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Introduction: the Crisis in Management Studies

I moved to Australia from Northern Ireland in 1999, to work in one of the country's leading business schools. Moving continents was a challenge. But it also involved relocating from the social science discipline of communication and into the new and unfamiliar world of management studies. In truth, both were nerve-shredding prospects. Yet this book has its origins in a conversation that took place during my time in Australia.

My colleagues and I were visited by a senior management academic from the USA for the usual research seminar, lunch and debate. Over coffee, talk turned to the state of research. Our visitor remarked casually, as if stating a well-known fact: 'Of course, most management research is rubbish.' Everyone laughed, although a little nervously. The view seemed to be that he was quite right, but he was saying things that shouldn't be said in sophisticated company, like someone who blurts out a preference for the bagpipes rather than opera. I remember feeling shocked at his nonchalance. It is a feeling that has long since worn off.

I have grown increasingly alarmed at the state of management research, the unreadability of our journals, the triviality of the topics that they mostly address, and the importance of the many issues that they ignore. As John Mingers reported in 2015:

> A very quick check in *Web of Science* (WoS) shows that out of 115,000 papers in management . . . published since 1990 only 328 (0.28 percent) were concerned with climate change, and that out of 222,500 papers in management, business and finance only 292 (0.13 percent) were concerned with the financial crises. Interestingly, the first paper on this subject was not published until

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> 2008 (it was the only one in that year) which shows how little foresight there was before the actual events happened.[1]

If this isn't a crisis, I don't know what is. It is also astonishing and disgraceful. To this day, none of the major journals issued by the Academy of Management have published a substantive paper dealing with the Great Recession of 2008. Remarkably enough, even top finance journals have published very little serious work examining the 2008 crisis, and the type of research that they publish remains pretty much as it was before the crash.[2] Perhaps what happened just wasn't important enough to discuss. Yet there is scant evidence that the banking and finance systems have been radically reformed. This offers the appalling prospect that another financial crisis is a matter of when rather than if. The consequences could be even more catastrophic, economically, socially and politically. In largely ignoring these issues, both mainstream and critically oriented management scholars have shown a diligent spirit of due negligence.

This sort of problem has been long recognised but still endures. Bill Starbuck complained as far back as 2003 that 'few organization theorists have focused on connections between organizations and social problems, although long-standing social problems persist and new ones appear'.[3] From his perspective, it follows that 'Organization theory can and should contribute more to human welfare. Efforts to make such contributions can inject vitality into organization theory as well as benefit our neighbors, our societies, and people far away.' Yet management scholars seem to engage with bigger issues in the same spirit that many of us approach visiting the dentist. We know that it is good for us and we know that we should. But we really don't want to do it. Why is this? It is a question I return to throughout this book.

Concerns of this kind are now widespread, and are increasingly articulated in our journals. For example, Bill Harley writes of what he terms 'an emerging crisis of confidence in management studies'. He goes on to say: 'Why might management scholars feel that their

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research and publishing activities were unsatisfying or even meaningless? To put it bluntly, this is a completely reasonable response to the fact that most of the research and publishing that most of us do has little or no impact.'[4]

In addition, many of our papers seem to be written by sadists who enjoy inflicting pain on masochists. To take a random example from the *Academy of Management Journal*, I find a paper entitled 'When does Medici hurt da Vinci? Mitigating the signaling effect of extraneous stakeholder relationships in the field of cultural production'.[5] Good luck to anyone trying to figure out what this is about from its title. Soldiering on through its abstract I find the following:

> We . . . confirm that the salience of the relationship with extraneous stakeholders – operationalized as the number of corporate donors – has a negative effect on peer recognition . . . We contribute to both the institutional logics perspective and stakeholder literatures by bringing in a signalling perspective: we show that peer recognition, upon which the maintenance of a dominant logic lies, is directly impacted by the nature of relationships with extraneous stakeholders.

And so on. This paper has value and even contains some lucid passages. But its abstract is the main part that most people will read. Why does it have to be written in such a way that only those already on the inside of the debates it refers to can understand it?

It has also become evident that various forms of research malpractice are common in our field. I discuss these throughout this book. I am talking about outright fraud such as inventing data, but also about plagiarism, self-plagiarism, poor-quality statistical analysis and *p*-hacking. The last-named involves such practices as dropping variables from analysis, terminating a study before it is completed, and 'rounding off' numbers obtained from statistical tests in order to obtain results that are statistically significant. The number of retractions in our field as a result of these practices is rising, while the

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volume of papers making extravagant claims that may be fatally flawed is also growing.

I offer one example of the latter from psychology. This is an adjacent discipline to management. Many of its findings have implications for management practice as well as theory, and our journals often overlap in the topics that they address. In 2010, Amy Cuddy was an upcoming scholar at Harvard Business School. With two colleagues, she published a paper entitled 'Power posing: brief nonverbal displays affect neuroendocrine levels and risk tolerance'.[6] The paper proposed that 'a person can, by assuming two simple 1-min poses, embody power and instantly become more powerful'. By adopting a particular posture that imitates power you seem more powerful to yourself and others. The researchers summarised some of the effects as follows: 'Results of this study confirmed our prediction that posing in high-power nonverbal displays (as opposed to low-power nonverbal displays) would cause neuroendocrine and behavioral changes for both male and female participants: High-power posers experienced elevations in testosterone, decreases in cortisol, and increased feelings of power and tolerance for risk; low-power posers exhibited the opposite pattern.' As they noted, this 'has real-world, actionable implications'.[7] I can, for example, imagine significant implications for leadership research and leadership development programmes.

