# The management academia: A naked carnival

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**Abstract**

*This paper describes the current state of the management academia as a naked carnival, namely, most of the management researches have no such a clothes called practical relevance. It is intended to provide an explanation why management research has become irrelevant to the real management practice. It argues there are three factors behind the irrelevance problem: first, the 'scientific model' of management studies generates an initial and internal force which pushes the management research away from practice management studies supposed to serve; second, paradigm maintenance effort of the mainstream management scholars prevents the irrelevant management academia moving back towards management practice; third, the surrounding environment provides the management academia anything but a strong counter force to change the irrelevance reality. This paper also argues any solutions under the 'scientific model' are doomed to failure; and the only way out is to completely abandon the 'scientific model' and adopt a 'professional model' of management studies. Unfortunately, this paper argues such a radical change from within is highly unlikely to happen.*

**Keywords**: management research, relevance, practice, scientific model, professional model

## 1. The problem of irrelevance of management research

### *1.1 The criticism of irrelevance*

Many management scholars have pointed out that there has been a disconnection between management research and management practice and called for making management research more relevant and practical[1] (e.g., AACSB[2], 1996; Anderson, Herriot, and Hodgkinson, 2001; Beer, 2001; Bennis and O'Toole, 2005 ; Bettis, 1991; Buckey et al., 1998; Chia and Holt, 2008; Copinath and Hoffman, 1995; Daft and Lewin, 1990; Hambrick, 1994; Jarzabkowski, 2003, 2005; Johns, 1993; Kondrat, 1992; Løwendahl and Revang, 1998; Mowday, 1997b; Nicolai, 2004; Nicolai and Seidl, 2007, 2008; Pettigrew, 1997, 2001; Prahalad and Hamel, 1994; Rynes,

[1] Of course there have been quite many good relevant researches. The argument is that the management research is becoming increasingly irrelevant post 1970s. We can understand this irrelevance debate in the 'broader practice turn in contemporary social theory' (Jarzabkowski, 2005: 2). Evidence of this practice turn can be found in Ortner (1984), Reckwitz (2002), Schatzki, Cetina, and von Savigny (2001), and Turner (1994), to name but a few.
[2] AACSB is the acronym of the American Assembly of Collegiate Schools of Business.

Bartunek, and Daft, 2001; Rynes, Colbert, and Brown, 2002; Sutton, 2004; Starkey and Madan, 2001; Staw, 1995; Tranfield and Starkey, 1998; Van de Ven and Johnson, 2006; Vermeulen, 2005; Whitley, 1995; Wren, Buckley, and Michaelsen,1994, among others)[3].

We acknowledge that there exists a divergence in views on the relevance issue. For instance, in the field of strategic management, Shrivastava (1987) studies the relative rigor and usefulness of scholarly publications of 23 research themes within strategic management and concludes that 'this field has emphasized the practical usefulness of research results'. 4 years later, Bettis (1991) argues 'most of this research [in strategic management field] is irrelevant to what is going on in such firms today'. In the accounting field, Leisenring and Johnson (1994) think the right term to use is 'useful' instead of 'relevance' and argue '[c]learly, a greater deal of research *is* [italic in original] relevant to practice, even though many practitioners don't see it that way'. With regard to articles published in *Academy of management Journal* (*AMJ*), Michael Hitt (1998), the editor of *AMJ* during 1991-1993, disagrees with his predecessor R. T. Mowday (1994), the editor of *AMJ* during 1988-1990 about whether we do good research in management. Similarly, R. Duane Ireland (2008: 9), the new editor of *AMJ* (for 2008-2010) comments '*AMJ* is engaging its second 50-year period from a position of great strength…*AMJ* is a healthy journal publishing high-quality empirical management research'[4]. Some scholars made a defense for the relevance of management research as a whole, e.g., Aldag and Fuller, 1995a, 1995b; Aldag, 1997; Baldridge, Floyd, and Markoczy, 2004; Hitt, 1995; 1998; March and Coutu, 2006; Pearce, 2004[5].

Specifically, the criticisms roughly fall into four categories: firstly, academic research lacks of impact on management practice (e.g., Ahlstrom, Mezias and Starbuck,1992; Aldag and Fuller, 1995b; Beyer, 1982; Daft and Lewin, 1990; Hitt,1995; Johns, 1993; Mowday, 1993; Rynes etl al., 2002; Susman and Evered, 1978); secondly, the content of academic research is largely theory- and method-driven therefore too abstract and irrelevant to the needs of practing managers (e.g., Ivory et al., 2006); thirdly, the research questions are too narrow, trival, and unimportant to business decision-making (e.g., Starkey and Madan, 2001); fourthly, the writing style of management research is too academic, obscure, inaccessible, and not interesting (e.g., Leisenring and Johnson, 1994).

More seriously, Ghoshal and Moran (1996) criticize 'the management theories we are teaching are as harmful and negative…much of what we wind up teaching is, in fact, "bad for practice"'(cited in Pfeffer, 2005). 10 years later, Ghoshal (2005) once again accuses management research has generated some bad management theories which 'are destroying good management practice'. Some scholars even charge the business schools as at large fail to prepare people 'who are equipped for the practice of management' (cf. Chia and Holt, 2008: 471-472; Wren, Buckley, and Michaelsen, 1994: 141). For example, many people criticize business schools overemphasize theories and analytical skills and quantification and specialization irrelevant to the management

[3] These are contributions to this topic post 1980s. Some earlier contributions can be found in section 1.3 'The persistence of the irrelevance problem'.

[4] In his *From the Editor* column, Ireland (2008) highly praises *AMJ*'s success as a leading empirical management journal having high-quality and positive influences without even mentioning the phrase 'relevant to practice', except when re-stating *AMJ*'s mission statement which states '… and contributes to management practice'.

[5] Pearce (2004: 178) makes a very interesting and balanced statement about the relevance of management research, 'most of it is quite relevant…much of it is relevant for those less critical, more technical problems, rather than for the most important problems that those coping with large, complex organizations face'.

practice (e.g., Bickerstaffe, 1981; Mandt, 1982; Miles, 1985; Muller, Porter, and Rehder, 1988; Waddock, 1991; Wren, Buckley, and Michaelsen, 1994). Mintzberg (2004) blames the MBA education 'has had a corrupting and dehumanizing effect not just on the practice of management, but also on our business, non-profit and community organizations, and even our social and cultural institutions' (cited in Lupoff, 2004).

