Received 24 August 2015
Revised 24 August 2015
Accepted 25 August 2015

DISTINGUISHED SCHOLAR ESSAY

Reflections on the so-called value-free ideal

A call for responsible science in the business schools

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Abstract

Purpose – Value-free science is an ideal that is neither possible nor desirable, especially for social sciences. The subject of social sciences is individuals and groups; hence social, moral, ethical, or political values are inherent and unavoidable in all steps of the scientific process. Further, the authority of science requires the scientist to be responsible experts in ensuring the reliability of knowledge and in assessing the risks in applying the research findings in social policies and practices. The purpose of this essay is to discuss the role of values in business school research.

Design/methodology/approach – The author explains the two primary types of values relevant for science: epistemic – norms and standards to ensure good science – and social – criteria not relevant for discovering the truth of knowledge but may influence decisions related to science especially in evaluating the cost of wrongful conclusions from the research evidence. Based on an analysis of published criticisms of business school research and the author's own analysis, the author describes how business school research is infused with social and political values, undermining the objectivity and quality of science by business scientists.

Findings – The author endorses the idea of responsible science – science that recognizes the mutual dependence between science and society, and that aims to satisfy both epistemic and social values. The author offers a modest proposal to encourage transformation of business school research to meet both rigor (valid and reliable knowledge) and relevance (useful for practice) – the hallmark of responsible science.

Research limitations/implications – The ideas in this essay have implications for further work on identifying the relevant epistemic and social values to guide business school research.

Originality/value – The idea of responsible science can potentially transform business school's research to become both scientifically rigorous and societally relevant.

Keywords Good science, Epistemic values, Responsible science, Socially responsible research, Socially responsible science, Sound science, Value-free ideal

Paper type Viewpoint

One of the central debates in the philosophy of science is the concept of "value-free ideal" (Churchman, 1948; Douglas, 2009; Levi, 1960; Reichenbach, 1951). Its basic premise is that scientific work should be guided only by internal scientific values (e.g. reliability, validity, explanatory power) and should be free from the influence of social values (e.g. justice, ethics) that are not an inherent part of the scientific process. The scientist's role is to discover knowledge with objectivity, unbiased by any contextual factor. Scientists are accountable only to other scientists and not to people outside the scientific fields (Kaplan, 1964). Therefore, the isolation of scientists from worries and social ethos is desirable and necessary to maintain scientists' autonomy of

Cross Cultural & Strategic Management
Vol. 23 No. 1, 2016
pp. 4-28

2059-5794
DOI 10.1108/CCSM-08-2015-0101

inquiry and independence of thought. This isolation "permits the individual scientist to concentrate his attention upon problems that he has good reason to believe that he can solve" (Kuhn, 1962, p. 164). Kuhn considers this autonomy to be largely responsible for success in the natural sciences.

Opponents of value-free ideal, on the other hand, view science as a value-laden activity, since the major purpose of science is to address human needs (Dewey, 1927). Therefore, a full account of scientific knowledge could not exclude values. Merton's (1942) famous "ethos of science" points out that the proper functioning of the norms internal to science depends on the values in the broader society. Recognizing the uncertainty inherent in scientific reasoning and inference, Rudner (1953) argues that social and ethical values are essential in judging the sufficiency of evidence when looking to accept or reject a hypothesis and to consider the risk of wrongful conclusions. At a very basic level, there are simple ethical demands on the scientist such as honesty, openness and integrity. Science "cannot succeed unless result are honestly reported, unless every reasonable precaution be taken to avoid experimental error, unless evidence running counter one's own view is fairly handled, and so on" (McMullin, 1982, p. 7).

After several decades of debate, the general conclusion today among philosophers of science (although some still hold out, e.g. Lacey, 1999) is that the value-free ideal is not only impossible; it is also undesirable (Douglas, 2009). Value-free science is not possible because values show up in every step of the scientific process, from choosing which problem to study to offering advice to policy makers in government or in business. Further, the value-free ideal is not desirable because societies rely on scientists' theories and research evidence in developing technology, policies and regulations (Douglas, 2009; Hempel, 1965). The authority of science creates expectations for scientists as responsible experts and public servants who ensure that scientific knowledge is reliable to guide policies. Therefore, value-free science (i.e. without the influence of social or ethical values) is an illusion inconsistent with the reality of scientific work, given its interdependence with the social world.

What is the relevance of the value-free ideal to science in business schools? This question is important for two reasons. First, business school research falls within the domain of social sciences, having earned legitimacy as a scientific discipline after business schools adopted the natural science model, following the challenge raised in the Ford and Carnegie reports (Gordon and Howell, 1959; Khurana, 2007). Additionally, research in business schools draws heavily on ideas in the disciplines of economics, sociology, psychology, anthropology and mathematics. Second, similar to other sciences, business school research aims to understand and explain empirical puzzles in the world of management, business, and organizations. The relevance to practice is central to business school research (Cummings, 2007; Fisher, 2007; Rousseau, 2006; Rynes and Shapiro, 2005). As such, the question of value-free ideal in relation to research[1] within business schools is a legitimate and important one to reflect upon.

Hence, the major focus of this essay is the question "What is the nature of values and science in business schools?" My analysis of the current state of business school research in search of an answer to the above question suggests a paradox. Business school research observes (or at least appears to observe) epistemic values, but it is also laden with social (or non-epistemic) values. The paradox is that these non-epistemic values do not integrate research with the social world (i.e. businesses). Rather, they serve to isolate the two worlds. This has occurred because of the dominance of two internal stakeholders (journal editors and school leadership) in assessing scientific work. This evaluation process systematically excludes a third group, managers or

decision makers who are external stakeholders (Aguinis *et al.*, 2012) and who may be considered the "ultimate constituency" (Davis, 2015) for organizational research. Further, even the application of values internal to science is laden with values external to the research process. I will show that this internal focus and the infusion of non-epistemic values, especially political values, in the scientific process and reasoning are detrimental to the integrity of science in business schools. It leads to a proliferation of junk science (Huber, 1991) – science that fails to meet the minimum standard of competence or integrity – and, concurrently, it leads to the misuse of the intellectual talents in business schools. This essay will conclude with an appeal for business schools to adopt the idea of responsible science (Kourany, 2010, 2013; Tsui, 2013b, 2015) [2] to restore the integrity of science, to improve the usefulness of this science to external stakeholders, and to provide true autonomy of inquiry to scientists. Without this transformation, both business schools and society might be better served by transferring the research function from business schools to research institutions specifically devoted to true science and scholarship.

