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Scientific Research in Information Systems

A Beginner's Guide

 Springer

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Preface

This book is designed to introduce doctoral and other higher-degree research students to the process of scientific research in the fields of Information Systems as well as in Information Technology, Business Process Management and other related disciplines in the social sciences. Information Systems is the research field concentrating on the impact of *information technology in use* by individuals and organisations alike, and is a fundamental element in the understanding of current and future business, management, societal and individual practices.

This book is based on the author's experience in information systems research, research-level teaching and postgraduate student supervision at Queensland University of Technology, the leading institute for information systems research in Australia. Being based on these experiences, the book is also positioned relative to the worldview and experiential accounts by the author. In consequence, while most aspects of the book relate to general experiences, challenges and processes in scientific research in general, it is mostly influenced by the domain and scientific paradigm in which the author conducts his research.

This book is not meant to replace but rather to complement existing textbooks for research students and scholars. Unlike other textbooks, this book is not primarily about research methods but instead covers the entire research process from start to finish. It places particular emphasis on understanding cognitive and behavioural aspects such as motivational components, various modes of inquiry in scholarly conduct, theorising, planning of research, as well as publication plans, and the ethical challenges of becoming a researcher.

The book is meant to guide research students in their process of learning the life of a researcher. In doing so, it provides an understanding of the essential elements, concepts and challenges of the journey of research studies. It provides a gateway for the student to inquire deeper about each element covered (such as a particular research method) by directing them to the appropriate resources.

The book is designed to be comprehensive and broad but also succinct and compact. It focuses on the key principles and challenges (namely, the core of the research process) without delving into the many associated challenges, exceptions and further issues. In essence, the book is meant to guide beginning doctoral

students in their quest to do scholarly work and provides an easy and comprehensive introduction that can assist them in developing their own answers and strategies over the course of their studies.

Structure

This book is organised into seven chapters, which span three parts:

Part 1: *Basic Principles of Research* introduces to the reader the journey and challenges of pursuing a research degree in information systems. Motivational components are discussed as well as key underlying principles drawn from the paradigms of the scientific method. It introduces the vocabulary of research and lays the foundation for reflecting upon research in Information Systems, be it the students' own research or the ones they encounter.

Part 2: *Conducting Research* dissects the research process in information systems as a science into the phases of planning, designing and executing. Each of these phases then receives due attention in terms of planning the research design, developing and appropriating theory in research and executing relevant research methods. In doing so, this part of the book aspires to give a general process model applicable to all doctoral students in information systems, and contains guidelines that can help in tackling challenges along the way.

Part 3: *Publishing Research* reflects upon the outcomes of the research process and provides practical suggestions for managing these outcomes. Publication strategies and writing tactics are the focus of this part alongside ethical considerations pertaining to research in terms of conduct and publication. It also offers a closing reflection on the key processes and outcomes of scholarly work.

Acknowledgements

There have been many people that influenced my work and more generally my life in significant ways. Certainly too many to mention here, and if I did I would only – in an undeliberate way – omit some of them, so I won't even start.

I do want to mention a number of people. My gratitude is owed to my colleagues in my department, especially our Head of School, Professor Michael Rosemann, for being a mentor and advisor, a critic and a friend – and for keeping my back free while I invested my time into this book.

My own thinking is much inspired by the academic scholars that I work with and who I hold in highest esteem for their success, professionalism, dedication and enthusiasm. I am a much better researcher than I could have ever hoped to be because I was allowed to work with people such as Peter Green, Marta Indulska, Jan Mendling, Hajo Reijers, Jan vom Brocke, Stefan Seidel, Michael zur Muehlen, Guy

Gable and many other esteemed scholars that I have met and worked with over the years. I owe a great deal of advice and experiences that I offer in this book from the knowledge that they, and others within and outside academia, were willing to share with me.

This book is also improved because of professional editing support that I received through manuscript editing services provided by Academic Research, Writing, and Editing Services Ltd., Pittsburgh, USA.

I also believe that acknowledgement is due to those that are in my life without crossing the boundaries of work – friends who share their life experiences with me and who are happy to indulge in mine. I know that they know that I am referring to them at this stage, so be thanked for making my life a wonderful and joyous experience.

Finally, my thoughts are with my family, my parents and sisters, my grandmother and all other members. Being an academic has meant for me physical separation but has also helped me learn the value in having these wonderful people as my family, and enjoying the precious moments we have together as much and as often as possible.

Laura, you are inspiring me all the time to be the best person that I can possibly be, and much more than that. You are my reason to go out and do the best I can – and more importantly, to come home.

June 2012

Jan Recker

Contents

Part I Basic Principles of Research

1 Introduction	3
1.1 Understanding Your Motivation	3
1.2 The Ph.D. Challenge	6
1.3 What This Book Is About	9
1.4 Further Reading	10
2 Information Systems Research as a Science	11
2.1 Principles of Scientific Inquiry	11
2.2 The Scientific Method	15
2.3 Essential Concepts in Information Systems Research	18
2.4 Further Reading	21

Part II Conducting Research

3 The Research Process	25
3.1 Research Questions	25
3.1.1 Motivation	28
3.1.2 Specification of Problem Statement	29
3.1.3 Justification	29
3.2 Research Design	30
3.2.1 Observation, Induction, and Deduction	30
3.2.2 Selecting a Research Design	33
3.3 Research Methodology	36
3.4 The Role of Literature in the Research Process	39
3.5 Further Reading	42
4 Theorising	45
4.1 What Is Theory?	45
4.1.1 Definition of Theory	46
4.1.2 Building Blocks of Theory	47

4.1.3	Nomological Nets	50
4.2	Types of Theory	52
4.3	Theorising as a Process	57
4.3.1	An Example	58
4.3.2	Practical Suggestions to Theorising	62
4.4	Further Reading	64
5	Research Methods	65
5.1	Quantitative Methods	66
5.1.1	The Focus on Measurement	67
5.1.2	Reliability	69
5.1.3	Validity	70
5.1.4	Measurement Development	71
5.1.5	Quantitative Method Procedures	75
5.1.6	Survey Research	76
5.1.7	Experimental Research	81
5.2	Qualitative Methods	88
5.2.1	Data Collection Techniques	90
5.2.2	Data Analysis Techniques	92
5.2.3	Case Study	95
5.2.4	Action Research	99
5.2.5	Grounded Theory	102
5.3	Mixed Methods and Design Science Methods	103
5.3.1	Mixed Methods	104
5.3.2	Design Science Methods	106
5.4	Further Reading	109

Part III Publishing Research

6	Writing IS Research Articles	113
6.1	Strategising	115
6.1.1	The Publishing Process	116
6.1.2	Key Publishing Decisions	119
6.1.3	Co-authorship	122
6.1.4	The Pre-submission Lifecycle	124
6.2	Structure and Contents	127
6.2.1	Introduction	128
6.2.2	Background	129
6.2.3	Research Model	130
6.2.4	Research Method	131
6.2.5	Results	132
6.2.6	Discussion	132
6.2.7	Implications	133
6.2.8	Conclusions	135
6.2.9	Abstract	135

6.3	Handling Reviews and Revisions	136
6.3.1	Understanding Reviews	136
6.3.2	Managing Revisions	138
6.4	Further Reading	140
7	Ethical Considerations in Research	141
7.1	The Role of Ethics in Research	141
7.2	Ethical Issues in Conducting Research	143
7.3	Ethical Issues in Writing	144
7.4	Further Reading	146
8	Instead of a Conclusion	149
	References	151
	Index	159

Part 1
Basic Principles of Research

Chapter 1

Introduction

1.1 Understanding Your Motivation

Welcome! You have just now commenced your journey as a doctoral scholar in the discipline of your choice. We call this research discipline “Information Systems”. Information Systems is mostly concerned with the socio-technical systems comprising organisations and individuals that deploy information technology for business tasks. In a broader way, the Information Systems research discipline is concerned with examining Information Technology in use [127].

This field of research is exciting, broad, and multidisciplinary in nature. It is an exciting field of research because the speed of technological advancements in the area of information and communications hardware and software has not only been rapid but is continuing to rise at an enormous pace. A life without smart phones, Internet, social media platforms, and micro blogging seems unthinkable as of today – but these are all developments of just the past two decades. Yet, for many of us, it is hard to recall a time when information technology wasn’t as pervasive, ubiquitous, and vital in our business and private lives.

Given this high impact of Information Systems in our lives, it is not surprising that the study of information-technology-in-use has become an important topic area for academic scholars. It is not so much the fact that more and more researchers devote their attention to such issues, but rather that the way they attend to the challenges around the topic take so many different forms and touch upon various other topics from several disciplines. Scholars around the globe are addressing a variety of key questions pertaining to information systems, which include but are not limited to the following:

- How do individuals use information technology?
- How does information technology contribute to organisational performance?
- How are organisations using information technology?
- How can societal issues be resolved through information technology?
- How can healthcare be assisted through information technology?

- How do we use information technology for learning and for entertainment? How do the two interact?
- How do we humans interact with information technology?
- What role does information technology play in global markets and local governments?
- How does information technology allow an organisation to innovate its business processes?
- How can information technology contribute to the greening of the planet?
- How do humans build capabilities in dealing with information technologies and related issues?

One may already anticipate that seeking answers to these questions can take many forms, and the potential pathways to these and other questions are rooted in a variety of well-established fields of scientific inquiry. The below is but a list of the most prominent types of inquiry and by no means complete:

- Scholars in information technology, software engineering, and computer science study the technical and computational attributes of information technology as such.
- Scholars in behavioural, cognitive, and psychological sciences study individuals' exposure, use, appropriation, and general behaviours within information technology domains.
- Scholars in organisational science, management, and business study how corporate environments shape, and are shaped by, information technology.
- Economists study the large-scale effects of information technology diffusion and innovation on organisations, markets, and societies.

