On synthesis of relevance and rigor: A critique of Freek Vermeulen's solution

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Abstract

This paper presents a critique of Freek Vermeulen's synthesis of rigor and relevance in management research, and argues (1) at the first glance, Vermeulen's papers are very appealing; (2) but with a closer scrutiny, we can unveil the weak and shaky foundations of his argument; (3) as a consequence, his solution of 'adding a second loop' to make management research meet dual needs of rigor and relevance is illusory and merely an applied science fiction; (4) and finally, there are two real contributions of his papers to the irrelevance debate, but they are not like what we might have thought.

Keywords: management research, synthesis, relevance, rigor, critique

Vermeulen (2005) is a very special paper appeared in *Academy of Management Journal* (*AMJ*). It is special, as I feel, because of two reasons. For one, it is neither an empirical nor a theoretical[1], rather a more philosophic, piece of research to be published in *AMJ*, a leading academic journal 'publishing high-quality empirical management research' (Ireland, 2008: 9), which is said all articles published in *AMJ* 'must make strong empirical contributions' and 'must also make strong theoretical contributions' (The *AMJ* editorial team, 2008: 1043). For another, given the fact that there has been a hot debate of the relevance of current management research at large and that this paper is a 'From the Editors' article, it must be a very cautious and serious decision for *AMJ* to publish such a short (less than 4 pages) philosophic essay (i.e., Vermeulen, 2005) to engage the debate. Maybe due to this unusual specialness, *AMJ* followed up a second paper (Vermeulen, 2007), and again as a 'From the Editors' article, to offer the author an opportunity to extend his initial argument with a balance of theoretical analysis and supporting evidence[2], and probably in the same time to justify *AMJ*'s initial decision of publishing the first article[3].

1. The appeal of Vermeulen's paper

It was my firm belief that there must be some deep insight or beauty in such a short philosophic essay (i.e., Vermeulen, 2005) to meet the expectations of *AMJ*'s editors[4] who have highly sophisticated minds yet critical eyes. I read through the text, and quickly had an answer[5] where the appeal of this paper is.

The attractiveness of Vermeulen (2005) is based on two tactics. One is his borrowing of Hegel's Dialectic Theory to reconcile the relevance-versus-rigor debate. Another is his slick handling of the call for change of the management academia. Let me explain these each tactic in more detail.

1.1 Vermeulen's reconciliation of rigor versus relevance

According to him, from a Dialectic Theoretical perspective (Hegel, 1812, 1830), the old trade-school model of management education in the first half of 20th century, which focused almost exclusively on vocational skills and lacked systematic research, can be seen as the initial thesis. The antithesis is the current 'scientific model' (cf. Bennis and O'Toole, 2005; Ghoshal, 2005; Thompson, 1956) of management education with promoting rigorous academic research, which, since the late 1950s, initiated 'a much-needed shift…toward academic rigo' and 'gradually crowded out much…even *most* [italic in original]…of the research's relevance' (Vermeulen, 2005: 979).

The current state of management research, according to Vermeulen, is much like 'the glass bead game'[6] that is characterized as 'sublime and aristocratic…not consciously serving something greater or profounder than itself. Rather, it tends somewhat toward smugness and self-praise, toward the cultivation and elaboration of intellectual specialism' (originally from Hesse (1943: 329), cited in Vermeulen (2005: 979)). Consequently, many people have called for change of our academic system and culture (Vermeulen, 2007: 754). Although Vermeulen agrees with those people there is a need for change, he does not support the idea of swinging back to the outdated trade-school model, i.e., relevance without rigor[7], because he sees non-rigorous research that 'would not pass the standards for acceptance[8] of' academic journals 'cannot be considered relevant', no matter how 'potentially interesting indeed' (Vermeulen, 2005: 979). Straightly speaking, what the synthesis in him mind is not something 'achieved by finding some balance between the two' (p. 979; cf. Staw, 1995), i.e., the thesis and the antithesis; instead, a transcendent reconciliation 'at a higher level of abstraction and understanding' of 'these seemingly opposing ends of rigor and relevance', put precisely, relevance without sacrificing rigor (p. 979).

Vermeulen (2005: 979) believes the researchers *per se* should not to blame for the lack of relevance of their rigorous research and the problem lies at the selection of research question in the first place, in other words, 'academic answers often lack practical meaning because the questions that were asked to start with lacked relevance'. In essence, this implies that scientific rigor *per se* has nothing to do with the outcome of management research lacking practical relevance[9]. To him, what makes a research one that 'would provide most value' (p. 979) is 'asking questions that are of importance to reality, while not making concessions in terms of rigor in developing theory and empirical evidence', i.e., both rigor and relevance.