Two types of values with potential relevance for science

Values relevant for science can be broadly classified into two types (Douglas, 2009). The first are the epistemic (including cognitive) values – norms or criteria used in judging the sufficiency of theory and evidence. "Something is an epistemic value when it contributes to good science. Epistemic values are part of the norms and standards of good scientific reasoning." (Risjord, 2014, p. 18). The second are the values unrelated to the conduct of scientific activities. McMullin (1982) used the word non-epistemic to refer to all values that are not directly relevant for discovering the "truth" of knowledge. These non-epistemic values, which broadly includes social and ethical, may affect decisions at many points in the scientific process, especially when evaluating the practical or policy implications of the knowledge produced and, in judging the consequences of error in the theory or the evidence. Risjord (2014) refers to these as moral and political values. Social or moral values are desired conditions that relate to a society, groups of individuals or individuals. For example, wealth or health is a social value that would apply to a society, a group, or an individual. Ethical values are beliefs and standards about what we should do. For example, fairness or honesty is an ethical value in many societies. Social and ethical values are often related, although they are not identical. For example, the social values of justice or integrity may have an ethical dimension, but compassion or success may not involve an ethical element, at least not explicitly. Political values may reflect the preferences of specific groups or individuals, e.g., the funding agency specifying the type of research it would support an evaluation committee defining the type of journals it would consider acceptable, or the reviewer preferring one theory or methodology independent of the problem or the data.

Epistemic values are standards for evaluating the adequacy of the theory or the evidence in meeting the goal of science, i.e., whether the research study produces reliable and valid knowledge, which would move us closer to truth. Examples of epistemic values familiar to business school researchers include construct validity, internal validity, external validity, and predictive validity. Failure to meet these values reduces the reliability or accuracy of the knowledge inferred from research findings. Epistemic values are essentially the criteria for sound science. In general, the scientific community has agreed on the relevant epistemic values for evaluating theory or the reliability of evidence (McMullin, 1982). The value-free ideal says that the researcher's personal values or personal preferences should not enter into this evaluation. However,

it has been argued that scientific work, either theory or methodological choice, involves value judgments (McMullin, 1982), and that personal values are unavoidable in the application of epistemic values in scientific activities or evaluation (Brown, 2013b; Douglas, 2009; Risjord, 2014). Let us take construct validity for a simple illustration. Usually, we use a level of 0.80 (whether it is internal consistency, inter-rater agreement, or test-retest reliability) as indicative of an acceptable level of measurement reliability. However, we also agree that for new measures a lower level is acceptable. For some measures, we do not even need a reliability estimate (e.g. ROA as a measure of profitability). Since there is uncertainty about the true reliability of a measure, social value is present when a researcher decides whether 0.80, 0.70, or 0.60 indicates an acceptable level of reliability. We leave it to the scientists (as authors or as reviewers) to choose the level that they consider appropriate to the study. Thus the scientists' personal values play a role in this decision when some are willing to accept a lower α coefficient and others insist on a higher threshold. Similarly, theories and evidence with stronger internal validity or greater predictive validity generally are preferred over those with less. However, non-epistemic or social value also is involved when the judgment is affected by researchers' individual differences, such as when the judgment takes into consideration the importance of the research topics, the difficulty of data collection, or preference for a particular theory or data collection method.

Cognitive values involve even more uncertain standards than epistemic values, even though the demarcation between the two can be vague. Some cognitive values used to evaluate theories include simplicity (vs complexity), scope (narrow or broad), and explanatory power. Usually, a simple theory, especially if it is elegant (which certainly reflects the taste of researchers), is preferred over a complex one. Agency theory (Jensen and Meckling, 1976) is an example of a simple theory, while institution theory (DiMaggio and Powell, 1991; Scott, 1995) may be considered a complex theory. Broad or general theories may be preferred over narrow theories, although in the management discipline, many consider "midrange" theories to be most useful in studying organizations (Merton, 1968; Pinder and Moore, 2012). Theories that have strong explanatory power are preferred over those with limited explanatory power. However, some researchers may be willing to accept lower explanatory power if the idea is novel and elegant, while other researchers may eschew a model that explains only a few percentages of the variance in the outcome variable. There is a trade-off between simplicity, accuracy, and generality (Weick, 1999). A simple theory may not be accurate, as has been shown of agency theory (Ghoshal, 2005). A general theory may be neither simple nor accurate. Scientists vary in their preference for simple or complex theories, and this preference may influence the evaluation (i.e. review) of another scientist's theory choice in a research study.

Scientists have generally agreed that epistemic and cognitive values are necessary to uncover flaws in theories or inadequacies of evidence and to insure against wrongful inference. However, they also agree that social and ethical values more often than not influence the research process indirectly (and sometimes even directly) and in fact may be useful in the interpretation of the adequacy of theory or of evidence in making claims about the acceptability of scientific work and the potential use of generated knowledge.

Social values are values that are not internal to the work of science but nonetheless may enter into the mind of the researcher (either consciously or unconsciously) when considering the claim of reliable knowledge. Social values important to a society such as justice, freedom, social stability or human dignity often overlap with ethical values.

Desire to protect humans from harm when they participate in research, for instance, led to the creation of institutional review boards (Ippoliti, 2015) that put limits on a scientist's use of humans in a research study. In this way, a social value – protection of human wellbeing – directly enters into the scientific process. Because of the influence of social values, the research project may be delayed or the research design may have to be modified, which sometimes may compromise the research from satisfying epistemic values. Choice of a research method can be influenced by a social rather than epistemic value. For example, Kulik (2011) describes the growing popularity of the multilevel, multisource, and longitudinal research design in the management research community, a research design that creates immense challenges for the researcher. These challenges include the difficulty of meeting the institutional review board's approval for this complex research design, increasing demands on the organization to provide such complex data, and impacting the progress of organizational research. Preferring or using a complex design can reflect epistemic value (if it is necessary for the problem being studied), but also can be driven by the personal taste of the scientists (reflecting a value of sophistication or pride, or a desire to placate the reviewer). Where social or ethical values come into play most critically is in considering the consequences of errors in the inference made from the results, a problem known as inductive risk (Hempel, 1965). Because evidence is never complete or flawless to support a hypothesis with certainty, there is risk to either accepting or rejecting a hypothesis based on the available evidence, producing the classic Type I error (accepting a hypothesis to be true based on the evidence when in fact it is false) or Type II error (rejecting a hypothesis to be false when in fact it is true). Social values are needed to weight the consequence of each type of error.

A frequent example in discussions of inductive risk is the use of chemicals in products for consumer (e.g. food or drugs) or industry (e.g. construction materials or pesticides). A Type I error (false positive, inferring harm when the truth is no harm) would lead to unnecessary regulation by the government that has the good intention of protecting the public. A Type II error (false negative, inferring no harm when the truth is harm) would increase the risk to public health because lenient regulation would be implemented as (wrongly) suggested by the research evidence. The scientist has the responsibility of advising policy makers on the consequences of each type of error. Policy makers in government and decision makers in companies have to weigh the cost to the consumers and the cost of production, respectively. Consumers may prefer stricter standards of statistical significance (to reduce Type I error or false positives) while industry may prefer more lenient standards (to reduce Type II error or false negatives). Increasing sample size will increase the power of the test and will reduce Type II error. However, increasing sample size will increase the cost of research. Whose values should take priority in assessing the consequence of the inductive risk in this example: citizens, industries, or scientists? Clearly, this is not a question of epistemic but of social value.