Others thought likewise. Cuddy went on to do a TED talk that has become one of the most watched ever.[8] In 2014 the *New York Times* published an article about her work that reads like the profile of a Hollywood movie star. It reported that she 'has attracted lucrative speaking invitations from around the world, a contract from Little, Brown & Co. for a book to be published next year, and an eclectic army of posture-conscious followers'. According to the article, the implications of her work are boundless. It claimed that 'Elementary school students, retirees, elite athletes, surgeons, politicians, victims of bullying and sexual assault, beleaguered refugees, people dealing with mental illness or physical limitations (including a quadriplegic): they have all written to say that adopting a confident pose – or simply

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visualizing one, as in that last case – delivers almost instant self-assurance.' The article includes a picture captioned 'Amy Cuddy strikes a winning pose outside her Harvard office'.[9] Her book was duly published in 2015 and garnered glowing reviews from readers on Amazon's website. Its title, *Presence: Bringing Your Boldest Self to Your Biggest Challenges*, chimes with the positive psychology movement.[10] This seeks to convince us that if we only developed the right mental attitude then many of our problems would disappear.

There is only one slight problem. The study that led to all this exposure was replicated by another team of researchers with a sample five times bigger than the original.[11] It did not find any of the effects that had been claimed. Yet, as Andrew Gelman and Kaiser Fung reported in 2016, the original findings continue to be widely publicised by the mass media.[12] By comparison, the failed replication has been virtually ignored.

Now, there may be many reasons for this failure, and no one has suggested that the original findings were faked. Researchers, like everyone else, like to show that their hunches are correct. We seek out evidence that confirms them and downplay the importance of evidence that does not. Perhaps further work will vindicate the original claims that were made. But it is clear that academic careers can be enormously advanced when people make what appear to be groundbreaking claims that also appeal to a media on the lookout for the latest sensational 'discoveries'. How many such claims rest on inadequate sample sizes, insufficiently rigorous analyses, over-hyping by authors keen to make an impact, and on occasion outright fraud? It is questions like these that led me to write the book you are now reading.

In Chapter 1, I look at the historical roots of some of these issues. In my view, there never was a 'golden age' of management research to which we can return. Many of our current problems were there from the outset, in one form or another. However, the never-ending processes of measurement and audit that now infect universities have made all these problems worse, as well as adding some new ones to keep them company. I examine these dynamics in Chapter 2.

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The effects on the quality of academic life are severe, as I outline in Chapter 3. In Chapter 4 I discuss the different forms of research misconduct and malpractice that can be found throughout academic research, and in Chapter 5 look in detail at how these are manifest in management studies.

A particular problem within management studies is that much of our research is far too preoccupied with creating the illusion of theory development, much of which is pretentious nonsense. I discuss this in Chapter 6, and in Chapter 7 illustrate it in depth by looking at an example of so-called 'theory development' in leadership studies. Some prominent researchers, such as Denise Rousseau and Jeffrey Pfeffer, have argued that one answer to the problem of irrelevance in management research is what they have termed 'evidence-based management'. Chapter 8 offers my take on this suggestion, and in particular argues that the poor state of management research makes it problematic. As computer scientists have long delighted in telling us, garbage in equals garbage out (GIGO). In Chapter 9 I suggest that our research should make a greater effort to address important issues, and offer some examples of what these could be. Lastly, I look in Chapter 10 at what we can do individually and collectively to reclaim traditional academic values of disinterested inquiry, and commit ourselves to doing research with a greater purpose than publication for its own sake.

I should also say that I am not putting myself forward here as a paragon of virtue. I have committed many of the faults discussed in this book, including poor writing and wasting time on topics that I should have ignored. In a sinful world, none of us are entirely free from sin. Nor do I want this book to be just a series of complaints. I offer suggestions throughout about what we can do to move forward. My premise, fundamentally, is that the world faces major challenges across numerous issues. Many of these are organisational. How can we respond to the introduction of new technologies that are transforming the nature of work? How should we deal with the changing culture within universities, where academics are pressurised more than ever

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to produce at least the appearance of ground-breaking research? They are rewarded handsomely if they do and penalised if they don't – a classic incentive for misconduct. What is the purpose of universities generally and business schools in particular in this ever-changing world that we inhabit?

It is my belief that management scholars can make important contributions to these issues. At present, we generally don't. Most managers don't read our research and most of the public is unaware that it even exists. Their instincts are sound. Turning this around is of course in our own interests. More importantly, we are missing tremendous opportunities to influence debate around us on a range of exciting, terrifying and important questions. Is the current state of affairs really what we want? Is it the best that we can do?

In the hope that your answer to both these questions is 'No', I offer the book that follows.

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