

Chapter 3: Concepts and Theory Building

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Introduction

Some 70 years ago, Kurt Lewin (1945, p. 132) wrote that 'much is gained, [however,] if one realizes that neither scientific nor practical results can be expected without adequate development of the theoretical aspect of the work'. Within the article, he argued that social science was reaching the stage where it could satisfy the requirements of practitioners' interest in social management. Of prime importance within this was the development of concepts and theories that combined 'generality with the power of reaching the concrete' (p. 132). His argument highlighted the high degree of complexity in the world, emphasising the need for careful diagnosis to enable the application of theory, and the need to avoid the danger of becoming a servant to very one-sided interests. Lewin argued for theory development to be linked closely with practice and to be useful, something we believe is crucial today for DBAs. This provides the focus and purpose of our chapter: to explore concepts and theory building within the context of the DBA.

The utility of management theory for professional practice has been the subject of much consideration in business and management journals in recent years, with a substantial body of literature arguing the need for relevance, alongside rigour, in management research (for example, Hodgkinson et al., 2001; Hodgkinson & Starkey, 2012; Huff & Huff 2001; Rousseau 2006; Starkey & Madan, 2001; Van Aken, 2005). This debate has highlighted the challenges faced by management scholars when deciding whether or not to undertake research at the interface between research and practice, and also potential issues associated with such ways of working (for example, Bartunek et al., 2006; Pollit, 2006; Macbeth, 2002). However, the role of concepts and theory building at this interface and how their utility and application might be made more accessible to practice have been discussed less widely (Bartunek, 2007; Saunders, 2011).

We have written this chapter from the viewpoint that, although not all management research can or should be of direct relevance to practitioners, demonstrating the relevance of theory to practice is an essential component of all DBAs. In particular, such research must address the needs of practitioners, ensuring that the theory they develop is, in Lewin's (1945) terms, both practical and useful. Working at the academic-practitioner interface, researchers need to maintain academic rigour while ensuring practical relevance (Hodgkinson & Starkey, 2012). We adopt Saunders' (2011, p. 243) term 'researcher as practitioner' to refer to those management scholars researching at this theory-practice interface. The chapter starts with a consideration of the nature of theory and concepts in management and, allied to this, the relationship between research and practice in theory building and the differing orientations of management researchers and professional practitioners. Within this, we consider an important debate that has emerged between design science (which is concerned with finding solutions to field problems and developing its own type of theoretical knowledge) and explanatory science (which is concerned with the development of 'traditional' theoretical knowledge to describe, explain and predict phenomena in the physical and social worlds), while also considering the related need for research to be of direct relevance and utility to managers. This debate has resulted in a shift in emphasis from Mode 1 research (which is designed and implemented by and for academics) to Mode 2 research, in which academics and practitioners collaborate in developing knowledge that is usable and developing practical solutions to organisations' problems (Gibbons et

al., 1994). We then consider inductive, deductive and abductive approaches to theory building, followed by discussion of some stage theories to explore how these approaches might relate to the researcher as practitioner.

The nature of theories and concepts

Within business and management, the terms 'concept', 'model', 'theory' and 'framework' are on occasions used interchangeably, sometimes with a degree of conceptual slippage. Indeed, there is lack of agreement as to whether a conceptual framework (or model) and a theory are different or can be distinguished, and even a lack of consensus regarding the definition of a theory (Sutton & Staw, 1995). Although the range of opinions is undoubtedly confusing and liable to lead to misunderstandings, some have argued that it simply arises from different ways of using these terms in the disciplines from which business and management draw, and also from changes over time (Lauffer, 2011). Whatever the reason, the lack of consensus emphasises a need for the management researcher as practitioner to be clear about how these terms are being and should be used in research.

In general, the term 'theory' is used to refer to a systematic body of knowledge, grounded in empirical evidence, which can be used for explanatory and predictive purposes. A theory brings together related facts and concepts that describe and interpret (Lauffer, 2011). It therefore explains or predicts, using supposition or a system of ideas based on general principles, delving into the underlying processes to provide reasons for occurrence or non-occurrence (Sutton & Staw, 1995). Theories are not static, but change on the basis of new, emerging observation and evidence, and must be capable of being verified or contradicted. In order to do this, you (in the role of researcher as practitioner) need to compare the predictions the theory makes with measurements made in the social world (Gilbert, 2008) – that is, the world of practice.

Where theories are presented as guides to action for people in organisations, they are often referred to as (professional) practice principles, or practice theories (Lauffer, 2011). There are three different levels of theory: grand, middle-range and substantive, and the differences between them depend on a theory's capacity to change the way we think about the world, and its general applicability (Saunders et al., 2012). A grand theory, such as Darwin's theory of evolution through natural selection or Einstein's theory of relativity, as well as being universally applicable, changes the way we think about the world. Middle-range theories, such as Taylor's (1911) scientific management, Maslow's (1943) hierarchy of needs or Herzberg et al.'s (1959) two-factor theory of motivation, are more restricted in their application and unlikely to change fundamentally the way we think about the world. However, while building on existing middle-range theories in their research, you may well develop only 'substantive theory'. Substantive theories are restricted to providing insights for a particular time, research setting and problem (Saunders et al., 2012) – and so are less likely than middle-range theories to have general applicability. This is not to say that substantive theories are of limited value. Substantive theories enhance our understanding of particular problems and offer guidance for actions that need to be undertaken in field settings. They may also, in combination with other substantive theories that present similar propositions, lead to the development or refinement of middle-range theories.