The inductive risk idea is also pertinent in evaluating business research. An example is the application of insights from the agency theory (Jensen and Meckling, 1976) to the development of executive compensation in dealing with the agency problem. According to agency theory, there is goal conflict between hired managers and owners (of private firms or shareholders of listed firms). Further, it is difficult to observe the behavior of managers. Therefore, an incentive system that increases the managers' ownership (actual or psychological) of the firm ought to increase goal alignment. Goal alignment is observed when managers make decisions that are beneficial to the firm, hence to shareholders. A series of studies indeed show that equity ownership of managers relates to strategic and financial decisions consistent with

shareholder interests (Eisenhardt, 1989). Practically, this means that stock-based executive compensation would increase shareholder returns. Armed with this knowledge, in the years since 1976, corporate use of stock-based executive compensation has exploded. However, Martin (2011) has provided a disturbing report: during the period while CEO compensation ballooned, corporate performance declined. Specifically, for the 20 years before 1980, CEO compensation per dollar of net income earned for the 365 biggest publicly traded American companies fell by 33 percent. In the same period, real compound annual return on the S&P 500 was about 7.5 percent. In other words, before 1976, CEOs earned more for their shareholders for steadily less and less relative compensation. After 1976, CEO compensation per dollar of corporate net earnings doubled from 1980 to 1990 and quadrupled from 1990 to 2000. In the same period, the compounded real annual return on the S&P 500 was 6.5 percent. Martin (2011) concludes that the idea of maximizing shareholder wealth and associated executive compensation is the dumbest idea in the world (Denning, 2011). In other words, stock-based executive compensation, instead of curing the disease of managerial greed, makes the patient sicker, that is, greedier.

Clearly, both the theory (Jensen and Meckling, 1976) and early research evidence (Eisenhardt, 1989) using either cross-sectional design or limited time periods suggest a positive relationship between ownership-based compensation of managers and corporate performance. Since there is inherent uncertainty in this hypothesis (both in theory and in evidence), it is necessary to estimate the inductive risk before applying the knowledge to practice. A Type I error of this hypothesis would mean that there is no relationship. In fact, experience from actual practice over a long period of time indicates that there is a negative relationship. The research on agency theory fails to meet both the epistemic value (estimating the reliability of the knowledge) and social value (evaluating the consequences of inductive risk). The false positive error in agency theory research was very costly to shareholders. While this is only money lost, false positive in scientific research could have serious consequences for humanity, such as in the development of the atomic bomb. Scientists had to estimate inductive risk before the Trinity test (the first detonation of the atomic bomb). They were concerned about the possibility of an atomic chain reaction that might create a nuclear holocaust (Rhodes, 1986). Only after a careful evaluation, arriving at the conclusion that it was scientifically impossible, did the scientists proceed to test the atomic bomb. Similarly, an analysis of the inductive risk associated with the application of financial instruments might have prevented or at least reduced the magnitude of the world financial crisis in 2008 (Wolf, 2014). These examples show that scientists in both natural and social sciences have a moral responsibility to consider both positive and negative consequences of their scientific work (Douglas, 2009). To meet this responsibility, social value is inevitable and desirable in evaluating any theory (such as organizational design, leadership, strategy, incentive systems) and associated empirical evidence that may have actionable implications.

In summary, both epistemic and social values are necessary and important in the responsible pursuit of science and the cautious application of scientific knowledge. Table I summarizes the two types of values and the role of each in the scientific process. It shows that epistemic values have primarily a direct role. However, research may lead to revisions of epistemic or cognitive values over time, e.g., development of new statistical methods to estimate measurement reliability. Social values, however, may have both a direct role and an indirect role in guiding scientific work, in evaluating the accuracy of the claims of reliable knowledge, and in considering the consequences of wrongful inference.

Role of Value	Epistemic/cognitive values	Social/ethical values
Direct	Serve as criteria to evaluate the adequacy of scientific reasoning and evidence: Nature of theory: scope (simple vs complex), accuracy (weak vs strong), generality (narrow vs broad) Clarity and reasonableness of background assumptions of the theory Choice of methodology, data collection procedure, data interpretation Validity, reliability, sample representativeness, appropriate statistical procedure Statistical sensitivity and Type I and II errors	Serve as constraints or goals but do not interfere with the internal scientific reasoning: Choosing which projects to pursue or to fund, e.g., pressing problems in society, such as climate change, food insecurity, discrimination, job stress, aging Weighing the costs and benefits of research on new technologies, e.g., GMOs Deciding on the appropriate method or sample, e.g., use of animals or humans in testing drugs or chemicals Protecting humans in research studies when humans are the objects of the study
Indirect	Revision of epistemic and cognitive values or criteria based on evidence, theory, or values (Brown, 2013a)	Serve to fill in the gap of incomplete information about the inference (inference gap) and to evaluate the consequences of wrongful inference (inductive risk): Economic vs social costs (e.g. consequences of Type I and Type II errors, increasing sample size to reduce Type I error) Justice, safety, privacy (e.g. cost and benefit of inductive risk to different affected groups)

Table I.
Direct and indirect role of epistemic and social values in science

Values and business school research

The discussion so far is based on the assumption that science is the development and accumulation of reliable (true) knowledge with potential application to solving problems in the natural and social worlds. Scientists, due to their status as authorities of knowledge, have a moral responsibility to minimize error in their scientific work (through applying epistemic values rigorously) and to estimate the degree of inductive risk due to uncertainty associated with scientific reasoning and empirical evidence (through applying social values thoughtfully and responsibly). There is a general agreement that business schools have a common mission to develop knowledge about business organizations and to train business leaders and managers using the knowledge from high quality research. Research in business schools, by necessity, is subject to assessment by both epistemic values (does the research qualify as sound science) and social values (does the research produce useful knowledge). Below, I will offer an analysis of how values may appear at the four main phases of research in business schools, first, the choice of topics to study; second, evaluation of theories and research methods; third, the interpretation of empirical findings; and fourth, the choice of outlets to publish research results.

Choice of questions to study

From the beginning, the field of business and management has been dedicated to the study of organizations with the purpose of improving efficiency of production, gaining cooperation of employees, and contributing to both the industrial and social development of society (Barnard, 1938; Khurana, 2007; Dauten, 1958). The Academy of

Management was founded in 1936 to support and foster the research and educational mission of business schools. The founding editor of the *Academy of Management Journal (AMJ)*, Paul Dauten, Jr, specified the objectives of research in the editorial statement of the inaugural issue:

> […] the general objective of the Academy shall be therefore to foster a philosophy of management that will make possible the accomplishment of the economic and social objectives of an industrial society with increasing efficiency and effectiveness: the public's interests must be paramount in any such philosophy, but adequate consideration must be given to the legitimate interests of capital and labor […] (Editor, 1958, pp. 5-6).

