

1 Introduction

The management and administration of business has been taught in various forms and under different labels for centuries. Business strategy is a relatively young subset; its core, as we think of it today, emerged in the 1950s and 1960s as a response to increasing pressure regarding the ‘big issues’ in the corporate world: long-term decisions, investments, diversification, geographical expansion and so on. In many ways, the strategy discipline has dealt with matters similar to other academic subjects in relation to ontology and epistemology: the view of the world of business, demarcation to adjacent fields, degree of subject integration, uncertainty concerning the object of study, a developing body of fora and outlets, ethical issues and more. In this Element we focus on another topic common to many academic subjects, not least the social sciences: rigour and relevance and the balance between them. We discuss the development of strategy research in Sweden from its inception as an academic subject until today. We single out a single country, albeit a relatively small and marginal one, to understand how, over time, the view on rigour–relevance is impacted by a range of epistemological concerns, the politics of academic governance, the relations between researchers and, not least, the dynamics of the societal and empirical setting. By understanding the longitudinal development of academic research within a specific institutional context, we learn about explanatory factors as well as the consequences of various rigour–relevance strategies.

We connect with the current debate around rigour–relevance in the field of management and strategy and delve into the relatively brief history of strategy research at Swedish universities. We look into the societal context as well as the immediate context of industry and markets; we discuss different theories and methods as well as international relations and empirical instances that are of interest to researchers. We argue that the Swedish example sheds light on a range of reasons why scholars choose (or end up at) a certain position on the rigour–relevance continuum and what might drive the development in either direction.

2 The Rigour–Relevance Debate

Management education and research can be criticised from many different angles. One is that insufficient attention is paid to practical matters or management practice. The rigour–relevance gap (e.g. Starkey & Madan, 2001) has been discussed in various forms among management researchers since the subject of management was introduced at universities. And scholars have debated this not just within the management field, even if management and business administration appear to be particularly exposed to these concerns. Viewpoints have varied depending on the situation. As early as 1918, Thorsten Veblen claimed that

business studies ‘belongs in the corporation of learning no more than a department of athletics’ (Veblen, 1918, pp. 209–10; Engwall et al., 2010). Similar arguments were heard in the 1950s, including Thompson’s (1956) claim that much of the literature used in education was ‘lore’ and that an administrative science must be developed. In 2008, Daft and Lewin argued along the same lines that the fundamental mission of academic journals should be to publish high-quality research ‘without regard to relevance of the world of practice’ (p. 181). Kieser and Leiner (2009) claimed that management science and management practice are too different from each other, and hence it is impossible to expect both rigour and relevance in research; the two systems can only ‘irritate’ each other. From the other side of the spectrum, however, particularly in recent times, scholars have argued in favour of the opposite (e.g. Hambrick, 1994; Van de Ven, 2002; Bennis & O’Toole, 2005; Augier & March, 2007; Latham, 2019; Gioia, 2022; Lawler & Benson, 2022): that the lack of relevance in research is detrimental to the education of managers. While the debate has long been simmering, some claim that it has intensified over the last decade (Birkinshaw et al., 2016), with the majority suggesting that relevance has been suffering but that there are ways to bridge the gap. Much of the debate takes place in America, even if Europeans have chipped in to the debate as well. It seems to be a bigger challenge in the American context than in the European – perhaps because Europeans have to a larger extent applied other ontologies and epistemologies, including constructionism, subjectivism and fine-grained empirical studies such as case studies, that have helped offer some level of relevance. There has been European criticism of American approaches in other subject fields, such as organisation, but within strategy Europeans have been keener to buy in to an American approach (although there are exceptions that we will come back to).

Much recent work on rigour–relevance can be said to focus partly on more direct aspects of knowledge differences and the nature of knowledge required in practical or academic contexts. A second angle of criticism of management education and research involves a category of factors that is more macro in character and includes the role of institutional forces for research (and higher education) as well as the relations between academia and industry. Some scholars have attempted to find answers to questions about the rigour–relevance dilemma by looking into historical and genealogical explanations of where we stand with management and strategy. Prominent is Khurana’s (2007) history review of business schools.