*1.2 What kinds of relevance to expect*

Before analyzing the irrelevance problem, it is important to clarify the meaning of relevance in this context. Nicolai and Seidl (2007, 2008) have pointed out that 'although many researchers talk about "relevance" they hardly[6] ever define what they actually mean by that'. Obviously, it is unrealistic and unfair to expect that management researchers know exactly what practicing managers do (cf. March, 2006: 85), which is against the principle of division of labor and specialization (Smith, 1776).

According to Nicolai and Seidl (2007: 2, 2008), 'scientific knowledge is said to be of relevance to management practice if it has some relevance for decision making' as 'management practice can be characterized by its focus on decision'. Building on Rich (1975), Knorr (1977), Pelz (1978), Beyer and Trice (1982) and other literature, Nicolai and Seidl (2007: 4) distinguish three types of relevance of knowledge to practice.

The first type is *instrumental relevance* which means the knowledge generated by scientific research can be utilized for action, therefore, such knowledge is 'knowledge for action' (Rich, 1975). There are three types of such knowledge: schemes, recipes and forecasts. Schemes can be relevant if they help reduce the complexity of the decision situation; recipes can be relevant if they can guide the choice between the alternatives; and forecasts can be highly relevant to practitioners since all decisions are based in one way or other on forecasts, precise or not (Nicolai and Seidl, 2007: 4-7).

The second type is *conceptual relevance* which means the knowledge generated by scientific research can be utilized for analyzing the problem or situation, therefore, such knowledge is 'knowledge for understanding' (Rich, 1975). There are also three subtypes of such knowledge: the first is linguistic constructs such as academic concepts, metaphors and stories; the second is knowledge uncovering contingincies which are the 'underlying social structures together with the restrictions they incur on human behaviour' (Nicolai and Seidl, 2007: 10); and the third is knowledge uncovering causal relationships. Linguistic constructs are of practical relevance if they have 'the potential of changing the decision maker's perception of his world and, thus, of his decision situations' (ibid.: 9). Knowledge uncovering contingencies can be relevant 'if it points out specific contingencies' (ibid.: 12), i.e., alternative routes of actions under different circumstances. Knowledge uncovering causal relationships are highly relevant 'in that they can provide a better understanding of the decision situation' (ibid.: 13).

The third type is *legitimative relevance* which means by referring to scientific knowledge, decision makers can justify their decisions. There are two types of legitimative use of scientific

[6] Mowday (1997b) is an exception as he focuses his paper on the criterion for measuring relevance. Vermeulen (2007) also devotes a section in his article to explain what being practically relevant means to him.

knowledge. One is for credentializing, for example, business school graduates can credentialize their management knowledge by their degree certificates. Another one is using as rhetoric devices, for example, 'managers often point to theoretical models or research findings to justify course of action' (Astley and Zammuto, 1992: 452).

From the abovementioned typology of relevance, we can now have a brief assessment of the relevance of current management research.

Current management research does not have instrumental relevance as the academic research is not interested in generating recipes and forecasts and most of the influential schemes having established in academic discourse, e.g., Ansoff's (1965) Product-Market Matrix, Miles and Snow's (1978) typology of strategy, McKinsey's 7-S-Framework (Peters and Waterman, 1982), come from management consultants and practitioners. Nicolai and Seidl (2007: 21) conclude that 'management science cannot be expected to produce instrumentally relevant knowledge (cf. Astley and Zammuto, 1992: 452) as it conflicts with the logic of science', which prioritizes scientific rigor over practice relevance (cf. Thompson, 1956).

Also, the management has seriously lost its conceptual relevance by detaching from management practice. This is evident given the specific criticisms of irrelevance aforementioned in section 1.1.

As a consequence, current management research and even the management education as a whole have lost their legitimative relevance, since the business education degree certificates do not credentialize business school graduates as much as the schools and students might have expected (cf. Pfeffer and Fong, 2002, 2004), and there is increasing rift between academics and practitioners therefore it is not often the practitioners would use academic research as a rhetoric device to legitimate their decision making, although the use of academic artifacts for communicational purpose is observed (cf. Jarzabkowski, 2003: 21).

### *1.3 The persistence of the irrelevance problem*

Mowday (1997b: 27) points out '[d]isscussions of the relevance of business school research are certainly not new'. Rynes, Bartunek and Daft (2001: 340) echo this opinion and find the discussions of the relevance-rigor-gap and its causes 'have been widely debated for some time'.

With respect to the early call for 'a coherent approach to linking research to practice', Cummings (2007) gives credit to Harold Smiddy's 1962 Presidential Address to the Academy of Management Annual Meeting (Smiddy, 1962). Johnson and Podsakoff (1994: 1392) attribute the earliest contribution to the inquiry of the quality of academic management journals to Coe and Weinstock (1969). Schon, Darke, and Miller (1984:7) trace the early contribution to 'the dilemma of rigor or relevance' to Campbell and Stanley's (1963) which 'not only described this dilemma but proposed a solution to it' which is the 'quasi-experimental method' (Schon et al., 1984: 7).

Other early contributions to the related discussion include Duncan (1974), Weiss and Bucuvalas (1977, 1980), Dunn (1980), Hayes and Abernathy (1980), Beyer (1982), Beyer and Trice (1982), Campbell, Daft and Hulin (1982), Hakel et al. (1982), Thomas and Tymon (1982), Miner (1984), Shrivastava and Mitroff (1984), Cheit, (1985); Lawler et al. (1985), Porter and McKibbin (1988), Oviatt and Miller (1989), Cummings (1990a, 1990b), the list can go long.

As early as in late 1970s, Susman and Evered (1978: 582) have warned us that 'there is a crisis in the field of organization science… as our research methods and techniques have become more sophisticated, they have also become increasingly less useful for solving the practical problems that members of organizations face…as a result, practitioners and their clients complain more and more frequently about the lack of relevance of published research [and] the lack of responsiveness of researchers to meeting their needs'.

Many journal special issues[7], academic conferences and workshops[8] have been devoted to address this problem, and the Academy of Management's presidents 'have persistently addressed the drive for relevance' (Cummings, 2007: 355), for example, Smiddy (1962), Hambrick (1993), Mowday (1997a), Huff (2000), Van de Ven (2002), Bartunek (2003); Pearce (2004), Rousseau (2006), and Cummings (2007).

