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Bridging the management theory and practice gap

Rob B. Briner, Lars Engwall, Tina L. Juillerat, Henry Mintzberg, Frederick P. Morgeson, Michael G. Pratt, and Stefan Tengblad

INTRODUCTION

The aim of the book is to contribute to the further development of a practice-based management theory. For this purpose, several experienced management scholars were invited to present their views on how to renew methodological approaches in management theory and on how to deal with the persistent divide between management theory and practice. The invited scholars – Rob Briner, Lars Engwall, Henry Mintzberg, Frederick P. Morgeson, and Michael G. Pratt – participated in a plenary session at the Academy of Management 2009 Annual Meeting in Chicago (Managerial Work in Modern Organizational Contexts: New Work or New Challenges). The organizer of the session, Tina Juillerat, also provides her reflections on the ideas presented in the seminar.

The chapter presents and discusses research strategies aimed at achieving a better grounding of management theory in managerial practices. For instance, the chapter focuses on areas neglected in previous research, readdresses topics from other theoretical framing and understandings, and identifies areas/aspects where empirical evidence is lacking. The chapter considers such research strategies as the more experimental research designs, the use of new research methods, the organization of research collaborations, and researchers' interactions with managers where the goal is the framing, analysis, presentation, and publication of new and/or improved research.

FINDING OUR KEYS: ON THE IMPORTANCE OF OBSERVING MANAGERS WHERE THEY WORK

Michael G. Pratt

A man approaches a second man in a parking lot. The second man is looking for something on the ground under a lamppost.

First Man: 'Can I help you?'
Second Man: 'I've lost my keys.'
First Man: 'Where did you lose them?'
Second Man: 'In the bushes.' (He points off to the distance).
First Man: 'Then why are you looking here?'
Second Man: 'Because there are no lights by the bushes.'

In thinking about how scholars study managing and managers, I am often reminded of this joke. Where do we look for insights about management? To get to the punch line, I argue we often look where it is easier to see, not necessarily where the keys are. As Combs (2010) argues, there is a trend among organizational researchers to gather increasingly large data sets; however, at the same time, the effects of sizes that come from our deductive analysis of these data are often becoming smaller. Combs reminds us not to equate statistical significance with theoretical or practical significance. I would argue further that perhaps the large data sets he describes are not where we need to be looking for managerial insights – at least in terms of understanding how managers do their jobs.

My apprehension about the utility of using large-scale quantitative data sets to study the intricacies of management and managing no doubt comes from my preference for qualitative, inductive work. I am trained to think of the people we study as 'informants' rather than as 'subjects' or 'respondents'. That is, I believe that to best understand managing and managers, one should talk to and observe managers in their element. This research approach is messy and time-consuming. It sometimes feels like looking for keys under an unlit bush; and you never know what you will find. However, I think your chances of success – of learning something worthwhile – are more likely with this approach than with a more distant, uninvolved approach.

However, I want to advocate for something more specific than 'inductive research'. I want to make a strong argument for observing, be it participant or non-participant observing. Although observation is a hallmark of 'empirical' research (even if the term, 'empirical', is often misused as a characteristic solely of quantitative, deductive research), I do not think we – as a discipline – take enough advantage of observation.

It is perhaps ironic, however, that my advocacy for observation not only comes from personal and powerful experiences with ethnographic research but it also comes from my work on intuition (Dane and Pratt, 2007, 2009): an area of research that is built on a theory very different from that of ethnography. By way of background, my interest in intuition involves understanding how managers make decisions rapidly and without conscious reflection. Such intuitive decisions may even be better than the so-called 'rational' decisions. In these situations, managers know what they have decided but cannot really explain their decision-making process; that is, they know what they want to do, but they do not know why.

In exploring the theoretical roots of intuition, I discovered a large body of research on 'automaticity', defined by Bargh and Williams (2006: 1) as 'the control of one's internal psychological processes by external stimuli and events in one's immediate environment, often without knowledge or awareness of such control'. This research argues that our lives are governed to a great extent by non-conscious processes (e.g. see Bargh and Chartrand, 1999). Claims such as this remind me of a psychology conference I attended in 2003 where a speaker said, 'I want to make the case that the conscious mind is good for something'. Whether you believe these claims in whole or in part, or in the deterministic ontology that underlies them, there is a growing body of evidence that suggests that we do not consciously think a lot about what we do. The first tests of these automaticity-related ideas involved 'relatively simple' notions, such as routinized performances and the activation of heuristics and stereotypes. However, later research suggests that automaticity is critical for higher-order functions such as goal striving, which is an activity normally attributable to managers.