1.2 Vermeulen's solution to change

Vermeulen (2005: 980) admits his 'appeal to synthesize rigor and relevance' will not 'change the behavior of academic researchers, at least of those who agree with', neither will 'any of the previous pleas for relevance'. The reason for his assertion is his conviction that '[t]he only way to change the attitude and behavior of people is to change the system that they operate in' (cf. Coleman, 1993; Ghoshal and Bartlett, 1997) and 'currently it [i.e., the management education/research system] does not do so [i.e., support and appreciate practical relevance of research]' (Vermeulen, 2005: 980).

Therefore, he 'do[es] not want to make more suggestions' about how to make change happen at a collective level because he trusts 'all such suggestions are likely to be futile until a systemic change is brought about that assures that, at the end of the cycle, practitioners also become valued as recipients of the knowledge that we produce' (ibid.).

Given the predictable complexity and daunting task of 'changing a system', Vermeulen thinks it is 'up to individual researchers to figure out what they want to examine' as long as their research is 'with a clear zest to tackle a real practical question while searching for the answer in a rigorous way', by doing which 'management [research] really can make things better' (p. 981). Hence, he feels 'this shift should [not] change the role of our academic journals', because the role of academic journals is 'to assure rigor (as they do)' of academic research. His solution to assure publication of practically relevant researches then is to have 'a different, separate track' of journals for managerial practitioners (p. 980)[10].

2. Vermeulen's synthesis: founded on sands

At the first glance, it seems Vermeulen's (2005) synthesis is seamless and delicate as so I felt initially. However, it is not difficult to spot the the weak and shaky foundation of his synthesis with a careful scrutiny. I will explain two aspects as follows.

2.1 His analysis does not touch on the root cause of the irrelevance problem

To him, the only reason for the problem is that many (not all) researchers have chosen the wrong research questions irrelevant to the practice, not because of, but despite of, the rigorous way they conduct the research. Therefore, to solve this irrelevance problem, we need 'to tackle a real practical question while searching for the answer in a rigorous way'. He thinks 'this is where the real lesson…lies' (p. 981). *It is so simple and easy*! Andrew Pettigrew would be astonished if he has read Vermeulen (2005) because it essentially implies that we as management academics should feel relaxed and carry on what we have been doing as if the 'Double Hurdles' (Pettigrew, 1997, 2001) were not existing any more. And many other scholars would be shocked how this gap of rigor and relevance can be quick bridged as if it were really not a gulf (e.g., 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; Jarzabkowski and Giulietti, 2007; Johns, 1993; Kondrat, 1992; Løwendahl and Revang, 1998; Mowday, 1997b; Nicolai, 2004; Nicolai and Seidl, 2007, 2008; Rynes, Bartunek, and Daft, 2001; Rynes, Colbert, and Brown, 2002; Sutton, 2004; Starkey and Madan, 2001; Tranfield and Starkey, 1998; Van de Ven and Johnson, 2006; Whitley, 1995; Wren, Buckley, and Michaelsen,1994).

However, while I hate to, but I have to say, this analysis of the cause of the irrelevance problem is naive, at best, and ignorant, at worst. If Vermeulen had read Mintzberg (2004), Bennis and O'Toole (2005), and Ghoshal (2005) carefully[11], he should have understood the route is 'the scientific model' of management studies *per se* we as academics have collectively adopted and pursued since the late 1950s (cf. Thompson, 1956).

Eschew this biased use of these works for a moment, and let us follow his argument first. Then, I cannot help wondering 'what is exactly a real practical question, in his mind?'. Is it really the case that most (if not all) academics do not choose real practical questions? I am not sure about this. But one thing I am really sure about is that every researcher can (if he/she wishes) justify his/her research is of a real practical question: on the one hand, it is easy to do so if the research question is an empirical one; on the other hand, s/he can also justify it is *of real practical use* if a theoretically- and even methodologically-derived research question. For the second case, we as intelligent academics can justify any kind of abstract research as scientifically rigorous by resorting to ontology, epistemology, and methodology something laymen can hardly understand, let alone debate with. I think most clever academics do this, and I don't lack of evidence! Because it is justified as scientific rigorous, then it must be genuinely good theory[12], then it must be potentially practical/relevant, because we can always legitimate this logic chain by quoting Kurt Lewin's (1946) well-known as well as well-used argument 'nothing is so practical as a good theory' (cf. Van de Ven, 1989)[13]. So Vermeulen's analysis is unconvincing and easily defeatable.