As detailed above, the study of information-technology-in-use can be very diverse, leading to both the exciting and the challenging nature of this research field. It is exciting because research in this area is multi-faceted and comprehensive, yielding insights from a variety of perspectives and lenses, and in turn assists our knowledge of information systems in our lives in a most comprehensive and multi-faceted way. It is challenging because scholars working in this field are constantly exposed to a wide variety of theories, methods, approaches, and research frameworks originating from various other schools of research traditions. To be able to make sense of the research, and contribute over and above the current state of knowledge requires a scholar to learn, understand, critique, apply and extend multiple forms of inquiry, learn multiple theories on various levels of abstraction and complexity, and consider multiple approaches to research conduct.

Before we get to delve deeper into the domain of information systems, and before we start exploring the challenges and intricacies of the research processes within this domain, as a beginning doctoral student, it is useful to revisit the reasons you have embarked on this scholarly quest in the first place. Consider one seemingly inscrutable question: What is your motivation? Is it the quest for knowledge? Is it the will to develop innovative solutions? Is it to use the degree as a stepping-stone to a professional career as a thought leader in a particular field or industry?

None of the possible answers to these questions is right or wrong. Instead, they just outline your intrinsic motivation to embark on a journey that will take you several years to complete.

I think there are three dimensions of motivation. Let us call them ambition, dedication and commitment:

One dimension is to understand your *ambition*. The ambition to complete a doctoral degree to enter (or progress) an academic career is different from the ambition to pursue a research degree as recognition of thought leadership for industry or corporate careers. The difference, amongst others, is for example in the emphasis of publishing academic papers from your research work. For an academic, this is the bread and butter; for an industry professional, papers have more of a “nice to have” feeling.

Dedication refers to the enthusiasm that you need to work on a novel and intrinsically complex challenge for a substantial amount of time – years, in fact. If you feel that your research topic does not excite you substantially, it will be very hard to sustain dedication to working on it over a prolonged period of time. In fact, many of the dropouts I have witnessed share a lack of or a drop in dedication to the topic they are working on.

Finally, *commitment* refers to the willingness to free time and resources to work dedicatedly on the research. Research work is quite unstructured and flexible, and in turn requires commitment to pursue the tasks that need to be completed. One particular obstacle to commitment is the requirement of working in a job. Notwithstanding high levels of ambition and/or dedication, the commitment of time and resources may very simply not be high enough to complete the research to the required or desired level of quality. My argument here is not that it is impossible to complete research work whilst working part- or full-time; instead, I am cautioning you about the implications in terms of resourcing and commitment that come with such work.

When thinking about ambition, dedication and commitment, don’t underestimate for how long these principles will accompany you on the research journey! I have over the years assisted many students on their path through the Ph.D. journey, I have completed this journey myself, and in my role as faculty member in the School of Information Systems at Queensland University of Technology, I have witnessed the paths of several young scholars. It is striking to see how their life proceeds over the course of a doctoral program, and seeing how the changes in their own private context influence their journey in terms of ambitions, dedication, and commitment.

Whatever your motivation is, and however the context of your own study may or may not vary, one element remains stable: the journey of doctoral education is, at its heart, a research journey. It requires you to be a scholar in the fullest sense of the term. A scholar develops the habits of mind and ability to do three things well: creatively generate new knowledge, critically conserve valuable and useful ideas, and responsibly transform those understandings through writing, teaching, and application.

What will be required of you during this journey? Being a research degree, the doctoral program will demand that you learn to demonstrate your ability to conduct research and scholarship that make a unique contribution and meet the standards of credibility and verifiability. These requirements pose several related demands:

1. A skilled scholar is able to ask and frame important questions.
2. A skilled scholar has abilities of knowledge generation and is able to assess, critique, and defend knowledge claims.
3. A skilled scholar is someone who has both a broad and deep understanding of important phenomena in his area of research, and also understands and is able to critically evaluate the current knowledge and its progression.
4. A skilled scholar is someone who is versed in theoretical and methodical approaches to examine and develop the current knowledge.

A doctoral program is demanding. The journey is long, the challenges manifold, and many of us do not start as a fully developed scholar but instead as an apprentice interested in, but not well versed in the domain, theories or methodologies. Moreover, I believe that some of the key challenges that surround a doctoral program are those that are not taught in method textbooks or in theoretical essays. Instead, those lessons only come through experience, personal observation, and advice from fellows or academics that are more experienced.

We will thus set out to discuss these important lessons, but also examine the key principles that inform a junior scholar on his or her pathway to becoming a researcher. The experiences and advice I offer in this book are drawn not just from the field of research that I find myself in – Information Systems – but also from the so-called reference disciplines; tangential and influential fields of research such as behavioural or organisational psychology, management, business, economics, computer or organisational science.

1.2 The Ph.D. Challenge

Let us examine what a doctoral program is about, in order to understand the requirements of being a scholar. At my own university, the Doctor of Philosophy degree is awarded in recognition of a student's erudition in a broad field of learning and for notable accomplishment in that field through an original and substantial contribution to [the body of] knowledge [132].

Other universities may frame this statement differently, but the essence of the requirement, in my experience, is quite constant across the world. Therefore, the main question is what this somewhat innocuous statement actually means, and what would be hard about it?

Most people equate the statement above with “doing research”. This analogy, while seemingly fitting, is a somewhat misleading one. This is because research can mean many things. For example, we often research “best buy options” when purchasing a new car. Most of us would have done some sort of student project in

which we were asked to “research” the latest information about an emerging topic. We may also “research” follow-up information about something we learned from the news. Consultants do research when identifying a problem solution for their clients. Similarly, software developers routinely do research when they design new applications (say, when designing a new iPhone App).

Are all these activities “research activities” in the sense that they contribute to the body of knowledge and by extension warrant the award of a doctoral degree? For example, should those iPhone App developers that brought us Fruit Ninja, Angry Birds, or Cut The Rope be awarded a doctoral degree for their obviously successful and arguably innovative games? The answer is no. Most research is simply that – research – in the colloquial sense of gathering knowledge about a specific subject of interest. Research that is recognised as a scholarly activity, however, is a way of searching for fundamental concepts and new knowledge that confirms to the key principles of *scientific research*:

1. The research work contributes to a body of knowledge.
2. The research work conforms to the scientific method.

In Chap. 2, we will return to these two principles and examine them in more detail. For now, let us simply acknowledge that these two principles of research in a scientific sense distinguish scholarly research from all other types of research activities; that confirming to principles of scholarly, scientific research is in fact a challenging task that, upon successful completion, is recognised through a doctoral degree.

That such principles exist to identify research does not necessarily mean that scientific research is a linear and sequential process. Nothing could be further from the truth. As all of us inevitably learn (doctoral students *as well as* the junior and senior academic faculty that assist the student), research is messy, and not always fully transparent or explicit. Likewise, a doctoral degree will not be awarded simply because we followed a particular research process mechanically. Instead, the process of research is merely the mechanism by we aim to achieve a particular outcome that is worthy of recognition. We learn about research from the scientific papers, books and theses constructed about it. Yet, all of these are merely “reconstructed stories”, written *ex post* (after the research) about what was done and what was found by doing it. We reconstruct the research reality in an utterly simplified way, brushing over the errors, mistakes, doubts, failures, and irrelevant results. In fact, most stories read as if we, as researchers, knew the expected results all along. We studied an important phenomenon, we conjured a theory, and guess what, our empirical evaluation proved that we were right!

The conclusion is that doctoral research in pursuit of a doctoral degree – or any scientific research – is not a mechanical process, and viewing research as being mechanical can be terribly misleading. Research simply doesn’t work like that. It may, in some instances, work that way, but more often than not, it simply doesn’t. In fact, the research “construction process” is messy, characterised by trial and error, failure, risk-taking, and serendipity, but above all, it requires perseverance – iterations of inductive, deductive and even abductive reasoning, of ideas and trials,

of tests and re-tests. A friend of mine spent 5 years setting up, executing and analysing experiments until he got the results that confirmed his (iteratively revised and re-defined) theory. It took Thomas Edison 2,000 times before he succeeded with his first innovation. Yes, 2,000 trials! So, when I say doctoral research is a challenging task, I do mean it. One of the common misbeliefs that I have encountered is that “those who start a doctoral degree will complete it”. This is simply not true. Like many other university courses, the doctoral degree program is also subject to attrition rates, unplanned extensions, and plain failure to complete. Statistics about these matters are hard to obtain, but those that can be found do not actually paint an optimistic picture:

- Fewer than 65% of students who start Ph.D. programs finish them [23].
- Attrition in (U.S.) doctoral programs is as high as 40–50% [49].
- Ten percent of doctoral students claim to have considered suicide and 54% of doctoral students, at various stages, have felt so depressed that they had difficulty functioning [125].

Now I don’t want to paint a gloomy picture of the scholarly journey towards a doctoral degree. Yet, I want to raise awareness for the complexity and challenge of this journey. One of the key reasons for the complexity of the journey is that a doctoral degree, at its heart, is fundamentally different from any other degree that precedes it. Yet, students most commonly do not understand the formal cause of earning a Ph.D., and in turn have a misconception that the program is similar to other degrees (say a Bachelor or Masters course). Let me use the analogy given by Tad Waddington [177] to illustrate this point:

The last step of the process is to contribute to knowledge, which is unlike the previous steps. Elementary school is like learning to ride a tricycle. High school is like learning to ride a bicycle. College is like learning to drive a car. A master’s degree is like learning to drive a race car. Students often think that the next step is more of the same, like learning to fly an airplane. On the contrary, the Ph.D. is like learning to design a new car. Instead of taking in more knowledge, you have to create knowledge. You have to discover (and then share with others) something that nobody has ever known before.

