.), *Advancement in Organisational Behaviour: Essays in Honor of Derek S. Pugh*, Dartmouth Press, London, pp. 277-96.

Pfeffer, J. (1993), "Barriers to the advancement of organisational science: paradigm development as a dependent variable", *Academy of Management Review*, Vol. 18, pp. 599-620.

Pfeffer, J. (2005a), "Why do bad management theories persist?", *Academy of Management Learning and Education*, Vol. 4 No. 1, pp. 96-100.

Pfeffer, J. (2005b), "Why do bad management theories persist? A comment on Ghoshal", *Academy of Management Learning and Education*, Vol. 4, pp. 96-100.

Pfeffer, J. and Fong, C.T. (2002), "The end of business schools? Less success than meets the eye", *Academy of Management Learning and Education*, Vol. 1 No. 1, pp. 78-95.

Pierson, F.C. (1959), *The Education of American Business Men. A Study of University-College Programs in Business Administration*, McGraw-Hill, New York, NY.

Polanyi, M. (1958), *Personal Knowledge*, Routledge, London.

Polanyi, M. (1967), *The Tacit Dimension*, Anchor Books, New York, NY.

Porter, M. (1980), *Competitive Strategy*, Free Press, New York, NY.

Porter, L.W. and McKibben, L.E. (1988), *Management Education and Development: Rift or Thrust into the 21st Century?*, McGraw-Hill, New York, NY.

Priem, R.L. and Rosenstein, J. (2000), "Is organization theory obvious to practitioners? A test of one established theory", *Organization Science*, Vol. 11, pp. 509-24.

Rorty, R. (1982), *Consequences of Pragmatism*, University of Minnesota Press, Minneapolis, MN.

Rynes, S.L., Bartunek, J.M. and Daft, R.L. (2001), "Across the great divide: knowledge creation and transfer between practitioners and academics", *Academy of Management Journal*, Vol. 44, pp. 340-55.

Sachs, W.M. (1976), "Toward formal foundation of teleological systems science", *General Systems Yearbook*, Vol. 21, pp. 145-53.

Sachs, W.M. (1977), "Thoughts on the mathematical method and future problems", in Linstone, H.A. and Simmonds, W.H.C. (Eds), *Futures Research: New Directions*, Vol. 164-174, Addison Wesley, Reading, MA.

المنارة للاستشارات

www.mana

Samuelson, J. (2006), "The new rigor: beyond the right answer", *Academy of Management Learning and Education*, Vol. 5 No. 3, pp. 356-65.

Sanchez, R., Heene, A. and Thomas, H. (1996), *Dynamics of Competence-based Competition: Theory and Practice in the New Strategic Management*, Elsevier, Oxford.

Sayer, A. (1992), *Method in Social Science: A Realist Approach*, 2nd, Routledge, London.

Schlossmann, S., Sedlak, M. and Wechsler, H. (1987), "The 'New Look': the ford foundation and the revolution in business education", *Selections, The Magazine of the Graduate Management Admission Council*, Winter, pp. 11-13.

Schön, D.A. (1982), *The Reflective Practitioner: How Professionals Think in Action*, Basic Books, New York, NY.

Schön, D.A. (1987), *Educating the Reflective Practitioner*, Jossey-Bass, San Francisco, CA.

Schulz, M. and Hatch, M.J. (1996), "Living with multiple paradigms: the case of paradigm interplay in organizational culture studies", *Academy of Management Review*, Vol. 21, pp. 529-57.

Searle, J.R. (1995), *The Construction of Social Reality*, Penguin, London.

Seth, A. and Zinkhan, G. (1991), "Strategy and research process: a comment", *Strategic Management Journal*, Vol. 12 No. 1, pp. 75-82.

Shrivastava, P. and Mitroff, I.I. (1984), "Enhancing organizational research utilization: the role of decision makers' assumptions", *Academy of Management Review*, Vol. 9 No. 1, pp. 18-26.

Simon, H.A. (1960), "The shape of automation", in Pylyshyn, Z.W. (Ed.), *Perspectives on the Computer Revolution*, Prentice-Hall, Englewood Cliffs, NJ.

Simon, H.A. (1969), *The Sciences of the Artificial*, MIT Press, Cambridge.

Skinner, B.F. (1974), *About Behaviorism*, Alfred A. Knopf, New York, NY.

Starbuck, W.H. (1976), "Organizations and their environments", in Dunnette, M.D. (Ed.), *Handbook of Industrial and Organizational Psychology*, Rand McNally, Chicago, IL.

Starbuck, W.H. and Nystrom, P.C. (Eds) (1981), *Designing and Understanding Organisations*, Vol. 1, Oxford University Press, Oxford.