2.2 His conviction of scientific rigor is the very vulnerable to attach

His insistence on using rigorous methods to research the real practical questions is also a target for serious attack. Again, if he had carefully read[14] Chia (2005), Hayek (1942, 1943, 1974), Susman and Evered (1978), and Whitley (1995), to name but a few, he 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).

If it seems a bit difficult to fully apprehend the central thesis of Hayek's essays, namely, using scientific methods to study organized complexity such like management and economy 'is decidedly unscientific' (Hayek, 1974: 3), now, let me switch to an easy topic which I trust laymen can understand – about using references in writing academic management article.

Borrowed from natural science, it is a norm in management studies that we as academics should use academic references to frame and support our argumentation in writing. It is perfectly understandable there are at least two reasons for using academic references, probably as many as possible. The first is to acknowledge other scholars' contribution and to avoid plagiarism. The second is to legitimate your analysis and thesis by referring to other published theories and arguments, which is what Nicolai and Seidl (2007) call 'legitimative/symbolic' use of academic knowledge[15].

In theory, both the two reasons are reasonable and valuable. However, in practice, the need of legitimative use of prior academic knowledge may become a straightjacket for many (if not all) management academics. Say, PhD training. It is quite likely that PhD students are advised to use published academic references as many as possible because most PhD students have no established research record therefore no academic credentials so that they must vastly use others' theories and empirical studies to legitimate their own PhD thesis[16]. This logic applied to the junior and mid-career (and even some senior) management scholars too. There seems to be nothing wrong with it. Right, but let's see the other side of the story. It won't be long before the young scholars, who may have been tired and frustrated with racking their brains to search and dig for any published references useful for their purposes, to find out the fact that it seems this troublesome norm does not apply (at least evenly) to those well-known senior professors. There is no lack of titles on the list of well-known academics' publications which use few references.

I wonder if this double-standard practice is against or for the claim of being scientifically rigorous. Paradoxically, I think it is both against and for: it is for scientism to put this straightjacket on the junior academics to force them to firmly ground their writing in published theories and empirical evidences; in the meantime, it is against scientism to relax this constraint on the established academics.

If someone is to dispute me by denying this unspoken norm and practice[17], fine, then I will compromise but to raise another related question: why on earth should we diligently seek references to legitimate our argument although I have no problem with the need for acknowledging others' contribution and avoiding plagiarism. In natural science, I can always refer to Newton's theory of universal gravitation to make an undisputable prediction that apple will fall from the tree to the ground. We all understand, in the world of Newtonian physics, that is a universal truth. However, in social science, for instance, how can we be assured by referring to Hofstede's national cultural differences theory we can *therefore* make undisputable claim that Spain is a quite famine society while Ireland a masculine one (see the debate between McSweeney and Hofsted, cf. Hofsted, 1980, 2002, McSweeney, 2002a, 2002b). More seriously, by referring to many Washington-Consensus-based theories, how can we be assured that the Shock Therapy derived from it can *therefore* rescue rather than devastate the post-Communist Russia, Poland and Czechoslovakia? Isn't it the power in reasoning is more powerful than the power in legitimation by referring to some so-called social *scientific* theories? Think about it, seriously, with mentality and morality!

3. Vermeulen's two-loops model of communication: An applied science fiction

3.1 An applied science fiction

Nicolai (2004: 957) argues, from a social systems theoretical perspective (Luhmann, 1982, 1984,1995, 1998, 2002, 2005, 2006), that due to the self-referential logic of the science system, there is a natural tendency or force to repeatedly separate scientific and practical communications from each other, as described as 'evolutionary drift' by Van de Ven (2000: 395). Due to the self-referential nature of any social systems (Luhmann, various years), 'it is improbable that a publication is 'connectable' to scientific discourse as well as to practice' (Nicolai, 2004: 957).

According to Nicolai (2004: 958), any claim that a particular academic management publication meets the duality of being both scientific rigorous and practical relevance is ultimately an 'Applied Science Fiction' (ASF), an illusion in which people mistakenly believe a linear chain of knowledge creation and diffusion in applied science really has happened. On the linear chain of knowledge, scientific knowledge is generated by rigorous academic research first and then is disseminated into practice. However, this believe, despite widely held in management field, is merely an illusion or fiction, precisely because it is contradictory to the Social Systems Theory and its fundamental notion of the self-referential nature of any social systems, such as science, art, legal, practice and even society itself, etc.

One well known ASF that Nicolai singles out is the case of Michael Porter's (1980) *Competitive Strategy* book, Porter's book and the 5-forces model have been widely used in academic and managerial discourses for more than a quarter century. Therefore some scholars would use it as a convenient and convincing evidence to justify the linear knowledge chain model, namely, Michael Porter, as an academic researcher, has done a scientifically rigorous research and generated the theoretical model and his theory has stood the test of practice and time, so it is one piece of business school research with both rigor and relevance. Most of us would take this for granted. However, in fact this is a merely ASF, i.e., an illusion.

