

A VERY SHORT,
FAIRLY INTERESTING AND
REASONABLY CHEAP BOOK ABOUT
MANAGEMENT RESEARCH

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In Search of Management Research

It is thus *possible* to create a tradition that is held together by strict rules, and that is also successful to some extent. But is it *desirable* to support such a tradition to the exclusion of everything else? Should we transfer to it the sole rights for dealing in knowledge, so that any result that has been obtained by other methods is at once ruled out of court? And did scientists ever remain within the boundaries of the traditions they defined in this way? (Paul Feyerabend, 1993: 11, emphasis in original)

This chapter, indeed this whole book, involves trying to understand what management research is and how it is done. The meaning of management research has come to be associated with systematic investigation using scientific methods. We are not opposed to this. But the tools of scientific investigation should simply be a means to an end, not ends in themselves. A parallel can be drawn here with bureaucracy and the notion of goal displacement. When the sociologist Robert Merton (1940) wrote about the bureaucratic personality he had in mind the possibility that bureaucratic forms of organisation could have unintended consequences when organisational rules were followed too literally or interpreted too narrowly. This, he argued, could result in rigid patterns of behaviour, as organisational members became obsessed with procedural compliance in a manner which was ritualistic and didn't necessarily contribute towards the ends that the organisation was supposed to be directed towards. This gives rise to a bureaucratic personality, guided by instrumental rationality and the pursuit of efficiency over and above values.

This relates to the point being made by the philosopher Paul Feyerabend above, when he argues that even if rule-following in scientific research is possible, it may not be desirable because the rationalist principles of the scientific method do not necessarily result in progress. What is more, he argues, the impression that we have of heroes of the scientific revolution, such as Galileo, is a misrepresentation of how these early scientists actually worked (based on rhetorical persuasion as much as empirical evidence). But despite Feyerabend's criticisms, conceptions of research remain founded on these popular stereotypes.

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This also gives a clue as to what we think is missing if management research is approached in a way that prioritises the following of strict rules. Because it means that research can become an instrumental, game-playing exercise, without there being a clear sense of why the game is played in the first place. It is this possibility that Grey (2010) has in mind when he talks about the skill and dedication involved in publishing a paper in a high status management journal being similar to that involved in becoming an expert cryptic crossword solver. Consequently, there is a risk that the etymological meaning of research – the activity of searching carefully for something – may be lost. Similar criticisms have been levelled at other social science disciplines, such as Sociology, which Berger (1992) says has succumbed to ‘methodological fetishism’, or the dominance of methods over content. In this chapter we will explain what is wrong with this approach and consider some alternatives.

Four kinds of management researcher

We think there are four kinds of management researcher. Let’s list them:

- *the practitioner-researcher* – this individual seeks out research situations where they can get close to managerial practice. They may appear to have no explicit research agenda, although this can be misleading as there is often a normative intent behind their research, in the sense that it prescribes a solution to a managerial problem. Systematic and scientific methods are seen as a means of enabling the immersion of the researcher in situations involving management practice. For this type of researcher, the purpose of study is often to improve management practice through change.
- *the management theoretician* – this kind of researcher makes theoretical arguments and may build theoretical models. They are sometimes called an armchair researcher, due to their lack of engagement with the empirical world. Their goal is to develop conceptual understandings by drawing on philosophical and social scientific knowledge. They often concentrate on secondary analysis, interpreting empirical research done by others, rather than doing primary research of their own.
- *the craft researcher* – for this type of person research is a creative as well as a technical-rational act; an art as well as a science. This requires not only skill and training but also a sense of imagination and the ability to switch perspectives in order to build up a complex picture of management. It can even involve an element of calculated risk, breaking away from established ways of doing things to enhance the possibility of learning something new.

- *the technical-rational researcher* – for this person the emphasis is on the rigorous use of particular research methods and methodologies, or working with methodological experts, to try to ensure publication in the highest status outlets. They often pursue topics and research designs that are likely to be popular with external funders.

Of course, these are ideal types; most management researchers are a combination of these, and the exact combination of characteristics changes over time. For example, when Richard started working at the shoe factory, he saw himself as a practitioner-researcher. Now towards the end of his career, he sees himself as more of a craft researcher.