Has research in management since 1958 lived up to this aspiration? Curious about this question, Walsh *et al.* (2003) analyzed 1,738 empirical articles published from 1958 to 2000 in the *AMJ*. They found that more than 70 percent of the articles focus on performance-oriented outcomes (efficiency, productivity, profitability, market value, innovation, etc.) and less than 30 percent on social outcomes (satisfaction, health, stress, and social responsibility). They reached this conclusion: "scholarship in our field has pursued society's economic objectives much more than it has its social ones" (859). Tsui and Jia (2013) updated and expanded this study by focusing on studies that involve Chinese samples in the period 1981-2012. Their reasoning was that management scholars in China, a socialist country, would be likely to value both social outcomes and economic outcomes of firms. They analyzed over 300 articles published in six leading English journals (*AMJ*, *ASQ*, *JAP*, *JIBS*, *OrgScience* and *SMJ*) and almost 3,000 articles in three leading Chinese language journals[3]. The results from this analysis are even more dramatic than those from the Walsh *et al.* (2003) study. Over 80 percent of the articles in the six English journals and over 90 percent in the Chinese journals focus on performance or economic outcomes. The trend lines are similar for studies at the individual, organizational, and societal levels. These results suggest that both Western and Chinese scientists value economic over social outcomes in their studies of organizations. This dominance of economic value has persisted over the years and shows no sign of abating. Management research has failed to achieve the noble objectives expressed by the founding editor of the *AMJ*, that research should address both economic and social objectives of an industrial society, with paramount attention to public interest, as well as adequate consideration of the legitimate interests of both capital and labor.

In recent years, journal editors have tried to shift attention to social outcomes. For example, in 2012, *AMR* published a special forum on the topic of care and compassion in organizations (Rynes *et al.*, 2012), and in 2014 *AMJ* editors called for research on the topics of aging and management (Kulik *et al.*, 2014), climate change (Howard-Grenville *et al.*, 2014), organizations with purpose (Hollensbe *et al.*, 2014), and grand challenges in management (Academy of Management Journal, 2015). *Management and Organization Review* calls for research on humanistic management (Tsui and Jia, 2013) and attention on Africa as a context for expanding management research (George, 2015). Tsui (2013a) encourages compassion in organization and management research. The concern for studying problems important to society is present in many other sciences and professions. US President Barak Obama has called on research universities, foundations, philanthropists, and companies to join him in addressing the grand challenges in the areas of energy, health, education, environment, national security, and global development. The US National Academy of Engineering (NAE) has identified 14 grand challenges (National Academy of Engineering, 2013), such as providing

access to clean water, preventing nuclear terror, and securing the cyberspace. It encourages engineering research to make the world a better place to live in through research in these challenge areas. In other words, both published work and calls for new research reveal the social values of the scientific communities as well as policy makers in government, business, and civil societies. Using these values to guide the selection of topics to study is permissible so long as the topics meet the needs of society (Kourany, 2010, 2013). Unfortunately, currently most researchers in organizational science parse the texts of research literature to look for "theoretical gaps" to fill (Alvesson and Sandberg, 2011; Davis, 2015; Suddaby, 2014) instead of exploring new research areas in the real world. Using the existing literature as the source of ideas for new research perpetuates the research focus of existing literature, accounting for the continuing domination of a focus on economic outcomes and contributing to incremental rather than ground-breaking research. However, studying new topics can be risky for beginning (or even experienced) scientists (Tsui, 2013b). Insecurity and maintaining the status quo (hence perpetuating the current paradigm) appear to be the main non-epistemic values guiding the decision of what topics to study by scientists in business schools.

Evaluation of theories and research methods

An interesting development in management research is the increasing emphasis on theory (Hambrick, 2007; Suddaby, 2014) with the purported aim to improve the contribution of research to knowledge. Contribution to theory becomes an important epistemic (in reality a social) value in evaluating the acceptability of a research paper. Curious about whether and how articles in *AMJ* contribute to theory, Colquitt and Zapata-Phelan (2007) analyzed a total of 667 articles published in the 45 years from 1963 to 2007. They coded these articles on a five-point scale in terms of the article's contribution to theory testing or theory building. They found most articles in the earlier 27 years (1963-1989, before 1990) to be primarily reporters (description of phenomena with no theory) or theory builders (development of new theories either inductively or deductively). Articles in the recent 18 years (1990-2007) were mostly testers, expanders, qualifiers and a small number of builders. There were no reporters. In fact, most of the articles after 2000 were theory elaboration studies.

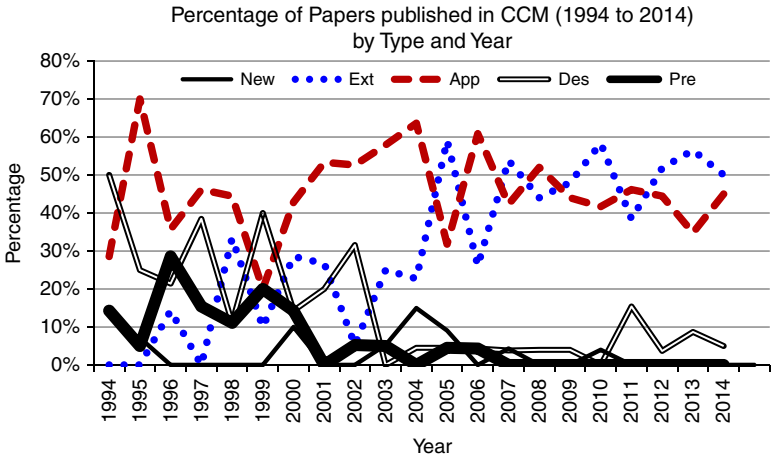
This pattern is not surprising since *AMJ* is a theory-focussed journal. Other journals, especially those that focus on phenomenon-based research (addressing empirical puzzles) might not have this strong theory orientation. However, *AMJ* leads the field. Most management researchers read *AMJ* and aspire for their research to meet the standards of *AMJ* papers. Most of the *AMJ* reviewers also review for other journals. For better or worse, the researchers (either as authors or reviewers) carry the theory focus to other journals. I sought some evidence on this conjecture. I analyzed the articles in *Cross-Cultural Strategic Management* (*CCSM*) for a comparative analysis. The mission statement of *CCSM*, from its founding year of 1994 through 2014, emphasizes addressing cross-cultural management issues. The statement does not even mention the word theory[4]. I expected much less theory focus in *CCSM* articles in these 21 years, unless, as I speculated, both authors and reviewers are influenced by the normative (non-epistemic) expectation of theory contribution.

CCSM published 441 articles from 1994 to 2014. Two students independently coded the articles into five categories: descriptive (without theory), theory application (use of existing theory to explain the problem being studied), theory building

(propose a new theory), theory extension (improving the theory as main goal by adding mediators or moderators, or testing boundary conditions), and prescriptive papers (not scientific papers since these do not contribute to new knowledge). The inter-coder agreement exceeded 0.80 on all the categories. I expected few *CCSM* papers would be theory building or extension given its mission of focusing on addressing substantive issues rather than on theoretical contribution. The results are in Figure 1.

The results show that in the earlier years (from 1994 to 2002), descriptive and theory application papers had the lion's share, along with some theory building and prescriptive papers. This pattern seems consistent with *CCSM*'s pre-2015 mission. However, prescriptive papers essentially disappeared after 2001. In the last decade (2005-2014), there was a sharp increase in the theory extension research, from around 20 percent before 2004 to around 50 percent beginning in 2005. In other words, in recent years, even an applied journal like *CCSM* values "contribution to theory" papers. This emphasis on theory is formalized and highlighted in the revised mission statement for this journal[5].