2.1 Knowledge-Related Differences

Baldrige and colleagues (2004) study the connection between research quality and practical relevance, with relatively disheartening results as the overlap between the two is rather small, thus confirming that there is a significant divide.

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They recommend that researchers study challenges to both practice and academia and also apply methods considered valid by practitioners. Most researchers studying the rigour–relevance gap suggest that it is a challenge rooted in questions related to knowledge. Starkey and Madan (2001) argue that one reason behind the lack of relevance is *the nature of the knowledge* produced by academia. Referring to Gibbons and colleagues (1994), they speak about Mode 1 (‘conventional’ research: homogeneity, disciplinary) and Mode 2 knowledge, where the latter is the result of applied research and characterised by heterogeneity, multidisciplinary approaches and a will to resolve practical problems. According to Starkey and Madan (2001), academic management research has clearly come to focus on Mode 1, and the solutions necessary to balance this in favour of Mode 2 involve radical suggestions such as reorganising universities, their incentive systems and how multi-stakeholder forums form around hot topics, and devising new ways of conceiving ‘impact’. They also propose that we review science funding and experiment more with industry–academy collaborations (Starkey and Madan also refer to a prominent Swedish research programme, which we shall come back to).

Likewise, Van de Ven and Johnson (2006) claim that the rigour–relevance gap relates to knowledge. This might be a *knowledge transfer* problem, or a problem in the sense that knowledge of practice and of theory are two *different forms of knowledge*. As argued by Starkey and Madan (2001), a third angle of criticism relates to the process of *knowledge production* (e.g. Mode 1 and Mode 2 knowledge) where the field has clearly opted for Mode 1-type research, while multidisciplinary Mode 2 projects are secondary. Solutions include research questions related to management practice, collaborative communities, multi-methods and rethinking the role of scholarship, orientating it towards clinical approaches with what is termed action research. Similarly, Birkinshaw and colleagues (2016) show that much of what is published by ‘bridging media’ between the ‘worlds of research and practice’, such as the *Harvard Business Review*, is inductive or theoretical (rather than statistical/deductive). These papers are often deemed more interesting by practitioners *and* also have high academic impact. Relevance impacts both application and research quality. Gioia (2022) is primarily concerned with the lack of relevance of much management research, and argues that the problem has ontological, epistemological and methodological roots. If we do not understand the differences between the science of the natural and the science of society, we run the risk of failing to understand the object of study, of failing to understand the narratives and perceptions of those managers we study, of failing to communicate how the results of our work can be used. Gioia suggests that it is difficult to be relevant if one does not apply a subjectivist/constructionist approach and an inductive method that truly is rooted in the realities of the actual object of study.

Another area focusing on knowledge transfer is the *evidence-based management* project (e.g. Rousseau, 2006, 2007), which is concerned with the ways in which researchers and others successfully bring tested, evidenced practices into organisations. This may relate partly to the distinction between facts-based decision-making on the one hand and intuition, bias or ‘gut feeling’, those inexplicable factors that typically form the bases of decisions, on the other. The rise of ‘the evidence-based zeitgeist’ might appear surprising as this is what managers and consultants (and practice-orientated researchers) have always tried to do, but its increasing popularity signals that something has been missing in our attempts to bring generalised theories into specific contexts. Rousseau (2006) talks about ‘Big E Evidence’ versus ‘little e evidence’, where the former refers to generally applicable theory and the latter to locally applicable theory. Rousseau further argues that one part of the problem is the propensity of teachers to alternate by teaching general management skills and using case examples as evidence, when they should instead focus on tested, evidence-based theories that work across cases. Weak research–education links further expand the divide. Solutions for becoming evidence-based include, as suggested by many scholars, promoting active use of evidence-based management and building collaborations among teachers, researchers and practitioners.

