Managerial Perceptions and the Essence of the Managerial World: What is an Interloper Business Executive to Make of the Academic-Researcher Perceptions of Managers?

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I argue that the Mezias and Starbuck (2003) research programme misses being managerially relevant by investigating the extent of perceptual inaccuracies among managers rather than the more valuable question of from where these inaccuracies arise and what their consequences are in practice. As a sometime business executive, it seems to me that the closer one gets to an actual managerial position, the less significant some of the explanations and recommendations in the academic research literature appear as regards managerial concerns. To ensure relevance in the managerial research enterprise, I suggest that academics should make concrete efforts to become reasonably conversant with the managerial world before undertaking any empirical research. I also make the point that the discussion of the Mode 2 approach to knowledge production in the management field needs to move away from the current focus on straightforwardly ‘bridging the relevance gap’ and toward finding ways to improve the competence of Mode 2 researchers by insisting on a minimum understanding of the managerial world before embarking on management research.

Introduction

My comments on the paper by Mezias and Starbuck (2003) are in large measure influenced by our different perspectives. I was a business executive before entering the scholarly life, and my work experience of over 35 years is about equally divided between business and academia. It seems to me, not only on the basis of the Mezias and Starbuck piece but also other research on managerial behaviour, that the closer one gets to an actual managerial position, the less significant some of the explanations and recommendations in the academic research literature appear as regards managerial concerns.

The invitation of the Editor-in-Chief to submit a commentary on this paper provides me an occasion for reflecting on the way academics typically approach management research. My general reaction is that this kind of research, as exemplified by the Mezias and Starbuck paper, is somewhat misdirected in terms of relevance and significance in understanding managers and management, even as it is technically rigorous. This is sad, because the old criticism of business research, noted by Porter and McKibbin in their 1988 study, seems to be true even today: ‘Research being turned out by business schools is largely trivial and irrelevant … In other words, and most damning, it can
The analysis in that study, I think, fell short in one major respect, namely, it failed to consider the need for the researchers to be conversant with the subjects of their study – here, the managers. That report, commissioned by the American Assembly of Collegiate Schools of Business, did not give any thought to the question that realistic perceptions on the part of the academic researchers about managers ought to be a precondition for ensuring the ‘pertinence to key business and management issues of the problems chosen for study’ (p. 168). I am not arguing for relevance over rigour – for we need both – but rather for infusing relevance into academic research in managerial behaviour. I will argue that for such research to be of any consequence, both for practice and scholarship, the crucial factor is the realistic perception of the manager by the researcher. But first, let me make a few comments specific to the Mezias and Starbuck paper.

Comments on the contents of the Mezias and Starbuck paper

The Mezias and Starbuck paper makes the point that the perceptions of managers are still as inaccurate as studies found two decades ago. The authors note that the studies conducted in the 1970s seemed to indicate that managers’ perceptions of their organizations and their environments did not correlate with ‘objective’ measures, and that these perceptions of individual managers were inconsistent with one another. They were, moreover, ‘astonished’ at the lack of such research on perception accuracy ever since. Given this situation, Mezias and Starbuck proceeded to collect data from executive MBA students and, later, senior managers in a large firm, to assess the extent of perceptual inaccuracy against respective objective measures. The authors are ‘disappointed’ to find that managers’ perceptions are not more accurate than prior studies suggested, and that the accuracy level did not improve even when the managers became more familiar with the subject matter. I believe managers are acutely aware of their limitations, but given their responsibilities and driven by peer competition and ambitions, they willingly suffer from an illusion of manageability (Das and Teng, 1999). Some other comments follow, in no particular order.

First, Mezias and Starbuck bring together eleven factors that can lead to inaccurate perceptions, such as cognitive limitations, experience, organizational information systems, culture, organizational hierarchies, availability of public data and volatility of the environment (pp. 4–5). Curiously, however, none of these factors receive any attention later by the authors in the context of managerial decision-making. In fact, the two studies by Mezias and Starbuck indicate that experience (relating to sales in Study 1 and quality in Study 2) did not make any meaningful difference.