There isn't one type of management researcher who is inherently superior to the others. But we do have some misgivings about the technical-rational researcher. This is because we agree with Morgan who says that 'a knowledge of technique needs to be complemented by an appreciation of the nature of research as a distinctively human process through which researchers *make* knowledge' (1983: 7, emphasis in original). Our main focus in this book is therefore on the craft of management research, because we think this is essential in positioning research as a moral, ethical and political (rather than just a technical-rational) activity.

— Differences between management research and consultancy

In considering what management research is, we also need to be clear about what distinguishes it from other types of information-seeking and analytical activities like consultancy, or even journalism. Let's start with a famous, or some would say infamous, example of knowledge based on management consultancy. Peters and Waterman's (1982) *In Search of Excellence* is one of the best-selling management books of all time. The authors, who were both management consultants, worked for the firm McKinsey. The book is based on their analysis of a small number of 'excellent' companies which they identified as having a 'strong' culture. In a scholarly critique, Guest (1992) makes a number of points that call into question whether *In Search of Excellence* can be trusted and relied upon as management research.

Peters and Waterman worked with a sample of 75 companies which were all considered to be 'highly regarded' in their industries. However, they rejected 13 of these 75 companies from the category of excellence fairly early on because they failed to represent a model of business that

the McKinsey consultants were interested in. Of the remaining 62, they concluded (based on analysis of a range of financial indicators) that 36 were 'excellent'. By 'boosting' the scores awarded for certain factors such as innovation, this number was raised to 43 companies. From their original sample of 75, the McKinsey consultants conducted interviews in around half of them, 21 in companies they judged to be 'excellent', plus another 12 which they deemed to be 'near misses' using their criteria for excellence. However, they only spoke to senior executives within these firms as well as outside commentators like business journalists.

There are numerous problems with this approach. First, it was not a representative sample of the employees in these organisations. Speaking to lower-level employees would probably have provided an interesting complementary perspective. This might have challenged some of the rather self-congratulatory assertions made by senior people in the firms, who said things like: the CEO 'praised initiative and staff skill' and 'rewarded innovation'. The *ad hoc* nature of the sample used by the McKinsey consultants meant that they may have omitted organisations that would have qualified at the outset. It is also questionable whether some of the qualifying attributes employed in the study, such as 'innovation', can be measured objectively. In addition, the authors 'boosted' the value of certain attributes, with little explanation, in order to make the sample bigger. Guest concludes that the whole process on which Peters and Waterman based their book lacked a methodology. In short, *In Search of Excellence* is not management research in any meaningful sense of the term.

So while the McKinsey consultants were searching for excellent management, they failed to base this on excellent research. This contradicts the view of Gummesson (2000) who argues that there isn't really much difference between a management consultant and a management researcher. Gummesson uses the metaphor of a pecking and defecating bird to represent both of these activities. The only significant difference between them is that the management researcher pecks at small aspects of managerial practice and contributes voluminously to theory, whereas the consultant pecks at a small amount of theory and contributes voluminously to managerial practice. For Gummesson, it is just a difference of degree.

According to Gummesson's logic, we might argue that managers, as well as consultants, often do research as part of their jobs; they collect data, analyse it and use it to draw conclusions. But the purposes that this activity is directed towards vary considerably. For example, Richard was once asked by ICI to do some research into how managers in the company made strategic decisions. They wanted him to use a qualitative

research method which involved asking managers to keep diaries where they recorded their daily activities. Richard was keen on this as he had used this method in previous research. He encouraged the managers to record their activities in detail – including time spent walking about the factory or discussing things over coffee. But as the study progressed it became clear that the research formed part of an initiative that managers were using to improve efficiency, and the data was going to be used as a rationale for cutting out ‘time wasting’ activities such as coffee breaks. Richard also realised that the senior team commissioning the study was unaware of previous management research, such as the study by Mintzberg (1973), which also used a diary method to study what chief executives do in their day-to-day lives. Crucially, Mintzberg found that tours of the work site or time spent talking with colleagues in corridors was of high value in enabling senior managers to gather information quickly that enabled them to make decisions and deal with the fragmented nature of their work roles.