If we take this research seriously, what are the implications for the study of managing and management? I suggest there are at least three implications worthy of consideration. First, and perhaps most importantly, managers themselves may not have a good idea of why they do what they do. This would seem to make survey methods problematic. Second, and in a related vein, the managers we study most in management research may be the managers least able to explain their decisions and actions. We often tend towards upper-level, experienced managers (e.g. CEOs). Such managers have built complex cognitive structures that allow them to act with great skill in relatively complex situations – yet without conscious thought (Dane and Pratt, 2007). This assertion is bolstered by the claim that situational cues can trigger even complex behaviours and that these cues may come to replace conscious thought. Therefore, watching what managers do (and maybe doing it ourselves) – rather than asking them what they do – may be a more fruitful way of exploring and understanding *managing*. A third implication, which arises from the second, is that since action triggers may be found in situations, research should expand beyond observations of managers to observations of their environments. That is, we need to take context more seriously.

The editor of this book has challenged its contributors to propose research strategies that better bridge the gap between management theory and management practice. I argue that one such strategy is to observe: to observe what managers do and to observe the environmental factors that influence their actions. Observing may be the best way to understand how experienced managers manage; and theories grounded in observations may have the best chance of being 'transferable' to management practices in a given domain. Returning to the joke that opened this essay, careful observations 'in the field' (and under bushes) may also give us the best chance of finding the keys – and if we extend this metaphor – of unlocking new insights.

BRIDGING THE GAP THROUGH THE CONSIDERATION OF EXECUTIVE CONTEXT

Lars Engwall

The significance of context

A significant finding of the earlier research on executive behaviour (Carlson, 1951/91; Stewart, 1967; Mintzberg, 1973; Kotter, 1982) has been the fragmentation of work. However, Stefan Tengblad's findings (2002, 2006), based on new studies in the 2000s, provided other results. His data indicated that today there are fewer disruptions in executive work than earlier studies had found. A possible explanation appears to be the changes in the executive context (Engwall, 2006; Engwall et al., 2010). This in turn implies that future empirical studies should focus less on individual executives and more on the executives' organizational context. In such research, executives' working conditions could be related to (*a*) the existence of buffering, boundary-spanning units, (*b*) the corporate structure, and (*c*) corporations' external relationships.

Buffering through boundary-spanning units

Thompson (1967) pointed out that organizations tend to protect their technical core through the creation of boundary-spanning units. The task of these units is to adapt the organization to the 'constraints and contingencies' it cannot control (Thompson, 1967: 67). Similarly, executives' boundary-spanning units aim to support the leadership by their careful selection of external contacts *as well as* to produce contacts in the interest of the corporation. Among these boundary-spanning units, those for media relationships play a particularly significant role. For example, communication units today are large, strategic groups closely linked with executives that are given the task of protecting and promoting the corporate brand. Similarly, investor relations units have the important role of communicating trustworthy information about the corporation to the equity and bond markets. Government relations units have to deal with the regulatory frameworks. And so on. In this way, boundary-spanners protect executives from disturbances as well as direct their stage performance. New research should examine these changes in the micro-environment of executives that have implications for executive behaviour.

The corporate structure

However, it is not enough to look at the organizational solutions close to the executives. It is also necessary to look at the corporate structure at large. In this context, it is relevant to recall the findings and arguments of Chandler (1962, 1977, 1990), who pointed to the strong movement towards divisionalization that results from corporate growth achieved by new product introductions and geographical expansion. These developments have had a significant effect on the working conditions of executives. According to Fligstein (1990), one effect is the change in the selection of executives. First, as a response to production problems, engineers made it to the top, but when markets expanded, marketing managers were promoted to executive positions. Then divisionalization allowed financial directors to advance to these positions. Given the increasing significance of the media, communications people may be promoted next to top positions. Irrespective of whether this prophecy is right or wrong, the media influence may be expected to affect CEO behaviour. Therefore, future studies of executive behaviour need to consider both the organizational set-up and the background of executives. It is of particular interest to study the interaction between CEOs and their subordinate directors as well as the working conditions of the latter. It may be that some patterns that were observed for CEOs are now found more frequently among subordinate directors where the buffering support is less.