Second, Mezias and Starbuck suggest that perhaps the unsatisfactory results of prior studies on perception accuracy were due to confounding research methods and inaccurate ‘objective’ measures. They therefore set out to choose the variables that are relevant to managers and more objective measures against which the perception responses were to be evaluated, and hypothesized that managers’ perceptions are more accurate than prior studies suggested. The Mezias and Starbuck finding about the prevailing inaccuracy in perception is in fact not surprising at all. The ‘objective’ measures of the organizational properties that the authors examined in study 1 (pp. 7–10) were obtained from the reports of three or more colleagues of each manager (i.e. on the basis of perceptions, p. 8). Only 5 of the 70 participating managers in Study 1 had Standard Industrial Classification codes for their industries, so that the authors could not obtain pertinent objective government statistics (p. 8).

Not satisfied with the diverse types of jobs of the MBA students and the lack of proper managerial background in Study 1, Mezias and Starbuck launched Study 2 in a large firm and surveyed senior managers, this time focusing on one subject – quality improvement – that was important to all of them. The authors were disappointed to find that the extent of inaccuracy stayed almost the same (pp. 12–14). They felt it was particularly odd that many managers retrieved wrong numbers from formal reports for some of the questions in the research. The rationalization that Mezias and Starbuck give to legitimize the study setting is less than convincing. It is true that ‘managers typically do not read formal reports when they are being
interviewed by academics or participating in group decision-making’ (p. 5). But, in basing themselves on this fact, Mezias and Starbuck fail to differentiate between ‘perception’ and ‘memory’ as the variable they actually studied. Admittedly, these two concepts overlap, but the consciousness that memory may be wrong would probably prevent such failing memory from becoming part of the perception. As the authors found, managers admitted that they did not know when they were not sure about the information requested. First, it is well recognized that human decision-makers have certain memory limitations, including memory distortions, so that they cannot carry properly all the information with them in their heads. Second, managers do not need to memorize some or most of the necessary information. Even if the quality improvement programme was very important to the respondents, it seems plausible that there was no reason for them always to have the exact numbers ready with them. Finally, an awareness of their vague memories may induce managers, if they felt necessary, to refer to relevant sources in the process of making decisions.

Third, Mezias and Starbuck seem to regard sales ‘perception’ errors within 50% as moderate, possibly acceptable, and more than 200% extreme and unacceptable (p. 9). To say the least, such criteria are somewhat arbitrary, given especially that many of the respondents had little to do with sales at work. Studies on managerial perception may be more productive if their objective is to gain more understanding about the impact of perceptions, including the ubiquitous inaccurate ones, on the effectiveness of managerial decision-making, rather than an emphasis on calculating the mathematical percentages of inaccurate perceptions.

Fourth, in reflecting on the two studies reported, Mezias and Starbuck suspect that maybe people need not be accurate in perceptions in order to solve problems after all, and that the assumption of the importance of accurate perceptions need to be reconsidered (p. 15). This insight seems too little too late, for it could have, and I believe should have, led to more interesting research questions that have relevance to decision-making in practice. However, Mezias and Starbuck disappointingly revert to their original emphasis at the end of the paper to the effect that ‘the accuracy of managers’ perceptions is an important topic’ (p. 16). This is, to say the least, disconcerting, given all the data they analysed. Where is the data to conclude that the accuracy of perception is important? I should have thought that they would convincingly conclude that the inaccuracies of manager perceptions should be taken as a given, and that the more significant purpose of research in this area, as I will suggest presently, is to see where the inaccuracies arise from and what their consequences are in practice.

My last point concerns the proposal of Mezias and Starbuck for ‘robust organizations’ as the means to combat managers’ inaccurate perceptions (p. 11). Such organizations, they prescribe, should allocate data-gathering resources to where errors occur, provide more feedback on performance and avoid drastic innovations. Informational education is another remedy that Mezias and Starbuck prescribe for the managers. On the face of it, these appear to be promising suggestions. However, the real challenge is to understand the internal mechanics that individual managers actually employ (intentionally or not) to make decisions in the context of inaccurate perceptions. Moreover, such informational education and resource allocation can also be impacted upon, following an identical logic, by inaccurate perceptions of the very decision-makers (including academic researchers) responsible for ‘curing’ the perceptual inaccuracies of managers.