It can often be the case that management research has already been done which, if known about, helps make sense of a current situation. Recall the accident in 2010 involving 33 Chilean miners trapped for 69 days 2,300 feet below ground in a copper mine before eventually being rescued. The men waited for 17 days after the huge landslide before any of the rescue team even made contact with them, eating just a teaspoon of tuna and drinking a few sips of milk each day to make their rations last longer. At this point, as far as the trapped miners were concerned, especially given the poor safety record of the mining company that employed them, they could not be sure anyone was seriously looking for them. And yet as the first grainy camera images of the men singing for the cameras emerged from the mine, they explained that they had tried to keep ‘everything organised’, by forming routines for games, prayer, and allocating different roles to individuals.

While recognising their undoubted bravery, we should not be too surprised at this. In 1958 a researcher named Donald Roy published an article documenting the social organisation of time based on his undercover study of a small group of factory operators. He called his article ‘banana time’, a phrase used by the workers to describe the ritual breaks they had devised to deal with the formidable ‘beast of monotony’ caused by the extreme repetition and tedium of the tasks they were employed to do. Roy’s point was that it wasn’t managers who ensured the social and psychological well-being of the workers, but the workers themselves, by organising themselves in ways that involved humour to punctuate their working day. Although the conditions faced by the Chilean miners were undoubtedly more extreme, Roy’s study helps us

to understand how workers cope with difficult conditions by establishing predictable patterns of social behaviour.

Similarly, when the scandal broke in 2004 revealing the abuse of detainees by US military police at the Abu Ghraib prison in Iraq, many commentators drew parallels with a study conducted by Zimbardo and his colleagues in the 1970s. Zimbardo's research involved an experiment (Haney et al., 1973) to see how people responded to pressures to conform in situations that resembled 'total institutions' (Goffman, 1961), where people are set apart from the outside world and all treated in the same way together.

The tendency to forget research that can help us to make sense of current managerial situations is a reflection of 'presentism', where 'the present is often assumed to be a period of unprecedented change, heralding the dawning of a new age' (Booth and Rowlinson, 2006: 6). We have noticed that students have a tendency to think that any management research which is more than ten years old is by definition 'out of date' and thus irrelevant, a situation that would be unimaginable in some other academic disciplines like politics or history. This is problematic in a number of respects, not least because there is a risk of reinventing the wheel, through conducting a study which simply repeats what has already been done before, perhaps without even realising it, and interpreting the data in a way which does not acknowledge the findings of previous studies. Burrell (1997) puts this down to the obsession with recency which is combined, among other things, with the desire for relevance, a theme we return to in Chapter 2.

A further problem arises when the management consultant or organisation that has commissioned the study has a strongly favoured outcome which they hope will arise from it. A recent illustration of this can be found in the UK TV series *The Apprentice*, where one of the candidates carried out what she called 'consumer research', by asking travellers on the Paris Metro questions about car usage to establish whether there was a market for a new car product. Despite the emphatic responses of Parisians that they were a city of car users and the product was a good one, she reported back to her team that people were not in favour of it because *she* didn't think the seat was very saleable. As this rather extreme example shows, researchers who are involved in the practice of management sometimes allow this to cloud their interpretation of data.

The way in which consultants and academic researchers engage with theory is also different. Management researchers are more focused on description (what is), whereas management consultants tend to be more interested in prescription (what should be done). Consultants are therefore more likely to apply normative theory. Like technical-rational researchers, they are also more likely to have an instrumental rather

than an intrinsic approach to the value of theory. Consequently, consultants and managers tend to approach research in a way that is more instrumental and solution-focused than management researchers. Theory is often interpreted relatively narrowly or not at all, and there is sometimes an element of crude empiricism, wherein research is seen simply as a means of presenting neutral facts.

But as our ‘practitioner-researcher’ category illustrates, some management researchers adopt an approach that is closely related to consultancy. In supervising students doing dissertation projects, we have noticed that many of them struggle to identify with the idea of being a researcher and sometimes fall back on an identity based on consultancy. This frames how they think of research questions and encourages them to approach them in a managerialist way, rather than being critical and analytical. It is also problematic because they fill the dissertation with bullet point recommendations, as though it was a business or management report to be submitted to a client or a line manager. Recommending a particular course of action to solve a problem is not the main point of research writing, although this may come later. Instead the primary purpose is to understand what is going on.