In my view, this analogy highlights that a doctoral degree cannot be earned by repeating, memorising and/or applying the bits and pieces of knowledge someone (like the supervisor) feeds to the student. This will not work because in this process, no novel and original knowledge can be created. Instead, students are required to identify, explore, and solve a problem that has not been solved sufficiently to date – or not at all. In turn, the student, by definition, must become the world-leading expert on that particular problem. Thinking like that also implies that the supervisor(s), again by definition, cannot take the student through that process. They can guide, they can assist, and they can criticise, but they cannot hold the answer to the particular problem – because otherwise it wouldn’t be a Ph.D. problem to begin with.

Now, the problem with interpreting the task laid out for the student, coupled with the problem with interpreting the definition of what scientific research is about, can lead to several archetypes of “interesting” but completely misleading Ph.D. ideas, proposals and strategies. The following list by Carsten Sørensen [157] classifies

these types of misleading views on Ph.D. activities, all of which are unique in their own way but share one common trait – they are not examples of scientific research warranting a doctoral degree:

- **The great idea:** “I have just had this great idea! I do not know if anyone else has ever had the same idea, because I’ve not checked, and I’m rather new in this field. Anyway, my idea is brilliant, so I really would like to share it with you all.”
- **Other people’s idea:** “I have just read this great book that I really like a lot. I’ll just give you a short resume of the interesting points in the book and apply it to this situation over here.”
- **The software hacker:** “I have just built this great computer system/software tool/mobile application. It is not based on previous theories or empirical findings. I am not very theoretical myself, but the system has a lot of fantastic features, and the interface is neat. Plus, people could really use it.”
- **The theory hacker:** “I have come up with this theory/conceptual framework/model/methodology. It is not related to other theories/conceptual frameworks/models, or any empirical data for that matter. Most of the concepts have been defined differently by all the big shots in the field, but I just do not like their categories so I have invented my own. And I think it is surely better (I haven’t checked that either).”

Now, in order to avoid these and other pitfalls, the remainder of this book seeks to introduce the key principles governing all sorts of valid research in Information Systems.

1.3 What This Book Is About

This book is about offering advice and guidelines for conducting good research as part of a doctoral education in information systems. Conducting good research requires students to train their mind and cognition to act in scientific ways – to think like a researcher – in terms of concepts, abstractions, hypotheses, operationalisations, and manifestations.

The book attempts to discuss several of the key essential as well tangential challenges in learning how to do good research. I believe three categories of such challenges exist, which are reflected in the three parts that constitute this book.

The first part, *Basic Principles of Research*, is concerned with understanding the key principles and concepts of scientific research that will constitute the key exercise of the doctoral program. This first section is around understanding the requirements of a doctoral education, while Chap. 2 introduces Information Systems research as a science. Therein, we will revisit scientific research, and the fundamental principles of the scientific method on which it is built.

The second part of this book, *Conducting Research*, is concerned with understanding three key stages involved in any research project. Chapter 3 is devoted to

theorising, and examines what a theory is, why we require them in research, how theories are composed, and how theories can be developed and applied.

Chapter 4 concerns research design, the act of developing a blueprint for executing a study of a particular phenomenon or problem of interest. Therein we discuss the formulation of research questions, the development of a research plan and the choice of an appropriate research methodology.

Chapter 5 then discusses various key research methods that can be elements of the research design. Broadly, we will distinguish quantitative methods from qualitative methods of inquiry, and we will touch upon other forms of research such as design research and mixed methods.

The final part in this book, *Publishing Research*, is then concerned with an essential stage of the research process subsequent to the actual research itself – that of publishing the findings from the study. In this part, I will address challenges that relate not just to doing good research but also to the craft of communicating effectively and efficiently about your research in the form of articles, reports or theses.

Chapter 6 presents advice on developing good research articles from the studies conducted. Therein, various writing strategies are exemplified, essential parts of good papers are reviewed, and advice is offered for handling reviews and dealing with rejections and revisions.

The final section, Chap. 7, then addresses the important ethical considerations in conducting and publishing research. Three aspects of ethics considerations will be discussed – ethics in research conduct, ethics in research publication, and ethics in research collaboration.

1.4 Further Reading

Many good readings are available as introductory books about the Ph.D. journey. David Avison and Jan Pries-Heje [6] compiled and edited a volume on the practices and recommendations from leading information scholars about the pursuit of a doctoral degree in Information Systems. In the early days of the discipline, the esteemed scholars Gordon Davis and Clyde Parker also shared their recommendations in a book to doctoral students [47]. There are also discussion papers available that summarise advice from senior scholars to students and early career academics, e.g., [50, 51, 95, 108, 173].

Chapter 2

Information Systems Research as a Science

2.1 Principles of Scientific Inquiry

At one time, a friend of mine gave me a book to inform me as to the nature of science.¹ My immediate response was that I did not need such a book; at that stage I was already tenured, promoted to Associate Professor with a good track record in publishing, and the promise of many publications to come. Clearly, I thought, I know what science is about.

I couldn't have been more wrong. This is not to say that all my prior efforts were fallible, misguided, and successful only by chance; rather that learning about the basic principles of science opened my eyes to some fundamental elements that govern much scholarly work.

I argued above that scholarly research worthy of a doctoral degree could be described as “scientific research” that conforms to a “scientific method”.

Science is the attempt to derive knowledge from facts through certain methods in a systematic and organised way. It is important to note that derivation is meant here in a logical rather than temporal way. It does not necessarily mean the facts come first. Historically, two categories of science have evolved:

The **Natural sciences** concern the study of naturally occurring phenomena and include such fields of inquiry such as chemical sciences, physical sciences, life sciences, and the biological sciences. The phenomena under scrutiny are real and tangible such as bodies, plants, or matters – although some objects in some fields like subatomic particles, chemical elements, or microscopic organisms are admittedly more difficult to observe.

By contrast, the **social sciences** concern the study of people or collections of people. These sciences are composed of psychology (studying individual behaviours), sociology (studying behaviours of groups of individuals), organisational

¹ The book, by the way, is Alan Chalmer's brilliant essay “What is this thing called science?” (3rd edition, University of Queensland Press, 1999).

science (studying the behaviours of corporate organisms), and economics (studying firms, markets and economies). I often make the general observation that all studies involving humans are part of the social sciences.

The distinction between natural and social is very important to understand because the inquiries or research processes for the two are very different. The natural sciences are referred to as the “exact sciences”²; these inquiries rely on precise and accurate measurements of phenomena or their properties. Examples of such work are readily found in any high school chemistry or physics department. In the natural science of physics, for example, properties such as the speed of light or gravity have been calculated and will remain invariant. This serves as an illustration but in the natural sciences, definitions and measurements are by no means always exact and precise. Heisenberg’s uncertainty principles state a fundamental limit on the accuracy with which certain pairs of physical properties of a particle, such as position and momentum, can be simultaneously known. Still, for the purpose of our argument here, let us presume that natural sciences are exact.

The social sciences are even further away from precision. In the social sciences, phenomena as well as measurements are often vague, imprecise, non-deterministic, and ambiguous. Think about a study that examines whether happy people sleep more or less than unhappy people. Problems abound when we try to understand let alone precisely measure what happiness means, or when we try to isolate the true cause of variation in sleep length. We all know that for any two given days people will not likely sleep for the same amount of time, and there are plenty of reasons for this variation: there could be noise in the bedroom, light shining through the window, more or less wind breezing through the room, the person could have eaten differently in turn affecting their sleep patterns, the person could have had a good dream or a nightmare, and so forth. Many reasons may contribute to different durations of sleep – and but one of them may be related to some form or other of happiness!

One of the many manifestations of this issue is the challenge of *measurement error*. Measurement error is invariably present in the social sciences. This issue recognises that because of the complexity, vagueness, and ambiguity of many of the phenomena under study, many of which we cannot faithfully define or isolate precisely, there will always be imprecision in the way we study our phenomena of interest, and the findings we in turn obtain.

We will return to this issue later in this book. The point I am making here is that students need to be cognisant of the fact that, as an information systems scholar – a discipline that concerns information technology in use by [individuals/organisations/economies/other groups of people], by definition, is a part of the social sciences. As soon as our investigation concerns a human element, imprecision, vagueness and ambiguity creeps into our research.

²I should point out here that this is an overly simplistic account of the relationship between the truly exact sciences (mathematics) and their applications in natural sciences such as physics. But I did promise not to delve into tangential discourses.

The view that I am subscribing to is that the goal of scientific inquiry is to discover laws and propose theories that can explain [natural or social, tangential or latent] phenomena in the worlds that concern us. This is what we call scientific knowledge produced as an outcome of scientific inquiry. Of course, this is but one view on the topic but a rather generally accepted view. Still, keep in mind that others (perhaps even yourself) may choose to phrase the goal of science in different terms.

The challenge as alluded to above is that often this knowledge remains imperfect, vague, and sometimes even incorrect. This is especially so in the social sciences because of the measurement error that creeps into scientific studies.

The key point to remember is that all scientific knowledge, by definition, is merely a set of *suggested explanations* of a particular phenomenon. I often illustrate this with the very basic example of stating that several hundred years ago we *knew* that the earth was flat. Our theories which were mostly inspired through western religion as well as the limited measurements of the time (look at the horizon and see how the ocean “ends” at a certain line), suggested this knowledge to be appropriate. Now we *know* that this theory of the earth wasn’t actually correct. For one thing, you can sail around the earth. Since, we have also ventured into space and seen from that distance that the earth has a spherical shape.