Some idea of the managerial world as a precondition for management research

As I see it, before one undertakes any academic research in management, including the topic of managerial perceptions, it should be a requirement that one has had some interactions with managers to have a minimal feel for the managerial life. One can read carefully the available writings by experienced managers or research monographs by those who have studied managers at close quarters. For instance, if one were to believe Chester Barnard – whose 1938 book, still very much in print, is arguably one of the most insightful books in management by a practitioner, trumping most academic research books – much of the decision making by managers, whether one believes their perceptions are sufficiently accurate or otherwise, consists of
not deciding. In his words: ‘The fine art of executive decision consists in not deciding questions that are not now pertinent, in not deciding prematurely, in not making decision that cannot be made effective, and in not making decisions that others should make’ (Barnard, 1938, p. 194; emphasis in original).

I continue to be puzzled, frankly, at the ‘picture’ many of my academic colleagues have of the managerial world, and often feel like an interloper among academic researchers who do not seem to value practitioner experience. It would seem their familiarity with the managerial world consisted primarily of ready anecdotes of executive doings, with a clear preference for name-dropping. Put simply, researchers make very little effort to acquire even a modicum of appreciation of the real-world managerial environment. The result can sometimes be seen in plainly vapid observations about managers and their milieu. By way of illustration, let me list a few banalities from Mezias and Starbuck, whose candour I find at once refreshing and disquieting: (1) in relation to the MBA student sample of Study 1, ‘some of the “managers” in Study 1 had had little managerial experience’, ‘we were surprised because we had taken the label “Executive” literally’, and the respondents ‘regarded some of these (survey questionnaire) variables as important and relevant to their jobs and others as irrelevant or unimportant’ (pp. 11–12); (2) ‘We discovered that using terminology that the managers actually used did not assure that they understood the terminology’ (p. 12); and (3) ‘The designation of specialists may be symbolic’ (p. 16). Because academic researchers such as Mezias and Starbuck have research and publication foremost in their mind – an admirable focus in itself – they routinely dive into the managerial world with their hypotheses and research instruments, etc. It is difficult for me to imagine researchers in other ‘applied’ and ‘practice-based’ disciplines akin to management, such as medicine and engineering, engaging in their research in the absence of some idea of what constitutes a patient or a building. So, here, the point is that management researchers ought to engage in research only after they have acquired some semblance of the managerial world.

For that to happen, I suggest that management researchers interact with managers to a much greater extent than I now see happening. I am not proposing the anthropological emic approach (à la Dalton, 1959), which may be both time-consuming and impractical, the latter because few academics would have the needed inclination and capabilities. An etic approach is what would generally be feasible, so that all that the researchers have to do is to devote some initial time and attention to learning about the world of management by at least conversing with managers. This they can do by spending time in business organizations and attending practitioner conferences. After all, if management students can obtain internships, then it may not be all that difficult for academic researchers to arrange temporary access to companies and conferences. Just as the medical researcher has to know something about the patient, management researchers should be conversant with managers and customers (in real life, that is, not the caricatures in college textbooks).

The consequence of this pre-sensitization approach would be a realistic appreciation of the variegated phenomenon known as managerial work in business organizations. Decision-making will then be appreciated as much more complex than can be eased by less inaccurate perceptions of information. Managers who make decisions need to be seen as, primarily, human beings, with all their frailties. It would then be clear that the perceptual inaccuracies that Mezias and Starbuck are interested in are an essential part of the managerial condition. As Sutcliffe (1994, p. 1374) observes, but only as a conjecture and only after carrying out a research project, ‘Having an accurate environmental map may be less important than having some map that brings order to the world and prompts action’.

So I would suggest that the starting point of management research should be the axiomatic fact that managers have perceptual inaccuracies and other ‘deficiencies’. To revert to the medical metaphor, there is not much point in medical researchers discovering after years of study that the human patients continue to be health-challenged. Perception researchers could easily learn most of the insights in the Mezias and Starbuck paper if they made the necessary effort to gain an appreciation of what it means to function as a manager. The interesting and significant research question would then become: in what ways managerial decision-making is affected by inaccurate perceptions and the nature
of the consequences of such ‘deficiencies’ in the light of known ‘objective’ conditions.

To buttress this point, I can do no better than describe the approach I adopted in my own early research that is related to the Mezias and Starbuck area of research. My research was on perceptions of consensus about planning objectives and planning horizons, as part of a larger study (see Das, 1986, for details). The critical point to note in this example is that it never alludes to executives not being ‘objective’, but rather investigates the managerial and organizational consequences of their perceptions (with the assumption that all managers, qua managers, necessarily have different perceptions).