Scholars have proposed many recommendations on how to remedy this irrelevance problem, some focusing on knowledge generation aspect while others on knowledge dissemination. For instance, Argyris's well-known 'action research' perspective initiated by Kurt Lewin's (1946) (Argyris, 1970, 1980; Argyris, Putnam and McLain Smith, 1985; Argyris and Schön, 1974, 1978, 1996). Boyer (1990) urges us to broaden our definition of scholarship to include scholarship of discovery, integration, application and teaching (cited in Mowday, 1997a: 339). Bettis (1991: 317-318) suggests to encourage 'more unstructured and exploratory research…the development of realistic prescriptive implications as a normal part of the research process…and methodological diversity'. Gibbon and his colleagues call for Mode 2 research (Gibbons et al., 1994; Nowotny, Scott and Gibbons, 2001, 2003). Leisenring and Johnson (1994) recommend academics to 'dumb it down' in order to communicate their research in ways practitioners can understand. Hitt (1995) believes a better channeling mechanism is needed to facilitate transferring of knowledge generated by the academic research to practitioners. Aldag (1997: 9-11) proposes 'heavier use of qualitative methodologies', proactive 'channeling of information from practitioners to academics', and engaging 'full research collaboration' between academics and practitioners'. Pettigrew's (2001) solution is to 're-engage and deepen our links with the social sciences and users' to complement to Starkey and Madan's (2001) solution of building 'new forms of research partnership and research training that will address the relevance gap'. Van de Ven and Johnson (2006: 822) build on Boyer (1996) to promote 'engaged scholarship' to conduct 'practitioner-meaningful research'. Vermeulen (2005, 2007) calls for improving research at individual level by 'adding a second loop [of relevance]' to the current paradigm in order to make management research matter more.

However, the problem is not only unsolved but deteriorated. We need take this issue seriously and disentangle the root cause of this problem (cf. Wing, 1994). I argue the failure of many existing solutions for change is due to first of all our incomplete understanding of the causes of the irrelevance problem. Van de Ven (2002: 179) points out 'the gap between research and practice of management is a complex and controversial subject'. Past diagnoses tend to simplify

[7] See the footnote 1 of Van de Ven and Johnson (2006: 802).
[8] See http://www.s-as-p.org/news_view.php?id=2 [Accessed on 9 January 2009]

and divide the complicated reality into separate issues, such as knowledge transferring problem, lack of collaboration between academics and practitioners, or non-user-friendly academic writing styles, etc. In fact, the reality of the irrelevance problem is a much more complex phenomenon than whatever the simplistic accounts can capture. I argue, we need a more holistic and systematic analysis in order to understand this complexity first and foremost. In the following section, I draw on extant literature to offer such an account.

## 2. Why has management research become irrelevant?

### *2.1 Two conditions and three forces of the irrelevance problem*

For the problem of irrelevance to emerge and persist, there must be two conditions: one necessary and one sufficient condition. The necessary condition is that there must be a strong force to push management research away from management practice which it researches. The sufficient condition which is there must be a strong force to prevent the deviating management research from moving back to management practice.

In physics, we understand to make a stationary object to move from the original position we need an initial force to push it. The moving object will gradually stop down if there is a counter force, say friction, with a reverse direction to be put on it. The counter force may be a net force resulting from two competing forces with different directions. However, if there is another prevention force to reduce the counter force, and only if the prevention force is equal to or stronger than the counter force, then the moving object will not stop down or even move forward more quickly.

In the case of management studies, our academia has been gradually moving away from the real business world, the original position. The initial force is the power of the self-referential and self-sustaining nature of management studies as pursued as a science. We may argue we are moving further away and even with a faster speed than before. Then we see another two forces having involved in this dynamics. One is a counter force to stop management academia moving away from business practice, both internal (i.e., scholars' call for relevance and change) and external (i.e., pressures from the business schools' external stakeholders for relevance and change). Another one is the prevention force to maintain the status quo. Given the reality that the management academia has not slowed down, we may argue there are two possible reasons: (1) the prevention force, i.e., paradigm maintenance of the mainstream academia, is stronger than the counter force, i.e., the internal call and external pressure for relevance and change; (2) the counter force is not a real counter force, i.e., with negative value, which means it will rather help moving the management academia further away from the practice. In the second case, it can be understood that probably there is no or little internal counter force or/and the external impact is negative as well, i.e., not only does not exert pressure for relevance and change, but also gives incentive for the system to value rigor more than relevance.

Gioia and Corley (2002: 107) call our attention to 'some metaforces that have dramatically affected the character of management education in the United States and now are spreading internationally' and argue that '[s]ometimes those things are of education's own making. Most times, however, they are a combination of external forces and internal willingness to be co-opted by those forces'. I cannot agree more with them.

In the following paragraphs, firstly, I will explain, from a social systems theoretical perspective (Luhmann, 1982, 1984,1995, 1998, 2002, 2005, 2006), that the initial force pushing management research away from management practice is the self-referential and self-sustaining nature of management science having been pursued by management academia since 1950s (cf. Thompson, 1956). Secondly, I will analyze the systematic paradigm maintenance of the mainstream academia to enforce and protect the established orthodox as the prevention force preventing the academia from moving back to management practice. Thirdly, I will examine why the external environment of the business schools not only exerts very weak pressure, i.e., the counterforce, but also puts some negative impact on management research and business schools as a whole.

### 2.2 The self-referential nature of management science: A trap of our own making

Van de Ven and Johnson (2006: 805-808) state '[t]he recognition that research and practice produce distinct forms of knowledge has been long-standing in the literature…[b]oth forms of knowledge are valid; each represents the world in a different context and for a different purpose…we take a pluralistic view of science and practice as representing distinct kinds of knowledge that can provide complementary insights for understanding reality'.

Van de Ven and Johnson (2006: 808) further point out '[e]ach kind of knowledge is developed and sustained by its own professional community, which consists of people who share a common body of specialized knowledge or expertise… [e]ach community tends to be self-reinforcing and insular, and limited interactions occur between them'. this view is certainly shared by Fendt and Kaminska-Labbe (2007: 3) when they announce 'the relevance gap is a natural consequence of the prevailing paradigms of management research'. The prevailing paradigm of management research is called 'the scientific model' (Bennis and O'Toole, 2005; Chia and Holt, 2008: 472; Ghoshal, 2005; Gordon and Howell, 1959; Locke, 1989: 1-3, 159-163; Pierson, 1959; Robertson, 1932; Smiddy and Naum, 1954; Susman and Evered, 1978, cf. Whitley, 1995: 81-82).