External relations

Stefan Tengblad's research also brought to our attention that today's executives participate 'more frequently in ceremonies (business dinners, inaugurations and other social gatherings)' (Tengblad, 2006: 1448). Besides managing the organization, increasingly CEOs have to act as 'ministers of foreign affairs'. Such responsibilities require them to work with various external intermediaries that generally fall into two groups: Consultants and Business Interest Organizations. In the first group, communication consultants and investment bankers play a particularly significant role by offering advice on communication strategies and financial solutions, respectively. Business Interest Organizations, which represent the interests of their members, have, of course, always existed. However, recently they have gone in new directions owing to the integrative process termed globalization. The most significant of these organizations are no longer local chambers of commerce but rather multinational NGOs that operate on a global scale. It is evident that the interactions of CEOs with these and similar intermediaries will be of vital interest to researchers of executive behaviour.

Conclusions

This discussion suggests that future researchers should take a wider perspective in the study of executive behaviour. In part, such studies can be conducted using traditional research methods. However, these studies should be supplemented by research into organizational structures at the top and below the top as well as into the external relationships. Such supplementary research is likely to require both quantitative and qualitative data collection and data analysis. For the latter, an appropriate research method is network analysis (e.g. Burt, 1982; Burt and Minor, 1983).

We should also expect that shadowing would be a particularly useful research strategy for data collection since shadowing can identify different aspects of contemporary executive behaviour (e.g. Czarniawska, 2007). In addition, modern information technology will make it possible to more closely analyse the communication behaviour of CEOs through e-mail correspondence, blogs, home pages, etc.

In terms of analysis, a particular interest is in explaining variations in executive behaviour. It is of value to learn what effect, if any, boundary-spanning arrangements, the corporate structure, and external relationships have on executive behaviour. Clearly, we also need to make more cross-cultural comparisons. The field of management research begun by Sune Carlson in his path-breaking study some sixty years ago still has the potential for a number of interesting future research projects.

SUPPOSE WE TOOK CONTEXT SERIOUSLY WHEN STUDYING MANAGERIAL BEHAVIOUR

Frederick P. Morgeson

There have been thousands of studies on the topics of leadership and managerial behaviour.[1] This research has enhanced our understanding of the nature of leader effectiveness and the innumerable ways in which leaders can impact individual, team, and organizational performance. Despite this impressive body of theoretical and empirical research, there is a curious gap in our understanding of how context can shape managerial behaviour. This is unfortunate because context can have a profound effect on managerial behaviour as well as influence the relationship between managerial behaviour and effectiveness. Here, I explain some reasons why leadership research should begin to take context a little more seriously.

As activity studies have shown, managerial work is inherently frantic, varied, fragmented, reactive, and disorderly (Mintzberg, 1873; Sayles, 1964; Kotter, 1982). This research has shown the complexity and diversity of managerial behaviour and acknowledge that leaders are often prompted into action by a variety of different factors (variously termed problems, fires, disturbances, crises, or events). As Sayles (1964: 47) noted over forty-five years ago, the managerial job involves 'stabilizing work systems in response to recurring disturbances of one kind or another'. But with only a few exceptions (e.g. Osborn and Hunt, 1975; Stewart, 1982; Hammer and Turk, 1987), scholars have tended to ignore how elements of the context (i.e. the disturbances) shape managerial work. This is unfortunate, in part because managerial work tends to be highly reactive in nature (in response to various contextual elements). This fact led Davis and Luthans (1980: 70) to conclude: '. . . if reactive behaviors are the rule rather than the exception, it follows that theory building and research need to give more attention to the important effects of the stimulus environment around the manager'. I reiterate this call and note that some colleagues and I have begun to explore these issues by looking at how elements of the task, and social, physical, and cultural context shape the managerial work role (Morgeson and DeRue, 2006; Shin et al., 2007; Dierdorff et al., 2009). We have found that context can have far-reaching effects on managerial behaviour.

Beyond this omnibus effect of context on managerial behaviour (see Johns, 2006), context is likely to make some managerial behaviours more or less effective. In essence, certain managerial behaviours are likely to better 'fit' particular contexts. This could manifest itself in a variety of ways. For example, it might reflect matching leader behaviour to broader work climates. As Hofmann et al. (2003) found, the impact of leader–member exchange (LMX) on subordinate role definitions was greater in positive safety climates. Or it might reflect tailoring leader behaviour to very specific events in the work environment. As Morgeson (2005) found, the novelty and disruptiveness of events in a team's context have significant implications for the effectiveness of different leader behaviours. Taken together, these studies suggest that considering the contextual contingencies on leader behaviour can provide insight into the effectiveness of leader relationships and actions.