In an interview (cf. Argres and McGahan, 2002), Michael Porter told the truth about his book. When Porter first finished his book, the book was completely seen as NOT rigorous from both Economics Department and Business School of Harvard University. The economists harped at him 'how did you derive those particular five forces? Why are these particular variables believed to be the most fundamental…?' and challenged his generic strategies model 'where are the statistical tests? What is the [economic] model?'. While his business school colleagues saw his generic strategies theory 'too abstract' and 'we can't generalize'. However, his book 'started to get a lot of traction in the practitioner community' and from his students. Gradually his book got widely accepted by the practicing world and in classrooms, as a result it was accepted by the academia.

In addition, I have found another such an ASF: the story of Jay Barney's (1991) *Firm Resources and Sustained Competitive Advantage* paper. Barney's 1991 *Journal of Management* (*JOM)* paper is a seminal research for the widely-followed Resource-Based View (RBV) approach to the studies of the firm and strategy. To many less informed strategic management scholars, Barney's VRIN[18] framework must be both rigorus and relevant to justify its cornerstone status of the RBV school with a more than 20 year history of development and adoption. However, in fact, this is again an illusion (ASF). This very important piece of research 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 (Smith, 2008: 306). We could speculate if the reason for initial rejection of his paper by *AMR* and *SMJ*, e.g., lack of scientific rigor, lack of empirical evidence, etc. Anyway, *JOM* accepted his paper (though by Barney himself), and more importantly, with more than 20 years development, RBV today is the dominant paradigm in strategic management field, being one of the kernel paper of this paradigm, most of strategic management scholar might trust this theory is well supported by empirical evidences. Unfortunately, some people have charged Barney's (1991) VRIN theory is essentially tautology (Priem and Butler, 2001; Williamson, 1999). More seriously, Newbert's (2007) first ever 'systematic assessment of the RBV's level of empirical support' found that 'the RBV [as a whole] has received only modest support overall and that this support varies considerably with the independent variable and theoretical approach employed'. Alas! What an inconvenient truth, to borrow Al Gore's phrase (cf. Gore, 2006).

In Vermeulen (2007: 755), he identified several scholarly rigorous research projects that, as he believe, are also practically relevant. I admit those works are well-known pieces of research in management field, but I am not sure how they are accepted in business world. It is beyond the scope of this paper to analyze whether Vermeulen has just seen an illusion, i.e., ASF. One thing I can recommend him to reexamine his claims is just to compare these works with Michael Porter's and Jay Barney's stories to see if those projects have transcended Porter's and Barney's works to be some genuine success stories[19].

3.2 Vermeulen's two-loops model of communication: nice try, but an ASF

In Vermeulen (2007), he made a concrete explanation how to make management research 'to matter more' or 'shall not remain insignificant' (p. 754). Briefly, his solution is 'adding a second loop', namely, 'Loop 2: Relevance', to 'Loop 1: Rigor'. By 'loop 1: rigor', he refers to the current dominant paradigm in management research, i.e., most researchers are solely doing research in an academic way – read academic journals, derive research questions from the reading, write scholarly research article aimed at almost exclusively other academics – they stay out of touch to the management practice.

To him, the second 'loop 2: relevance' functions in this order: individual researcher makes 'regular, direct interaction with practitioners intended to enrich understanding of [his/her] research subject' by *conducting case interviews* and *writing teaching cases*, and then 'try to force [himself/herself] to communicate with managers about [his/her] research and *the insights it provides* – for instance, through executive education and managerial articles' (p. 757). In so doing, management academics can 'use [academic] research to help practitioners learn'[20] (p. 759). By adding the second loop 'will stimulate me to examine issues that are also of interest to them and to ask more relevant questions about those issues', therefore, to perfectly synthesize rigor and relevance. Simple and easy, end of story!

Although I have to admit his synthesis is perfectly easy to understand, for me, it is just too simple to be convincing me not to take a deeper thinking about it. Thanks for my instinct and rethinking, the loopholes on his second loop gradually became visible. Let me share with you my observation by starting with his 'first element of the second loop: regular, direct interaction with practitioners intended to enrich understanding of my research subject'.