2.2 Relations and Institutions

Bartunek (2007) and Bartunek and Rynes (2010) also take a knowledge perspective and show how the ‘implications for practice’ sections of papers rarely provide practitioners with useful knowledge. One of their solutions is *relational*. They suggest that we look at *integrative scholarship*, that is, research that takes practical concerns seriously and integrates findings within the disciplines into a meaningful whole, where (practice) communities can be researched and related to their everyday concerns. Such scholarship includes textbooks and blogs, consulting, artistic expressions and more. However, as suggested by Bartunek (2007), the practice side probably needs to change too; this might include boundary spanning and relationship building and, by extension, opening up for collaborative research. Bartunek and Rynes (2014) extend the discussion further beyond knowledge and identify a number of tensions connected with the relationships between practitioners and academics. The tensions include *differing logics*, not least when it comes to use of the results of knowledge production, time perspectives, communication forms and both rigour and relevance. They also include *institutional forces*, such as government funding, ranking systems, journal orientation, to which one might add recruitment policy, career management, status and more on the academic side.

Institutional forces on the practitioner side matter too, such as funding, intellectual property (IP) and competition law, and relations to consulting firms.

Another important institutional force in its own right is the subject itself. Agarwal and Hoetker (2007) use industry development theory to describe the evolution of the subject. They scrutinise articles published in the *Academy of Management Journal* (AoMJ) between 1980 and 2005 and observe that the number of citations from outside the management field fell significantly, in favour of references to management research literature itself. Although there were differences according to whether the article in question was of micro or macro nature, in general the once so popular adjacent fields of sociology and economics had become less important, whereas psychology remained strong, particularly in studies at the micro level. They conclude that the subject of management has matured and become a valid discipline in itself. In so doing, it has abandoned much of its multidisciplinary nature and thus lost an important factor behind practical relevance. In the words of Agarwal and Hoetker (2007, p. 1319), the increasing focus over the last decades on discipline and rigour results in ‘theoretical and conceptual frameworks that do not inform those who manage firms and lead people’. That subject fields are born and grow in close connection to existing fields is not new; neither is the importance of economics for business administration and management, even if some argue that, originally, the more valid and rigid discipline of economics offered not only content but also academic legitimacy, which management research itself did not have (e.g. Holmqvist, 2018).

The way in which management research has developed is not unique. According to Whitley (1984), subject disciplines typically emerge as disintegrated and dispersed, and with an object of study that is characterised as uncertain. Here one can assume, as a parallel to Agarwal and Hoetker’s (2007) findings, that the field is more integrated now compared to the 1950s; consequently, the management field may lack some of its multidisciplinary nature, which in turn might explain why we talk about a greater divide between theory and practice nowadays than ever before. Theoretical parochialism typically drives rigour, not relevance, and logically and as we have already stated, many researchers interested in relevance value the disintegrated nature of management and business administration research precisely because it allows the field to respond more dynamically to emerging matters and concerns (e.g. Engwall, 2020).

2.3 ‘Scientification’

One of the more prominent studies of the field of management is Rakesh Khurana’s (2007) review of the history of American business schools. Many people probably think of the ethical issues addressed by Khurana, but one other

matter of significance, at least for the last decades of management research, is declining relevance. Like others, Khurana highlights the importance of the reports known as the ‘foundation reports’ (by Gordon & Howell, 1959 and Pierson, 1959), which were highly critical of the status of management education at the time, a criticism that naturally spilled over to management research. The authors of the Ford and Carnegie foundation reports (who were all economists) had produced thorough pieces of work (the Ford report was almost 500 pages long) and covered a range of measures including faculty, students, curricula, pedagogics and more. They made observations that the standards were ‘embarrassingly low’, that it was in violation of the Association to Advance Collegiate Schools of Business (AACSB) standards and that it was mere ‘vocational training for trade techniques’. The findings led Gordon and Howell (1959) to question whether business management is a profession – something which had been the mission of institutions such as the AACSB to establish since its inception in the early twentieth century. The resolution suggested was to think of business education as a combination of potentially unrelated disciplines rather than as one coherent subject, where these disciplines typically relied on one or more fundamental subject areas, such as economics, sociology and/or psychology (and also maths, statistics and others). Paired with the strong ambitions to improve the education by applying more rigorous scientific standards, the focus gradually shifted away from the world of the general manager to the world of the specialists (e.g. finance, human resources (HR), marketing). ‘Management science’ was the new concept, and to the extent that it meant helping the manager, this was done through the daunting task of establishing a broad understanding of a range of different disciplines; the concerns of the manager were translated into set disciplines. Khurana (2007, p. 271) writes: ‘In contrast to the old model of business training, this new science would allow managers to make decisions solely on analytical and rational grounds, without recourse to fuzzy notions such as intuition or judgment (the latter being a quality that Harvard Business School explicitly tried to cultivate in its students).’