Sutton and Staw (1995) offer further insights by clarifying what (new) theory is not. They argue that theory is not simply alluding to (or describing) other theories already developed by researchers. Rather, new theory needs to build a theoretical case logically by drawing upon the concepts, causal relationships and explanations used in existing theory. You are likely to find that the building of this case involves the use of middle-range theories. Sutton and Staw (1995) also emphasise that theory is not just data. While data derived from research can be used to describe *what* has been observed, highlighting patterns and providing support, theory explains through reasoning *why* what has been observed, or is expected to be observed, happens. Consequently, reasoning and explanation are crucial to a theory. Moreover, while a theory can be presented diagrammatically and contains

propositions or hypotheses, it does not need to be represented diagrammatically, and is more than a listing of propositions or hypotheses.

In contrast to theory, the term 'concept' refers to a mental image or abstraction of a phenomenon (Lauffer, 2011). A concept in its broadest sense therefore summarises ideas or observations about all the characteristics of the mental image of the phenomenon (Lauffer, 2011), describing rather than explaining why through reasoning. Using the analogy of a box for the mental image or abstraction, a concept is the box in which we place things we believe to have aspects in common. The concept of organisation therefore includes a wide range of elements, such as people, structure, roles and responsibilities, learning and so on. If this term is amended to the concept of organisational structure, the box becomes smaller and the concept more focused. Some concepts, such as age and gender, are well defined, are relatively easy to understand and can be directly observed. Others, such as culture and trust, are more complex and abstract; sometimes they have competing alternative definitions or are difficult to observe in reality. Researchers of organisational culture (for example, Hofstede et al., 2010; Schein, 2010) emphasise the complexity of the concept, highlighting the different ways and levels at which it is manifest. Some manifestations, termed 'practices' by Hofstede et al. (2010) and 'artefacts' by Schein (2010), are visible and easy to discern when studying an organisation, but – because of their superficial level – difficult to decipher. Other manifestations, termed 'values' by Hofstede et al. (2010) and 'basic underlying assumptions' by Schein (2010), are considered to be of a deeper level and core to the culture but are more difficult to discover, often being invisible. In addition, Schein (2010) introduces a further intermediate level of manifestation into his conceptualisation: 'espoused values' connected with moral and ethical codes that determine what people think ought to be done. Similarly, while an employee's trust in a line manager may be inferred through that employee acting with assurance and taking the initiative, the concept is more complicated than just these manifestations, as it incorporates expressions such as faith, confidence and hope (Saunders et al., 2014).

Consideration of this example of organisational culture as a concept and the related notion of trust highlights the importance of clearly defining concepts that are integral to the research. The definition you use needs to satisfy both academic and practitioner needs, and enable the concept to be communicated to and understood the same way by both the academic and practitioner communities, even if they do not agree on the detail. These two communities have different orientations requiring a focus on different aspects, requirements that you, the researcher as practitioner, must satisfy. Clarity in communicating concepts is particularly crucial where there are competing or varying views on what a concept entails. For example, Taras et al. (2009) point out that over 160 definitions of culture were already in existence more than 60 years ago, and this number is still increasing.

You may combine concepts into a conceptual model or framework. Such models and frameworks represent how the concepts and information relevant to the research are likely to be connected, in effect providing a guide upon which theory might subsequently be built. We have found it helpful to distinguish between models and frameworks and using the term model to refer explicitly to the representation of concepts and their interrelationships. The term framework also includes a consideration of the ontological and epistemological assumptions and previous research upon which the model (the concepts and their interrelationships) is built. For this reason, the term 'conceptual framework' is sometimes referred to as a pre-theory (Lauffer, 2011). However, as we pointed out earlier, terms such as 'conceptual framework' (or 'model'), 'concept' and 'theory' are on occasions used interchangeably. In some journal articles and text books, the term 'conceptual model' or 'conceptual framework' may therefore refer to a theory!