It is crystal-clear that his intention is to *enrich* understanding of *his research subject*. Seems nothing wrong? Indeed, nothing wrong with *enriching*, but the problem I see is the point of departure – to enrich understanding of *his research subject*, rather than *real practical problems*. As academics, we all know our management education and research training are almost exclusively academically- and discipline-based. Except few, most of the young, mid-career and even some of the senior academic researchers start research career without any prior real managerial experience. Then our strength and core competence is 'doing research to create *scientific* [italic added] knowledge' (Cummings, 2007:356). Not *practical* knowledge! So the young scholars, although cannot be compared with established researchers, still have a core competence of *applying academic knowledge* to frame our understanding of management phenomena[21].

Therefore, before we go to conduct our case interviews, we should utilize our core competence, i.e., discipline-based and academically-derived '*scientific knowledge*' to frame our initial understanding of the research subject, derive a research question, and design a structured or semi-structured interview *accordingly*. Aha! Have you seen what the problem here is? It is there is an innate tendency and '*strength*' to frame our *academic* understanding of what we want and how to research, so we are naturally, normally and even proudly pre-equipped with an initial *academic* understanding[22] of the research project before we go interviewing our accessible practitioners[23]. So it is for us to enrich *rather than reach* our understanding of our research subject/question.

In short, Vermeulen's first element of the 'loop 2: relevance' cannot guarantee relevance. Simply because, academically-minded researchers *do interviews* (the available, not necessarily the suitable, of course) *to selectively enrich or enhance* their understanding of the research questions they derived academically in the first place, in that, if the interviewees confirm his preliminary (some people may be definitive) understanding, then by conducting interviews whatever they can get access, their *academic* understanding is ensured and claimed as '*scientific*' knowledge. But if the interviews contradict their academic knowledge, then the only choice is to either discard or selectively use (i.e., enrich) the data collected in the interviews. There won't be a lack of good reasons to believe that the contradiction-type interviews make up the vast majority of all academic-led interviews[24].

Someone may question what if a researcher go interviewing without a preliminary academic makeup and adopt an unstructured interview, will his/her interaction with practitioners (again whoever accessible) guarantee relevant knowledge? Well, eschew the argument that most people will have some at least deeply embedded and taken-for-granted belief therefore we can hardly be non-pre-minded, the answer may still be, I am afraid, a big No! Why? Because even if the researcher in question is not academically pre-minded, we can still not expect s/he can learn much from the interaction for several reasons. Firstly, how much will the interviewers tell you? Don't expect the interviewers will be so patiently teaching you what they really do. Why should the practitioners teach someone with a decent title yet without basic managerial experience? Why should they spend this time on teaching university academics without any benefit while at much cost of time? Someone may say, but my research will generate *scientific* knowledge and ultimately benefit the business world including you. My dear, be realistic. They know you are doing irrelevant researches. You can fool them once, probably twice, the third time, No[25]! Secondly, in case some warm-hearted practitioners do want to teach you, then the question comes how much you can learn. It is not that straightforward to digest and absorb what is heard from a practitioner who is down-to-earth and so practical, i.e., non-academic, non-theoretical, when the audience has no real managerial experience. Someone may argue, I have learnt much, really a lot, in my BSc, MBA, and PhD management courses, I should have no problem to understand conversation of management topics. Again, my dear, be realistic, the business world is far more complex, fuzzy, and unpredictable than whatever your business school management education can teach. Lastly, if they want to teach and if you may understand, but how much can the practitioners explicitly explain to you because much of what the real practicing knowledge is tacit, learnt by getting your hands dirty, and perfected by practicing again and again. In short, no matter how you come to an interview and with what purpose, as an academic being outside the real practice for most of his/her time, you simply cannot have or *enrich* much real practical management knowledge. Unfortunately!

With respect to his second element of the ' loop 2: relevance', which is for the same researchers who have done the case interviews to try to communicate with managers about their researches and the scientific knowledge these researches arrived, from above detailed analysis, it is obviously it is of little use for managers to learn your scientific knowledge (as the reality shows) simply it is not practical, no relevance to the needs of the practitioners who face very complex, fuzzy, and unpredictable business reality. This is evident as James G. March, a leading influential management thinker, once comments that '[m]odern organized life poses problems that are not trivial… [d]ealing with the simultaneous demands for self-respects, autonomy, control, coordination, order, freedom, imagination, discipline, and effectiveness that are essential parts of modern organizations seems to me worthy of respect, even when it is done in a less than perfect way' (Coutu, 2006: 89).

From his second dimension of the 'loop 2: relevance', I have not only NOT seen any relevance necessarily automatically generated, but also ONLY seen what Sutton (2004: 28) described as 'ignorance…paired with arrogance', precisely due to a blind belief in what Nicolai (2004: 953) calls 'the aqueduct model', or 'the linear model' by some others, of knowledge chain, in which scientific knowledge is believed to be on a higher level than practical knowledge (cf. Camerer, 1985). In Vermeulen (2007), I see such ignorance because we as academics normally lack of knowledge about what and how the real business and management are going on there. I also see such arrogance because Vermeulen's second dimension of his added-loop of relevance is for us as management academics to teach and 'help practitioners to learn' (Vermeulen, 2007: 759) about the scientific knowledge we produced by doing academic research.