Again, what is the point? The point here is that the body of scientific knowledge, the outcome of all research to date, is the *current accumulation* of theories, evidence, and measurement methods in a certain domain (for example, medicine, management, education, information systems and so forth).

As you can see, this definition makes no statement about the quality of the body of knowledge. Thereby, it recognises that the theories, evidence, and measurement methods may be good or poor. It does however allow us to reason about how a contribution to the body of knowledge can be made. This indeed is the objective of a doctoral study: progress in scientific inquiry can be examined by comparing how well we can improve the current accumulation of theories, evidence and measurement methods in a certain domain. For instance, a contribution could be to improve the explanatory power of a theory of a certain phenomenon. We could also add to the body of knowledge by arriving at better evidence or better or more accurate measurements.

How can one achieve such a contribution? You may notice above that the body of knowledge focuses essentially on two concepts and their relationship, as shown in Fig. 2.1.

We can contribute to the body of knowledge in three ways or combinations thereof:

1. **We can improve our explanation of a particular phenomenon.** This can be done by arriving at a better theory for such a phenomenon, or in other words, by extending a theory. For example, research on theories explaining why people accept or reject information technology over time has improved the theory by showing how additional factors (such as habit, emotion or anxiety) add to our initial understanding, which essentially stated that we accept technology when it

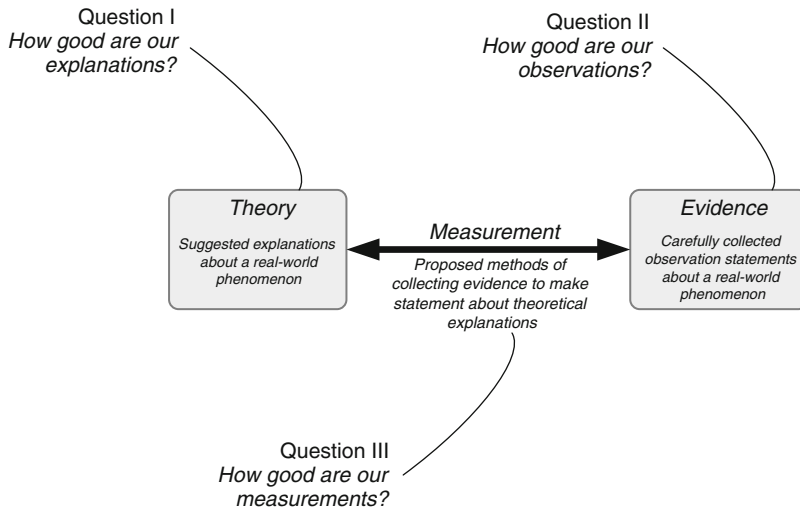


Fig. 2.1 The body of knowledge

is useful and easy to use. In Chap. 3 below, we will return to the question of how we arrive at better theories in more detail.

2. **We can improve our collections of scientific evidence.** For example, we may be able to collect data about a phenomenon where no observations existed to date. A prime example is the famous voyage of Darwin on the Beagle, where he encountered and systematically described many previously unknown species of plants and animals. This evidence in turn allowed him and other scholars to refine their theories about plants and animals. In fact, it laid the groundwork for a whole new theory, the theory of evolution. Arriving at this theory was only possible because firstly systematic statements about observable facts were created through careful exploration and observation. We return to methods of observation later in this book (in Chap. 5).
3. **We can better our methods for collecting observations in relation to theory.** Again, let me give you an example from the history of science. One of the most important contributions Galileo Galilei made was through the improvements he invented for telescopes, which initially were invented by Hans Lippershey. Galileo made a telescope with about $3\times$ magnification and later made improved versions with up to about $30\times$ magnification.

Through a Galilean telescope the observer could see magnified, upright images of the earth or the sky, a greatly improved measurement compared to previous instruments, which largely relied on the naked eye. It was only through these refined instruments that Galileo noted how the positions of some “stars” relative to Jupiter were changing in a way that would have been inexplicable if they had been fixed stars (the current theory at the time). For one thing, he discovered that the “fixed stars” at some points in time were hidden behind Jupiter.

The improved measurements of the satellites of Jupiter created a revolution in astronomy that reverberates to this day: a planet with smaller planets orbiting it did not conform to the principles of Aristotelian Cosmology – the then prevalent astronomical theory, which held that all heavenly bodies should circle the Earth.³ Still, we know now that Galileo was right and that this breakthrough was possible because he initially did not refine the theory or the observations but instead improved our ability to measure relevant phenomena.⁴

The above examples are meant to illustrate the manifold ways in which scientific progress can be achieved. Yet, it does not answer the question of how recognisable progress can be achieved. To that end, we need to look at the process of scientific inquiry and the postulates of the scientific method.

2.2 The Scientific Method

In Chap. 1, we ascertained that in doctoral research we are asked to learn and execute studies that comply with two key principles of scientific research, namely, the research work contributes to a body of knowledge and the research work conforms to the scientific method.

We then illustrated several ways in which one can contribute to the body of knowledge through the scientific output created. Let us now turn to the notion of the scientific method. The scientific method describes a body of techniques and principles for investigating real-world phenomena with the view to adding to the body of knowledge.

Above we argued ambiguity in the connotation of the term “research” and that a doctoral program is about one type of research only, the class of scientific research. For research to be called scientific, the scientific method postulates that the inquiry must be based on gathering empirical and measurable evidence subject to specific principles of reasoning.

Although research procedures vary from one field of inquiry to another, the scientific method provides us with some common features that distinguish scientific inquiry from other methods of obtaining knowledge. Most notably, scientific inquiry is generally intended to be *as objective as possible*, to reduce biased

³ We should note here that Galileo initially endured significant resistance against his findings, because his measurement was not trusted as a scientific instrument. It took decades of *replication* (another scientific principle), until his findings were confirmed to the extent that they were trusted as valid observational evidence.

⁴ Refining measurements is still very much prevalent to date. To note just one example: the improvements in neuroscientific measurement methods such as fMRI scanners provide much more precise measurement of brain activities than any other measurement instrument used in cognitive psychology to date.

interpretations of results and to reduce dependency and partiality of the research team or any interpreter of the findings.

To ensure as much objectivity in research as possible, several principles are provided by the scientific method as a sort of checklist:

1. **Replicability**

Replicability is a term that characterises the extent to which research procedures are repeatable. The principle states that the procedures by which research outputs are created should be conducted and documented in a manner that allows others outside the research team to independently repeat the procedures and obtain similar, if not identical, results. Put in simple terms, the question to ask is “if I repeated your research based on how you conducted it and described it to me, would I get to the same results?” Replicability relies to an extent on the expectation to carefully and in great detail, document, archive, and share findings, data, measurements, and methodologies so they are available for careful scrutiny by other scientists, giving them the opportunity to verify results by attempting to reproduce them.

2. **Independence**

Independence is closely related to reliability. It concerns the extent to which the research conduct is impartial and freed from any subjective judgment or other bias stemming from the researcher or research team itself. Independence is sometimes easier to achieve (when working with factual, objective, precise data) and sometimes harder (in interpretive research where we attempt to explain a phenomenon by interpreting available sentiments or statements about the phenomenon from participants). As we will discuss in Chap. 5 below, different research methods are challenged by and deal with independence in different manners; for example, in some interpretive research teams of external coders are used to verify the impartiality of one’s subjective judgment.

3. **Precision**

The precision principle states that in all scientific research the concepts, constructs, and measurements should be as carefully and precisely defined as possible to allow others to use, apply, and challenge the definitions, concepts, and results in their own work. I noted above that especially in the social sciences many important concepts like happiness, satisfaction, joy, anxiety, and so forth are in essence very hard to define and carry many connotations. A precise definition and measurement is therefore critical to ensuring that the interpretation of the chosen research concept can be comprehended by others, used, or perhaps even challenged.

4. **Falsification**

Falsification is probably the most important principle in scientific research. It originates from the thinking of the philosopher Karl Popper [131] who argued comprehensively that it is logically impossible to conclusively prove theories in scientific research. Instead, scientific theories can only be disproven, i.e., falsified. In other words, falsifiability describes the logical possibility than an assertion, hypothesis, or theory can be contradicted by an observation or other

outcome of a scientific study or experiment. Importantly, that a theory is “falsifiable” does not mean it is actually false; rather that if it is false then some observation or experiment will produce a reproducible and independently created result that is in conflict with it.

The falsification argument has at least two important implications. First, it draws a clear boundary around the possibilities of scientific research: our theories are sets of suggested explanations that are assumed to be true because the evidence collected to date does not state otherwise. Newton sat under the apple tree and apples were falling on his head, which allegedly gave him inspiration about a theory of gravity. As per that theory, apples fall to the ground because of gravitational forces exerted by the earth’s core that pull them towards the ground. Following this anecdote the question is: does the theory conclusively and irreversibly predict that all apples will always fall to the ground? No, it cannot. There is no logical way to prove conclusively that apples will continue to fall to the ground even if all apples to date have done so. Instead, if we were to find an apple that, say, scoots off into the sky we would have found evidence that is contrary to the theoretical prediction and in turn, we would have falsified Newton’s theory. Second, falsification also implies an important criterion for the theories that are created as scientific output: a good scientific theory is one that can be falsified (i.e., a falsifiable theory). This principle implies that theories must be stated in a way that they can, hypothetically, be disproven. If we do not define a theory in a way that allows us or others to disprove the theory using carefully measured observations then we have not complied with the scientific method and in turn have not offered a scientific contribution to the body of knowledge.