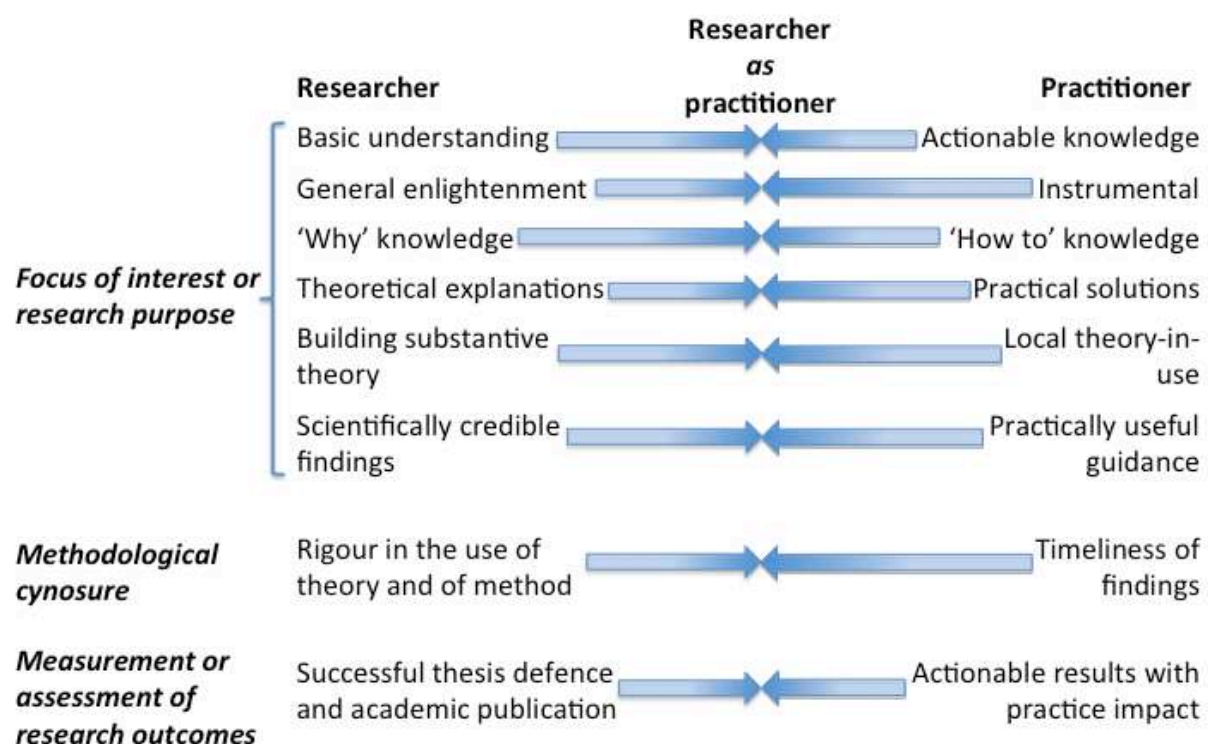
The relationship between research and practice in the building of theory

The literature mentioned in the introduction on the relevance of management research offers a number of useful insights you may use in building a theory. It highlights the tensions that are likely to

be created, and that you can expect to find and you may need to defuse. These tensions relate to three broad areas (Saunders, 2011), the first two being the focus of interest, or purpose of the research; and those aspects of methodology considered most important, which we call 'methodological cynosure'. Management scholars see purpose and methodology as a double challenge: they must be theoretically and methodologically rigorous, while also embracing the world of practice and having practical relevance (Hodgkinson et al., 2001; Wensley, 2011). The third area of tension relates to how the outcomes of the research are measured or assessed (Saunders, 2011). These areas of tension, summarised in Figure 1, reflect broader differences in outlook between some researchers and practitioners. Negative views are represented by those academics who disdain scholars who seek to communicate with practitioners (Bartunek, 2007), and practitioners who see no value in collaboration and who deprecate or ignore academic research (Kerr, 2004). Conversely, positive views include wanting to make a difference and encouraging management researchers (such as the researcher as practitioner) to develop valid knowledge to support organisations (Huff et al., 2006). This positive approach is supported increasingly by government drives for relevance in academic research, such as the UK's Research Excellence Framework.

Figure 1: Tensions when building theory for the researcher as practitioner

Source: Developed from Saunders (2011)



Within the academic literature, several commentators highlight a fundamental separation between researchers and practitioners in relation to the focus of interest or research purpose (Van Aken, 2007). Much of this debate has centred around whether management is better considered a design science or an explanatory science (Van Aken and Romme, 2009) and, allied to this, Gibbons et al.'s (1994) work on how knowledge is produced. The debate is basically between Mode 1 and Mode 2 concepts of knowledge creation (Tranfield, 2002). Mode 1 refers to knowledge produced by scientific theory alone and which is of a fundamental rather than applied nature with little, if any, focus on the use of research by practitioners. In contrast, Mode 2 refers to knowledge produced by interdisciplinary teams of an applied nature being governed by the world of practice, and highlighting

the importance of collaboration with and between practitioners. The focus of explanatory sciences, which include disciplines such as a sociology, psychology and the natural sciences is to ‘develop knowledge to describe, explain and predict’ phenomena in the natural or social world (Van Aken, 2005, p. 20). In contrast, the focus of design sciences (Simon, 1969/1996), such as medicine and engineering, is to develop actionable knowledge that can enable ‘organizational problem solving in the field’ (Huff et al., 2006, p. 413).

Mode 1 knowledge creation is based on research in which questions are set and solved according to academic researchers’ interests, and emphasises basic understanding and general enlightenment. Such research focuses upon description, explanation and prediction, and the building of substantive theories to explain why. This mode can be considered akin to much of the research in the explanatory sciences. Mode 1 researchers strive for generalisable cause–effect relations (Gray et al., 2011). In contrast, Mode 2 knowledge creation is grounded in the world of the practitioner, with a focus on developing knowledge that is usable and developing and testing practical solutions to organisations’ problems (Huff et al., 2006). This aligns with the concerns of the design sciences, which highlight ‘how to’ rather than ‘why’, and strive for the creation of actionable knowledge (Argyris, 1996). This is not to say that design science does not develop theories, but rather that the theories and the knowledge developed support professionals in taking decisions and solving problems (Van Aken, 2005). Practitioners need techniques and methods that can be applied immediately, and that may rely on a different evidence base to that required by academics. They may have to take it on trust that these techniques can deliver robust, practical and valuable outcomes. Anderson et al. (2001, p. 405) refer to this condition as short-term ‘faith validity’, arguing that it can be delivered only by Mode 2 research.