If you think I am too judgmental about him in this respect, please read the section of 'smell the beast' of Vermeulen (2007). In that section, he tells us how he defends himself when challenged by 'nonacademics who learn that [he] work [s] at a business school [and] ask [him] whether [he] should not gain real work experience [himself]' (p. 756). Surprisingly, his answer usually is that he studies business managers like a zoologist might study mountain gorillas, and similarly, like a perfectly good criminologist might study criminals. His analogy is you do not have to have been a gorilla yourself to understand them and similarly you do not have to have stabbed someone to understand the criminals who do so.

I can defeat his analogy by pointing out that there is a logical defect, namely, criminology is different from management studies as the former is aimed to help get rid of criminal practice while the later to help strengthen management practice; and also if you want to get rid of crimes then you as criminologists have no any need of ever having stabbed someone while if you want to advice your customers/audience, i.e., criminals on improving their skills, you would better have had some stabbing experience, at least some inhuman items like a sandbag, like boxing coaches do. Nevertheless, I prefer not to have the luxury to defeat him as I now feel very fortunate not to be that poor nonacademic guy to ask such a provocative question to him, otherwise, I would have been treated as either a mountain gorilla or a criminal therefore I would have to be so hesitant to choose which one I would better to be seen by him. It is a really dilemma: in reality, a nonacademic businessman is much clever and sophisticated than a mountain gorilla and his business world is much complex, fuzzy and unpredictable than the mountain; so he would better choose to be a criminal who is more close to the reality, namely, the criminals are much clever and sophisticated than mountain gorillas and their world is much more complex, fuzzy and unpredictable than the gorillas' mountain. But I don't know how many business people would like to be treated like that, in the eyes of the business school academics!

4. What is the real contribution of Vermeulen's solution

4.1 His synthesis of rigor and relevance: it is actually barren

The real contribution is not his synthesis of rigor and relevance. On the contrary, his synthesis is actually a barren idea distantly remote to what he has self-claimed 'a higher level of abstraction and understanding' (Vermeulen, 2005: 979). To be honest, given the fact that this paper is published in a top-level scholarly management journal (i.e., *AMJ*) and his position[26] as a current member of the editorial board and former member of the advisory board of the *AMJ*, which may very likely give much credential to his papers immediately, if I haven't misinterpreted what he wrote in his first paper with respect to his *synthesis* of rigor and relevance, or the two models of business management education, then, in my opinion, Vermeulen really owes a big apology to G. W. F. Hegel for his misapplication of the Dialectic Theory.

It is correct for him to label the two models of management education/research as thesis and antithesis. What is wrong with Vermeulen (2005) is his synthesis is not only *NOT* 'a higher level of abstraction and understanding' of the two opposing models, but also *NOT* different from the antithesis, the current 'scientific model', at all.

His two-loops model of communication or interaction between the two communities of management academics and management practitioners community is exactly what the reality is and has always been. So, he did not 'add' anything into the current paradigm. If we presume the claim true that he did add the second loop, i.e., 'Loop 2: Relevance', into the current paradigm, then we have to accept the corollary of this claim that we only had one loop, i.e., 'Loop 1: Rigor' before and now. Is that true? Of course, that is absurd! If we had only had the first loop, i.e., 'Loop 1: Rigor', then by Vermeulen's Figure 1 we would have been completely living isolated to and insulated by the business community. If so, we would never have been able to have Michael Porter's (1980) *Competitive Strategy* book in today's bookshops, and also we would never have Ansoff's (1965) Product-Market Matrix, Miles and Snow's (1978) typology of strategy, McKinsey's 7-S-Framework (Peters and Waterman, 1982), which Nicolai and Seidl (2007: 4; 2008) categorize as practitioners' contributions having penetrated into academia, appeared in our management textbooks.

Even if we assume his claim true for a while as if we were only doing the research in the first loop, i.e., 'Loop 1: Rigor', then it is still not his contribution to advise us to add a second loop because many people have long before said that. Isn't Kurt Lewin's (1946) call for action research *a*[27] well-established solution to combine practical relevance and academic rigor (cf. Argyris, 1970, 1980; Argyris, Putnam and McLain Smith, 1985; Argyris and Schön, 1974, 1978, 1996)? Then what is new about Vermeulen's (2007) idea of 'adding a second loop to matter more' (p. 754). Nothing!