There are five assumptions of 'the scientific model': (1) natural science is the ideal model for social science research (Susman and Evered, 1978: 583); (2) scientific research can generate knowledge and theory that are definite and generalizable (Chia and Holt, 2008: 473); (3) knowledge flows in a linear (and even hierarchical) way: academics generate, consultants disseminate, practitioners implement them (Mckelvey, 2006; Nicolai, 2004; Rasche, 2007: 3; cf. Van de Ven and Johnson, 2006: 805; Whitley, 1995); (4) scientific rigor should be given priority over practical relevance (Cummings, 2007; Mckelvey, 2006); (5) the pressure for immediately applicable results must be reduced (Thompson, 1956; cf. Van de Ven, 2002). In practice, 'the scientific model' requires (Cummings, 2007: 357): firstly, an almost single-minded focus on theory and research; secondly, a skillful interplay of inductive and deductive methods; thirdly, research questions being largely theory-driven, or method-driven; fourthly, data gathering and analytical methods mainly quantitative (cf. Robertson, 1932; Thompson, 1956). Consequently, the scientific research is detached from the practice.

Some scholars and practitioners see the root cause of the irrelevance problem of management research is 'the scientific model' (Bennis and O'Toole, 2005; Ghoshal, 2005; Wing, 1994: 388[9]) we as management scholars have collectively adopted in pursuit of management studies as a science, like other natural sciences, such as physics and chemistry.

According to Niklas Luhmann (2006: 37), a German sociologist and philosopher, the development of the systems theory has three stages (i) the theory of closed systems; (ii) the theory of open systems; and (iii) the theory of observing or self-referential systems. Luhmann's own contribution centers on the third and last stage. My analysis here is derived from his social systems theory.

From a Luhmannian social system theoretical perspective, social systems include society, organizations and interactions (Luhmann, 1995: 408-410) while society, as a social system itself, encompasses the other two forms of social systems as it includes all communications. All societal subsystems, e.g., economy, art, science, religion, have and serve particular social functions, which is an important concept of functional differentiation (Luhmann,1982).

According to Luhmann (2006), all functional systems and all social systems in general are autopoietic, i.e., self-referential, which means, social systems reproduce themselves from within themselves. All social systems reproduce their own elements on the basis of these elements, which is communications. Each communication of a system relates to other communications of the same system on the function-specific code, such as the code true or untrue in the functional system of science, the code payment or non-payment in the system of economy, etc. (Luhmann, 1982).

Because of the autopoietic nature and the unique codes used, different functional systems represent 'environment', a important concept distinct from system, for each other. According to Luhmann (2006: 38), 'a system *is* the difference between system and environment. Therefore, social system though can be influenced but not determined by its environment, and the same logic applies to different social systems. Any social system reproduces itself based on its own communications and registers other system's as irritations only (Rasche, 2007: 10).

Science and practice then can be viewed as two distinct social systems as Luhmann (2005: 378) calls practice as 'the system of application', therefore, both science and practice are self-referential closed and functional system. Management studies, as having been pursued as an applied science, is made to be self-referential. The element or communication of the management science is the scholarly publication which adopts stylized writing formats and goes through the peer-review process. It is self-referential because scholarly management research publication is written based on and referred to the past scholarly publication and peer-reviewed based on the quality assessment criteria set by the management science community itself. Though it may be influenced, but it is not determined by any external systems. The same self-

[9] Kennard T. Wing is a practitioner and a member of the Academy of Management. He believes the root cause is the 'positivist approach to research' which 'makes it hard to do practice-relevant research' (Wing, 1994: 388).

referential logic applies to management practice, as a system of application, too. Therefore, management science is naturally and gradually divorced from management practice[10].

Management science is gradually detached from management practice because the whole management education was not like, and completely opposite to, the current model in the first half of the 20th century. Business schools used to be completely, what some scholars term, a trade-school model (Bennis and O'Toole, 2005; Vermeulen, 2005) where the whole business education focused exclusively on vocational training with little systemic research. Since the call for making management studies a scientific discipline in late 1950s (Gordon and Howell, 1959; Pierson, 1959; Thompson, 1956), we as management academics have collectively adopted and wholehearted pursued 'the scientific model', the current orthodox, to study and teach business management.

To many people, the pendulum has swung so far away that the need of scientific rigor has gradually crowded out much even most of need of practical relevance (cf. Chia and Holt, 2008: 473; Van de Ven, 2002: 180; Vermeulen, 2005: 979). Therefore, many responsible scholars call for a balance of rigor and relevance and proposed many suggestions for change of all sorts. However, all such solutions have not made the management academia more relevant in any strict sense. Most people still have no clue why we are stuck here and even drifting further away from real business world. They just don't understand management studies as pursued as a science, like any other social system, has inherited the self-referential and self-sustaining nature. Since management studies needs not necessarily to be pursued in a scientific way, as we used to do before mid-1950s, we have to accept this reality that we are stuck in a trap of our own making.

### *2.3 Paradigm maintenance of mainstream management academia*

Ever since the management academia as a whole collectively and gradually adopted 'the scientific model' in order to make management studies as a science, the mainstream of management scholarship, i.e., the orthodox, has seen there is a need to maintain and strengthen the established paradigm. And this paradigm maintenance is being done in a systematical way, from researcher training, i.e., PhD programmes, to faculty recruitment, from peer-reviewing to promotion, i.e., tenure system, and business schools as academic institutions is profoundly change-resistant. Let me explain each of these 5 aspects in more detail.

Firstly, the PhD training is a starting point because the purpose is to prepare for future's management researchers (as well as educators[11]). PhD programs normally have a compulsory research methodology training which forcefully and systematically instills the students a set of orthodox philosophy (i.e., ontology, epistemology and methodology) of scientific research and a set of research methods, quantitative and/or qualitative. PhD students systematically learn what rigorous research means, how to use published works as references, and most importantly how to

[10] Ironically, even the present paper, which criticizes the self-referential nature of management studies as a science, has to follow the stylized writing format and the referencing norm and go through the peer-review process in order to get this paper published. This is to say, the management academia effectively decides how the criticisms on it are presented and published.

[11] In practice, the demand for preparing good management educator is not effectively emphasized during the PhD studies.

appreciate 'the scientific model' of management science itself. In a sense, PhD training is so rigid and conservative that any deviating attitude and behavior will be discouraged and even punished immediately. For instance, PhD students are normally advised to choose very specific and narrowed-down topics in the name of the need for more in-depth research. Ambitious plans, say interdisciplinary topics, are discouraged because of the complexity and difficulty[12]. Any challenge to the orthodox should be avoided because the assessment committee would not like that. There are many other constraints on what PhD projects can do, like a straightjacket[13]. If you like the jacket then you feel quite comfortable; if you don't like it then you will soon feel frustrated. Given the fact that the trend is that a PhD degree is more and more becoming a requirement for entering into management academia, the PhD training is a very important way to ensure the orthodox or the dominant paradigm in management academia is to be maintained and strengthened.