Illustration of an early (1986) empirical study of managerial perceptions

The critical role of individual perceptions in organizational decision-making has long been acknowledged in the literature, such as in Anderson and Paine (1975). Graham (1968, p. 292) pointed out that ‘an individual’s reaction within a situation is a function of his perception of the situation rather than his interaction with a solitary combination of “real” stimuli and constraints’. Hence it would seem crucial, particularly in the area of plan implementation, for organizations to ensure that a sufficiently high degree of at-large consensus is perceived by all concerned.

I studied this topic of perceived consensus, as part of a larger investigation, with data gathered from 207 senior executives (out of 298 originally sampled) working at different levels in the headquarters of one of the largest commercial banks in the USA. The respondents consisted of 31 who had corporate titles of either Executive or Senior Vice President, 114 Vice Presidents, and 62 Assistant Vice Presidents. I found, as hypothesized, that the respondents perceived less agreement prevailing among organizational members concerning planning horizons than concerning planning objectives. As I have indicated elsewhere (Das, 1986, 1987, 1991), the planning period is in reality, though at an implicit level, a potent area for what can be termed as the perception-based ‘silent politics of time’. People negotiate and adapt objectives by ‘trading-off’ perceptions of the relative salience and depth of the temporal dimension.

My study also dealt with the perception of time among managers, and how these temporal perceptions affected their perceptions of consensus mentioned above. It was found that near-future oriented managers were more likely to perceive a higher degree of consensus about both planning objectives and planning horizons than managers who were more distant-future oriented. Another research question in that study related to Thompson’s (1967) model of means and ends, confirming that when agreement about objectives and horizons was perceived, there was a tendency to also perceive the use of standardized procedures in decision-making, whereas when disagreement was perceived about objectives and horizons, the tendency was to perceive the use of subjective and intuitive methods.

Hence, perceptions of (inadequate) consensus is a topic that advances our understanding of the managerial phenomenon. I would thus make the point that the subject of relevance is not perceptual inaccuracies, but rather, given such inaccuracies, what kind of research questions can be helpful in better understanding the managerial world.

Concepts to build the architecture of the managerial world

In the light of the above remarks, there is a case to be made for developing the conceptual architecture of managerial work much more strenuously than hitherto, and certainly before one takes a headlong plunge into empirical endeavours. My own belief is that in academic research the principal emphasis should be on discovering and developing concepts and theoretical hypotheses, relegating empirical studies to a subsequent and subsidiary phase (Das, 1984). Indeed, while studies with student samples are valuable for honing research techniques and for the testing of tentative hypotheses about the managerial world, management journals should refrain from publishing the results of these preliminary student-based studies (diverting them perhaps to research outlets properly dealing with student behaviour). It may seem like a trite comment, but given the deluge of non-manager research outputs that insidiously claim to deal with managers, it may not be a bad policy for journals of managerial behaviour to restrict
themselves to studies with managers as participants. Far too many empirical investigations are undertaken, it would seem, on account of the enticing prospect of some seemingly relevant ‘data’ being readily available, such as students in the classroom, losing sight of the original aim of gaining insights into managers and management.

It is admittedly quite difficult to gain access to managers for academic research purposes. As the Mezias and Starbuck paper also reveals, the moment they made the laudable effort to elicit data from real-life managers in a real organization (i.e. quality programme in Study 2), drastic compromises in research rigour had to be made. Specifically, Mezias and Starbuck discovered (a) that they had to abandon their desire for ‘data from managers working in diverse industries so there would be variation in the “objective” measures’, claimed as a strong point in Study 1 (p. 7); (b) they ‘could have access to its executives only if we would study variables relating to its quality-improvement programme’ (p. 12); and (c) that ‘when we returned for a follow-up study a year later, we were unable to obtain sufficient, dependable data’ (p. 14).