Differences between management research and journalism

So what is the difference between journalism and management research? In the 1970s, Studs Terkel wrote a book called *Working: People Talk About What They Do All Day and How They Feel About What They Do*. Terkel, who was from Chicago, was a journalist, a radio presenter, a novelist and an oral historian. He was also an expert in getting people to tell their stories. Key to this was his skill in listening, giving people the time and space in which to talk about their experiences. In the book he presents a series of first-person accounts. Such stories are referred to in research as vignettes. The stories were based on interviews he carried out in which he asked people to talk about their working lives. Stories were told by American men and women who did all kinds of work, from a policeman to an airline stewardess, a supermarket checker and a plant manager. They illustrated the mundane experience of work and the importance of routine for the people who did them.

To take another example, in 2001 American journalist Barbara Ehrenreich wrote a book called *Nickel and Dimed: On (Not) Getting By in America*, based on her attempt to gain low-wage, low-skill employment in companies like *WalMart*. Five years later she published

another book, this time about white-collar, managerial work. In both cases, she didn't tell her prospective employers that she was a journalist doing research for a book. In management research we refer to this as covert study, because the people being studied don't know the person they're speaking to is a researcher. We would describe Ehrenreich's research as participant observation, because the researcher is seeking to understand the social phenomenon they are interested in by participating directly in the setting, rather than observing it from a distance. Both of these techniques were also used by Donald Roy (1958), mentioned earlier.

However, neither Terkel's nor Ehrenreich's books would be seen as management research, and nor would the authors describe themselves as management researchers. The reasons for this are complex and some of these distinctions might seem relatively pedantic. The first relates to conventions entailed in creating knowledge. In management research there is an expectation that the researcher gives an explicit account of how they conducted the research and how they analysed the data collected. This involves providing the reader with information about the people or situations that constitute the focus of study and showing how this is related to an existing body of knowledge about the subject. To do this, the researcher must demonstrate their understanding of existing knowledge, usually by reviewing the accumulated research literature. They need to cite this literature, providing a set of references that enables the reader to go and access this published work for themselves.

Researchers also need to demonstrate an understanding of the principles of knowledge creation, including what knowledge is and how it is generated. These are termed epistemological issues. They also need to consider the reality status of their subject of study – whether it is something that has an objectively real existence or is dynamically constituted through the actions and perceptions of the people who engage with it. This is an ontological issue. Researchers must also show skill in use of research methods. They must also demonstrate that they understand how the quality of research is assessed. This includes awareness of notions of 'good practice' established over time, and how these sensibilities have changed (so that certain research practices which might once have been considered acceptable, like covert research, are now rarely considered ethically acceptable).

Neither Ehrenreich nor Terkel do these things. What is more, the publication of their work is not reliant on any form of peer evaluation. This is the process whereby research is reviewed by other management researchers, who decide whether or not it is good enough to be published. Instead, Ehrenreich and Terkel rely on the judgements of publishers, who determine whether or not there is an audience for

their work, and on readers, who decide whether or not they find the work meaningful and interesting. You might be thinking that management research *should* be evaluated like this. And you might have a point. After all, why shouldn't management research be evaluated according to whether people find it interesting and consider it worth taking the time to read and think about it?

Physics envy in management research

To explain why these are not the primary criteria used to define management research we must turn to a particular affliction that management researchers tend to suffer from, popularly referred to as physics envy (Thomas and Wilson, 2011), so named because it involves a degree of anxious comparison. Physics envy is a term used by those who are critical of the emulation of positivist methods associated with natural or 'hard' sciences¹ like physics.

According to the positivist tradition of knowledge creation, research is a neutral, value-free enterprise, in which researchers go about collecting objective data through empirical observation. This is called phenomenalism. Positivist researchers approach their research subject by reviewing existing knowledge and using this to generate hypotheses, which they seek to test empirically. If a hypothesis is rejected, the theory must be modified. This is known as a deductive approach to theory building. The purpose of this hypothetico-deductive method is to generate law-like theory that applies independently of time and place – known as nomothetic knowledge. The goal for positivist researchers is to generate knowledge about management that enables explanation of how and why things are as they are, and through this to influence the future.