The foundation reports’ recommendations were to increase the proportion of the research faculty, to put a stronger emphasis on quantitative analysis and to develop a clear disciplinary orientation, where hiring from other fields outside of management became the norm; by the mid-1960s, business schools were the major recruiters of graduates in psychology, sociology and economics. Both foundations spent large sums on the business schools that clearly adhered to the new recommendations, including the Graduate School of Industrial Administration (GSIA)/Carnegie Mellon, which became a model for the future business school. Stanford, Chicago, Wharton, Massachusetts Institute of Technology (MIT) and others

followed the recommendations (and were rewarded). Harvard, however, had difficulties adjusting to the new deal, with its general manager-centred programmes. Most importantly, its case-based approach in both education and research was deemed obsolete in the foundation report framework; it was not considered scientific in comparison to the disciplines. Khurana (2007) states that how management problems were resolved in a case was of little interest to the majority of faculty as ‘it was assumed that theoretically deduced solutions would be superior anyway’. The business schools’ efforts at ‘scientification’ in the first half of the 1960s were also aided by the formation of scientific societies and their associated journals (compare with the observations by Whitley, 1984 and Agarwal & Hoetker, 2007). Apart from the worldwide standardisation of curricula and disciplines, changes were made to doctoral degrees. The old, practically orientated DBA (doctor of business administration, particularly popular at Harvard) was gradually abandoned in favour of the discipline-orientated PhD (doctor of philosophy). The DBA typically rested on case-based fieldwork with ‘real’ problems, in clear empirical proximity to the ‘real’ world. The weight Khurana gives to the two reports when it comes to tilting the continuum towards rigour rather than relevance has been contested. McLaren (2019), for example, argues that Gordon and Howell (1959) and Pierson (1959) cannot bear the sole blame for our current-day propensity to choose rigour over relevance. Many other forces, often of an institutional nature, helped as well: the Cold War (and the idea that the enemy must be met with rigorous, hard science), the maturing of the subject field (compare with Whitley, 1984) and the search for academic legitimacy among emerging business schools are also listed as key factors behind the transformation to the research-based model of education.

According to Khurana (2007), the institutionalisation of the 1960s had some major implications. For one thing, MBA students started to complain about the lack of relevance of the programme, as they were taught theories rather than solutions. Their teachers were now young scientists rather than experienced managers or faculty with business experience, which, for obvious reasons, affected many MBAs. Furthermore, Khurana (2007) argues that the era of institutionalisation also produced managers who were less able to understand the impact of business on society. The increasing orientation towards ‘science’ and ‘discipline’ led to the depoliticisation of the profession. Historically, managers in the United States were assumed to, and perhaps did, consider themselves instrumental to American society and its institutions. Students, too, cared less and were more concerned with understanding and using tools and models rather than understanding societal challenges. Teachers did not teach ethics as this was considered outside the scope of disciplines and competence. Khurana (2007) argues that by the end of the 1960s, the foundation reports had ‘dramatically