Starkey, K. and Madan, P. (2001), "Bridging the relevance gap: aligning stakeholders in the future management research", *British Journal of Management*, Vol. 12, pp. 503-26.

Steiner, G.A. (1979), *Strategic Planning*, Free Press, New York, NY.

Suddaby, R. (2006), "From the editors: what grounded theory is not", *Academy of Management Journal*, Vol. 49 No. 4, pp. 633-42.

Thomas, K.W. and Tymon, W.G. (1982), "Necessary properties of relevant research: lessons from recent criticisms of the organizational sciences", *Academy of Management Review*, Vol. 17, pp. 345-52.

Thompson, J.D. (1967), *Organization in Action*, McGraw-Hill, New York, NY.

van Aken, J.E. (2004), "Management research based on the paradigm of the design sciences: the quest for field-tested and grounded technological rules", *Journal of Management Studies*, Vol. 41 No. 2, pp. 219-46.

van Aken, J.E. (2005), "Management research as a design science: articulating the research product of mode 2 knowledge production in management", *British Journal of Management*, Vol. 16, pp. 19-36.

Van Strien, P.J. (1997), "Towards a methodology of psychological practice. The regulative cycle", *Theory & Psychology*, Vol. 7 No. 5, pp. 683-700.

von Bertalanffy, L. (1968), *General Systems Theory*, Braziller, New York, NY.

المنارة للاستشارات

www.mana

Weick, K.E. (2001), "Gapping the relevance bridge: fashions meet fundamentals in management research", *British Journal of Management*, Vol. 12, pp. 71-6.

Weizenbaum, J. (1976), *Computer Power and Human Reason: From Judgment to Calculation*, W.H. Freeman and Company, San Francisco, CA.

Wernerfelt, B. (1984), "A resource-basel view of the firm", *Strategic Management Journal*, Vol. 5 No. 2, pp. 171-80.

Whitley, R. (1988), "The management sciences and managerial skills", *Organization Studies*, Vol. 9 No. 1, pp. 47-68.

Wicks, A. and Freeman, R.E. (1998), "Organization studies and the new pragmatism", *Organization Science*, Vol. 9 No. 2, pp. 123-40.

Wooldridge, B. and Floyd, S.W. (1990), "The strategy process, middle management involvement and organizational performance", *Strategic Management Journal*, Vol. 11 No. 3, pp. 231-41.

Further reading

Fals-Borda, O. and Rahman, M.A. (1991), *Action Knowledge: Breaking the Monopoly with Participatory Action Research*, Intermediate Technology/Apex, New York, NY.

Testimony before the Subcommittee on Urban Growth (1970), 91st Congress, 2/III Sess. 205-265.

About the authors

Jacqueline Fendt is Professor of Strategy and Entrepreneurship at ESCP-EAP European School of Management in Paris and Director of the school's Chair of Entrepreneurship. Prior, she was Dean of GSBA Zurich, Switzerland. Jacqueline has come to academia from a long-standing corporate career, *inter alia* with executive positions at Ciba-Geigy Ltd, DEC Switzerland and chief executive responsibilities of Swiss Shipping Company Ltd and Expo.01. She holds a PhD in organization science from Leiden University, The Netherlands and an MBA from GSBA Zurich and Boston University.

Renata Kaminska-Labbé is Professor of Strategic Organizing at CERAM Sophia Antipolis European School of Business where she also directs the continuing master's education program. She is a visiting professor at the Krakow University of Economics and at Leon Kozminski Academy of Entrepreneurship and Management in Warsaw. She has a doctorate in management from the University of Nice, and an undergraduate degree from the University of Western Ontario. Renata Kaminska-Labbé is the corresponding author and can be contacted at: renata.labbe@ceram.fr

Wladimir M. Sachs was Associate Dean of Research at ESC Rennes School of Business. He was director of TEMA School of Technology and Management; an undergraduate business program jointly operated by Reims Management School and Ecole Nationale des Ponts et Chaussées. Previously he was on the Faculty of Wharton School and of Rotterdam School of Management, as well as high-tech entrepreneur, manager and management consultant to companies and other organizations in the USA, Latin America and Europe. He held a PhD in management from the Wharton School and a D.E.A. (advanced post-Master's degree) in mathematics from the University of Paris at Orsay. He died in October 2007.

To purchase reprints of this article please e-mail: **reprints@emeraldinsight.com**
Or visit our web site for further details: **www.emeraldinsight.com/reprints**

المنارة للاستشارات

www.mana

Reproduced with permission of the copyright owner. Further reproduction prohibited without permission.

www.manaraa.com