The positivist tradition has profound implications for the way management researchers see themselves in relation to their subject of study. If you were asked to picture a typical scientist, the image conjured up might be of a bespectacled man or woman wearing a white lab coat, perhaps examining something through a microscope, maybe carrying a clipboard – the dispassionate, neutral observer who studies her subject from a distance. This stereotype reflects an *etic* view of research, a 'fly on the wall' or outsider perspective. It assumes that the phenomenon being studied exists independently of the person who studies it. It also assumes that the researcher can reach an understanding of the phenomenon they are interested in by studying it from a distance. What is more, this distance is seen as desirable because without it there is a danger that the researcher will affect their subject of study by studying it, thereby invalidating their findings.

Yet very little management research has resulted in the kind of explanatory, generalisable knowledge that positivist researchers aspire to create. A further problem with this approach is that it assumes it is possible to generate management theory that is neutral, detached and free from value-judgement. This is a highly problematic assumption, for reasons which will become clear later. Plus, in contrast to the natural sciences, and in common with many social science and humanities disciplines, management research is characterised by so-called 'soft' knowledge, based on recursive development, the same issues being returned to over and over again. It is also characterised by a lack of consensus surrounding what questions should be asked and what constitutes respectable or legitimate knowledge. The affliction of physics envy is therefore crucial in understanding the ideas about scientific knowledge upon which some management research is based.

— The interpretive other

There is another group of management researchers who go by the label interpretivists, or sometimes social constructionists, who claim that the study of social systems is not amenable to exploration using methods and standards traditionally associated with the natural sciences. If we had to imagine what this type of researcher would look like we might think of someone who is less 'buttoned up' and more casually dressed, wearing jeans or even, as Learmonth and Humphreys (2011) observe, shorts and sandals to work, perhaps having a slightly hippyish style about them, as though they came of age in the 1960s and 1970s. This is not that surprising because a significant growth of the interpretive research tradition in the social sciences² can be linked to currents that flourished in these countercultural decades which fostered a willingness to question established ways of doing things and to experiment with alternatives. This built on an earlier era of interpretivism, in the form of the Chicago School, a group of US based sociologists who were committed to developing naturalistic ways of studying social life based on detailed, qualitative investigation, partly a reaction to the positivist tradition which was particularly dominant in the early part of the twentieth century in this discipline. The interpretive researcher might therefore be characterised as something of a rebel in contrast to the dominant positivist tradition.

Interpretivists would suggest that it is very difficult, if not impossible, to generate nomothetic knowledge in relation to complex domains of human activity like management because they are so dependent on the

social actors who are involved in them for the meanings that are generated. They would further argue that management knowledge is situationally specific, owing to the complexities and the unique character of the particular cultural and historical moment that is being studied, and therefore not able to be generalised from, except in the most tentative and thought-provoking ways which cannot form the basis for changing organisational behaviour or making managerial decisions. This is known as ideographic knowledge.

The goal of this type of researcher is therefore to generate understanding through knowledge creation. They don't approach their research subject by generating hypotheses in relation to it. Instead they use existing literature on the subject to form a research question which they take into the fieldwork setting, and adjust or adapt depending on the themes and findings that emerge during data collection. We call this an inductive and iterative (cyclical) approach to theory building. Interpretive researchers argue that in order to understand management, we need to get close to the people who are affected by it. They therefore adopt an *emic* or 'experience-near' approach to study (Geertz, 1974), whereby management is understood from an insider perspective. Consequently, the researcher gets involved with the people they study, possibly even affecting outcomes and events through their presence.

Of course, these portrayals are crude oversimplifications which cannot do justice to the diverse array of practices and people that exist within and outside these traditions. However, we make no apology for introducing them at this stage because they provide a useful starting point.

Research communities

So now we have a sense of what management researchers look like, the next question is where and how do you find them? Geertz (1974) suggested that if you want to understand a field of scholarship, you need to start by looking at what its members actually do. This is because in order to become established as a field of science, management researchers must develop a shared understanding of what constitutes proper behaviour and what matters in their field of inquiry. Such understandings will be heavily influenced by certain 'leading lights' (Burrell, 1996: 643), scholars who exercise a degree of political influence.