So, if context has largely been neglected in past leadership research, what can researchers do to address this gap? There are at least three specific things that could be done. First, in order to more fully incorporate context into the study of managerial behaviour, it would be helpful to study managers in situ (i.e. in their context). This has been done in the past to great effect, but seems to have fallen out of favour as a research approach. To understand the role that context might play, it is helpful to talk with managers about their specific context and how they both influence and are influenced by it. This would generally involve qualitative methods that have been used in past research, but the focus would be different. In addition to focusing on managerial behaviour, scholars should explicitly assess and explore how leaders' contexts shape their activities. This would include studying different leaders in different contexts as well as leaders as they move through different contexts in the performance of their work. Context is rarely directly examined, but attention to context can inform our understanding of leadership behaviour.

Second, scholars can begin to incorporate context as a substantive variable in their theorizing and empirical data collection efforts. There now exist models of context that can help scholars identify the particular contextual elements that might be relevant given their substantive research questions. For example, most contexts can be described in terms of discrete task, social, and physical contextual elements. Research linking these contextual elements to broad categories of leadership activities (e.g. conceptual, interpersonal, and technical) can enhance our understanding of the links between context and leadership role enactment. In addition, theory and research should begin to articulate how different aspects of context can moderate the relationship between leadership activities and outcomes. For example, are certain leadership behaviours more effective in certain contexts than other behaviours? In addition, leadership challenges arise from the different contexts in which leaders operate. These challenges arise from the team, organizational, and environmental context (Morgeson et al., 2010). Attending to these challenges and the contexts from which they arise can enhance our understanding of the interplay between leadership and performance.

Third, utilizing different event methodologies can provide insight into managerial behaviour and context. Such methodologies may include experience sampling where managers report on their behaviours over time (as they occur) and on how these behaviours vary depending upon the context they find themselves in (e.g. dealing with a problem employee; briefing upper management). Event-based methodologies may be another technique where managers recount what they did when certain kinds of events occurred. As Mintzberg (1973: 223) has noted, this technique is 'interesting and useful because it focuses on concrete examples, allowing the manager to describe what he knows best (actual events), and leaving interpretation of data and development of theory to the researcher'. Regardless of how one approaches the study of context, however, research that explores contextual factors should take a high priority given its potential to help us better understand managerial behaviour.

BRIDGING THE GAP THROUGH EVIDENCE-BASED MANAGEMENT AND MANAGEMENT-BASED EVIDENCE

Rob B. Briner

The idea that there is a gap between management research and management practice is now well established. The nature of this gap and how it can be narrowed have been described in different ways using different language. For example, Huff (2000) discusses the role of Mode 2 research that attempts to generate knowledge in the context of practice while Cummings (2007) draws on the notion of engaged scholarship. While there are subtle but sometimes-important differences in the way this gap is described and understood, it is generally seen to have two main consequences. First, what researchers study and how they choose to study it – in this case, managerial behaviour – are insufficiently informed by what managers and organizations actually do. While research produced in this way may have many qualities, relevance is unlikely to be one of them. The second consequence of this gap is that what managers and organizations do is insufficiently informed by those research findings that are of relevance. Managers do not make full use of what we know from research about the way management and organizations work.

Within management, the most recently proposed approach to closing this gap is *evidence-based management*. Although this approach represents a family of approaches rather than a single practice, evidence-based management generally involves organizations 'making decisions through the conscientious, explicit, and judicious use of four sources of information: practitioner expertise and judgment, evidence from the local context, a critical evaluation of the best available research evidence, and the perspectives of those people who might be affected by the decision' (Briner et al., 2009: 19). While managers and organizations routinely make use of at least three of these sources of information, they make relatively little if any use of research evidence.

How then can evidence-based management approaches be used to develop research on managers and managing? A useful starting point might be to conduct a systematic review and research synthesis (e.g. Rousseau et al., 2008) of what is known and, equally importantly, not known about managerial work. Systematic reviews adopt an explicit methodology and use focused review questions to identify and critically appraise relevant evidence in order to more clearly establish what is known and the basis of such knowledge. They are an essential element of evidence-based management in that they help make evidence accessible to managers who wish to use it in their decision-making. They are also extremely useful for researchers in that they help to resolve debates about what is known and not known and identify gaps for future research. While narrative reviews of managerial work do exist, there are, to my knowledge, no systematic reviews of this sort.