Secondly, the faculty recruiting is another important issue because you don't really want to risk hiring someone who does not appreciate the norm and culture of your organization. So if it is very likely that the PhD students may well deviate from the school's expectation after being admitted to the PhD programs, it is a different case for faculty recruitment because the applicants' research records are quite reliable indicators whether they are standard-followers or grave-digger. So, the recruitment process can effectively filter the applicants the schools do not want in order to maintain the paradigm. A well-known case is that F. A. von Hayek, a brilliant economist and Nobel Prize Laureate in 1974, was rejected to be a professor in the Department of Economics of the University of Chicago, precisely because of his association with the Austrian School of Economics, which is a competitor to the Chicago School.

Thirdly, when it comes to publishing scholarly articles in management academia, the practice of peer-review is the strongest pillar of intellectual enterprise of the management orthodox. Since the editors and the reviewers of the mainstream management journals are normally reputed senior management academics, these people will act as what Smith (2008: 307) calls 'the gatekeepers of orthodox', because they all have grown with the same system, tradition and practices of mainstream management, and they are so used to the paradigm, and finally there seems to have no good reason why they want to relax the straightjacket for the later comers after they have endured for their whole career. Two very interesting stories are Mike Porter's (1980) Competitive Strategy book and Jay Barney's (1991) *Journal of Management* paper. Both works are well-known scholarly works and each represents the cornerstone of the two dominant competing schools of thought in strategy field, i.e., the Industrial Organization school and the Resource-Based View school. However, these two works were initially rejected by their respective orthodoxies. Porter's later-landmark 5-forces model and generic strategy theory were seriously questioned and doubted by both Business School and the Economics Department of his employer Harvard University (cf. Argres and McGahan, 2002). Similarly, Barney's later-most-cited RBV paper had been rejected twice by *Academy of Management Review* (*AMR*) and once by *Strategic Management Journal* (*SMJ*) before finally Jay Barney, as the editor of an special issue of the *Journal of Management* accepted his own paper (cf. Smith, 2008: 306).

[12] Discipline-based PhD supervisors may not be able to supervise on these ambitious research projects.
[13] Bettis (1991) use this word to describe his feeling that the whole strategic management field is like having a straightjacket being put on the strategy scholars by adopting and following the 'scientific model', i.e., the orthodox.

Fourthly, the promotion policy favors research as opposed to any other activities, i.e., teaching, consulting, servicing. This is understandable because the performance of any non-research activity is hard to measure while the quantity of the publication is a comparable indicator. Although quality is highly concerned, it is still quite controversial because there is no single precise criterion to assess the quality of a published research project. For instance, in practice, since we cannot directly compare the qualities of two papers, we heavily rely on the reputation (i.e., impact factor) of a journal in which the assessed research appears, however, it is disputable if a paper published in a higher-impact-factor journal is better than another in a lower one, because even the how to measure the quality of the journals is disputable and controversial in the first place. So, gradually the importance of quantity of publication is highly visible in the promotion decision-making, although quality is also very important. This practice as a consequence gives individual researchers an increasing vast incentive to produce as more publications as possible. Therefore, there is a natural result: scholars choose to study more and more narrower topics to research in order to produce quick output. No wonder why these narrowed-down researches are of little practical relevance because in practice business practitioners face much more complex, fuzzy and unpredictable reality than what these narrow researches can study.

Finally, as a result of the abovementioned paradigm maintenance, the business schools as academic institutions are in general extremely conservative and change-resistant (ref). It is a norm rather than an exception to do irrelevant researches in business schools as Smith (2008: 306) sharply remarks '[there is] no need to be practical. I could pursue an idea for the sake of that idea'. As an experienced and successful businessman prior to his academic life, Smith (2008) confessed he felt 'it was liberating to be free of the "read world"' once he re-entered the world of management scholarship. Unfortunately, an increasing number of management scholars feel 'the academic system hampers our research from fulfilling its potential in terms of relevance for practitioners' (Vermeulen, 2007: 759) and have called for change. However, '[c]hange is never easy' (Mowday, 1997a: 343; Pfeffer, 2005:99) because the management academia is running in 'an incestuous, closed loop' (Hambrick, 1994: 13). Without any strong counter force, from within or without the academia, we seems to have no way out.

### *2.4 The external environment of business schools: anything but a counter force*

The business schools have several groups of stakeholders, i.e., the Ministries governing the education sector, management students as both the buyers and supplier of the business education, business corporations as customers and donors, funding agencies as financial suppliers as well as proxy consumers of management research, evaluation agencies as quality assurance body, advisory boards, and the general public. They all together consist of the external environment of business schools.

According to Luhmann's Social System Theory, a system's environment, although cannot determine, can indeed influence the internal function and dynamics of a system. If the external environment had exerted a strong counter force on the management academia, then there is a possibility that the speed and direction of the moving of the academia would have been different. However, this is not the case. In a sense, we can even argue, the external environment has exerted not only NO strong counter force, but EVEN some negative impact which reinforced the deviation of management academia from the practice.

Oviatt and Miller (1989) convincingly explained why the industry cannot exert a strong pressure on business professors by using Porter's (1980) five forces framework. In his account, the suppliers and buyers, i.e., being both the management students and business executives, have low bargaining power over the business professors as a whole; and the threat of entry of new business professors is low due to the high entry barrier, i.e., PhD degree requirement, and low return on investment, i.e., huge suck cost of time and salary-lose during the PhD training; and the substitution threat from corporate and non-university providers of management education is moderate because of the excess demand for academic services. Therefore, business schools professors enjoy enormous bargaining powers and tend to be intransigent.

An important reason for business professors' intransigence and the persistence of irrelevant problem of research is due to the structural problem of proxy consumption of management research, namely, the funding body (normally the research councils) acts as a proxy agent standing in between the producers of research, i.e., academics, and the real consumers, i.e., the corporate world, so the natural demand-supply ties are broken by the funding agency. In natural sciences, this arrangement breaking the tie is understandable and even desirable because the basic researches normally take time and may not have immediate relevance in practice so it is necessary to insulate the natural scientific research and their communities from the relevance pressure (cf. Conant, 1945; cf. Oviatt and Miller, 1989: 307). However, this is right for pure science research, not for artificial science (Simon, 1969, 1996) research, such as management studies (Romme, 2003:715; cf. Argyris et al., 1985).

