

Book review
*A Research Agenda for Management and
Organization Studies*, edited by Czarniawska
Tom S. Karlsson*

Tom S. Karlsson
School of Public
Administration
University of Gothenburg
tom.karlsson@spa.gu.se

The field of management and organization studies has developed – perhaps even matured – over the course of more than a century. Early works from scholars such as Frederick Winslow Taylor, Henry Fayol, Mary Parker Follet, and Chester Barnard, together with later works by, for example, Peter Drucker, and James March and Johan Olsen have greatly affected the manner in which we understand, conceptualise, and analyse contemporary management and organization. In many ways, these scholars have created a solid base of knowledge that researchers employ in order to legitimise their own research.

It is in this setting, indeed with this backdrop exemplified, that the book *A Research Agenda for Management and Organization Studies* (edited by Barbara Czarniawska) makes an attempt to reveal what future opportunities there are within this field. At 188 pages long it is a concise book, with a preface and fifteen individual chapters. It brings together nineteen scholars, representing different empirical foci but also different scientific backgrounds. However, the academic fields and empirical interests represented in this book are often interrelated: organization, accounting, sociology, anthropology, and information systems. In the following sections, I will briefly summarise the contents of each chapter.

Marie-Laure Djelic (Ch. 1) engages in a discussion about the *fashion* and *fads* of management in order to unpack what management means and is today. This is done through a historical exploration. She concludes by arguing that the words we use today to understand management carry historical connotations that may be disturbing.

Paolo Quattrone (Ch. 2) follows the ideas presented by Hopwood (1987) in envisioning what *accounting is not* in order to make future crossroads for accounting research visible. I found the sections about the historical roots of accounting combined with an analysis of why accounting in many ways supports the capitalist system especially fascinating and worth reading.

Franck Cochoy, Jan Smolenski, and Jean-Sébastien Vayre (Ch. 3) discuss possibilities for future marketing research. They focus on (1) *market-things* and (2) *market-ITing*. The first refers to the processes and tools which redefine consumers and the effects they have on marketing. The second refers to the use of digitised tools for consumption. The authors' main conclusion is that, rather than focusing on business to business or customer to customer, marketing needs to expand to a framework of business to business to customers.

Martin Koernberger (Ch. 4) revisits scholarly work within the field of strategy, attempting to break free from the taken-for-granted assumptions within the field. By opening closed doors (which Koernberger uses as a paraphrase of Kierkegaard), novel insights might be found in an extensively explored field.

* **Tom S. Karlsson** is an assistant professor at the School of Public Administration, University of Gothenburg. His main research interest concerns managerial and market reforms over time, with specific interest in how central government agencies – and the actors within – adopt and adapt to new practices.

Monika Kostera (Ch. 5) explores the field of *humanistic management*, arguing that management theory is not merely science, but ultimately ideology. As such, she engages in unpacking the management concept by adopting perspectives from the humanities and urges future research to focus on the human conditions of management. Kostera then concludes the chapter by reviewing ten works that are of importance to the field.

Yvonne Benschop (Ch. 6) focuses on gender studies within organizations. She argues that future research agendas should expand from two meta-theoretical approaches: the *gender-in-organizations* and the *gendering organizations*. She argues that new and novel questions might be developed by combining the two sets. Benschop concludes the chapter by urging scholars to engage in discussions about how organizational gendering practices connect to broader societal phenomena.

Andreas Diedrich (Ch. 7) takes on the task of bringing diversity research into management and organization. More specifically, he argues that diversity needs to be more integrated in management and organization research. With an empirical example drawn from a Swedish context, he engages in a review of several different approaches to studying diversity and multiculturalism. Diedrich concludes by urging future research to turn its attention away from diversity as an effect of organizing and towards the enactment of everyday assumptions that manifest themselves through discrimination and inequality.

François Cooren (Ch. 8) presents a wish list for the next fifteen years of research into organizational communication. He argues that in contrast to much of the present research undertaken about communication, future attempts should restrain from relying mainly on post-hoc reconstructions and instead turn to more direct methods of inquiry. Cooren recommends observations or shadowing as important methods and concludes the chapter by highlighting the importance of keeping an open mind to where redefinition and reconceptualisation might take the research.

Wanda J. Orlikowski and Susan V. Scott (Ch. 9) discuss the future of research into digital work. One of the first things they establish is that the separation between *digital* and *non-digital* is meaningless, moving attention back to work and action itself. Since today most organizations engage in digital work, it becomes more meaningful to simply undertake research about different kinds of work. The authors argue throughout the chapter for more research focusing on sociomaterial practices through the concepts of *entanglement* and *performativity*.