In Mode 2 research, knowledge is generated in the context of multi-disciplinary teams, working on problems found in everyday working life. Teams create theoretical frameworks in the context of the application of knowledge, and often include members who are potentially the users of the new knowledge (Gray et al., 2011). Mode 2 research requires both relevance and academic rigour (Anderson et al., 2001). Hence, MacLean et al. (2002) point to five key features of Mode 2 research: knowledge is produced in the context of application; it is transdisciplinary and involves different sets of skills; it is tackled by transitory, heterogeneous teams whose members come and go as the situation unfolds; it is socially accountable and involves greater levels of communication and transparency; and it requires a more diverse range of quality controls. So, while in Mode 1 research the quality of knowledge is usually judged from the standpoint of the discipline, its most respected scholars and a ‘blind’ peer review process, in Mode 2 quality controls have to reflect a much broader community of stakeholders (MacLean et al., 2002).

Some, however, have now progressed beyond Mode 2 and have called for Mode 3 research, defined by Huff and Huff (2001, s. 53) as: ‘knowledge production ... to assure survival and promote the common good, as various levels of social aggregation’. Some of the reasons why the basic issues of human existence cannot be addressed by Mode 1 and Mode 2 research are connected to how and why knowledge production is activated and by whom (as represented in Figure 1). In Mode 1, members of disciplines advance their own and their discipline’s work when they identify gaps in theory; in Mode 2, problems are encountered in specific practices in field settings, often connected with the pursuit of profit. Indeed, Mode 2 projects often operate on national and international scales that exceed the capabilities of legal systems, hence bypassing the larger social consequences of their work. We only need to look at the recent media reports of large corporations’ application of carefully researched legal corporate tax loopholes to see the consequences for governments and the revenues they have to provide services (Barford and Holt, 2013). However, in contrast to this are the Mode 3-type activities (often undertaken by not-for-profit organisations) that unite action and research in projects directed to help humanity (Huff & Huff, 2001).

Table 1: Alternative modes of knowledge production

(adapted from Huff & Huff, 2001)

Descriptors	Mode 1	Mode 2	Mode 3
Activity triggers	Theoretical or empirical gaps	Practical problem	Appreciation and critique
Participants	Homogenous Sub-disciplines	Activity-centred Transdisciplinary (including Mode 1)	Diverse stakeholders (including Mode 1 and 2)
Goals	Truth, theoretical extension	Solution, improvement	Future good
Methods	Pre-tested, paradigm-based	Often invented, based on experience	Collective experience, conversation
Activity site	Sheltered, laboratory, 'ivory tower'	Practice, the workplace	Society, the community
Time horizon	Individually driven, often unimportant	Often immediate or urgent	Immediate to very long term
Boundaries	Disciplinary, pure/applied, institutional	Transdisciplinary, often proprietary	Multiple modes of knowing
Beneficiaries	Individual scientists, professional groups	Firms, government, commercial/regulatory bodies etc.	Society
Quality control	Elite-dominated, peer review	Utility, efficiency	Community agreement
Funding (primary source)	University, government, EU	Business	Philanthropy, university, business, government
Dissemination	Scholarly conferences, academic journals	Practitioner conferences, policy documents, internet	Local to global debates and action, media reports

The methods and products of design science and Mode 2 research cannot be equated to the managerial anecdotes often found in the populist management literature (see Figure 2, Quadrant 1: Popularist Science) or the 'theories' that are implicit in the actions of practitioners. Although these are termed local theories-in-use (Argyris & Schön, 1974), in many instances they would not satisfy our earlier definition of theory as they neither explain explicitly nor predict, and are limited in their transferability to different contexts (Denyer et al., 2008). While popularist science may have a high practical relevance, its methodological rigour is low – take, for example, some of the popular 'how to' management books on subjects such as emotional intelligence, leadership and coaching. Pedantic science (Quadrant 3) is the result of research that adopts sophisticated designs but produces findings of low practical relevance to organisations or practitioners. Quadrant 4 is what Anderson et al. (2001) term 'puerile science', where researchers produce studies of limited practical value, using methods that lack rigour (for example, using small samples and a single, non-validated data-gathering instrument).

Figure 2: Fourfold typology of research (Gray et al. 2011, adapted from Anderson et al., 2001

		Methodological Rigour	
		Low	High
Practical Relevance	High	Quadrant 1 ‘Popularist Science’	Quadrant 2 ‘Pragmatic Science’
	Low	Quadrant 4 ‘Puerile Science’	Quadrant 3 ‘Pedantic Science’

Case study 1 : An example of pragmatic science

Sakellariou (2008), in a DBA thesis, sought to gain a deeper understanding of the product innovation process, both to generate practical ideas that could be implemented in her employing organisation (a large multinational conglomerate) and to make a contribution to theory. In doing this, she examined the literature on new product development and in particular the ‘innovation funnel’, where she found a lack of consistency – different authors proposed different steps for the innovation funnel. These steps in the innovation process were analysed at a conceptual level and were synthesised and classified under three major stages, namely: Ideas/Concepts (Stage 0), Feasibility/Capability (Stage 1) and Launch (Stage 2). After reflecting on the theoretical model (and discussing her ideas with her organisational sponsors), she decided to focus on Stage 0: Ideas/Concepts for her study, partly because, from a practical perspective, this stage remained ‘fuzzy’ and insufficiently understood. In conducting the research she adopted an action research methodology, working collaboratively with the organisational team responsible for front-end product development and with consumers who worked with the team to test and provide feedback on potential new products. Due to time constraints, it was possible to work through only one phase of the action research cycle (i.e. planning, acting, observing and reflecting). The research had two positive outcomes: first, new approaches to product innovation were identified that were of practical importance to the organisation; second, the research provided an opportunity to modify the innovation process model – a contribution to theory.