For example, the assertion that “all swans are white” is falsifiable, because it is logically possible that a swan can be found that is not white. By contrast, consider the example of the Rain Dance Ceremony theory:

If you perform the Rain Dance Ceremony and all the participants are pure of heart, it will rain the next day.

Proposing this theory is not a scientific undertaking because this theory is not falsifiable: If you perform the ceremony and it rains, the theory is confirmed. If you perform the ceremony and it doesn’t rain, it would suggest that one of the participants was not pure of heart, and again the theory is confirmed. Unfortunately, being pure of heart is not a property that we can precisely, reliably, and independently measure and therefore we cannot possibly create a scenario in which we could disprove our Rain Dance Ceremony theory.

The idea behind the scientific method is not to accredit or discredit research endeavours of any sort. It is, however, used to separate scientific research from other fields of research. A common example is that of theology, which is not a science because its inquiries do not conform to the principles of the scientific method. For one thing, the principle of falsifiability is violated because phenomena such as divine intervention cannot independently be tested or verified. Similarly, humanities, literature, and law are not sciences in that their work relies heavily on

the ability to interpret complex material in a sense-making process, a procedure that, by its very nature, is not independently repeatable as it is subject to the individual performing the inquiry.⁵

2.3 Essential Concepts in Information Systems Research

One of the most frequently occurring problems that I encounter with doctoral students is that, simply put, our conversations are hampered by us using “standard” research concepts and terms in different denotations. In a way, the problem is not so much that the theoretical construct, operationalisations, measurements, and observations we are discussing are not precise enough; rather that our definitions of terms such as construct, concept, variable, etc. differ.

To resolve this problem, let us have a close look at the way that I define some essential concepts for usage in this book. I have tried to relate them in Fig. 2.2.

First, we need to define the term *concept*. A concept describes an abstract or general idea inferred or derived from specific instances that we perceive in the real world. Concepts are thus mental representations that we develop, typically based on experience. Concepts can be of real phenomena (dogs, clouds, pain) as well as of some latent phenomena that we can agree upon (truth, beauty, prejudice, usefulness, value, and so forth).

We use concepts as a language mechanism all the time to describe general properties or characteristics that we ascribe to certain things or phenomena. For example, we use the concept of weight to describe the force of gravity on objects. Weight is a general property that applies to all tangible things in the real world. But we can also use the same concept, weight, to illustrate the psychological state of someone experiencing stress, tension, and anxiety as we do when we refer to the “weight on their shoulders”. We sometimes also develop new concepts to describe a new or newly discovered property. Emotional intelligence, for example, is a concept that purports to describe our self-perceived ability to identify, assess, and control the emotions of oneself, of others, and of groups. This concept has gained some prominence in a debate regarding whether it is a personality trait or form of intelligence not accounted for in currently prevalent theories of intelligence or personality (which, by the way, are also concepts).

As abstract units of meaning, concepts play a key role in the development and testing of scientific theories. They give us a vocabulary to reason about some real-world phenomena (or the linkage between real world phenomena, as shown in Fig. 2.2) and a means to ascribe characteristics or properties to those phenomena and their relationships. Concepts can be linked to one another via propositions – suggested tentative or conjectured relationships between two or more concepts

⁵ Note again that these statements do not qualify these research inquiries but are merely used to distinguish different strands of research.

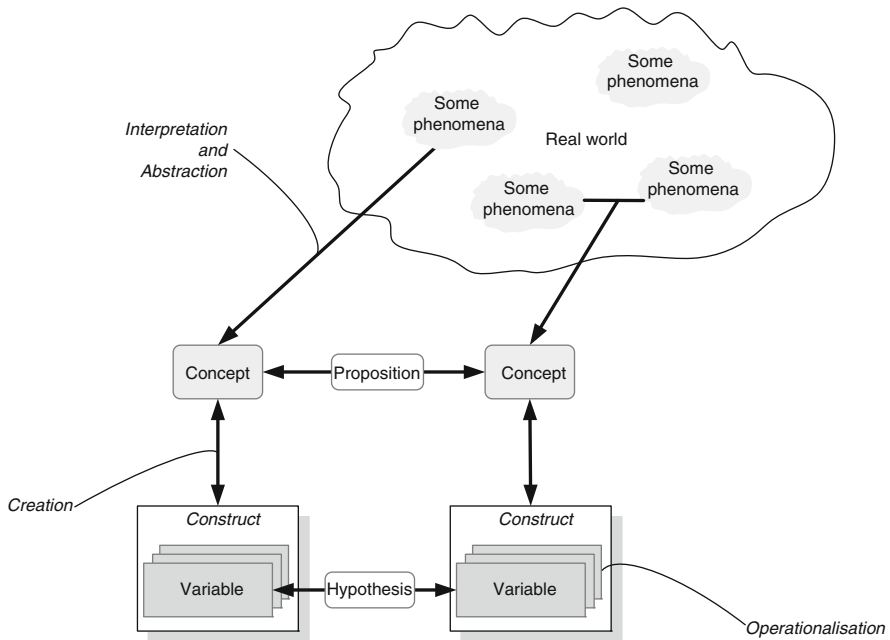


Fig. 2.2 Essential concepts in the research process

that are stated in a declarative manner, more intelligence leads to better decisions, for example.

Note the key words *suggestion*, *tentativeness*, and *conjecture* that I apply to the notion of a proposition. These terms characterise the fact that propositions are proposals for an explanation about how phenomena are related. Whether or not the propositions hold true is an entirely different question and typically, an empirical one that we need to answer carefully through dedicated research methods.

The problem with concepts is that many of the *phenomena* we are interested in such as satisfaction, empathy, intelligence, anxiety, skill, and so forth, are fuzzy and imprecise. This is mostly because most phenomena of interest are not directly observable, and instead rather abstract and difficult to capture, define, or visualise. It is also because in the social sciences we often are concerned with understanding behaviours, processes, and experiences as they relate to “information technology in use”.

For example, take the simple proposition “education increases income”. The concepts of education and income are, by definition, abstract and could have many meanings. Conclusively, there are many, potentially unlimited ways in which such a proposition could be tested – and in turn, many different results could be obtained. Therefore, a proposition (also called a conceptual hypothesis) cannot be tested. They need to be converted into an operational hypothesis.

As per Fig. 2.2 above, we note that hypotheses are suggested linkages between constructs. *Constructs* are operationalised concepts, where we attempt to take the

abstract meaning of a concept such as education, for example, and operationalise it to something in the real world that can be measured. The concept education, for instance, could be operationalised as “highest degree earned”, which in turn could be measured by ascertaining what type of course (high school, under-graduate, post-graduate, etcetera) a person had completed. Income could be operationalised as the construct “Yearly Salary”, which can be measured by comparing annual salary figures before tax in US Dollars. Importantly, we can see how “income” is a fuzzy concept as it remains unclear which specific meaning we apply to this notion (are we talking about annual or monthly income? Before or after taxes?). A construct by contrast is specified to the required level of precision; in the case of our example, it might be annual gross salary.

A construct is thus the creation of an operationalisation of a concept in a way that we can define it in terms of how we can measure the construct. In this process, we attempt to describe fuzzy and imprecise concepts in terms of their constituent components that are defined in precise terms. By doing so, we eliminate *vagueness* (how many centimetres exactly is a “tall” person?) and *ambiguity* (“I purchased a bat” could have many meanings).

This process is a challenging mental procedure. For instance, to operationalise the concept prejudice we would have to ask ourselves “what does prejudice actually mean to us? Are there different kinds of prejudice (race, gender, age, religion)? How could we measure them?”

Depending on the answers chosen we can, generally speaking, create uni- or multi-dimensional constructs. *Uni-dimensional constructs* are composed of only one underlying dimension like weight, height, speed, and so forth. In turn, these uni-dimensional constructs can be measured through one *variable*. A variable is thus the empirical indicator that allows us to approximate the underlying latent construct. You can also call a variable a measurable representation or manifestation of a latent construct in the real world. For example, when we define the concept “weight” as the construct describing the force on an object due to gravity, then we can define a measurement variable that specifies different levels of weight, for instance, using a metric scale (in kilograms). Because weight is a relatively simple, uni-dimensional construct, there is typically not a need to define multiple measurement variables. This is also the case because even if you measure a person’s weight in kilograms and pounds you would obtain the same result since the scales are equivalent. Other good examples for uni-dimensional constructs include age, gender, time, income, and others.

Most constructs, however, are more complex. By complex, I mean that they are composed in a multi-dimensional set of underlying concepts. Intelligence, for example, can hardly be measured by one variable only, because we understand this concept to pertain to abilities for abstract thought, understanding, communication, reasoning, learning, planning, problem solving and others, including emotional intelligence as mentioned above. Such constructs we call *multi-dimensional constructs* because they possess several underlying dimensions, all of which are relevant to our understanding and use of the construct and all of which we consequently need to measure separately through dedicated variables. Taking the example

of intelligence again, we now see the reason for the existence of the IQ (intelligence quotient) score, which is the standardised outcome of a complex test that contains measurement variables to ascertain the levels of intelligence of individuals alongside a number of ability dimensions such as abstract thought, communication, creativity, learning, memory, problem solving, reasoning, visual processing, and others.