Although business schools and professors can be arrogant and intransigent, they also have to be subject to the evaluation process. In theory, all aspects of business schools activities are to be assessed, i.e., teaching, researching, servicing. But in practice, due to the difficulty and controversy of the assessment of teaching quality and servicing performance, research becomes the most important item of the evaluation system. Due to the similar reason I have explained in respect of faculty promotion, the evaluation gradually focuses more on the research output of a school and a department, which in turn give the faculty members a strong incentive to favor research more than any other activities and to favor quantity more than quality of their researches. The dominant culture in academia is 'publish or perish'.

With respect to business schools' advisory boards, Bellizzi (2008: 3) comments '[a]dvisory boards have advised faulty and administrators, ad infinitum, of the need for more relevance at both undergraduate and graduate levels. They have punctuated the critical need for personal and interpersonal skills; for first-hand, on-the-job experience; and for a greater understanding of diversity and ethical issues. Somehow the need to change and redirect efforts are slow to occur, as in most organizations, especially when what is being rewarded and reinforced is the very thing the change is directed at'. So we cannot expect any effective impact of the advisory boards on business schools for change the paradigm and culture.

The business world in general does not really press the business schools for relevance. On the contrary, they often make huge donations to business schools for public relations purposes, like corporate social responsibility claims, benefits of having their corporate names in the schools', and corporate image building to attract best students, etc. Essentially, by donating large amount of money, the corporate world is sending the wrong message of encouraging and rewarding what the business schools have been doing. In addition, corporations and executives are to blame as they close the doors for researchers, although they might not be interested as they do not see any benefits or they don't have any expectation the academic research can generate some relevant knowledge from business school professors who are often ignorant and arrogant (Sutton, 2004: 28). Some scholars complain business practitioners do not read academic management journals therefore do not know the new value of the management research (Aldag, 1997: 14; Gopinath and Hoffman, 1995; Hitt, 1995; Leisenring and Johnson, 1994: 75-76; Rousseau, 2006: 261; Rynes et al., 2002). Van de Ven (2002: 178) claims that the criticism of irrelevance 'goes both ways' as '[m]anagers and consultants are not doing enough to put their practice into theory'.

Finally, the general public has also had some negative impact on the behaviors of business schools. For instance, Gioia and Corley (2002: 107) argues '[t]he forces of greatest moment in the management education domain are the media rankings of business schools…the rankings are producing an accelerating, Circe-like transformatition of business schools from substance to image'. All kinds of ranking and accreditation have forced business schools to act accordingly to meet expectations of the general public and to compete with other schools for ranking in order to attract more resources and improve the reputation in order to attract students. Research output is normally an important criterion in the ranking design. Pfeffer and Fong (2002: 91) criticize the tendency and problem of making business school a business and comment 'as with any status-based system, it is scarcely in the interests of those schools winning the competitive war for status to change the rules of the game that have put them on top'. An evident problem in competing in media ranking and even the international accreditation like AACSB[14]'s has been we take for granted that there is a best practice, say, US-style MBA education, and therefore every business school should strive to converge to it. This is fads-following (aldag, 1997) which is dangerous. For example, Pfeffer and Fong (2002, 2004) make serious criticisms of the failure of US-style MBA education which has been widely followed as the ideal model worldwide.

In short, the external environment as a whole has given business schools at least no positive pressure for change, and we may even argue it exerts negative impact. Together with the innate driving force of the self-referential nature of management science and the paradigm maintenance of the orthodox, we have seen the ultimate result: management research and education as a whole have become irrelevant!

## 3. Management academia: a fast train going to nowhere

### *3.1 Any solutions designed under 'the scientific model' are doomed to failure*

[14] Pfeffer (2005:99) states '[m]uch of what we do is truly taken for granted and there are powerful organizations such as the Graduate Management Admissions Council and even the AACSB that have some vested interest in the status quo' and '[b]usiness education is heavily institutionalized' so '[c]hange will obviously be difficult'. Cousins (1994) laments the pressure for his school (used to be a very good teaching-oriented one) to conform to AACSB guidelines to focus on generating research (become a purveyor of third-rate articles).

Although the irrelevance problem of management research has been raised long time ago (i.e., since late 1970s), and there has existed plenty of solutions and recommendations (e.g., partnership, incentive reform, practitioners join the review board, two faculties for two types of research, action research), the situation has been not only unimproved but also deteriorating (cf. Mckelvey, 2006: 822). To many people, this is surprising (Pfeffer and Fong, 2002: 92) and not understandable (Ayas, 2001, in Beer, 2001: 65) given the increasing awareness of the problem and appeal for closing the gap.

On the other hand, people who wholeheartedly believe in and pursue 'the scientific model' will have very different feeling about this irrelevance charge. Some people are feeling proud of what the management academia has achieved in pursuit of a management science. For example, Thomas G. Cumming (2007), the President of Academy of Management for 2005-2006, address the Academy's 2006 Annual Meeting 'doing research to create scientific knowledge is what we do best. It is our core competence and main claim to legitimacy as a professional society…[t]oday, we have gained scientific legitimacy and can afford, at this stage of our profession's growth, to devote more attention to making sure our knowledge is relevant and useful'. Similar attitudes can be found with Aldag (1997), Hitt (1995), and Leisenring and Johnson (1994). Some other people may argue whether an emphasis on relevance is desirable (cf.Rynes, Bartunek, and Daft, 2001), e.g., Earley (1999), Fagenson-Eland (1999), Gillespei (1991) Knights (2008) makes an epistemological defense of independence as academics and warns it is dangerous to enslave us to practical relevance. Mckelvey (2006: 826) disputes with Van de Ven and Johnson's (2006) engaged scholarship solution and see it as 'illogical' and 'could produce…bad science'. Nicolai (2004: 972) calls for 'respecting the self-referential nature of science' which will serve a better dialog between science and practice than 'demanding an identity that cannot exist'. Rousseau (2006) in her Presidential Address to the Academy of Management 2005 Annual Meeting complains 'why managers don't practice evidence-based management' (Rousseau and McCarthy, 2007) and argues '[t]he research-practice gap among managers results from several factors. First and foremost, managers typically do not know the evidence…few practicing managers access this work', and she urges management educators to teach what the up-to-date academic research findings support to business students, MBAs and executives.