As a researcher-as-practitioner needing to inhabit both the academic and practitioner worlds, you have to address and resolve the tensions created by these differing foci of interest. It could be argued, for example, that these tensions are fuelled by fundamental ontological and epistemological differences about knowledge production and consumption – especially between an academic’s search for the generalisable and a practitioner’s search for specific solutions. Short (2006) refers to the tension in a variety of ways: the *research–practice gap*; the *implementation gap*; the *research–*

practice divide; and the *theory–practice void*. In the management/organisational studies field, it is often characterised as the ‘rigour–relevance’ debate (e.g. Aram & Salipante, 2003; Fincham & Clark, 2009), also represented in Figure 2.

While both groups may be interested in the same subject matter, the management researcher’s focus can be characterised, broadly, as pushing back the frontiers of knowledge (Macbeth, 2002) through scientifically credible findings. These findings will support the genesis of theoretical explanations for problems. Producing such explanations is crucial to your success as a management researcher-cum-practitioner in the academic world. Yet such scientifically credible findings can also be used to meet the practitioner’s focus, typified as improved understanding of a particular business problem to provide results-orientated, practically useful guidance (MacLean & Macintosh, 2002). For some writers this tension between researcher and practitioner foci is intractable. Kieser and Leiner (2009, p. 517), for example, argue that researchers and those they research inhabit separate social systems, leaving an unbridgeable gap ‘not only attributable to different languages and styles in the scientific community, but also to different logics – to differences in defining and tackling problems – that prevail in the systems of science and practice’. Others, such as Hodgkinson and Rousseau (2009, p. 538) disagree, arguing that collaborative research can be both rigorous and relevant; ‘developing deep partnerships between academics and practitioners, supported by appropriate training in theory and research methods, can yield outcomes that meet the twin imperatives of high quality scholarship and social usefulness’. They argue that the gap between research and practice may be due to little more than differences in style and language and that management researchers can generate knowledge that is both socially useful and academically rigorous. Indeed, according to Starkey et al. (2009), relevance is a necessary condition for rigour. Through also using findings based on rigorous research to develop both theoretical explanations and (professional) practice principles, the researcher as practitioner might overcome the potential tensions and uphold the foci of interest for both researchers and practitioners.

The methods and methodology used to collect and analyse the data from which findings are derived represents a second area of potential tension. As we have noted, albeit briefly, academic research as explanatory science is expected to be theoretically and methodologically rigorous – a point emphasised in numerous publications (for example, Garmen, 2011; Saunders, 2011). However, Hodgkinson et al. (2001) argue that the pedantic nature of social science, characterised by an increasing focus on methodological rigour, is to the detriment of results that are relevant. In addition, ensuring such rigour is invariably time-consuming, and this can cause tension with the need for findings to be timely if they are still to be of relevance to practitioners. Where organisations require urgent solutions to problems, pragmatic organisational pressures can compromise theoretical and methodological rigour (Van De Ven & Johnson, 2006). You may find this tension between rigour and relevance impossible to defuse. The different foci of interest (outlined earlier) set the researchers’ requirement for theoretical and methodological rigour against practitioners’ need for a timely (often urgent) solution. We will return to this in our examples. Help, however, is at hand. MacLean and MacIntosh (2002) offer some practical guidance on how management researchers and practising managers can collaborate effectively, based upon their experience of various Mode 2-type projects. Table 2 provides a summary.

Table 2: Dos and don'ts for researchers and practitioners in conducting Mode 2 research projects

Adapted from: MacLean and MacIntosh (2002)

	Researchers	Practitioners
Do	Have a clear view of research questions/issues at the outset Select partners carefully Search for real business issues that allow for the exploration of these questions View everything as constantly negotiable Ensure the research is sponsored, financially or in other ways Contribute to the development of process ground rules for the interaction	Be clear and realistic about the desired practical outcomes Select partners carefully – ask about and examine previous work Choose an area of research of interest as a co-researcher – matching area of expertise Set aside necessary resources (and time) and contributors Read up on the research area – demand clear explanations Contribute to the development of process ground rules for the interaction
Don't	Expect to design the research process and stay in control of it Work with people who have no direct stake in the research Over promise on results Get diverted by the business problem – it is research not consultancy	Expect automatic success Take unacceptable risks Try to separate the problem from the research Force staff to participate

As we can see in Table 2, both researchers and practitioners need to be aware that compromise is often necessary. This must be based upon an understanding of the other stakeholder's needs. The next section on approaches to theory building provides further evidence of this.

Approaches to theory building

Earlier in this chapter, we highlighted that clear, reasoned argument was central to building theory. This raises an important question about how argument can be used to build theory and how this theory can be tested subsequently. Although often portrayed simplistically as two contrasting approaches, deductive and inductive reasoning, there are in fact three widely used approaches, the third being abductive reasoning (Suddaby, 2006).