transformed American business education’ (the only exception being Harvard, which did not fully conform to the Ford model). These forces did not terminate at the end of the 1960s, however. On the contrary, the institutionalisation continued with ever more focus on disciplines rather than general management, which led to a declining interest in the lives of managers. Khurana (2007) lists the recession across many American industries in the early 1970s and the rise of investor capitalism in the mid-1970s as factors or indications that management had lost its status. The shareholder value perspective (Jensen & Meckling, 1976) was an attempt to pit shareholders and managers against each other, exposing corporate management to the market for control. The principal-agent model effectively offered a logic to this arrangement whereby the actions of the chief executive officer (CEO) were expected to be in line with the outcomes defined by the shareholders. This meant rewarding managers to the extent that they delivered on owner ambitions. Management education followed suit and the perception of the general manager became even more like that of a machine, held back not only by subject discipline but also by the stock market. These movements helped push education and research away from the everyday concerns of the general manager. Many people also claim that these were, by extension, major factors behind the divide between rigor and relevance that apparently is still widening.

2.4 The Field of Strategy: Problem and Purpose

Khurana’s (2007) historically based argument is compelling, and we have no reason to assume that it does not also apply to the discipline of strategy, even if the debate has been less intensive in strategy than in other disciplines such as HR and organisation studies. Schwenk (1982) discusses strategy research and how we can balance or combine case-based field research and lab experiments, but the more noteworthy observation here at this point in time is that it is perhaps case-based fieldwork that appears to be the norm – not large quantitative studies (and certainly not lab studies). Mintzberg’s (1977) perspective assumes case work to be the main form of empirical research, as experiments cannot create conditions similar to real-life decision-making, and suggests that strategy researchers ‘have been forced to study real behavior in real organizations, amidst their complexity’ (p. 93). Perhaps this indicates that the strategy field had stayed out of at least some parts of the institutionalisation campaigns, at least until the late 1970s. The Harvard case approach still dominated, but things were changing.

A strong statement that there is also a major rigour–relevance divide within strategy surfaced at the Strategic Management Society’s (SMS) annual conference in Houston in 2017. Some fifteen or so seasoned scholars (who had all attended the founding 1977 Pittsburgh conference, which came to result in the

SMS) debated the importance of the SMS over the four decades, among other things as an educational resource for practitioners. There appeared to be consensus among panellists that the products of the SMS had very limited value outside the immediate academic circle. Someone stated that ‘we have nothing to offer managers nowadays, and they tell us that’, and most of the panellists and audience seemed to agree. Most were saddened, but others appeared fine with this state of affairs, reinforcing that the whole point of the SMS was to steer strategy research away from the case-based business policy traditions most notably associated with Harvard Business School. The aim of the *Strategic Management Journal* (SMJ) and the SMS conference, it was stated, had been to make the subject ‘more scientific’, more ‘rigorous’, more ‘deductive’ and more ‘quantitative’. Thus, most of the panellists argued that increasingly less relevant ideas had come out of the research conducted within the field. The community had chosen rigour over relevance, and perhaps done it well.

So there seems to be little disagreement about the rigour–relevance divide, even if scholars have different views on whether or not it is in order. The nature of the knowledge, the relations between researchers and those who use the models and, as outlined by Khurana (2007), a range of institutional forces, all play an important part in understanding this. Solutions suggested range from quick-fix efforts such as ‘relationship building’ to more fundamental takes on institutional change (e.g. funding). Understanding the entire picture around the increasing gap between research and practice requires more than just grasping the practicalities around knowledge, relations and communication. We argue that this is a matter linked to institutional differences across universities as well as differences across businesses and across the societies in which they act. Business administration in general and strategy in particular are two relatively young scientific fields and, furthermore, they were established to help local industry improve and survive in an increasingly competitive environment; consequently, it is not surprising that the first decades or so provided the scientific field with a local touch and local ‘recipes’. For better or worse, these differences fade over time as the field matures and becomes truly international, with unified models, theories and methods; they become more integrated, to use Whitley’s (1984) terminology. At the same time, as the discipline matures, the impact of the national or the institutional context is eroded. We argue, in this Element, that understanding this dynamic between national, institutional context and the creation of a research community or discourse helps us grasp what can be done to balance rigour and relevance.

We will use the historical development of strategy research in Sweden as an example of said dynamic. The changes described earlier, concerning the way in which rigour has taken over from relevance, have happened in Sweden too.