We find it useful to think of management research as a community of practice. To learn how to do management research it is helpful, if not imperative, to become part of this community. In a book which influenced

how we understand the process of learning, Lave and Wenger (1991) describe a community of practice as a group that shares certain understandings about what they are doing, including what this means in their lives and for their communities. This provides the basis for learning. Belonging to a social learning system is reliant on three things: first, engagement, by talking to people at conferences or producing artefacts like written papers; second, imagination, in that you have to be able to look in the mirror and 'see' yourself as a management researcher – this involves having some role models with whom you identify; and third, alignment, the feeling that your activities are in line with respected ways of doing things that you perceive to be related to a collective enterprise or shared goal. To join the management research community, you need to establish relationships of mutuality with other members, contributing reciprocally to it as a trusted member, and also to have access to a shared repertoire of language, sensibilities, routines, tools and stories, that enable you to demonstrate your competence (Wenger, 2000). Importantly, participation in a community is experienced as empowering, whereas if you are prevented from participating very much this is a source of powerlessness.

Our own experience illustrates how this works. Around a decade ago Emma was a PhD student supervised by Richard studying payment systems in the chemical industry. She was advised for a while by Tom Lupton, who in the 1950s was one of the first anthropologists in the UK to study managerial systems. Richard's career as a management researcher began ten years prior to that. He was supervised by Angela Bowey, who had been one of Tom Lupton's PhD students. Richard's PhD was also about payment systems.

The point of this story is that management researchers learn their craft from each other, often through the supervision relationship which is similar to an apprenticeship. At times you may think that your supervisor is a God-like figure who has always known how to do research. At other times you may think they are a bit irritating or odd, but you probably won't forget them and they definitely won't forget you. The reasons for this relate to the length and complexity of the process of becoming a management researcher in a community that becomes a bit like an extended (and perhaps dysfunctional) family. We suspect that many other management researchers could tell similar stories about how they learnt their craft.

Sometimes this learning isn't based on a supervision relationship but on knowledge of and respect for the person's research. For example, in the 1970s, management researcher Michael Burawoy did research in the same US factory where Donald Roy carried out his study of output restriction and informal work groups in the 1940s. Burawoy walked along the same corridors, sat in the same offices and may even have

spoken to some of the same employees. It seems reasonable to assume that these experiences shaped Burawoy's understanding of what it means to be a management researcher, and that they were more powerful because he knew that a classic study of organised work had been carried out by a management researcher in the same location 30 years earlier. Interestingly, it was Roy who also inspired Tom Lupton and convinced him in 1955 to set out to test the findings of the earlier Hawthorne studies by observing work practices in a naturalistic setting, rather than under experimental conditions. This resulted in Lupton's book *On the Shop Floor* (1963).

Becoming a management researcher thus positions you as part of a community. Some of the members you know personally, others you do not. This is similar to Bourdieu's (1984) notion of fields, which are distinguished by shared practices and relations between social actors who share a common interest or desire to play certain social games. The process of learning how to do management research is therefore based on socialisation. This is as much about spending time with and talking to other management researchers who hold various forms of professional and educational capital (Bourdieu, 1984), as it is about learning how to do regression analysis or transcribe an interview from a book. In workplaces this is sometimes referred to as 'sitting by Nellie', or more grandly as 'situated learning'. It involves learning how to do a job by watching someone more experienced do it. Seeing how another management researcher interviews a senior manager or negotiates research access has helped us develop our own research practice and experiment with different ways of doing management research.

If you are a newcomer and you want to join the management research community, you learn the rules by participating in the group's shared socio-cultural practices.³ One way of doing this is by participating in a gathering of management researchers, such as an academic conference, where you will see old-timers in action (so to speak), presenting papers and conducting themselves in ways that reveal to you their particular values and attitudes.

As you might imagine, management research is not one big unified community of practice, but rather a series of smaller communities, many of which have quite distinct practices. For example, the Critical Management Studies community, which has its own research gatherings, displays certain cultural rituals of behaviour and practice which reflect a kind of masculine, critical, rebellious identity, for example by favouring drinking rituals and eschewing the suit-and-tie presentation of a 'businesslike' self (Bell and King, 2010). A sharply observed article by Ford and Harding (2008) suggests the kinds of behaviours one sees at conferences provide a means whereby a small number of powerful people dominate a large

number of others. This is achieved through infantilisation, for example by speaking to them as if they were children, but also through seduction, as important speakers parade themselves before their audience, stroking their hair and wearing smart suits. The point of all of this is to say that you can learn a lot about a community by observing how its members behave at public events like this where they give what Goffman (1959) calls 'frontstage' performances.