If you start with theory, perhaps developed from reading the academic literature, and design a research strategy to test that theory, you are using a deductive approach. Alternatively, if you start by collecting data to explore a phenomenon observed in practice and from this you develop a conceptual model upon which theory is built, this is an inductive approach. However, where you collect data to explore a phenomenon, identifying themes and explaining patterns to generate a new or modify an existing theory, which is subsequently tested through additional data collection, you are using an abductive approach.

The deductive approach to theory building commences with the development of a clear argument, usually based on general principles derived from the literature. This seeks to explain or predict a particular phenomenon and is then subjected to a rigorous empirical test. Blaikie (2010) argues that a theory in deductive research comprises a series of general premises. These are testable hypotheses or propositions that operationalise the concepts, explain the relationships between the variables associated with these concepts in a way that can be measured, and outline the conditions under which these relationships are likely to hold. Within this approach, the logic of the argument on which the theory and its component premises are based and their grounding in academic literature is crucial. You, the researcher as practitioner, test the theory through its component premises by collecting and analysing data within the same conditions as those the theory is predicted to hold. Such testing usually adopts a highly structured methodology to facilitate replication (Gill & Johnson, 2010) and to help ensure reliability. When the results of this analysis are consistent with the premises, the theory is corroborated; where they are not consistent, the theory must be either rejected or modified.

Some management researchers are critical of deductive approaches to theory building, arguing that the cause–effect link between particular variables can be made only by understanding the way in which humans interpret their social world (see also Chapter 3). Developing such an understanding is a strength of building theory inductively. Those using an inductive approach to building theory would also criticise deduction because of its tendency to construct a rigid methodology that does not permit alternative explanations of what is going on. While alternative theories can be suggested by a deductive approach, these will invariably be within the limits set by the associated structured research design. A less structured inductive approach might reveal alternative explanations.

If you use an inductive approach, you are likely to be particularly concerned with the research context within which the theory is being built. You would start by developing a feel of what is happening so as to better understand the nature of the problem. Different possible views of the phenomenon would be established, having used a variety of methods to collect data, which would often be qualitative (Easterby-Smith et al., 2012). Theory would then be built inductively from these data.

Rather than moving from theory to data (as in deduction) or from data to theory (as in induction), an abductive approach moves back and forth, in effect combining deduction and induction (Suddaby, 2006). Abductive reasoning begins with the observation of a ‘surprising fact’ or phenomenon and then develops a plausible theory of how this could have occurred. Van Maanen et al. (2007) note that some plausible theories can account for what is observed better than others, and these theories will help uncover more ‘surprising facts’. Data that are sufficiently detailed and rich are used to explore the phenomenon and identify themes and patterns, which are located in a conceptual framework. This framework is tested using existing data and through subsequent data collection in an iterative process to build a theory. The theory is modified as necessary, using a process that moves between testing the theory with data and using data to further develop the theory.

The question of whether you should develop theory predominantly deductively, inductively or abductively depends on the emphasis of your research and the nature of the research topic. Topics or problems where there is a wealth of literature from which to define a theoretical framework and hypotheses or propositions are often considered more suited to deduction. Where the topic or problem is less well defined, or there is limited literature available, or there is considerable debate in the literature, an inductive approach to building theory may be more appropriate. Alternatively, a topic about which there is a wealth of information in one context but far less in the particular context of interest may lend itself to an abductive approach through which an existing theory is modified.

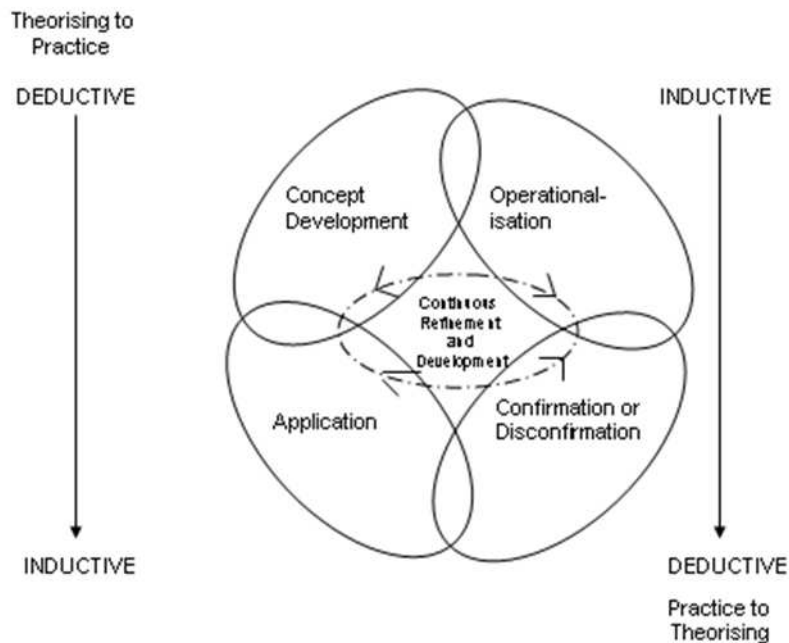
As we have already highlighted, time – and, in particular, a requirement to meet organisational deadlines – is a particular issue for those working with practitioners. Building theory deductively can be quicker, even though it will take time to set up the study prior to data collection and analysis. As

the data are collected during one time period, it is often easier to plan the time required to complete the project. In contrast, abductive – and, particularly, inductive – theory building can be much more protracted. Often, the concepts and conceptual model from which the theory will be built emerge only gradually, necessitating a longer period of data collection and analysis. Deduction is argued to be a lower-risk strategy as you already have a theory to test, even though there are risks, such as the non-return of questionnaires (Saunders et al., 2012). In contrast with both induction and abduction, it may well be that no meaningful or useful data patterns and theory will emerge from the analysis of the data.