Thus, we use variables as measurable representations of constructs that create precise operationalisation of concepts that in turn present a mental abstraction of some property of some phenomenon in the real world. By doing so, we can also speculate about linkages of phenomena not only in conceptual propositions but also in so-called operationalised hypotheses. A *hypothesis* is the empirical formulation of a proposition that is characterised as a testable relationship between two or more variables. Hypotheses need to be formulated such that they are directly empirically testable and such that they allow for precise reasoning about the underlying proposition they represent. For example, “highest degree earned is related to annual gross salary” is what we call a *weak hypothesis*, because it fails to specify *directionality* (does the earning of a degree cause an increase or decrease in annual gross salary?) or *causality* (does annual gross salary cause a specific degree or vice versa?). A *strong hypothesis*, by contrast, would be the statement “the higher the degree earned, the more annual gross salary will be earned”. As this example shows, hypotheses need to clearly specify directionality as well as causality by clearly delineating which variables lead to which effect on which other variable. A statement “Europeans earn high annual gross salaries”, for example, is not a hypothesis because it does not specify a directional/causal relationship between two variables. In turn, we cannot collect meaningful data to evaluate the hypothesis – which in turn violates the principle of falsification.

2.4 Further Reading

As alluded above, I found Alan Chalmers’ introductory book on the philosophy of science an immensely worthwhile read for understanding common principles of good scientific inquiry [33]. Karl Popper’s seminal article on “The logic of scientific discovery” is also a good and more detailed follow-up to the simplistic explanations I gave about falsification [131]. For a more critical view about the principles of scientific method and their limitations, you can consult Paul Feyerabend’s “Against Method” [63].

A good introduction to essential concepts in IS research I found to be Anol Bhattacharjee’s book on social science research [20]. Similar term definitions can also be found in other introductory textbooks such as those by Paul Creswell [40] or Paul Reynolds [141]. Finally, a key paper that defines constructs and operationalisations as elements of a nomonological net (see Chap. 4) is that of Lee Cronbach and Paul Meehl [41].

Part 2

Conducting Research

Chapter 3

The Research Process

3.1 Research Questions

The Australian Ph.D. program lasts approximately 3–4 years, with the average closer to 4 years. Doctoral programs in other countries tend to last a little longer, up to 7 years in some North American programs. In most countries and across most individual cases, virtually all of the first year is dedicated to two challenges of doctoral study:

1. First, you will read the available literature to learn about methods and theories. This part is nicely structured – there is a set of relevant books and papers, and there are typically good classes, workshops, and tutorials to attend. So while this challenge is not to be underestimated, it is a problem with structure and one where considerable guidance is (typically) available. The very structure of this challenge comes with the expectation that you will master the relevant methods and theories. Your supervisors, colleagues, examiners, reviewers, and journal editors will expect you to understand and apply these methods and theories appropriately. Anything less will simply not be acceptable. This challenge (reading papers, learning about methods and theories) is largely *domain-agnostic*. This means that independent of the field of study or the specific phenomena of interest, we can learn about relevant theories in the field, and we can build capacities for utilising a research method or a set of methods well.
2. Second, you will formulate and develop your research questions and propose a plan to tackle them. This is a much more difficult problem, primarily because this challenge is not as structured but instead undefined and highly contextual. Formulating and understanding the research questions is by definition a *domain-specific* challenge. This means that this challenge pertains closely and directly to *what* we want to research, whereas the first challenge – methods and theory, pertains mostly to *how* we want to research.

The difference between points one and two above is very important. One of the great scholars in Information Systems, Ron Weber, has put it much more aptly than I have [186]:

I believe that the choice of research problem – choosing the phenomena we wish to explain or predict – is the most important decision we make as a researcher. We can learn research method. Albeit with greater difficulty, we can also learn theory-building skills. With some tutoring and experience, we can also learn to carve out large numbers of problems that we might research. Unfortunately, teasing out deep, substantive research problems is another matter. It remains a dark art.

The important point we are both attempting to make is that a good doctoral study starts and ends with the right research questions. Finding and specifying them (I am not even talking about answering them!) is by no means easy. Some of the problems I have witnessed related to the formulation of research questions are:

1. **The “elevator speech” problem:** You cannot tell me which question you are asking unless you engage in a 5-min monologue. At that time I will have left the elevator. The problem here is that you don’t fully understand your research question yourself, or you cannot articulate it properly. A good research question is simply a short question. You should not need to elaborate for 5 min on this question or to explain the components of the question. Doing so indicates that you have not yet fully grasped what the essence of the problem is, or you have not yet fully discovered how you can scope your research question in a way that you can easily distinguish from related phenomena or problems.
2. **The “so what” problem:** You have a research question, but it simply doesn’t matter to anyone. Research in an applied discipline such as information systems “apply” knowledge to practical problems (for instance, how new technology shapes the work practices of employees). Consequently, there should be practical merit (utility) in the research. You notice that you may have a so-what problem if you have trouble motivating and justifying your questions.
3. **The “solving-the-world” problem:** Your research question is indeed important. But it simply cannot be answered given the resource constraints – it’s basically only you – and/or the time constraints – you have around 2–3 years, depending on the regulations prescribed by your institution, and typically you don’t know enough yet to find the solution quickly.
4. **The “insolvability” problem:** Your question simply cannot be answered meaningfully. Sometimes this is because of a logical problem in the question, because the information needed to answer the question cannot be logically or legally obtained, or because the answer is so hard to obtain that feasibility of the research within the constraints is not possible. A good example that I often give is that of longitudinal studies (research that examines the evolution of phenomena over time). Such research, by definition, must be carried out over many years. For instance, to study the impact of early childhood abuse on the development of social morale in adolescence you would need to examine relevant individuals in your sample over years if not decades. Unfortunately, most Ph.D. programs should or need to be completed in much less time – which is why we very rarely see longitudinal work in doctoral theses.

5. **The “multitude” problem:** You are simply asking too many questions. In turn, most of your questions are too narrow, too irrelevant, too grand, or otherwise deficient. I always tell my students that a good study sets out to answer one question. Maybe two if you are ambitious. Nothing is gained by setting out to answer six questions. Most likely, most of them will fall under the following categories – all of which in turn describe them as not being a proper research question:

- (a) *Obvious questions:* “Are there challenges in using information technology?” Of course there are. Obvious questions have answers to which everyone would agree.
- (b) *Irrelevant questions:* “What is the influence of weather on the salaries of technology professionals?” There is no reason to believe that there is any influence whatsoever.
- (c) *Absurd questions:* “Is the earth flat after all?” Absurd questions have answers to which everyone would disagree.
- (d) *Definitional questions:* “Is technology conflict characterised by disagreement?” That is simply a matter of creating a concept that says it does. Definition is a mere form of description, not research.
- (e) *Affirmation questions:* “Can a decision-support tool be developed to facilitate decision-making for senior retail executives?” I sure hope so. There is no reason to believe that it cannot be done.

Use the above bullet points as a black list (an exclusion list). Compare your proposed research questions to see whether they fit any of the problems listed above and if they do, go back and revise them. Alternatively, use the questions above as a checklist of what not to do when creating your research questions in the first place. With these caveats and flaws in mind, go back to the start. Why are research questions important?

A research question should be a key statement that identifies the phenomenon to be studied. **The research question(s) is/are the fundamental cornerstone around which your whole doctoral research revolves and evolves.** For you, your supervisory team as well as any outsiders (external examiners, for example), the research questions provide the frame that brackets your whole investigation and its representation in the final thesis. We set out in our study to answer a particular question, and upon doing so we turn back to the question to reflect whether we have sufficiently and comprehensively answered the question through the research we have carried out. To find a good research question, we can utilise a number of guiding questions:

- Do you know in which field of research the research question(s) is/are?
- Do you have a firm understanding of the body of knowledge in that field (how well do you know the relevant domain literature)?
- What are the important established research questions in the field?
- What areas need further exploration?
- Could your study fill an important gap? Will it lead to greater understanding?

- How much research has already been conducted in this topic area?
- Has your proposed study been done before? If so, is there room for improvement?
- Is the timing right for the question to be answered? Is it a sustainable and important topic or is it currently hot but at the risk of becoming obsolete (a fad)?
- Who would care about obtaining an answer to the question? What is the potential impact of the research you are proposing? What is the benefit of answering your research question? Who will it help, and how will it help them?
- Will your proposed study have a significant impact on the field?

Using these criteria, guidelines to formulating a good research question can be developed. In my view, three key components should be considered:

3.1.1 *Motivation*

A good research question does not fall out of thin air. Instead, asking a research question is the logical, necessary, and inevitable conclusion to a set of arguments. These arguments stress that there is (1) an *important problem domain* with (2) an *important gap of knowledge* about (3) a *particularly important phenomenon* that deserves attention from the research community. A motivation is not necessarily extensive, as long as it addresses these three points. A simple, pertinent example is the following chain of arguments:

Organisations invest heavily in new information technology, seeking benefits from these investments.

In this statement we learn about a problem domain: investments into IT and benefit realisation from IT. It should be evident that this is an important problem because of the large amount of money involved. As a tip: motivating research problems by citing data that confirms the quantifiable value (dollars spent, money lost, for example) is extremely valuable.

Many of these benefits never materialise because employees do not use the technologies.

Here we drill down to a particularly important phenomenon of interest – individual rejection of IT. This narrows down the problem domain to a particular phenomenon within it, which will be useful in focussing the research later on.

The literature to date has only studied why individuals accept new technologies but not explicitly as to why individuals reject technologies.

Here we make a statement about the current body of knowledge. We could develop a set of arguments to strengthen the proposition that our theories on technology acceptance [e.g., 175] fail to conclusively predict the opposite, rejection of technology, for example. The logical conclusion to this is the following research question:

Why do people reject new information technology?

By the way, this question is one that Ron Centefelli and Andrew Schwarz [32] set out to answer in their paper “Identifying and Testing the Inhibitors of

Technology Usage Intentions”. I mention their paper here because I think Ron and Andrew have done a very good job in motivating why research on technology rejection is important and why an answer to their question is needed.