A second way in which evidence-based management approaches may help develop research in this field is through promoting *practice-based* or *management-based evidence*. Evidence-based practice approaches, particularly when used in medical and related fields, have been criticized for focusing exclusively on existing empirical evidence when conducing systematic reviews. As already mentioned, an important limitation of much existing research is that it often bears little or no relation to what practitioners are doing in their daily work. One solution to this problem proposed in other fields (and also by other contributors to this chapter) is to gather practice-based evidence to better understand what managers actually do as well as the consequences of such actions. Evidence gathered in this way can then be used to help inform practice. Where practices are found to be effective, the boundary conditions or contexts in which they are more or less effective can be identified. Where practices are found to be ineffective, the reasons can be further explored and alternative practices drawing on existing evidence can be developed and implemented. These practices can in turn be empirically evaluated, thus adding to the body of practice-based evidence.

A third way in which evidence-based management approaches could be used to develop research in this field is through evidence-based management interventions and experiments. Introducing evidence from systematic reviews into the managerial decision-making process will tell us much about managerial work. For example, do managers see such evidence as relevant or useful? What factors make it more or less likely that such evidence will be used? When might managers place more emphasis on other sources of information such as their experience and intuition? While, as suggested elsewhere in this chapter, close observation of managerial work is essential, interventions and experiments, whether they are evidence-based or in some other form, may also provide important insights.

Common concerns about evidence-based management approaches are that they are overly rationalistic, too closely based on evidence-based medicine where the evidence available has more in common with the physical than the social sciences, and privilege evidence from academic research over other sources of information. However, as mentioned above, evidence-based management also involves the use of three other sources of information and evidence: the manager's experience and judgement, evidence from the local context, and the views of those who may be affected by the decision. In many cases, it may be that these sources of information are much more relevant and useful than any formal research evidence. In addition, research evidence also needs to be critically evaluated for relevance and applicability. Here, too, managers' experience and judgement are vital for appraising whether the research evidence available applies to their context. Evidence-based management is about using the best available evidence. While such concerns are understandable, a more recent attempt to clarify what evidence-based management is and what it is not (Briner et al., 2009) places great emphasis on the importance of *combining* these four sources of information and evidence in the decision-making process. Ultimately, people and not evidence make decisions.

There is little doubt that a gap exists between research on managerial work and what managers do. What is far less clear is the nature of this gap, the purpose of narrowing or filling it, and how it can be done. I believe that evidence-based management approaches provide a framework for addressing each of these questions.

THAT RESEARCH ON MANAGING BE DEVELOPED

Henry Mintzberg

I have been asked to write about *how* research on managing can be developed. But first we have to be concerned *that* research on managing be developed. How extraordinary that the most fundamental subject for the field of management – namely what the job of managing is all about – has been all but ignored by researchers and by the programmes of the business schools. Sure, there is plenty on leadership – ad nauseam. But that is mostly about the grandeur of the great leader, not the nuts and bolts of the down-to-earth manager. The former may be more enticing, but that has led to an awful lot of hubris in organizations these days: heroic leadership disconnected from the requirements of plain old managing.

So we need to get back to managing. Actually researchers in the business schools need to get to managing. They have never been there.

When I published *The Nature of Managerial Work* in 1973, I could not find much research on simply what managers do – what the job is all about, based on empirical evidence. There was Sune Carlson's original study of the 1940s, Len Sayle's excellent book of 1964, and quite a lot of research by Rosemary Stewart, but not much more. (My own book gave rise to a number of studies, especially in the realm of school management, most of them replications in one form or another, but that soon died out.)

When I set out to publish *Managing*, which appeared in 2009, the situation was hardly better – in fact, worse. Stefan Tengblad was the only very active publisher of research that I found. In fact, even on such an important subject as the impact of the Internet, especially e-mail, *on the practice of management*, I could find nothing of an empirical nature.

Why? I have no idea. Do we avoid facing our own deities? (Probably) Are business schools that detached from managerial realities? (For sure – just read the titles of the articles in the journals.) Is managerial work that complicated, or at least not amiable to 'rigorous' methods of research?

Let me pick up on this last point, because it brings us back to what I was supposed to write about here – how research on managing can be developed.

'Rigorous', when used in this sense, usually means fancy, also deductive, so that statistical methods can be applied. It is too bad that the really interesting questions in our whole field, not just this one, do not lend themselves to such 'rigor'. Hence, we get so many banal studies of uninteresting topics, chopped up beyond what could possibly be of interest to even the most thoughtful managers – although the journal editors love them.