From our experience, most managers are more familiar with the methods associated with deductive processes of theory building, in particular statistical testing. Although this is changing, we have found that many organisations like research that presents quantitative data, and are more likely to prefer practical guidance emanating from such outputs. Similarly, you will also have a preferred style for theory building. This is important because, as Buchanan et al. (2013, p. 59) argue, the ‘needs, interests and preferences (of the researcher) ... are central to the progress of fieldwork’. However, if you are assigned a research question by a client in an organisation, it is important that your personal preferences do not lead you to amend the essence of the research question. Any such changes will at best result in your research findings being ignored. At worst, they could result in the access to undertake the research being withdrawn, or even loss of employment!

So far, we have assumed that approaches to theory building adopt one approach (inductive, deductive or abductive). However, Lynham (2002) suggests a more complex model based upon five stages, which she calls the General Method of Theory Building (see Figure 3), and which includes both conceptual development and application. ‘Conceptual development’ requires that ideas are formulated in a way that reflects the most informed understanding of a phenomenon within a relevant world context. The building of this conceptual framework is not limited to one epistemological position, but can apply both hypothetico-deductive and inductive/qualitative approaches. After conceptual development comes ‘operationalisation’. This seeks to make explicit the connection between the theoretical framework and practice. This stage allows theory to be empirically tested in a real-world context. The third phase, ‘confirmation or disconfirmation’, involves conducting a research study to confirm (or refute) the theoretical framework. A fourth phase, ‘application’ is then conducted, in which the theory is refined through further studies, and where the relevance of theory is tested in practice. These phases, however, are not as neat and sequential as implied. Lynham (2002) notes that applied theory building can begin with any of the phases. However, while the general method allows for multi-paradigm research, Storberg-Walker (2006) warns that it does not offer explicit steps for completing each phase.

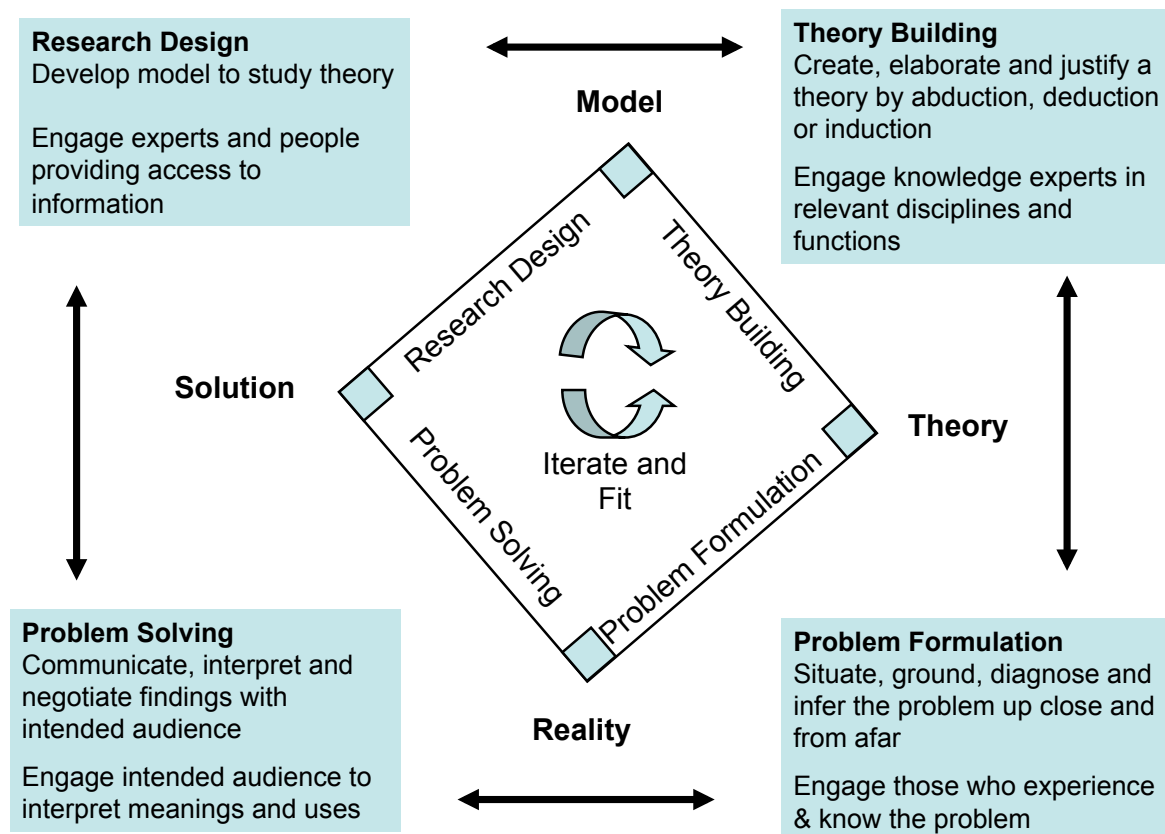
Figure 3: The general method of theory-building research in applied disciplines (Lynham, 2002)