The importance of understanding how to become part of a research community applies to undergraduate, Masters and MBA students doing a small research project, as well as PhD students and business school lecturers and professors. But being apprenticed into a community is often characterised by asymmetrical power relations. The apprentice is often heavily dependent on their supervisor not only for advice and guidance but also to confer legitimacy upon them and give them access to contexts where they can participate in community activities.

But there are alternative ways of learning how to become a management researcher. Under conditions of what Lave and Wenger (1991: 93) describe as benign neglect, groups of novitiates can organise their learning among themselves, such as by meeting up informally to talk about their research. This can make them less isolated and more mutually supporting. It provides an alternative to the master–apprentice model of research supervision which is rather paternalistic, and can be experienced as disappointing and frustrating, especially if the student ends up pressured to pursue research avenues that align with their supervisor's research identity, rather than their own (Jones, 1995).

Communities in conversation

To learn how to do management research, one of the first things you need to do is to engage in conversation with people who are doing it. Reading a book about management research, such as this one, is all very well, but you also need to join a community of management researchers so that you can have conversations about research. This might sound rather grand, perhaps conjuring up images of men with beards and bald heads sitting smoking pipes in deep leather armchairs in a book-filled room exchanging intellectual ideas. Whereas, in fact, some of the best research conversations we have ever had have been in rather unlikely and uninspiring places, such as drab university coffee lounges or on long train journeys.

Huff (1999) describes management research as a conversation based on ongoing dialogue that takes place in the classroom, at conferences, by email, or perhaps nowadays on Skype or Twitter. It involves newcomers

as well as experienced researchers from different universities and different parts of the world. But the most important aspect of the conversation takes place in published work – in journals, books, and their electronic equivalents. If you want to participate in the research conversation, Huff recommends that you write. Writing is not just a way of communicating findings, it also enables you to understand how scholarship works, including what Huff (1999: 5) calls the ‘tacit norms and subtle nuances that characterize good scholarship’. Important questions to ask are: ‘what conversations do I want to participate in?’ and ‘what audiences do I want to reach through my research?’ Huff even suggests that you imagine yourself having conversations with researchers in your community so you get used to the idea that you might have something to add. She also gives advice based on the rules of normal conversation which is helpful in considering how to frame your contribution to the conversation, in a way that others are likely to respect:

- 1 *listen before you speak* – don’t pile in without having listened to (i.e. read) the work of other conversationalists;
- 2 *connect with points already made* – don’t try and change the track of the conversation onto something you are interested in; instead make an effort to connect to what others are saying;
- 3 *be interesting* – be clear, concise, try to avoid saying something that they already know;
- 4 *be polite* – the desire to be noticed can push you towards bravado or even aggression, try to resist this, it is not good conversational practice. (Huff, 1999: 47)

The bottom line is that you are unlikely to be listened to (i.e. published) if you are talking to yourself. Similar conversational practices have been noticed in other kinds of problem-solving work, such as that done by photocopier technicians who, as organisational ethnographer Julian Orr (1996) observed, spend a great deal of time telling stories to one another about the process of fixing machines.

Research communities as material, virtual and textual

Increasingly, communities of management researchers are virtual rather than actual, gathering in cyberspace via websites, discussion forums and listservs, rather than on a university campus. Virtual communities of practice also often have the advantage of being very international, and

participation is not reliant on having the funds to be able to travel to far-flung destinations.

An example relates to the field of strategy research, which has traditionally been dominated by a small handful of strongly positivist-oriented publications such as *Strategic Management Journal*. Since the turn of the millennium, however, there has been something of a shift with the development of a community oriented towards the study of what is termed 'strategy-as-practice'. This forms part of a broader interest in the so-called 'practice turn' in social theory, which has led researchers to turn their attention to the processes through which management and organisations are constituted on an ongoing basis. In the case of strategy-as-practice, this has opened up spaces for more interpretive research.

By forming a management research community, comprising several prominent as well as some less well-known management researchers, starting in 2003 members began to set an agenda, by publishing several 'special issues' about strategy-as-practice in high-status journals like *Long Range Planning* and *Journal of Management Studies*. They also ran regular streams, tracks, symposia and workshops at leading management research conferences, the proceedings of which are often published electronically. This helped to encourage established researchers and doctoral students from related fields to explore and identify with the strategy-as-practice community and orient their research in ways that related to it. One of the most innovative things this community did was to establish a website which acts as a resource repository and a discussion space for researchers, listing recently published articles in the field. The site name was later changed to the Strategy as Practice International Network, reflecting the community's growing international membership. They also published several handbooks or edited collections with prestigious academic publishers like Cambridge University Press, which gave a comprehensive overview and mapped the current progress of the field (e.g. Golsorkhi et al., 2010). In a fairly short space of time, through developing a strong identity, a successful and relatively influential research community was constructed.