3.1.2 *Specification of Problem Statement*

With an appropriate motivation, a good research question is precisely defined. Research questions are typically categorised in one of two types, based on the issues that they delve into:

- “What”, “who,” and “where” questions tend to focus on issues where we seek to explore and describe a topic where little knowledge exists to date.
- “How” and “why” questions are explanatory and seek to provide an answer about the causal mechanisms that are at work regarding a particular phenomenon.

Developing the question as your problem statement is probably one of the most important steps in a doctoral study, and you should thus be patient as well as flexible. You should give yourself time to revise questions as your knowledge and experience grows over time; you should also prevent yourself from being rigid and fixed on one type of problem if you realise that your doctoral study takes you down a path where in fact what you do is not quite what you originally set out to do. A research question also unveils cues about how the research question can be answered. In other words, certain types of questions favour different research methods – we will turn to this issue below.

I have found that guidelines for structuring research questions hierarchically can be beneficial to doctoral students. A common approach to hierarchical structuring attempts [38] to distinguish *managerial* from *research* and *investigative* questions:

A managerial question states the driving question of the study.

A research question captures the general purpose of the study. It is derived from the managerial question, and translates it into a research problem.

An investigative question then identifies question(s) that must be answered in order to address the research question(s) more precisely.

3.1.3 *Justification*

As a final step, a good research question comes together with a convincing argumentation for why the particular problem is significant and deserves our attention. This means that for each question indicating a gap of knowledge within one particular area (our problem statement), we should be able to offer an argument regarding why that particular focus, aspect, or question is important to focus on. Think of the research question formulation as a top-down process in which you narrow down a particular domain or field of investigation to bring it to one specific question within the domain. In the motivation, you argue why the domain should be

examined and which phenomena within warrant attention. In the specification of the research question, you define one particular aspect of some phenomena that you propose to examine from a particular angle. In this final step, you should now offer some arguments as to why this particular focus, demonstrated in the research question(s), is warranted.

After specifying your research questions, it can be useful to reflect upon them. To evaluate the quality of your research questions, the following criteria can be applied in order to assess critically your set of research questions. A good research question is:

- *Feasible*: Adequate subjects of study are available, technical expertise is available, time and money are available, and the scope is manageable.
- *Interesting to the investigator*: You are confident that you can maintain an interest in the topic and maintain your own motivation to study it for several years. If you are not interested in pursuing the answer to the question, no one will be interested in hearing about the results.
- *Novel*: An answer to the question will confirm or refute previous findings or provide new findings.
- *Ethical*: Pursuing and answering the question will not violate ethical principles for the conduct of research, and will not put the safety of the investigators or subjects at risk. We will discuss such challenges in detail in Chap. 8 of this book.
- *Relevant*: both the question and the future answer(s) are important in the sense that they inform scientific knowledge, industry practice, and future research directions.

3.2 Research Design

Once your research question is well specified (for the sake of brevity we will assume one question for the moment), the next challenge is to craft a plan of action to answer the question. This is what is called research design. A well-specified question will suggest to you the most appropriate course of study that could be undertaken in order to answer the question.

Research design is the blueprint for the collection, measurement, and analysis of data. It ought to be relevant and economical. It reflects complex research planning decisions, requiring compromise and trade-offs between the demands of research resources, time, quality, and data access. Before discussing different types of research designs, let us explore some themes that are common in all types of research designs: observation, induction, and deduction.

3.2.1 Observation, Induction, and Deduction

There is no series of events that commonly and consistently unfolds in a scientific process and which would therefore be common to all research designs.

However, a number of themes can be found that appear to be common to all scientific inquiries and are thus manifested in most, if not all, research designs. We call these common themes *observation*, *induction*, and *deduction*.

Observation concerns the discovery of things encountered in common experience. It promotes the desire to understand these observable things by discovering some systematic order in them. Forms of observation involve *exploratory* or *descriptive research*. These research strategies have in common the aim of carefully observing and documenting previously unknown or under-studied phenomena of interest and scoping the magnitude, extent, or boundaries of the phenomena. As part of a scientific study, observation research must be conducted in adherence to the principles of the scientific method; it should be precise, reliable, and independent. Observation research is conducted to gain an initial understanding of a phenomenon within its context, and to generate ideas about the phenomenon or relationships to other phenomena, which may therefore lead to the formulation of speculative propositions or theories.

Induction is reasoning that involves moving from a set of specific facts to a general conclusion or from specific observations to broader generalisations and theories. By developing patterns and commonalities, tentative hypotheses and propositions can be formulated in order to develop general conclusions or theories. Induction can be thought of as trying to infer theoretical concepts and patterns from observed data or otherwise known facts. A simple example would be:

- Every life form we know of depends on liquid water to exist.
- Therefore: All life depends on liquid water to exist.

Inductive arguments can be weak or strong. The induction “I always hang pictures on nails. Therefore: All pictures hang from nails” is an example of a weak induction because the generalisation is too broad to follow directly from the observations. They can be difficult to justify, however inductions are an accepted pathway for hypothesis construction, on account of the fact that conclusions are offered based on educated predictions. Case study research is a good example of induction because in such research we often collect carefully measured observations that form the basis for educated speculations that could form the basis of more general theory. The inductive reasoning typically goes as follows:

I studied phenomenon *X* in *Y* amount of cases and I have always found the particular relationship/phenomena *Z* to be at work. Hence, the evidence collected in my observation lead me to formulate the tentative proposition that *Z* is related to *X* in this or that way.

Deduction is reasoning that involves deriving arguments as logical consequences of a set of more general premises. Deduction is commonly used to predict the results of the hypotheses or propositions. That is, in order to predict what observations one might make if an inquiry is conducted, the hypothesis is treated as a premise, and from it some not currently obvious conclusions are logically derived, tested, and if necessary, revised. In other words, through deductive reasoning we attempt to show that a conclusion necessarily follows from a set of premises or hypotheses. Deduction can be viewed as an attempt to test concepts and patterns known from theory using new empirical data. A simple example would be:

- All men are mortal
- Socrates is a man
- Therefore, Socrates is mortal

Similarly to induction, deduction has potential drawbacks. The most obvious challenges in deduction are related to deductive soundness and validity. Consider this deduction:

- Everyone who eats steak is a quarterback.
- John eats steak.
- Therefore, John is a quarterback.

We can see here that the deductive reasoning as it is applied is logically sound (the conclusion about John being a quarterback), however we don't actually know whether the final statement is valid. The reason for this is that the premise "Everyone who eats steak is a quarterback" may or may not actually be true. In other words, we can deductively reason in good faith but still end up with incorrect conclusions.

Each of the principles *observation*, *induction* or *deduction* that we use to reason about phenomena, facts, or assumptions in our effort to generate new knowledge, will be insufficient in itself. Observations are useful to developing an understanding of a domain or a phenomenon but are not sufficient for explaining or reasoning regarding phenomena. Induction is useful for theorising from observations and other singular facts and evidence, but insufficient for demonstrating the validity of any emergent theory. Deduction in itself can be used to test theories using some or many individual cases but it must rely upon the formulation of a robust set of premises to begin with.

Sound research design, therefore, should strive to employ *combinations* of observation, induction, and deduction. That way we can achieve a meaningful mix of **exploration** – where we build an understanding of the phenomena that interests us, **rationalisation** – where we begin to make sense of the puzzle or problem that interests us, and **validation** – where we appropriately subject our emergent or developed theory to rigorous examination.

One aspect worth highlighting regarding exploration, rationalisation, and validation is that they are not necessarily related in a linear or temporal manner. Instead, good research typically moves back and forth between them, as shown in Fig. 3.1.

Exploration of a phenomenon of interest can provide the basis by which we can begin to rationalise the phenomenon. For instance, based on observations we can rationalise a solution to a problem, or explain a specific behaviour. The rationalisation process, in turn, might create the demand for further exploration (as shown by the arrow moving from rationalisation back to exploration). We may find that in order to explain a particular behaviour, we need to collect further observations about other behaviours that we didn't deem relevant in our initial exploration. We can also interpret the interplay between rationalisation and exploration as providing the set of initial evidence against which we can test the

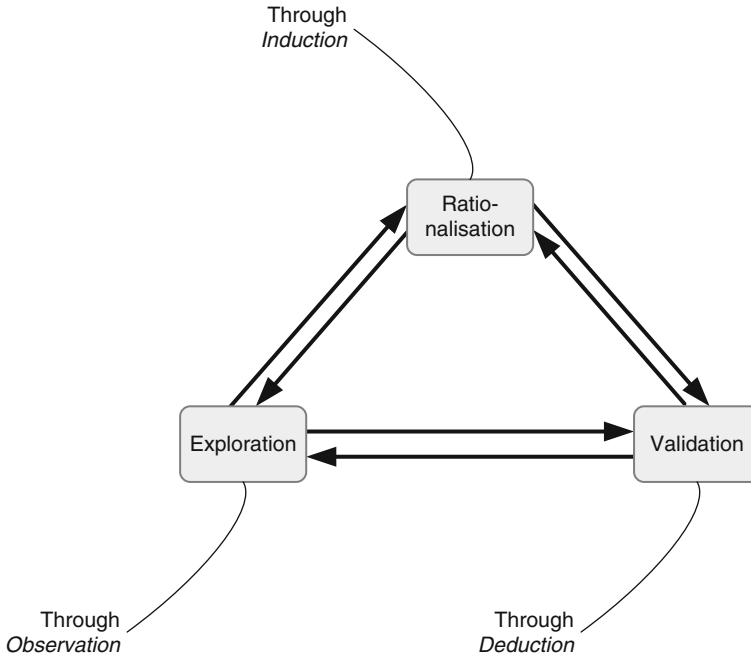


Fig. 3.1 Exploration, rationalisation and validation in research design

outcomes of our rationalisation process or evaluate the set of tentative propositions between constructs that capture the phenomena. The rationalisation should be valid in light of any observations that were collected.