In this paper, I would argue that any solutions to tackle this irrelevance problem designed within and based on 'the scientific model' is what I call '*zhi biao bu zhi ben*' solutions, a household phrase in China borrowed from the Chinese medical philosophy. The phrase of '*zhi biao bu zhi ben*' can be literarily translated as 'relieve the symptom without cure the root cause'. Therefore all such solutions will turn out to be in vain because as Vermeulen (2005) firmly puts that 'without a systemic change [to the academia]' (p. 978), any pleas for relevance, including any of previous ones and his own, will not 'change the behavior of academic researchers, at least of those who agree with' (p. 980). Mckelvey (2006: 827) forcefully articulates '[t]here is nothing in the current rules of scientific realism that allows paradigms to accept site-and time-specific findings as broad "scientific" truth claims' because '[w]hat gets stamped as legitimate research … is discipline-centric quantitative research with large samples, Gaussian statistics, findings reduced to averages, and confidence intervals for statistical significance based on finite variance'. As a consequence, any partnership by the two distinct self-referential systems – science and practice – in fact does not exist, as Kieser and Leiner (2007) pointed out that '[r]esearchers and practitioners cannot collaboratively produce research; they can only irritate[15] each other'. If any such a combination or balance of rigor and relevance within 'the scientific model' of management research is to be claimed, it is merely an illusion, or what Nicolai (2004: 958) calls 'Applied Science Fiction' (ASF)[16], which is 'dangerous' as 'ASF leads to mutual deformation rather than information between science and practice' and 'ASF paradoxically leads to practical irrelevance' (ibid.: 970-971).

If the problem is with the Positivist quantitative research methodology applied to management studies, then what about the qualitative research? Is it a solution?

Daft (1983: 539) contends '[i]ndeed, qualitative and quantitative approaches can be used side by side, as in the natural sciences'. Morgan and Smircich (1980) make 'a case for qualitative research' (as the title of their paper) and Tellis (1997) analyzes the history of case study methodology to show it was extensively used before the quantitative turn and its later dominance.

I argue the promotion of qualitative methodology, i.e., case studies, is a good initiative to make management research more relevant because management practice is ultimately context-dependent, i.e., vary from case to case. Nevertheless, I disagree with the tendency for qualitative researchers to argue and justify their research methods are also 'scientific' as the quantitative ones so claimed by the Positivist management scholars. In so doing, I am afraid the qualitative researchers are putting a straigtjacket on themselves and create a risk of falling into a self-made trap, like what we have done in the pursuit of a management science.

### *3.2 A radical systemic change needed – Towards a 'professional model'*

Vermeulen (2005, 2007) proposes a synthesis of the outdated vocational model and the current scientific model is needed. However, his solution of 'adding a second loop', i.e., 'Loop 2: Relevance', has contributed nothing new, does not touch on the root cause of the irrelevance problem, and calls for no systemic change but individual responsiveness. So his solution is merely another '*zhi biao bu zhi ben*' solution.

More fundamental changes to social science research has been suggested by some others, e.g., Kurt Lewin's (1946) 'action research', Campbell and Stanley's (1963) 'quasi-experimental method', and Glaser and Strauss's (1967) 'grounded theory' building. Fendt and Kaminska-Labbe (2007: 19) label some scholarly suggestion of abandoning 'the scientific model' in order to tackle the irrelevance problem as 'radical manifestos for the redesign of academia, such examples are those of Susman and Evered (1978), Romme (2003), Gosling and Mintzberg (2004), Mintzberg (2004), Ghoshal (2005), Pfeffer (2005), and Bellizzi (2008).

Why do we need to abandon the dominant paradigm, i.e., 'the scientific model'?

[15] Scientific model-minded management academics tend to believe that academic knowledge generated by scientific research is at a higher level of knowledge chain than the practicing knowledge the practitioners have (cf. Camerer, 1985; Nicolai, 2004: 953). We can imagine how irritating this mentality can result when contradiction of knowledge emerges in the interaction between down-to-earth practitioners and ignorant yet arrogant academics (cf. Sutton, 2004).

[16] Unfornately, Nicloai's own suggestion to this irrelevance problem is to ask people to respect the self-referential nature of [management as a] science, which to my understanding is paradigm maintaining.

If we had carefully read Chia (2005), Hayek (1942, 1943, 1974), Susman and Evered (1978), and Whitley (1995), to name but a few, we should have understood the fundamental problem of using scientifically rigorous methods to study 'phenomena of organized complexity', such as management and economy, in contrast to 'phenomena of unorganized complexity' (Weaver, 1958; cited in Hayek, 1974),.

There is no need for me to repeat what the structural feature of an organized complexity system, e.g., management and economy, are and why therefore it is fundamentally wrong to use scientific methods used in natural sciences such like physics and chemistry to study the social phenomena of such an organized complexity. Here let me just quote an opening passage of Hayek (1943), a classical piece from the then Nobel Prize winner for Economic Science in 1974, who titled his Nobel Memorial Lecture as 'The pretence of knowledge' which echoed once again the argument of his 1942 and 1943 essays on 'scientism and the study of society'.

> 'The great differences between the characteristic methods of the physical sciences and those of the social sciences make it not difficult to understand why the natural scientist who turns to the works of the professional students of social phenomena should so often feel that he has got among a company of people who habitually commit all the mortal sins which he is most careful to avoid, and that a science of society conforming to his standards does not yet exist' (Hayek, 1943: 34).

Some scholars have pointed out that management is not a science, in one or another way. For instance, Eccles & Nohria (1992 'classify management as a practicing art (cited in Ghoshal, 2005:77). Chia (2005: 1092) echos this views that 'managing…is an art, not a science'. But some others see a more balanced picture. For example, Cioffi (2002: 3) sees '[m]anagement is a continuum with Art on one end and Scientific Process on the other' while Gosling and Mintzberg (2004: 19) believe 'management is neither a science nor a profession…management is a practice'.

There is a more interesting viewpoint which I completely agree that management is more of an art than a science. For instance, Pfeffer (2005: 99) contends 'management and organization science must be concerned with more than science and theory, although they are obviously important foci'. Bellizzi (2008: 1-2) observes '[b]usiness is about people interacting with people, and no scientific equation exists which purports to explain how to go about this…the know-how…cannot be obtained solely from a textbook or "scientific" research…Business is a profession not a hard discipline like physics or chemistry…the fact that effective business leadership is more of an art than a science'.

Gosling and Mintzberg (2004: 19) point out 'management may use science, but it is an art that is combined with science through craft…business schools have important things to teach about managing'. I cannot agree more! But in order to fulfill this important role to teach about managing, I believe, business schools and management research have to adopt a[17] 'professional

[17] Notice I use the word 'a' rather than 'the' because different people may have different conceptualization of 'professional model', e.g., Bennis and O'Toole's (2005). So here I propose my own conceptualization of such a 'professional model'.

model' (cf. Bennis and O'Toole, 2005), which I refer here as a synthesis of the outdated vocational model and the gone-wild scientific model.

What should the professional model look like?