This deficiency does not occur, however, with what Van De Ven (2007) calls the Diamond Model (see Figure 4), which represents part of what he terms ‘engaged scholarship’, a participative form of research that seeks to obtain the perspectives of key stakeholders (researchers, users, clients, sponsors and practitioners). Research is undertaken *with* stakeholders, not *for* them; hence, stakeholders are engaged at the four stages, as follows:

- Problem formulation – you talk to those who experience the problem, as well as reviewing the literature.
- Theory building – you create the theory either inductively or deductively, and validate it by having discussions with knowledge experts from disciplines and functions that have addressed the problem.
- Research design – you formulate an appropriate methodology, but also discuss this with technical experts. You also talk to people who can provide access to data, as well as respondents or informants.
- Problem solving – you apply the findings, but also engage with the intended audience to interpret meanings and uses.

Figure 4: Practising engaged scholarship (adapted by Gray et al., 2011, from Van De Ven, 2007)



However, as Gray et al. (2011) point out, if researchers seek to make theory more relevant to practitioners, what do we mean by 'relevance'? Practically relevant research should be seen as subject to negotiation between stakeholders, and may change between one time period and another. It will also depend on the particular needs and interests of stakeholders (researchers, practitioners, managers, sponsors, fundholders, policy makers etc.). Similarly, what constitutes methodological rigour is contested, and depends on the epistemological and ontological position taken. Mode 1 research will often address more focused and discrete questions, and will emphasise research rigour. Mode 2 research, however, is often more concerned with deadlines and an urgent need to address a current problem or issue. So how can the two be reconciled? Iles and Yolles (2002) provide an example of a project aimed at developing 'technology translators', the aim of which was to bridge the knowledge gap between key staff in small/medium-sized enterprises and the academics involved in the research. Such translators could be practitioners with academic research backgrounds (e.g. DBAs, PhDs) or academics with practitioner knowledge and experience – the researcher-as-practitioner (Saunders, 2011). Case study 2 provides our second example.

Case Study 2: The researcher as practitioner – making a contribution to theory and practice

Alison is a professional coach and also a coach supervisor – that is, she provides support and help in the professional development of fellow coaches. She is qualified to do this given that she has several decades' experience as a coach, and is also trained and accredited as a supervisor. Three years ago, Alison embarked on a DBA that sought to investigate what 'goes on' in the supervisor-coach relationship, particularly in terms of learning processes. Being both a coach and a supervisor, Alison

considered it legitimate to bring elements of her own story into the research. Hence, she provides biographical details that illustrate the twists and turns of her career, showing where her curiosity about supervision developed, and how the problems and disappointments she faced have generated learning that influence her role as a coach and supervisor. In the thesis, she describes the development of coaching and supervision, including the significant lack of engagement with supervision within the coaching industry. She also shows that the subject of supervision in coaching is under-researched. In other words, she identifies the gap in knowledge that she intends to fill.

In her methodology chapter, Alison describes what she calls a 'magical mistake' in terms of the panic and meltdown she endured on her academic journey. Often, as an experienced practitioner but not an experienced academic, she felt doubts about her own research capabilities and even wondered whether she would complete the journey. Her academic supervisors, however, gave her reassurance and support. In the thesis, she reflects on how she came to grips with the issues and comments on her own learning. Key to the methodology is action research and a desire not only to investigate practice but also to improve it. The overarching research design, using separate action learning sets of coaches and supervisors, is described and justified. The collaborative inquiry approach appears to work in terms of generating learning within the groups and the kinds of valid data required by the study. Alison skilfully demonstrates the process involving the co-creation of knowledge between the researcher and professional practitioners (of which she is one). She is also highly reflexive in terms of how the research impacts on her own professional practice as a supervisor. She is also prepared to comment critically on her own performance as a researcher – for example, posing ambiguous questions. In doing this, she demonstrates that she is also learning to do research.

Alison's analysis presents a contribution to theory through what she terms the 'three pillars of supervision', in which she talks about the need for supervision to engage with the theories of both adult learning theory and reflective practice. This is a significant contribution because, to date, most supervision theories have been developed within the discipline of psychology rather than adult learning (andragogy). Hence, in this DBA, Alison has been able to make a contribution towards the practice of coaching and supervision, to the theory of supervision and, of course, to her own professional practice.

Conclusion

Our overview of management researchers' and practitioners' differing orientations and requirements highlights that when building theory, the researcher-as-practitioner needs to:

1. satisfy both management researcher and practitioner foci of interest
2. utilise theoretically and methodologically rigorous research designs in a timely manner
3. meet the impact requirements of both academic publication and practitioner practice (i.e. make a contribution to both theory and practice)
4. adopt an approach to theory building that meets their preferences as researchers and is acceptable to practitioners.

As we have seen, overcoming these tensions is no simple endeavour. We have attempted to show, however, that they are not insurmountable. A key approach is for you as a researcher as practitioner is to engage with stakeholders to tackle research *with* them and not *for* them. This engagement should be enacted at four stages: problem formation, theory building, research design and problem solving. The relevance of research to both academia and practice is not predetermined but an element that is established through negotiation. Clearly, for this to work, there must be trust between all stakeholders. Increasingly, however, some (for example, Huff and Huff, 2001) have come to question the legitimacy of even Mode 2 research, calling for research to engage with wider, societal issues that embrace the common good and the nature of human existence.

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