Hambrick (2007) discussed the problem of too much emphasis on theory in management research, relative to the natural sciences or even other disciplines in the business schools (e.g. accounting, finance, and marketing). In science, theories are developed or used to explain and understand empirical puzzles. Theories in management, however, have become greatly divorced from empirical puzzles, contributing to this "research-practice gap" that has been discussed for many years by many scholars (Cummings, 2007; Hambrick, 1994; Pfeffer, 2014; Rynes, 2007; Walsh, 2011). Many scholars have tried to understand why management research has reified theory as the quintessential focus of research and why it is so divorced from phenomena in the world of business and management. Birkinshaw *et al.* (2014) and Suddaby (2014) cautioned us to distinguish good theory from "fetishistic theory" which refers to "theory that has become ritualized, mechanical and artificial, or theory that



Notes: New, new theory building; Ext, extension of existing theories; App, application of existing theories; Des, descriptive studies with no theory; Pre, prescriptive studies with or without theory

Figure 1.
Types of papers published in *Cross-Cultural Management* 1994-2014

has, largely, been drawn from parsing texts rather than experience," and that this ritualization and rationalization of theory is "largely in response to the increasing career pressures to publish" (448). Fetishistic theory requires theorists to identify and fill increasingly narrow gaps in existing theories and thus becomes increasingly distant from practice. Suddaby continues with the following thought:

> As theories become more fetishistic, they also become more "scientistic". That is, they adopt the appearance – the form, language, and presentation of science – without the functional contribution of new knowledge. Scientism, thus, applies the formal language of theory and method to explain the obvious or to re-characterize existing knowledge […] applying a more sophisticated method or an embellished theoretical framing to solve an old issue, becomes more important than the production of new knowledge or a fresh insight (2014, p. 449).

Beyond theory fetishism and scientism, there also are concerns of questionable research practices, ranging from falsification and fabrication to selective reporting of findings (cherry picking), or *post hoc* hypothesizing (Bedeian *et al.*, 2010; Tsui, 2013a; 2015). Although there are debates on how serious the problem of misconduct is in science (Fanelli, 2009), studies have reported occurrences that range from trivial to alarming. A meta-analysis of 18 studies on misconduct in the natural sciences found a pooled weighted average of 2 percent ($n = 7$, 95 percent, CI: 0.86-4.45) of scientists admitting to have fabricated, falsified or modified data or results at least once. Up to 33.7 percent admitted other questionable research practices. Because this is self-reported, the results may be an under-estimation of real instances. Bedeian *et al.* (2010) surveyed 384 tenured and non-tenured faculty in business schools and asked if they had observed colleagues engaging in falsification, fabrication, and plagiarism (FFP), as well as a number of other questionable practices. A majority (27-79 percent, depending on the specific practice in the FFP category) reported that they had witnessed these misconducts. The percentages reach 92 percent on other questionable practices. It is possible that researchers might over-report colleagues committing these practices. However, based on social expectations, scientists should have the motivation to protect the reputation of their profession and minimize reporting of misconduct (Fanelli, 2009). The truth may lie somewhere between self-report and other report.

There are many ways that authors can write articles full of intriguing results that are in fact false or misleading, oblivious of the serious conflict of interest between publishable results and accurate results (Nosek *et al.*, 2012). Davis (2015) agrees with these observations, "There is an emerging consensus in some quarters that the system of journals and academic career incentives often favors novelty over truth in publications, that individual academic researchers are often rewarded for being interesting rather than getting it right, leading to systematic biases in the published record" (181).

Fetish theories have the form of theory, but they are not directly connected to any empirical phenomenon. Scientism encourages technicality, favoring fashionable modeling approaches, fancy statistical procedures, or new and clever ideas regardless of their substantive importance or advancement of understanding (Birkinshaw *et al.*, 2014; Pfeffer, 2014). These research practices reflect values of expediency, instrumentality, faddism, personal preferences, (job or career) insecurity, or careerism. These values are neither epistemic nor social or ethical but are certainly dysfunctional by turning potentially good science into junk science.

The problems of theory fetishism and questionable research practices should be detectable during the review process of the scientific work. Unfortunately, the review process also is fraught with problems (Bedeian, 2003, 2004; Starbuck, 2003; Tsui and

Hollenbeck, 2009). The most serious and prevalent problem is the domination of personal preferences (a social value) over scientific criteria (epistemic values) in the evaluation of manuscripts. Bedeian (2003) surveyed 178 authors of the two leading journals in management, *AMJ* and *AMR*, about their experiences with the review process. He reports "Among the survey's more disturbing findings, more than one third of the responding authors reported that recommended revisions in their manuscripts were based on an editor's or referee's personal preferences, and almost 25% indicated that in revising their manuscripts they had actually made changes they felt were incorrect" (2003: 331). Starbuck (2003) offers evidence to show that "editorial decisions incorporate bias and randomness" (344). In essence, non-epistemic values are rampant in the evaluation of scientific work (during the manuscript review process), leading to randomness in which papers get published (Starbuck, 2005), threatening the integrity of science and the production of valid knowledge.

Interpretation and presentation of empirical findings

Epistemic values support the detection of flaws in an empirical study and ensure the degree of uncertainty about the evidence is within acceptable limits. Replications and reporting of both positive and negative results are important epistemic (and social) values. However, in many fields, there is a strong preference for hypotheses confirming results. Non-significant results, results inconsistent with the hypotheses or with prior findings, and, especially, anomalies are consistently unreported. This confirmation bias is ubiquitous in psychological research (Nickerson, 1998; Nosek, *et al.*, 2012), and it also pervades the management literature (Davis, 2015; Leung, 2011). This selective reporting of hypotheses, cherry picking results, and formulating hypotheses based on the results are inconsistent with both the scientific principles of falsification and replication, and the social expectations of honesty and integrity. Reviewers, unfortunately, instead of detecting such practices, may even coach or insist authors to remove unsupported hypotheses during the review process. As a result, published work is potentially infested with false positive conclusions. This confirmation bias in part reflects the domination of the normal science paradigm (Kuhn, 1996), which encourages scientists to work "in the neighborhood of existing theories and paradigms" (Barkema *et al.*, 2015, p. 465), with anomalies or surprising results systematically suppressed or ignored. While epistemic values serve to ensure rigor in methods and reliability of the evidence, social values condone the prevalence of Type I errors (associated with confirmation bias, selective reporting, or post hoc hypothesizing). These conditions suggest that authors may be influenced by non-epistemic values of insecurity and careerism while reviewers may be influenced by personal preference (for certainty theories or methods) and power (punishing authors with a rejection recommendation for non-compliance to their directives for change).

Articles in management or business journals rarely discuss the problem of inductive risk associated with Type I and II errors. Doctoral students learn about these ideas in their statistics or research methods courses. Then, they essentially forget about them because these terms rarely appear in journal articles. As discussed above, there is no expectation that research in business schools will be applied to practice. The goal of business school research is publication. This lack of attention to the consequences of research (in terms of both the quality and the usefulness of knowledge) is both a cause and a result of the inward focus. According to the value-free ideal, this isolation from the social concerns of society should free scientists to concentrate their attention on the problems that they want to solve

(Kuhn, 1962). This independence has led to successes in the natural sciences. Why is business school research not enjoying this same success? The answer is obvious. Science in business schools is highly value-laden, and scientists do not have independence in their scientific inquiries.