As this example shows, one of the most important means through which particular management research communities are constructed involves researchers committing their thoughts to print. This constitutes a way of saying 'this is the direction that management research is going in and we think it overlooks [x] and therefore we have published this handbook or started that new journal to try to redress this imbalance and reflect the growing interest in this emerging and important area. Through this new journal/book we will endeavour to take management research in a new, interesting and potentially more worthwhile

direction'. This is what organisational aesthetics researchers have recently done, by setting up their own online journal.⁴ As these examples show, management research communities are thus material (in the form of getting together through events such as conferences and workshops), virtual (forming communities through online forums and networks) and textual (creating groups of like-minded management researchers through publishing activities).

Communities that remember – or forget

The management research community can also be understood as mnemonic, a group that develops a commonly shared understanding of the past (Rowlinson et al., 2010). According to this view, the collective memory of the management research community is constructed by expressing attitudes toward the past and attaching meaning to them. One of the things we hope to do in this book is to provide a focus for remembering, rather than forgetting the past as a basis from which to form identities in the present. We do this mainly by telling stories. We think this provides a valuable counterweight to the prescriptive tone sometimes associated with methods textbooks, showing the foundations of the field to be built on methods that rarely follow a normative, 'best practice' model of research design. It also helps to demonstrate the historical contingency of management research, as specific to the time and place in which it is done.

Take the case of Melville Dalton, a Chicago School trained industrial sociologist and author of the classic study of informal organisation and unofficial reward, *Men Who Manage* (1959). Emma has argued that the intensive, ethnographic research done by Dalton (who took jobs in the organisations he studied and spent a number of years researching them), could not be carried out in the same way today (Bell, 2011). This is in part because the methods Dalton used would not be considered respectable in today's climate. Dalton argued that it was impossible to study unofficial action other than by using covert methods which enable the researcher to get sufficiently close to the subject. But this argument would not cut much ice with a university ethics committee today. One reason why management researchers do not tend to remember their collective past is because the reality of management research is often messier, more complex and contingent than the community is inclined to admit. Added to this, the demand for scientific rigour in publishing, means that 'warts and all' methodological accounts tend to get written out of published articles.

Conclusion

In this chapter we have introduced four different kinds of management researchers. We have been careful to point out the strengths and weaknesses associated with each of these types, and to show that in practice, researchers often take on identities that combine elements from all of them. One reason for introducing these different types is to encourage you to think about what kind of researcher you want to be.

Becoming a management researcher relies on a process of socialisation through which you become part of a community and develop an identity as a researcher. This can be a strong community, within which you participate fully, or you can be an occasional member of several communities, engaging with each more casually. But not everyone becomes a full participant, and the process of trying to join a community can sometimes be experienced as quite isolating, socially as well as intellectually.

We have also suggested that, contrary to accounts that emphasise the systematic and scientific nature of research as a planned process, management research is often a messy, unpredictable and politicised process which does not readily lend itself to stereotypical ideals of objective neutrality. This is a theme we will return to again and again. And yet we are not advocating the abandonment of all attempts to pursue systematic, scientific management research, but we do think it is important to retain sight of the reasons why management researchers do what they do. In other words, there is a need to search for the purpose of management research, rather than unquestioningly accept conventions surrounding the production of management knowledge, without asking why they exist in the first place.

Notes

- 1 So called because they generate hard knowledge, which has well established criteria for judging claims to new knowledge, and is characterised by steady, cumulative growth.
- 2 Marked by publications like the influential *Sage Handbook of Qualitative Research*, first edited by Denzin and Lincoln in 1994 and now in its fourth edition (2011).
- 3 This is similar to Becher's (1989) analysis of how academics in various university disciplines saw themselves in relation to their research subjects. Sadly for us, Becher did not include management researchers in his study.
- 4 <http://ojs.wpi.edu/index.php/orgaesthetics/>