It is important to also note the quality and relevance of the insights that Mezias and Starbuck obtained from their empirical studies. My sense is that the managerial practitioner would not be overly impressed by the following illustrative observations in the paper, all of which are in the nature of commonplace bromides in the managerial world: (1) ‘most problem solving, possibly almost all problem solving, does not depend on accurate knowledge of current situations’; (2) ‘In most situations, people can act effectively without having accurate perceptions’; (3) ‘managers’ ability to improve product quality depends hardly at all on their knowledge of current quality measures. The managers mainly need to accept the idea that they should improve quality’ (all on p. 15); (4) ‘Although most managers know they do not know, recognizing that ignorance does not ensure that someone in the organization actually does have essential information’ (p. 16); and (5) ‘organizations cannot rely on internal specialists to provide expertise’ (p. 16). It seems we are faced with a situation in which the topic of inaccurate manager perceptions is purportedly being studied by academics with patently inaccurate perceptions of those very managers. It is apparent, however, that this kind of situation poses no obstacle in the triumphant march of empirical research, no matter that the object of study is perceived or grasped only barely.

As every street-smart researcher in the management field finds out, it is a blessed convenience to carry out research that has empirical data readily available and, perhaps even more to the point, to have the relatively easy assurance of such empirical research published. In a recent study of the contents of the Strategic Management Journal over the past 20 years, Phelan, Ferreira and Salvador (2002) found that ‘the proportion of empirical papers outnumber nonempirical articles 7:1’ and observed that ‘it has become harder to make a competitive contribution’ with nonempirical papers (p. 1167). Most people would agree that the crucial issue here is whether the research question is potentially valuable. This is an objective, I submit, that the research community (including the journal editors) has to be clear about. My remarks here are intended to underscore the point that one cannot begin to identify such significant issues without first appreciating the authentic managerial world. I believe that journal editors, along with doctoral advisors, ought to emphasize this point. I have seen, as have my colleagues, young management scholars prosper solely on the basis of their bare-bones statistical expertise (often aided and abetted by their collaborating mentors, who may well stand in need of this expertise for their research) without ever caring to acquire any real sense of the managerial world. How can we, in this situation, ensure that management practice animates management research? It is worth pondering whether academic researchers should be feeling self-satisfied doing grounded research concerning managers, on the infrequent occasions when they do so, unless they somehow mean to help improve the ground itself, namely, the world of managerial practice.

Re-aligning the debate on research
Mode 2: from relevance bridge to understanding essence

Finally, I wish to make a brief comment, against the backdrop of my remarks here, on the current academic debate over the traditional academic-
centred Mode 1 and the more application-oriented Mode 2 approaches to knowledge production in the management field (Gibbons, Limoges, Nowotny, Schwartzman, Scott and Trow, 1994; Tranfield and Starkey, 1998), a debate that the Editor-in-Chief of this journal recently helped encourage, by editing a set of thoughtful papers by leading commentators (Hodgkinson, 2001). In that debate forum, the report by Starkey and Madan (2001) has comprehensively reviewed the case for addressing the relevance gap in management research, advocating a Mode 2 agenda and suggesting measures to bridge that gap, including involving additional stakeholders in the research enterprise, research-oriented partnerships, and changing the mindsets of academics and managers.

I agree that the move toward Mode 2 is generally worthwhile. However, I believe that the debate has not adequately addressed the nub of the problem, namely, how to implement the Mode 2 kind of approach to producing relevant knowledge. In this respect, I welcome the call by Hodgkinson, Herriot and Anderson (2001) to focus on the ‘competency gap’ of researchers to help develop a ‘pragmatic science’ that values both rigour and relevance. In that spirit of suggesting ways to improve the general level of competence of management researchers, I discuss how we first need to examine the fundamental status of the managerial relevance gap.

It strikes me that the debate is predicated on a misplaced diagnosis of the managerial relevance problem. I suspect that the intellectual enthusiasm brought to bear on the topic has masked a kind of Type III error (Mitroff and Featheringham, 1974). In this case, the wrong problem (namely, the lack of a ‘relevance bridge’) is being addressed by an excellent solution (namely, build a bridge through Mode 2 research). On the face of it, the Mode 2 solution would appear to reasonably address the problem, but only so if we accept that the deficit in managerial relevance is in the nature of a ‘gap’ that can and needs to be ‘bridged’. I submit, however, that the debate is somewhat off-centre because it is based on the general belief that the obvious solution lies in constructing a bridge that can be crossed by academics to reach the practitioners in order to then deliver managerially relevant research insights.