Paradoxically perhaps, many ideas from business school research do appear in textbooks and are taught to students in all levels from undergraduates to MBAs to executives. Textbook authors (most of whom are accomplished scholars) select the scientific ideas or results based on their perceived relevance for practice and contribution to a body of knowledge. But since high quality journals are not a guarantee of the quality of articles, textbooks may consist of both good and bad theories or results with unknown inductive risk. Two examples will illustrate this problem. First, most business schools today still teach their students that managers' responsibility is to maximize shareholder values and managers must be incentivized to focus on this goal. As discussed above, the influence of agency theory on executive compensation is undeniable, but with dismal results in terms of increasing return to shareholders (Martin, 2011). Ghoshal (2005) explains in detail why agency theory in fact is bad for management practice. A second example of a management idea that appears in most strategy textbooks and that received enthusiastic support from managers is Porter's (1985, 2008) theory of competitive strategy. Porter earned the reputation that he was to strategy as "Aristotle was to metaphysics" (Denning, 2012). His consulting firm The Monitor Group enjoyed exceptional success from its founding in 1983 until it filed bankruptcy in November 2012. Even before its bankruptcy, critics (Stewart, 2009) suggested that Porter's idea was lacking in both fact and logic. In Denning's (2012) words "there was no evidence that sustainable competitive advantage could be created in advance by studying the structure of an industry."

A commonality of these two examples is the lack of an evaluation of "inductive risk" in both theory and evidence[6]. In fact, most management theories that appear in our journals on any topic (e.g. leadership, job design, organizational development, reward and punishment systems) were not subject to assessments of consequences associated with inductive risk. A consequence of wrongful inference in business research is a lot of wasted money and negative spill over on employees and society. The agency theory and the competitive strategy theory are examples of management theories that would benefit from a risk assessment informed by social or ethical values. The following statement reflects the desirable role of social values in addition to epistemic values in judging a management theory. Denning said Porter's strategy framework:

> […] isn't just an epistemological black hole; in its essence, it's antisocial, because it preserves excess profits, and it's bad for business, because it doesn't work. It accomplishes the unlikely feat of goading business leaders to do wrong both to their shareholders and to their fellow human beings (Denning, 2012).

Clearly, society as a consumer of scientific knowledge has the right to complain about unproven ideas, and scientists have a moral responsibility to get it right (Davis, 2015; Douglas, 2009; Kourany, 2013).

Reporting of findings

The role of non-epistemic values is most evident and deterministic at this final stage of the scientific process – the reporting and dissemination of research findings. The research is shown in public forums, orally or in writing, and is necessary for knowledge sharing and accumulation. There are a variety of venues for this dissemination, including conferences, workshops, books, journals articles, and research monographs.

Books and research reports are appropriate venues for large projects. Journals are suitable for self-contained ideas or studies that can be described in sufficient details within a limited number of pages. All works that appear in these public forums have already gone through scrutiny, and are now available for public debate and discussion. Published works are subject to further reviews and commentaries, and this is a healthy exchange that advances future work on the topic. Normally, individual scientists can choose the venue that is most appropriate for their ideas to reach the right audience and to maximize exposure of the new ideas or discoveries. In earlier years in the history of business schools, most research findings were published in books, possibly due to the lack of journals as well as the large-scale nature of research projects (Barkema *et al.*, 2015; Smith and Hitt, 2005). In the recent past twenty years, however, journal articles have become most valued (Adler and Harzing, 2009). Business schools even specify the "list" of journals where their faculty should publish their research. The list usually consists of high status (A) journals, ranked by their impact factors (number of citations to the articles published in the journal in a year by articles in the other journals over the following two years). Books, book chapters and articles in lower tier journals are eschewed, scorned or even punished (Tsui, 2013b; Walsh, 2011). What are the values that drive business schools (or other disciplines across the university) to adopt this publication policy, constraining scientists' autonomy in choosing the appropriate venue to share their work, and specifically to shy away from writing books or more applied outlets?

The answer is the commercialization of the business school mission. An industry of ratings and rankings of business schools emerged in recent years (Walsh, 2011). The Bloomberg *Businessweek* MBA ranking started in 1988. The *Financial Times* global MBA program ranking began in 1999. Beyond these commercial rankings, scholars are also eager to measure research productivity as an indicator of scholarly success. There is an abundance of articles comparing the research productivity of schools and individual scholars (Azar and Brock, 2008; Certo *et al.*, 2010; Podsakoff *et al.*, 2008). Science in business schools has become an annual pageant show with multiple judges rating schools on teaching and research, using criteria that are observable and countable to achieve some degree (or semblance) of objectivity. Prestige or fame of individual scientists and the reputation of the schools are defined by the number of A level journal publications, regardless of the actual content of the research or the actual quality of the papers that appears in these A level journals (Macdonald and Kam, 2007). Nosek *et al.* observed, "To the extent that publishing itself is rewarded, then it is in scientists' personal interests to publish, regardless of whether the published findings are true" (2012, p. 616). Rewarding scientists for "impact" (citation counts) and "productivity" (article counts) is a perverse incentive system leading to many questionable research practices including the inappropriate manipulation of peer reviews (Moylan, 2015).

There is a strong assumption that all the articles in the high status A journals are of high quality. However, many scholars (Rousseeuw, 1991; Starbuck, 2005) point out that this is a fallacy. Based on the disagreement between reviewers on their evaluation of manuscripts (between $r = 0.08$ and 0.45, Table 2, Starbuck, 2005) and the low correlation between reviews and the later citations of the paper ($r = 0.24$, Gottfredson, 1978), the true value of a manuscript is uncertain. This randomness means that top tier journals publish many low quality papers, and some excellent papers get published in lower tier journals. The famous Peters and Ceci study (1982) proved the point when eight of nine published papers[7]were rejected when they were resubmitted to the same journals that had published them 18-24 months earlier. The authors used fictitious names and affiliations and made cosmetic changes to the

title, abstract and opening paragraph. This evidence indicates that there is hardly a relationship between the quality of the journal and the quality of any paper in it. The focus on top tier journals pervades all sciences, leading to the San Francisco declaration (Cagan, 2013) signed by 155 editors and publishers of scholarly journals in a variety of disciplines representing 82 organizations worldwide at the December 2012 meeting of the American Society for Cell Biology[8]. This declaration decries and calls for a halt to the practice of using publications in high impact factor journals as a measure of a scientist's contributions.

Despite these warnings, business schools worldwide are continuing the practice of using this single criterion in hiring, promotion and tenure decisions. This counting method of evaluation essentially eliminates the intellectual autonomy of scientists. Researchers focus on writing for these journals, perusing them for ideas to study, following the dominant theories and research methods, avoiding new topics that might be risky and focusing on filling the theoretical gaps in the literature. It is a vicious cycle that begins and ends with the journal list. The values that dominate this final stage of the research process are tied primarily to the school's aspiration to rise in reputation and the scientists' desire for career success. The consequence is damage to the integrity and usefulness of science since such research fails to meet both the epistemic (reliable knowledge) and social values (beneficial knowledge) of scholarship.