4.2 The real contribution of Vermeulen's solution

Based on the analysis above, the audience of this paper might be wrestling with one puzzling question: if Vermeulen's two papers are full of defects as the aforementioned implies, how come the well-respected *AMJ*'s editors accepted and published them? Good question!

It took me several days and short-of-sleep nights to find out a plausible reason. About the following analysis, you may treat as merely my conjecture or the truth. Whatever, so far, this is the only answer I can come up with. I think the real contribution of Vermeulen's papers is twofold. The first is his solution is the best of the class (at the time of publication) which 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'. The second is his papers offer the most magical solution of paradigm maintenance.

His synthesis of the rigor and relevance is a '*zhi biao bu zhi ben*' solution because he suggests to change our individual academic's research behavior and attitude without calling for systemic change of orthodox and culture of the management academia, which the latter is precisely the root cause of the irrelevance problem while the individual's behavior and attitude only a symptom or result of this root cause. His solution is the best of the class because it, with such a short length and a plain language, is relatively better than all previous calls for collective action at once. Vermeulen (2005: 980) rightly points out '[t]he only way to change the attitude and behavior of people [at the collective level] is to change the system that they operate in'. however, he completely understands that 'changing a system usually requires more than just changing incentives; it also requires adaptations of culture, people, and more'. It is far more complex and difficult than any of those suggesting collective action could have imagined. So Vermeulen's solution, i.e., encouraging change at individual level, is better than any other in his class of '*zhi biao bu zhi ben*' solutions.

Don't get me wrong. I say his solution is the best of his class, not *the best* solution we can have. It is beyond the scope of this paper to present my own solution which I have done it elsewhere (not yet published). Let me give you a brief idea why his solution is not what I fancy: he has not seen a *dilemma* built in his solution. The dilemma is: although we cannot imagine change to happen at the collective level right now or in anytime without actions at individual level, however, you cannot succeed in encouraging individual to change their attitudes and behaviors without showing the sign that collective action (at least to an enough degree) is taking place. Therefore, his ideal 'some relative simple changes … could set in motion a chain of systemic reactions that just might alter our world' (Vermeulen, 2005: 981) is ambitious yet naive.

Now comes to the magical power of his solution to maintain the paradigm: the scientific model of management studies we have been living with since late 1950s (cf. Thompson, 1956). The magic is very delicately designed. By showing other people this is the best solution so far (of course in his '*zhi biao bu zhi ben*' class in which most who have made a suggestion for change, except Bennis and O'Toole (2005), Chia (2005), Ghoshal (2005), Hayek (1942, 1943, 1974), Mintzberg (2004), Susman and Evered (1978), and Whitley (1995), among others), most people would be allured to his solution for change, but due to the dilemma built in his solution, then no substantial change will effectively happen. Isn't this what most of us really want: showing responsiveness yet doing nothing?

Although I completely don't agree with his solution and his reasoning, I have to say, I have definitely surrendered to the artistry of skillful and tactful protecting his and many others' beliefs in the orthodox, i.e., 'the scientific model'. I think *AMJ*'s editors of Vermeulen's two articles have done a great job to help him to design such a master piece, because I always think, why not to have some interesting and inspiring papers appearing in mainstream journals that normally publish technical and abstract works? James March once contends 'a basic goal in writing is to choose words that can evoke beautiful and useful meanings that were not explicit in your own mind' (Coutu, 2006: 89). I think Vermeulen's two *AMJ* papers are just a case of such inspiring writing. Congratulations to *AMJ*!

NOTE

[1] Although Vermeulen (2005) uses Hegel's Dialectic Theory to frame his argument, it is a philosophical theory, rather than a management one. So I argue it is neither an empirical (i.e., no any empirical data involved) nor a theoretical paper (i.e., no any existing management theory used).

[2] I am quite careful in choosing the word of 'theoretical analysis' rather than 'theory' and the word of 'supporting evidence' as opposed to 'empirical evidence'. This is because, in my opinion, Vermeulen (2007) is still not grounded in well-established management theories. His analysis of 'the features of relevance', though interesting, still falls somewhat short of such thing that can be called a theory. The references he chose to support his analysis are therefore not empirical evidence.

[3] From Rynes (2007: 745), we understand that Vermeulen (2007) was an invited essay by the former *AMJ* editor Sara L. Rynes (2005-2007) to address 'combining rigor and relevance' during the First Annual Sumantra Ghoshal Research Conference at London Business School in May 2007 where Freek Vermeulen was an organizer of that conference as well as an *AMJ* board member.