Gili S. Drori (Ch. 10) discusses the need for comparative studies of organization and management by engaging the concepts of *glocalisation* and *orientation*. Glocalisation refers to corporations that work in a global organizational context with varying jurisdictions. Orientation is engaged as a concept of referencing or identifying. Combining the concepts brings forth new research opportunities, for example, for studying how such global organizations construct their identity.

Hervé Corvellac (Ch. 11) argues that one crucial challenge for the future of management and organization concerns waste management. The idea that one

person's waste can be reconstructed into another person's resource is pivotal to current understandings of the contemporary economy. Corvellac urges future research to study waste management in order to make it possible to understand the concepts of *production*, *distribution*, and *consumption*.

Timon Beyes (Ch. 12) takes a slightly different approach and discusses the need for arts and aesthetics in management and organization research. Although claiming that the scope of the chapter is not large enough to provide a comprehensive overview of the literature, Beyes engages in a number of fascinating perspectives wherein arts and aesthetics can prove beneficial for organization research.

Carl Rhodes (Ch. 13) discusses how popular culture can be analysed in order to further understand managerial activities. Using the empirical example of SpongeBob SquarePants, issues of understanding cultural logics emerge. It is a fascinating and slightly provocative perspective, but one which seems inherently beneficial for future research. Rhodes argues that by exploring popular culture in relation to management and organization, lived realities within organizations might be explored.

Christopher Grey and Jana Costas (Ch. 14) discuss invisible organizations, which they delimit to *secret organizations*. Studying secret organizations is inherently difficult because, as Grey and Costas accurately claim, if they really are secret it infers that we do not know that they exist. This means that research focusing on secret organizations should focus on such organizations that we know of, but where our knowledge of what they actually do is limited. The authors conclude the chapter by urging researchers to engage in a process aiming to *demytify* rather than *mystify* such secret organizations.

Martin Parker (Ch. 15) discusses the issue of moving towards an alternative business school. He argues that rather than engaging in trying to satisfy neoliberal think-tanks and management consultants – he calls this ‘teaching what will sell’ – business schools should move towards embracing a *school of organizing* instead. Parker concludes the chapter by arguing that by not making such changes, business schools continually decrease their own legitimacy as they keep studying only a small part of what is expected of them.

Although not explicitly formulated, *A Research Agenda for Management and Organization Studies* comes across as a book with an emancipatory agenda: the reader is challenged to question the manner in which current research is performed, and what it can do in the future. This challenge can entail fundamentally new angles where we find ourselves questioning the validity of our assumptions. This is good. More than that, I would argue that this is pivotal in itself for the continuation of research in the field.

As I read the book, I thought about other plausible readers. Firstly, I would say that any scholar even remotely interested in organization and organizing should take the time to read at least some of the chapters. From my perspective, this book provides quite a number of intellectually stimulating ideas that – without a doubt – could be implemented in research. I imagine that, for example, junior PhD students struggling to formulate their own research topic, as well as

more experienced scholars who are ready to expand their own field of interest for future research projects, would benefit from reading this book. A critical reading of *A Research Agenda for Management and Organization Studies* provides insights as well as inspiration for everybody who has an open approach to the possibilities of research.

Writing a review in a public administration journal naturally prompts the question of whether this book can be beneficial within the field of public sector research. Although the book lacks a specific section where reflections for public administration are raised – which surprised me – there are still reasons for public sector scholars to read it. The suggestions for future research agendas are primarily of a generic order; the propositions focus on management and organization at an abstract level. As a consequence, the application of such foci becomes one in which the empirical setting can be engaged as one sees fit. That is, understanding the historical roots of accounting, as Quattrone discusses, or the struggles of understanding how strategy comes into play, as Kornberger discusses, can without much effort be applied to a public administrative context.

A Research Agenda for Management and Organization Studies provides a rich and interesting flora of possibilities for future research. One reason for this is probably due to the heterogeneity of the contributors. I frequently found myself inspired, intrigued, and challenged during my reading. Yet, though I found the book well-balanced in terms of different scholarly traditions and research scopes, it lacks a chapter that could bring the different perspectives, ideas, and traditions together. Czarniawska makes it clear in the preface that she has no intention of providing a synthesis. As a reader, however, I would have liked to see one. Having said this, I recommend this book to anyone interested in new ways forward for management and organization research.

References

- Hopwood, A., G. (1987). The archeology of accounting systems. *Accounting Organization and Society*, 12(3), 207-234.