A call for responsible science

Kourany (2010) introduced the idea of socially responsible science that involves a joint consideration and satisfaction of both epistemic and social values. Socially responsible science recognizes the mutual dependence between science and society. Tsui (2013b, 2015) uses the term socially responsible scholarship to encourage transformation of business school research to meet the criteria of both rigor (valid and reliable knowledge) and relevance (useful for practice). How should we move forward with responsible science in the business schools?

One encouraging development is the proliferation of "Code of ethical conducts" in many scientific disciplines (Brown, 2013a). These codes are designed to ensure the application of epistemic values and the appropriate use of social values in every stage of the scientific process. Many such international codes of ethics already exist in the fields of biomedical research, medical sciences, sociology, archaeology, and psychology. In the management discipline, scholarly associations like the Academy of Management, the Academy of International Business, and the International Association for Chinese Management Research have similar codes focusing broadly on professional conduct of their members. However, these codes tend to be general. We need codes specific to the scientific process to guide scientific behavior and to support "the autonomy of science, the accountability of science, and the public's trust in science" (Kourany, 2013, p. 96). To encourage integrity in scientific conduct, *Management and Organization Review* devoted an entire issue to "Research and Publication Ethics" (Tsui and Galaskiewicz, 2011) with twelve articles or essays. This is social value of a scientific community at work.

Moving forward with responsible science for business schools, I offer a modest proposal. I suggest the community of scholars in the business school disciplines jointly develop a list of epistemic, cognitive, social, and ethical values that are relevant in the pursuit of science. I suggest defining the values appropriate at each stage of the scientific process, and clarifying the role of each type of values, either directly or indirectly. Below I share some preliminary thoughts on this value clarification exercise for each of the main four stages of a research project.

Choosing topics to study

At this first stage, social values should play a direct role. This ensures that scientific resources and talents are applied to problems that are important to society, as defined by the external constituency of schools or of disciplines. I have mentioned earlier that the NAE has identified fourteen grand challenges and that *AMJ* has called for research on the topics of aging, climate changes, and purpose. Recently, the *Journal of Service Research* developed five categories of research priorities through a multi-stage process of polling the opinions of experts in the field of service research, in marketing, operations, and management (Ostrom *et al.*, 2015). Learning from these experiences, the professional association for each discipline (finance, accounting, marketing, operations, and management) can define the 'grand challenges' in the world of practice. This could be a biennial exercise to identify new emerging issues. Each school can further choose a sub set of these grand challenges that would support the mission of the school or the mission of the university. For example, the Engineering College at the University of Notre Dame is one of 122 engineering schools in the USA that has pledged to train a minimum of 20 "Grand Challenge Scholar Engineers" each year to specially prepare them to lead the way in solving large-scale engineering problems. At the same time, the research program of the College also aims to directly contribute to solving some of these grand challenges. These strategies are designed to support the mission of the College of Engineering with an aspiration of contributing to the "the greatest good for society" (https://engineering.nd.edu/about/visionmission). Likewise, business schools can aspire to contribute to a better world by solving the grand challenges of the business world globally. I suggest a few important challenges that I have observed and state them in the form of research questions. How are organizations addressing issues such as global warming, resource depletion, and poverty, social justice, or corporate greed and corruption? How and why are some corporations contributing to while others are extracting from the common good? How do society (employees, customers, suppliers, government) respond to corporations or businesses that contribute negatively rather than positively to society? There are many challenging and important questions that we can and should study in the global context. Many emerging economies, including Africa, China, India, are experiencing exciting growth and development. Tsui and Jia (2013) encouraged more research on social outcomes of management. George (2015) focuses on Africa and encouraged studies on the interplay between foreign aid and local entrepreneurship, the effect of colonial imprinting on foreign investment patterns and international business development, and the role of business entrants in stabilizing conflicts in local communities? Other editors have called for more research on organizational justice, business ethics, and corporate social responsibility (Rupp *et al.*, 2015). These are but a few of the many important problems in both local and international arena that await systematic analysis. The social values of economic success, social development, and the common good are certainly appropriate and desirable in guiding the choice of research topics by business scientists.

Conducting the research

At this stage, epistemic values play a direct role while some social values (e.g. protection of research participants) may play an indirect role. The community of scholars can review and revisit the epistemic and cognitive values currently in use and determine any necessary revision. Existing codes of research ethics should be strengthened, and the training of doctoral students should include a course or more on the philosophy of science and values in science. Scientists should be aware of their personal

preferences at each stage of the research process, and understand how these values might interfere with epistemic decisions along the way. Researchers should pay greater attention to the problem of inductive risk, identifying the consequences associated with Type I and II errors in the specific study, providing insight on the extent and nature of the risk in the inference. I suggest the "limitations" section of a manuscript to include this risk analysis if appropriate depending on the research problem being studied, as well as an explicit statement of the evocation of any social values by the researcher either directly or indirectly through each stage of inquiry. This risk analysis must precede any discussion of managerial or policy implications. Both competence (epistemic values) and integrity (social values) contribute to sound science and responsible actions.

Publishing the findings

The best judge of how and where to disseminate the important research findings should be the scientists themselves. Larger sized projects can be reported in their totality in the form of books or monographs. Smaller sized projects can be summarized in journal length articles[9]. Scientists aspire to publish in the most prestigious journals for a wide exposure, but the choice should instead be based on the domain of the journal and its readership. Further, scientists should recognize that scientific achievements (e.g. the Nobel prize) are based on the importance of the ideas and not where the work is published. Aguinis and colleagues (2014) explain how number of articles and citations of a scientist's work in other scholarly journals are extremely deficient measures of the scientific contribution. As Walsh (2011) states, scholarly contributions should be based on "a body of work that might be revealed in books, articles, book chapters, simulations, cases, and, who knows, maybe even movies and more. The key is to appreciate the import of the questions asked and the quality of the answers offered" (227).

Many scholars have identified, as I have attempted to do in this essay, the unintended and undesirable consequences of forcing scientists to publish their research in a pre-defined list of journals; the consequences take the form of both unreliable knowledge and limited social utility. I encourage the community of scholars to clarify scientists' responsibility in both knowledge production and knowledge application, as has been done in other fields such as medicine, law, and engineering. Scholars should seek to publish their work in the most appropriate format or venue. At this stage on the dissemination of scientific discoveries, similar to the first stage of topic selection, social values should appropriately dominate. Which venue would provide the most appropriate space and audience for the sharing of discoveries on a timely basis?