[4] According to an email I received from the current *AMJ* editor R. Duane Ireland (2008-2010), neither of these two papers (i.e., Vermeulen, 2005, 2007) were double-blind reviewed because they were 'From the Editors' essays.

[5] Please notice the word I use is 'an answer' not 'the answer'. You will find out why I make this distinction later.

[6] This phrase is borrowed from Nobel Prize laureate Hermann Hesse's novel of the same name.

[7] In fact, few (if not no) people have proposed such an idea, as far as I know.

[8] Mowday (1997) expresses his concern about using acceptance as a criterion to assess if our research is relevant. Because he believes that 'some of the most important research throughout history was not well accepted at the time it was conducted' and he worries that research following fads may be quite accepted but have dangerous consequences (cf. Aldag, 1997). Jay Barney's (1991) seminal RBV (resource based view of the firm) paper (cf.

Smith, 2008: 306) and Michael Porter's (1980) landmark Competitive Strategy book (cf. Argres and McGahan, 2002) are two typical evidences to the belief that some very important research may not be accepted by the time of their birth.

[9] If both the researchers and the scientifically rigorous research of management topics should not to blame, then there is nothing to blame except the damn research questions *per se* (the reader may start to wonder who designed the irrelevant questions and why on earth do irrelevant research? Me too!)

[10] I wonder, isn't the current publishing practice exactly like this, only in the strict sense that we have academic journals like *Academy of Management Journal*, and we also have managerial journals like *Academy of Management Executive*. Then what is the difference of Vermeulen's solution of having two separate tracks of journals from the reality?

[11] Actually he has read them and made reference to all these three works in his first paper (Vermeulen, 2005) while only made reference to Bennis and O'Toole (2005) in his second paper (Vermeulen, 2007). However, Vermeulen (2005) only *selectively* uses the descriptive part of these works, i.e., the statement of the resulting status quo of 'the scientific model' of management studies, namely, rigor gradually crowded out most relevance (p. 979), rather than the most valuable and critical part of these references, i.e., their analyses of the fundamental flaws of adopting 'the scientific model' in management education and inquiry (you will find another two cases of selective use of information later).

[12] This is in line with the principle and logic of science.

[13] People may also cite James March's (1995) argument 'good scholarship is relevant, and bad scholarship is not' (Schmotter, 1995: 3; cited in Mowday, 1997: 341).

[14] My guess is that he did read them but choose not to use them as these references contradict to his very belief (a case of selective use of academic knowledge).

[15] In this paper, sometimes I use 'academic' and 'scientific' interchangeably although they are fundamentally different. But sometimes I purposefully use the wording 'scientific' to connotate the truth-claim tendency in management studies.

[16] This is exactly what I and my fellow PhD students have experienced some years ago. In my own case, one of my PhD supervisors, who was a mid-career management scholar not yet tenured, once told me it was safe and in my own best interest to legitimate every my single point in my writing by finding any published references as many as possible, the more the better.

[17] Since it is unspoken, there is a lack of published (rigorously) empirical evidence in this respect. So any charge of this kind can be legitimately denied based on the scientific principle.

[18] VRIN is the acronym of 4 words: Valuable resources, Rare resources, Imperfectly imitable resources, and Non-substitutable resources, which are the resource base which gives a firm competitive advantage.

[19] To be honest, I am not sure about the answer. Yet, don't get me wrong. I do not necessarily intent to give you any hint in the comparison.

[20] Sutton (2004:28) vividly explains how ignorant and arrogant our business school professors who have lack of real business experience can be, when we interact with business practitioners who normally have only practical knowledge rather than academic theories. Vermeulen (2007) just confirms me this image of management academics. I will explain this later in the section 3.2.

[21] Here I use 'management phenomena' rather than 'management practice' to imply that what our academics see tend to be separate and isolated phenomena of management, say, corporate culture, leadership, governance structure, etc. while in reality what the business practitioners have to face and deal with is more complex, fuzzy and unpredictable management problems, which are, often and not rarely, rather an integrated organized complexity.

[22] Largely because we do not really have a practical understanding of the questions we are to address.

[23] Bear in mind there is an access issue. It is often rather than rare that the researchers cannot access the right person s/he needed, no matter junior or senior academics.

[24] Again, you can dismiss this argument as it seems to me there is no published rigorous empirical study on this issue.

[25] Bennis and O'Tool (2005) quote a renowned CEO's opinion that labels academic publishing a 'vast wasteland'.

[26] Source: the official resume of Freek Vermeulen, http://faculty.london.edu/fvermeulen/assets/documents/Resume_August_2008.pdf (Accessed on 14 January 2009)

[27] Here, I describe action research as *a* solution rather than *the* solution.

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