Since we have so little fundamental knowledge of the essence of managing, we have few interesting hypotheses to test (let alone the means to test them so that they produce insights, which is the real purpose of research). So research on managing has to be inductive (which is usually wrongly labelled 'qualitative' – my first book, wholly inductive, was loaded with numbers). The problem is that many journal editors are suspicious of this approach. A rigorous deductive study that proves nothing can usually be published more easily than an insightful study that is inductive.

So, how can research on managing be developed? Easy. Replace many of the journal editors with those concerned about insights. (Good luck.) Institute an 'intelligent practitioner' review of all articles, submissions to conferences, and tenure files too. (Good luck again.) Bring business schools down to earth by stopping them from pretending that they are developing leaders out of barely experienced twenty-somethings instead of working with real managers on *their own* concerns – which of course might promote a change in the nature and the interests of the faculty themselves. And then, just maybe, the latter might get interested in the most fundamental subject in the whole field of management. I wish us all a very great deal of luck.

WHAT HAPPENED AND WHAT NEXT? REFLECTIONS ON THE ACADEMY OF MANAGEMENT PANEL SESSION

Tina L. Juillerat

What happened?

As a graduate student in management (and a former practicing manager), I was surprised to discover the lack of research focusing on managerial work (Mintzberg, 1990; Morgeson and Campion, 2003; Hambrick et al., 2005). I also shared scholars' concerns about whether existing management theories and knowledge were still accurate given dramatic changes in organizational contexts (Barley and Kunda, 2001; Parker et al., 2001; Rousseau and Fried, 2001; Johns, 2006), several of which I had personally observed or experienced in the workplace.

To help address these issues, I thought it might be useful to talk with researchers who shared my concerns and interest in this gap in organizational research. To reach a broad audience, I hoped to organize a panel discussion at the Academy of Management's 2009 Annual Conference in Chicago. I was very fortunate and thrilled that several distinguished scholars agreed to contribute to the symposium and to serve as panellists: Rob Briner, Lars Engwall, Henry Mintzberg, Frederick Morgeson, Michael Pratt, and Stefan Tengblad. Our goal was to help ensure that future management research and theoretical development would be guided by an understanding of 'what managers actually do' (Mintzberg, 1990) in modern organizations. As such, the symposium was designed to both stimulate interest in studying managerial work and provide suggestions to guide future research. The session resulted in a lively and insightful exchange that spawned further collaborations, including this chapter. It has been exciting to see the symposium elicit further dialogue and efforts to connect management theory and practice.

This dialogue has generated several insights and future research directions that could substantially improve our understanding of managerial work. Clearly, we need to know more about the role of organizational contexts (Engwall and Morgeson, this chapter), as well as how managers make decisions (Pratt, this chapter). Integrating these perspectives, I also suggested that we more explicitly consider the interplay between the organizational context and managerial decision-making. In particular, before criticizing managers' decisions or practices, we need a better understanding of the interpersonal, institutional, or political goals they are trying to achieve in a particular context (Lerner and Tetlock, 1999).

What next?

Given that the symposium highlighted both significant challenges and opportunities for research on managerial work, perhaps it is not surprising that this chapter presents diverse views on the likelihood of reducing the gap between management theory and practice. The authors' assessments range from a less optimistic 'good luck' (Mintzberg, this chapter) to greater optimism driven by specific strategies believed to represent unexploited opportunities. My own view is mixed. Certainly, the challenges are not imaginary, and the next generation of management researchers needs to have a realistic view of what is possible. Moreover, unless scholars develop greater *interest* in managers and the nature of their work, a less optimistic view is warranted.

On the other hand, I remain reasonably optimistic and have been further inspired by the panellists, who have successfully pursued an array of interests in managerial work and contributed to management theory and practice. Hopefully, these role models can change attitudes about the possibilities for achieving high levels of academic career success *and* for reducing the gap between management research and practice. These authors also provide excellent guidance for current students, including research strategies and methods to learn and employ during our training and future careers. Armed with this knowledge, future generations of scholars can be better equipped to conduct management research that is both more grounded and more clearly linked to management practice. Still, the next generation of researchers is unlikely to be able to lead the way. We need more established scholars who are willing to serve as role models, provide mentoring, and pursue new research questions and new methods.