Evaluating the research

Peer review is an integral part of the scientific process (Merton and Zuckerman, 1971/1973). It is a necessary stage to evaluate the quality of scientific work and the credibility of findings before the work becomes part of the public record. Peer reviewers should have similar if not higher research qualifications than the authors whose work is being assessed (Bedeian *et al.*, 2009). If a reviewer is to assess a specific aspect of the manuscript due to his or her expertise, the reviewer should disclose this expertise. If a reviewer is using social values in making a judgment, this should be disclosed, just as the authors of the manuscript do. Given that reviewers have different expertise to bring to the evaluation and are judging different aspects of the research, disagreement is to

be expected. It is rare for a piece of research to be excellent in both theory and methods, while at the same time also important in both the research question and the policy implications. Since all research has some flaws, both epistemic (e.g. validity) and non-epistemic (e.g. novelty) values might be helpfully applied, to provide an overall assessment of the research's acceptability to be made available in the public domain. However, there is no clear epistemic criterion for determining overall acceptability. Since reviewers subjectively use different epistemic or social values when assessing research, it would be desirable for the reviewer to articulate the basis of the overall evaluation to help guide the editor's publication decision, a decision which is also ultimately driven by a combination of epistemic and non-epistemic values.

Reviewers can make either Type I error or Type II error also. Junior reviewers tend to be conservative (to prove that they hold high standards) and tend to make a Type II error. Some reviewers are not open to perspectives different from their own (Starbuck, 2003), also resulting in a Type II error. Different reviewers may use different non-epistemic values in their evaluations, leading to potentially random reviews. Editors also have personal preferences; and some like to follow the reviewers' recommendations while others do not, contributing more randomness to editorial decisions (Starbuck, 2005). Therefore, I favor editors also disclosing the values or criteria that influence the final decision, be they epistemic, social, or both. Such transparency will contribute to less randomness in editorial outcomes (Aguinis and Vaschetto, 2011; Clark and Wright, 2007).

The above observations suggest that reviewer training on the role of epistemic and social values in the evaluation of scientific work is critically important. Currently, the focus of reviewer guidelines is on social values primarily, as the reviewer guidelines for the journal *Academy of Management Discoveries* (and common in most other journals, e.g. Lewin, 2014; Hempel, 2014) show: be constructive, focus on the core issues, be concise and specific, be polite and conversational, do not be two-faced, be sensitive, and be punctual (Miller and Van de Ven, 2015). I encourage future reviewer training to include the appropriate use of epistemic and social values in the judgment of another scientist's work. I also encourage reconsidering the role of doctoral students in formal reviews. Their training in progress, lack of research experience, and desire to prove their worth may all result in a high likelihood of both Type I and II error, which detracts from a fair evaluation of scientific work. This training along with practice reviewing should be provided under the guidance of experienced scientists/reviewers (Bedeian, 2004; Tsui and Hollenbeck, 2009).

Conclusion

It is beyond question that science has contributed greatly to progress in civilizations and to the quality of life for humanity in general. We grow food in greater abundance and variety, cure many deadly diseases, build more comfortable homes, engineer faster planes and trains, access information on any subject in an instance and in abundance through the Internet, design mobile devices that link us to others in any corner of the earth, and connect people instantly through ever expanding social media. However, science also has incidentally brought about destructive forces including deteriorating air and water quality, global warming, toxic wastes, diseases caused by unhealthy food and harmful chemicals, economic crises, and so on. Science-and-technology-based economic growth has greatly increased inequality in both developed and developing regions (Kourany, 2013; Piketty, 2014). These developments mean that it is even more important for science to be guided by social values so that we can benefit from the

positive contributions of science while avoiding or minimizing its negative consequences. Social values are both necessary and desirable in ensuring that science and scientific communities contribute to the thriving if not the survival of humanity.

Science and the seeking of truth is a universal activity. Criteria of sound science, i.e., epistemic values, should transcend cultural and national boundaries. Social values relevant for science, such as the protection of humans during the research process, integrity when conducting the study and honesty in reporting the research finding, should be universally endorsed as well. Local contexts may suggest differences in what problems are considered important and what social values may take priority. However, the scientific and social responsibility of scientists should remain invariant across space and time. Responsible science is at the service of society by applying rigorously and objectively the relevant epistemic and social values in the choice, execution, evaluation, and dissemination of results of scientific studies.

The authoritative and privileged status of scientists implies a corresponding moral responsibility to ensure that their advice is based on sound evidence and that the knowledge they produce is used to advance and not to thwart human progress. All the problems that we have observed and documented about the misconduct of scientists, the pursuit of self-interest and the proliferation of useless research will disperse when the scientific community embraces and enforces responsible science or socially responsible scholarship. Science cannot be so-called value free, but it should be free from the dysfunctional values that this essay has documented. Clarity on the nature and role of both epistemic and social values will restore the integrity of science in business schools.

Notes

1. I use the words "science" and "research" interchangeably. Similarly, the terms "scientists" and "researchers" are inter-changeable.

2. Kourany (2010, 2013) uses the term "socially responsible science" and Tsui (2013, 2015) uses the term "socially responsible scholarship". The word "social" implies both social values and social implications of scientific work and scientific knowledge. However, some may misunderstand this word "social" to mean research on "social responsibility topics" (e.g., justice, poverty, environment topics). Hence, I use the term "responsible science" to avoid this misunderstanding. Responsible science refers to scientists' responsibility to ensure both sound science and positive impact of their work on society. I thank Paul Adler, President of the Academy of Management, and Jonas Haertle, Head of PRME secretariat, for pointing out this potential confusion and suggesting the deletion of the word "social".

3. The six English journals are the *Academy of Management Journal (AMJ), Administrative Science Quarterly (ASQ), Journal of Applied Psychology (JAP), Journal of International Business Studies (JIBS), Organization Science (OrgScience), and Strategic Management Journal (SMJ)*. The three Chinese language journals are *Management World* (管理世界), *Nankai Business Review* (南开管理评论), and *China Industrial Economics* (中国工业经济).

4. The mission statement of *CCM* as stated on the journal website prior to 2015 was this: "*Cross Cultural Management* seeks to be the leading source of research on multicultural management issues. The journal addresses cross cultural management from all management angles. The journal welcomes contributions addressing intracultural, intercultural and transcultural management issues." Beginning 2016, *CCM* will change its name to *Cross-Cultural and Strategic Management*. The journal's mission also has changed to include theory as an important condition for submission. The revised domain mission as it

appears on the 2015 website begins with this statement: "The goal of *CCSM* is to publish discerning, theoretically grounded, evidence-based and cutting edge research on issues relevant to all aspects of global management. *CCSM* is especially interested in theoretical and empirical papers that investigate new and unique ideas and/or are multilevel (micro-meso-macro) and/or are multidisciplinary in nature." This change is consistent with the overall trend of emphasis on theory in management research.

5. In a personal communication, the new Editor in Chief of the retitled *Cross-Cultural and Strategic Management* journal explained the motivation for emphasizing theory requirement in the revised mission statement. She said that, "the reward/incentive system is such that publication in highly ranked journals with high impact factor is valued. The reality is that atheoretical papers have no room in many high impact journals."
6. It is not my intention to be critical of these scholars who have offered innovative ideas to the field. I use them to illustrate the need for assessing inductive risk before applying the ideas to practice.
7. They initially sent out 12, but three were detected that they were published papers.
8. The location is symbolic as San Francisco was where the United Nations charter was signed on June 26, 1945 and effective on October 24, 1945.
9. Large and small is defined in terms of scope, not in terms of significance.

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