To my mind, the issue of relevance has to be analysed with full recognition of the natural divide that separates the two worlds of academic research and management practice, remarkably akin to the disjuncture between the two cultures of the scientist and the non-scientist (Snow, 1969). I would suggest that the issue could be resolved when we accept the two worlds as they are. By that I mean acknowledging their very different performance goals and meaning systems, and appreciating each other on their own terms. Such an approach would argue for academics understanding the essence of the world of the manager rather than straightforwardly ‘bridging the relevance gap’.

Weick (2001) notes that the ‘infrastructure of the bridge is unreliable because the ideas are too abstract, the research findings arrive too late, the principles are too general, the industry lore is too lean and the takeaways are too obvious’ (p. S72). I would agree with Weick when he says that it is a ‘lousy bridge’. Even so, the reality is that business practitioners are hungry for knowledge that they would consider relevant. They would dearly welcome and embrace any insights from academics that might make practical sense. The fact that they gravitate toward fads and fashions, ‘complaining about relevance’, corroborates my contention that they are constantly looking for useful insights that seem ultimately not to be available.

The key to solving the ‘managerial relevance’ puzzle, I submit, lies not in dwelling inordinately on Modes 1 and 2, the thrust of the current debate, but in examining the so-called solution of ‘bridging the gap’. The ‘relevance’ in the term ‘relevance bridge’ is not so much about reformulating the research approach (here, Mode 2 from Mode 1) in order to achieve research output that would be relevant to practice. Rather, it is about what should be done to enable the two disparate worlds of research and practice to understand and communicate with each other. Thus, the issue is not how best to bridge the relevance gap in management research through Mode 2, but how academic researchers can understand the essence of the managerial world. In turn, practitioners may wish to improve their own understanding of the academic world in order to be able to select the research insights that seem right for application. Or, perhaps, place ‘orders’ for...
specific insights to be developed by academics for consumption.

Applying the marketing concept, if academics are interested in furnishing practitioners with research insights, then the best way to do so would be to find out what practitioners want and need and then try to make that available. If the task is beyond the interest or capability of researchers, then the proper course would be that they should either try harder or take their wares elsewhere. What does not make sense to me is to blame practitioners for being too ignorant or too mindless regarding their own interests. If the academic research community wishes to do relevant work, then it has to begin its labours by acquiring an appreciation of its chosen customer, namely the managerial practitioner. Such a customer-oriented approach would be much more likely to ensure managerial relevance, because it would avoid the unrealizable task of ‘bridging’ academia and business, and instead would accomplish a better understanding of the practitioner world the way it is. Let us call this the Mode C approach (‘C’ for customer), so that my preference is for a clear emphasis on producing customer-directed and customer-relevant knowledge, or MCK. A ready example of research leading to MCK is the study of effective decision-making by managers who are necessarily subject to perceptual inaccuracies.

Thus, under Mode C, as I have argued above, any improvement in the relevance of academic research is predicated on an initial familiarity with managers and management. Academic researchers need to develop the requisite variety in their competence to be able to at least moderately grasp the complexities of the practitioner world (Ashby, 1956). Furthermore, Mode C would open up the possibility of identifying the unrecognized ‘white spaces’ (Brown, 1998) lying between disciplinary mindsets that I believe are more likely to be germane for addressing the concerns of the managerial world. My point is that, whatever the approach to knowledge production, Mode 1 or Mode 2 or Mode C, we ought to recognize the need for a minimum immersion in the management world before engaging in management research. Thus, the solution to the relevance issue lies in ‘appreciating’ managers and their messy world. Accordingly, what we need is a serious discussion about how academics can best be persuaded to first gain an understanding of the essence of the managerial world before embarking on any management research – and thus reduce their critical ‘competency gap’ – as a reliable basis for ensuring the managerial relevance of their research.