The professional model of management studies, as a synthesis of the vocational and scientific models, should have the merits of both academic rigor and practice relevance, but it is not like what any other published solutions have depicted. First and foremost, it is a different combination of relevance and rigor in which I argue we give priority to relevance over rigor, which is completely opposite to the 'scientific model' that prioritize rigor over relevance. Secondly, it is a special combination of relevance and rigor because the structure of this combination is unique: conceptual relevance and rigor + instrumental relevance and rigor. Thirdly, it is a new combination because it requires a new triadic relationship between research, consulting and practice. Let me explain more.

Firstly, I think practical relevance should be pursued constantly even at the cost of academic rigor. Simply, I see in a professional school, like engineering, medical, legal, and management, the practical relevance is the ultimate end while the academic rigor is the means. This is to say, it is perfectly understandable we can choose different means to reach an end while it is irrational to change the end (i.e., relevance) in order to accommodate the means (rigor). Business school, among other professional schools, now looks very special in terms of the degree of detachment from the profession they serve (ref). This is entirely due to the wrong emphasis which puts the carts before the horse. It is time for redirection now.

Secondly, as many people have realized how difficult to reconcile the ideal of balancing rigor and relevance and the harsh reality of trade-off, the problem is these people have not yet really understood the relationship between relevance (and its subcomponents) and rigor. According to Nicolai and Seidl (2007, 2008), we know there can be at least two different types of relevance: conceptual and instrumental relevance. And from the analysis of section 1.2, we know academically rigorous research cannot be expected to have instrumental relevance, but can have conceptual relevance. So we should not waste time to argue and hope both rigorous and instrumentally relevant research done by pure academics. Then we need consultants who are used to and good at doing instrumental researches. If consultants conduct instrumentally relevant research based on rigorous academic concepts and theories (as opposed to based on the rigorous research methods), then consulting research can have both instrumental relevance and rigor. So the synthesis of relevance and rigor can only be done in this way.

Thirdly, the structural characteristic of this special combination of relevance and rigor calls for a new triadic relationship among management academics, consultants and practitioners. In the current model, these three groups of people are living in separate worlds while consultants are widely seen as a bridge between the two isolated communities of academics and practitioners. This traditional way of division of labor among these three groups is not effective largely due to the fact that academics are so ignorant and arrogant to believe there is a hierarchical chain of knowledge (cf. Van de Ven and Johnson, 2006: 805), namely, academics as scientists generate scientific knowledge (which is at the highest level) and consultants disseminate the academic knowledge and help practitioners to implement in/apply to the management practice. So although some scholars acknowledge 'academic researchers do not have a monopoly on knowledge creation. Practitioners and consultants discover anomalies and insights from their practices' (Boyer, 1990; Starkey and Madan, 2001; Van de Ven, 2002; cited in Van de Ven and Johnson, 2006: 805), practicing knowledge (e.g., Nonaka, 1994; Nonaka and Takeuchi, 1995) has been largely ignored by management academics and only paid lip service. No wonder why they are living in two isolated worlds. So the new professional model prescribes a closer collaboration triadic relationship between these three groups of people on the basis that they must understand and accept the reality that each of the three have their distinctive yet non-substitutable knowledge, and furthermore both academics and consultants should work closely to co-service the management practice. In this new triadic relationship, the practical needs (relevance) are at the core and the rigor is at the periphery.

### *3.3 Unfortunately, the change from within is highly unlikely to happen*

Academic institution like business schools is profoundly conservative and change-resistant due to the vast and strong vested interests situated at every corner of the institution. There are several reasons.

Firstly, it is due to path-dependency. 30 year has passed since Susman and Evered's (1978) warning of the irrelevance crisis. The history simply tells us an inconvenient truth, to borrow Gore's phrase, that the management academia is too inertial and resistant to change. As Cummings (1990a) correctly predicted that drifting, rather than thrusting (cf. Porter and McKibbin, 1988), is more consistent with the essentially conservative attitudes of business school faculty toward change.

Why inert? It is said that there are three kinds of people: those who make things happen, those who watch things happen, and those who wonder what happened (cf. Hambrick, 1994: 16). In our management academia, we see this pattern too. If I am allowed to make a rough estimate, I would say at least 50 per cent of management researchers are those who have no idea about the irrelevance problem or don't understand why, who are mainly PhD students and junior researchers; another 45 per cent are watching this irrelevance going on, some (say 80 per cent) don't bother (e.g., those who are close to their tenure) while some (say 20 per cent) resist to change (e.g., those tenured and powerful). So only 5 per cent scholars support change and call for action. However, among this 5 per cent, it seems to me 90 per cent of them do not see the root cause of the irrelevance problem and have just come up with many '*zhi biao bu zhi ben*' solutions, such as call for academic-practitioner partnership, using user-friendly writing style, and call for 'research advisory boards' (Aldag and Fuller, 1995), etc. Since these '*zhi biao bu zhi ben*' solutions do not address the root cause of the problem, they are doomed to failure. The reality has proven this by not showing any sign of improvement. Therefore, only 10 per cent of the 5 per cent call-for-change scholars (e.g., Susman and Evered, 1978; Romme, 2003; Gosling and Mintzberg, 2004;, Mintzberg, 2004; Ghoshal, 2005; Pfeffer, 2005; and Bellizzi, 2008) have found the disease of the irrelevance, i.e., the 'scientific model', and called for completely abandon it.

It is of little hope to expect this 0.5 per cent can shaken the rest 99.5 per cent, let alone the powerful resistance from those the 20 per cent of the 45 per cent who are watching and standing-by, i.e., 0.5% < 9%. Even if we presume that there is no resistance for change, the 5 per cent people's can still not effectively set a change in motion due to a dilemma, namely, even if individuals are not change-resistant, they normally wait and see what *others* do, i.e., if they don't change then I won't because in an academic community conformity is valued. On the other hand, if individuals do not take action, the whole system won't change in any substantial way, then the above-referred *others* won't change. It is a dead-circulation.

Vermeulen (2005, 2007) encourages individual responsiveness in this change call and he hopes 'some relative simple changes … could set in motion a chain of systemic reactions that just might alter our world' (Vermeulen, 2005: 981). Due to the dilemma, we could not expect this chain reaction to happen. Management academia today is like a huge naked carnival. Here, we are all naked. Not only the king, but also everyone else is! All of us don't have the clothes called relevance. I argue, the only way out and the urgent task for us who care about the prospect of management academia is rather to make voices, the more the better, to shout to the *others*, in order to awaken our long-lost sense of shame! We need the clothes now!

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