However, I believe that all scholars, not just those whose work is explicitly focused on managers or primarily qualitative research, can contribute to our understanding of managerial work. To this end, I highlight a few basic guidelines which can be applied by any researcher and that I would like to implement in my own research. First, I would like to incorporate a qualitative component into all future studies by simply asking several open-ended questions. Second, to identify influential yet underexplored contextual moderators and mechanisms, I will talk to more practitioners when both planning and interpreting future studies. Lastly, to help reduce the traditional challenges of studying managers in situ, I plan to search for innovative ways to apply newer technologies to the study of managerial work. Although researchers have discussed the challenges of technological changes for managers, we have only begun to leverage the opportunities these changes can provide in our own context.

Most organizational scholars seem to hope that our individual or collective work 'matters' (Hambrick, 1994), and we often lament our perceived lack of influence on management practice (Rynes et al., 2001; Pfeffer and Sutton, 2006; Rousseau, 2006). Is there a connection between our perceived (in)ability to influence managerial practice and our admitted lack of interest in and understanding of what managers do? Wouldn't management scholars who acquired new insights about the nature of managerial work be much more likely to influence managerial practice and 'matter more' (Hambrick, 1994)? We need to 'get back to managing' (Mintzberg, this chapter) to further develop and connect management theory and practice.

IS THE GAP BRIDGEABLE?

Stefan Tengblad

Summarizing the contributions

There are some important similarities among the contributions in this chapter. These include a call for studying micro-practice, preferably through observations and encounters with managers, and an awareness of the importance of contextual factors. Managers work in many different settings, and even if there are general traits in managerial behaviour, great variances have been identified over the years. It is also agreed that mainstream research based on large-sample surveys, using correlations between various standardized questionnaire items, cannot bridge the gap between theoretical frameworks and real-life managerial practices. This is true, not least in the sense of the so-called 'rigorous' management theory that without any evidence maintains that complexity and hard performance pressures are best handled by a greater emphasis on systematic knowledge gathering and on designing formal management routines for planning and decision-making.

These contributions also address particular topics. Mike Pratt argues effectively for the need to study non-conscious and intuitive managerial behaviour based on experience and routine. In our research, we should never take-for-granted that managers can provide accurate accounts about what they are doing and why. Managerial routines are, to a large extent, habitual and non-reflective. There is therefore a definite risk that such managerial accounts reflect more their notions of what managers are supposed to do in accordance with popular myths and beliefs. I highly support the study of non-conscious and intuitive managerial behaviour through direct observations.

Lars Engwall's contribution concerns the need to more systematically address some important contextual factors, especially the influence of external stakeholders. In particular, large organizations that are publicly owned and/or are subject to political influence need to be sensitive to external relationships. In such organizations, top leaders should devote considerable energy to those relationships in order to maintain much needed legitimacy.

Fred Morgeson continues the discussion on the importance of context. He suggests useful methods for studies of context, namely experience sampling and event-based methodologies. He advocates the development of managerial behaviour theories that integrate core contextual factors, especially the stimulus environment managers face in different settings.

Rob Briner makes an interesting advocacy for an evidence-based management theory based in management practices rather than in large-scale surveys. By systematically reviewing previous studies of managerial behaviour and engaging managers in dialogues and experiments, solid, practice-based management theories can be developed.

In his advocacy of a more holistic and practice-oriented management research, Henry Mintzberg's outlook is less optimistic. As a proponent of this approach for some three decades, Henry is critical of the general development within management research that he thinks has gone in the wrong direction. However, he pinpoints some important structural features that hamper the development of the approach: the career/tenure system that is oriented towards publication in A-journals and the associated conservative effect of the reviewing processes. Additionally, the lucrative business of promoting simplified and overly rationalistic conceptual models for popular consumption contributes to the vast divide between management theory and practice.

Finally, Tina Juillerat's contribution calls for greater attention to managerial practice that deals with the relevance problem in management science through increased interaction with practitioners and increased collaboration between young and senior researchers in our discipline.

I can only concur with the contributors' wish to deal with the gap through new and better methods and approaches for capturing the essence of management. To conclude the chapter, I expand this discussion of the need to develop a more holistic and practice-based understanding of management. The emergence of a multipolar world may provide great opportunities to suggest viable alternatives to mainstream research.