**Concluding remarks**

As a general proposition, I believe that the prevailing approach to management research, such as is reflected in the Mezias and Starbuck paper, needs to be reconsidered so as to provide for a rigorous initial evaluation of the research question in terms of its relevance and significance to real-life managerial phenomena, irrespective of whether an empirical study ensues. After that, and only after that, an empirical follow-up should become a legitimate activity. And, of course, it will then become clear to everybody that data on student perceptions would rarely be appropriate as a basis for examining significant managerial issues. After all, the eminent principle of garbage-in-garbage-out is not in the least bit threatened by the mere display of the niceties of empirical rigour if all one has is quotidian student data to stand in for all kinds of manager-related phenomena. A mandated focus on the real world should also serve, at least partially, to disabuse academic researchers of one of their reigning conceits, namely, that the boundary between the managerial world and academic representations of it is almost inconsequential insofar as doing research is concerned. Imagine the kind of progress management researchers could have made over the last decades had they taken to heart the fact that it is in the nature of the managerial beast to suffer from perceptual inaccuracies, and that these inaccuracies somehow do not deter managers from making substantive decisions. That would have, for instance, prompted Mezias and Starbuck to acknowledge at the beginning of their research odyssey, rather than as the very last observation of their paper, that ‘occasions when erroneous perceptions have serious consequences are unusual’ and that ‘managers seem to be aware that there is a lot they do not know’ (p. 16). One wonders if a proper initial construal of the managerial world would have stimulated an investigation about the determinants and consequences of impugned inaccurate perceptions...
instead of being fruitlessly exercised about the continuing existence of such cussed perceptual inaccuracies.

I am sure some management researchers have work experience in business and other organizations, but the relevant point here is the extent of comprehension of the managerial world. It is not sufficient for the academic researcher to interact, even extensively so, in the particular context of carrying out a research investigation or a consultancy project. Within the specific research context, the participating managers already would have placed the researchers in the ‘external’ sphere, in which maintaining a proper public image becomes the dominant concern, no matter how the interaction is framed in terms of serious academic objectives. We know that very few researchers have shouldered the substantive responsibilities of supervising people engaged in customer-and-profit-driven work. I am aware that an outright insistence on the anthropological emic approach would be unrealistic in the management research area. What I am arguing for is that the researcher needs to make concrete efforts to become reasonably conversant with the managerial world before initiating empirical research concerning that world. As I observed earlier, surely, no medical researcher jumps into empirical research on the treatment of patients without some idea of what constitutes a patient. Hence, in short, all that management researchers have to do, I should think, is to make an attempt to talk to managers in extenso outside of research and consulting contexts to get an idea of the managerial phenomenon. This suggestion is far from expecting researchers to acquire some actual managerial work experience, which ordinarily would not be possible, for that would involve an investment of many years in a career quite removed in terms of capabilities and/or inclinations.

In summary, let me suggest the following basic steps for management research to ensure relevance and significance: (1) Start with significant questions suggested, implied or hinted by managers, either directly or to fellow researchers and, of course, in discussions and publications; (2) stress concepts, ideas, understandings of phenomena and so on, rather than rush into empirical investigations of possibly vacuous issues; (3) use appropriate study participants for the selected questions, such as senior executives in the case of strategic decision-making topics, not executive MBA students, who are typically junior managers having neither sufficient book knowledge nor the thoroughgoing work experience; and (4) discuss the impact of known limitations; if one has convenience samples (such as students) that have severe constraints, then say so, and evaluate the validity of the findings for the managerial issues being examined. I believe the most reliable way for researchers to ensure relevance is to spend some serious time talking to business executives and observing them, if not also joining them in projects, looking for intimations of topics of some significance – taking care to do so, invariably, before undertaking any academic research.

I should be remiss if I did not add that a sense of naïve wonderment about the managerial universe may be a romantic notion in academia, and perhaps a certain sense of mystery is desirable for motivating research in management. But all phenomena not experienced first-hand need not be construed as exotic, so that it ought not to take such leading researchers as Mezias and Starbuck to treat essentially well-accepted facts of managerial life as discoveries, such as the notion that perhaps managers ‘can act effectively without having accurate perceptions’, leading to the acknowledgment that it is time to ‘reconsider our assumptions about the importance of accurate perceptions’ (p. 15). The lesson learned by Mezias and Starbuck from their odyssey, one would have thought, is that the accuracy of managers’ perceptions is not as important as it might have seemed initially. Curiously, however, Mezias and Starbuck conclude their paper by reaffirming their pristine position: ‘We remain convinced that the accuracy of managers’ perceptions is an important research topic’ (p. 16). It appears that perceptions not only tend to be inaccurate but can also be pretty obdurate, permeating the decision-making of academic researchers as well as managers.

References