The need to establish a holistic understanding of the realities of management

In Chapter 2, Ola Vie and I wrote that acting rationally by making decisions based on objective facts is a cornerstone of the management institution and a main reason for allocating so much power and so many economic resources to managers. Highlighting the emotional, habitual, and reactive aspects of management is therefore to question the management institution as such to a fairly large degree. Thus, many managers and management educators often prefer the fairy tales of well-run administrative processes built on enlightened foresight, systematic planning, and smooth execution. As a consequence, however, individual managers may feel inadequate and therefore reluctant to subject their real behaviour to external scrutiny. Instead, they adapt to management rituals and ceremonies when needed in order to establish their legitimacy as competent managers. This requires a significant amount of effort aimed at producing various written documents and compliance with superiors' instructions.

Therefore, in the renewal of management theory, there should be a quest to find a new way to legitimize management as an institution. A suggestion is that emotions, improvisation, and reactive - or better - interactive behaviour are useful in dealing with typical management challenges such as unexpected events, open-ended work tasks, complexity, incompatible expectations, and performance pressures.

To effect such a change, we need to develop a strong alternative to the physics-inspired methodology of developing general (context-independent theory) and hypothesis testing based on simple cause–effect assumptions. If we accept that managerial processes are very complicated, with numerous important factors that interact with each other in unpredictable ways, then we need to study management in an integrated fashion. The increasing fragmentation of management research contributes to the relevancy problem since managers, unlike researchers, need to address the whole picture. It is probably impossible to integrate all relevant factors in a single piece of research, but a synthesis of various streams of research and many factors (at least) may contribute more than a purely theoretical approach focused on a single factor. Even when it is necessary to study management in small chunks, the research community still needs to understand the whole picture of management and to communicate this understanding to management practitioners. Otherwise the researcher–practitioner gap will widen into an unbridgeable divide.

The need for interactive research

The dominant research tradition tells us to ignore the impact of the field when planning a study. Empirical research is supposed to be theory-driven and context-independent without any interference from the study objects. The data should preferably be pure and untouched like an old document from the past. But our inability to develop empirically grounded theories that are context-independent is a sign that the research community alone cannot satisfactorily develop a new understanding of management. Both researchers and managers in practice suffer from the success of normative management theory that tells us management should be a well-ordered and deliberative process.

In order to deal scientifically with the high level of complexity in management, new understandings of this complexity as well as new methods of studying it are required. One such methodological approach is to invite practitioners to become 'co-researchers' who participate in the exploration of everyday management activities where reflective action, rather than orderly control, is more the ideal.

The chapter cannot describe all the implications of the interactive co-research approach. However, Ingalill Holmberg and Mats Tyrstrup (Chapter 3, this book) offer an excellent example of the approach. Another example is Kristian Kreiner and Jan Mouritsen's concept (2005) of the 'the analytical interview' that proposes that the researcher and the respondent can reach a complex and rich understanding through an in-depth conversation with each other. From this perspective, the interview becomes an opportunity that permits both researcher and respondent to learn something they had not known before. This method can be pivotal for generating scientific insights (as suggested by Mintzberg, 2009) or wisdom – both practical and scientific (as proposed by Czarniawska, 2003).

Management research in a multipolar world

Modern management is to a large degree an American innovation. American companies, educational institutions, and research institutes – as well as management gurus – have dominated for some decades although there have also been significant achievements in these areas in Great Britain, France, Japan, Canada, The Netherlands, and other countries. However, many countries have followed the American business model, with its focus on innovation, technological progress, and a consumer economy. Given the increasing economic problems in the United States with its national budget and trade deficits (and indeed in many European countries as well), combined with the rise of new economic powers, the twenty-first century business world will inevitably be more multipolar. For example, in their business practices, South Korea, China, India, and Brazil reflect a new and increasing diversity in the world of management. The expectation is that such diversity will increase the competition between different management cultures and institutional frameworks. Management educators in the future will hopefully teach case studies of corporations such as Samsung, Huawei, Hyundai, and Infosys as well as Google, General Electric, Procter & Gamble, Nokia, Toyota, or Unilever.

This increased multipolarity should also promote the development of a rich discourse in management science in which managerial experiences and behaviours worldwide are described and analysed. The American influence on management science and education will endure, but there will be other important influences from other researchers, other countries, and other models. The outcome will be the realization that management research can expand beyond a strong paradigm based on a 'Fayolism' theory of management (established in a totally different time period) to a paradigm that captures the complex and information-rich world of management. It is perhaps not possible to build a single bridge between theory and practice in our discipline, but it should not be impossible to build a strong, practice-based management theory.

NOTE

1. Although some have made distinctions between leadership and management, for the purposes of this chapter I treat them as largely equivalent.

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