Once a rationalisation has been achieved, in which tentative general propositions are created through inductive reasoning from observations, we can proceed to validation, where we deductively derive testable hypotheses from our general theory. These can be subjected to further empirical tests using specific new or existing cases. In light of the results or evidence collected, we may need to revise our rationalisation (moving from validation back to rationalisation). The validation also employs as a basis the findings from the exploration. This can be done by defining the sample frame, target population, or other contextual parameters that emerged as important through initial observations, (thus, moving from exploration to validation). Likewise, the results might prompt further exploration of the phenomenon (moving back from validation to exploration). For instance, observations can be collected that can be used to refine specific results, such as those that are on the surface contradictory to the developed theory.

3.2.2 *Selecting a Research Design*

The discussion above provides common ground to all types of research designs; in the sense that good research design typically includes work that combines

Table 3.1 Research design decisions (Adapted from [38])

Spectrum	One end of continuum		Other end of continuum
Method	Qualitative	vs.	Quantitative
Aim	Exploratory	vs.	Explanatory
Boundary	Case	vs.	Statistical
Setting	Field	vs.	Laboratory
Timing	Cross-sectional	vs.	Longitudinal
Outcome	Descriptive	vs.	Causal
Ambition	Understanding	vs.	Designing

observations, induction and deduction. These three themes can be utilised to reflect upon your research design (does it involve an appropriate way to rationalise? To validate? To explore?).

Through the perusal of the framework in Fig. 3.1, it is evident that research design is the outcome of a process with many choices. These choices are not trivial and their implications regarding what the potential and likely research outcomes will be are significant. Table 3.1 summarises some of the important design decision parameters. Note that by no means are these decision binary (either-or) in nature; rather, they exist as two key points along a continuum of choices.

The rows in Table 3.1 describe different spectra by which we can examine research design choices. For instance, the first row may relate to the design spectrum “method” and considers qualitative versus quantitative modes of inquiry. The aim of a piece of research, similarly, may fall between exploration and explanation or a combination thereof. The boundary of the research might be limited to a particular case (such as an interesting case of an organization or an individual) or by statistical properties (such as the required sample size for a survey or experiment). The focus of the timing might be one case over time (longitudinal) versus at several cases at one point in time (cross-sectional). Again, there might be other variations such as several cases over time or one case at one point in time. Our research outcomes might focus on descriptions of a previously undiscovered phenomenon or rather on the discovery of certain causal mechanisms that explain why a phenomenon manifests the way it does. The ambition, finally, might be to understand a problem or situation or to design a solution or a novel artefact.

I wrote above about the criticality of the research question, and I will repeat it here: The key benchmark against which your research design must be aligned is the problem statement as specified in the research question(s).

One key downfall of doctoral students is the “*I do research method X syndrome*”. When asking students about their research, I often receive statements such as “I’m doing case study research”, or “I do design science”, or “I’m doing a survey”. In all these cases, the students focus on a singular aspect of their research design – the choice of a research method – independent from the projected research outcome or the research question they seek to answer.

Let’s put it straight: the research question determines, at least to a large extent, the choices required in selecting a research design. It dictates whether a more

qualitative, interpretive inquiry is warranted or whether a more quantitative, statistical examination is the more appropriate strategy.

One of the key design choices in research design relates to the use of the **research methodology**. We will return to this challenge in Sect. 3.3 below. At the same time, research design needs to account for several other considerations. The most important ones I will summarise as follows:

- **Data:** What type of data is required? Where can I collect observations or other forms of evidence? How will I sample the relevant data?
- **Risks:** What are the potential dangers associated with execution of the research design? For example, what is the likelihood of a case organisation not being available for study anymore? What are strategies available to minimise or mitigate these risks?
- **Theory:** Which and how much literature concerning the phenomena of interest is available? Where are gaps in the knowledge base? What findings have been produced to date that might have an impact on my work and influence choices in my research design?
- **Feasibility:** can the research design be executed within the constraints associated with a doctoral study such as time limitations, resource limitations, funding, experience, geographic boundaries, and others?
- **Instrumentation:** how will my constructs of interest be measured? Will my construct operationalisation be appropriate given the choice of research methodology and set of data available?

In selecting a research design, progress may be evaluated by examining whether (a) you have appropriate answers to the questions above, and (b) you maintain alignment to the type of research problem that is specified in the research question. The alignment does not necessarily need to be unidirectional (from the question to the design); in fact, in most cases it is observed that research questions, over time, get tweaked and altered to reflect an updated research design, and still, research questions should retain prominence over and above the research design. In our research efforts, we set out to answer an important question; it is not appropriate to find an answer and then devise a question that fits the answer.

In making research design decisions, students, together with their supervisors need to select research designs that they feel comfortable with and in which they have experience. There is logical validity to such an approach. This is not to say that new research designs should be abandoned and not pursued. Still, many doctoral problems I witnessed originated largely from the fact that neither the student nor the supervisory team had any experience with a particular research design (say, based on a survey strategy). The execution of such studies is then unnecessarily hard because resources to give meaningful feedback based on experience are necessarily limited if available at all.

3.3 Research Methodology

Hand in hand with the development of a research design is the selection of the appropriate research methodology. Most scholars would even argue that the selection of a research methodology is the most important design choice in the research process.

Research Methodology is a term that describes the **strategy of inquiry** used to answer a specific research question. Creswell [40] states that strategies of inquiry are “types of qualitative, quantitative and mixed methods designs that provide specific direction for procedures in a research design”. I agree with him, but will also add design science methods as an additional orthogonal strategy of inquiry to his list:

- **Quantitative Strategies** are procedures that feature research methods such as experiments or surveys and which are characterised by an emphasis on quantitative data (think of these procedures as having a focus on “numbers”).
- **Qualitative Strategies** are procedures that feature research methods such as case study, ethnography or phenomenology and which are characterised by an emphasis on qualitative data (think of these procedures as having a focus on “words”).
- **Mixed Methods** are procedures that feature combinations of both qualitative and quantitative strategies in either sequential or concurrent fashion (think of these procedures as having a focus on “numbers and words”).
- **Design Science Methods** are procedures that feature methods to build and evaluate novel and innovative artefacts (such as new models, methods or systems) as the outcome of a research process and which are characterised by an emphasis on the construction of the artefact and the demonstration of its utility to an organisational problem (think of these procedures as having a focus on “artefacts”).

When we leave out a mixed method strategy for a moment (because it combines characteristics of both qualitative and quantitative strategies of inquiry), we can differentiate the other three strategies alongside a number of dimensions, as summarised in Table 3.2 and discussed below.

Controllability refers to the extent to which events during a study are under the control of the researcher. In a qualitative inquiry where the researcher often enters an organisation to observe the behaviours, processes or events, controllability over what happens is comparatively low when compared to quantitative inquiries such as surveys or experiments, where control is exerted, for instance, through the operationalisation of a measurement instrument that precisely defines what will be measured and how. In design science research, control over progress and effects is typically at the hands of the person designing, i.e., the researcher.

Deductibility refers to the extent to which the strategy allows for deductive reasoning. Through the emphasis on quantitative data, quantitative strategies allow for strong deductive reasoning through statistical or other quantifiable conclusions,

Table 3.2 Differences in research strategies (Based on [66])

Requirement	Qualitative	Quantitative	Design science
Controllability	Low	Medium to high	High
Deductibility	Low	Medium to high	Very low
Repeatability	Low	Medium to high	High
Generalisability	Low	Medium to high	Low to very low
Explorability	High	Medium to low	Medium to low
Complexity	High	Medium to low	Medium to high

whereas deductibility is typically limited when doing qualitative inquiries such as single case research or ethnography. Deductibility is often extremely low in design science research because of the challenge involved in embedding hypotheses testing into the design of an artefact.

Repeatability refers to the extent to which the findings are reliable in the sense that the research procedures can be repeated with similar if not identical results. This requirement is easier to meet in quantitative inquiries where instruments of measurement tend to be more precisely defined. Repeatability can be said to be high for design science research as the artefact is typically designed to be stable and thus should lead to similar usage behaviours.

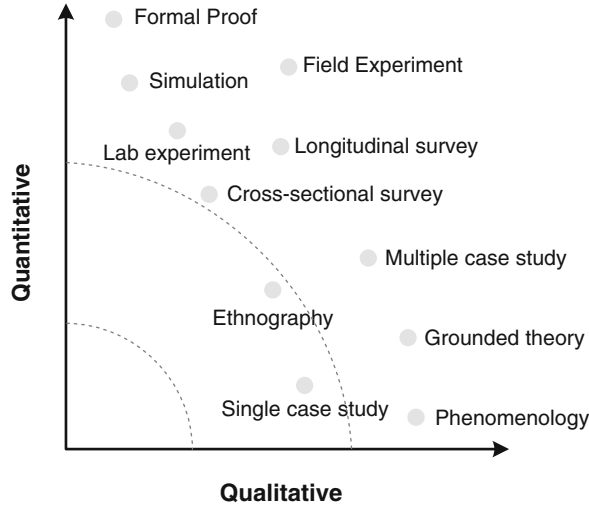
Generalisability refers to the extent to which the findings and observations can be generalised beyond the data being observed or examined. Quantitative inquiries, especially surveys, provide a greater ability to generalise beyond the sample; qualitative inquiries are more deeply immersed into the context of the inquiry.

Explorability refers to the extent to which a research strategy encourages or enables the discovery of previously unknown or unconsidered observations or findings. This emphasis is typically built into qualitative inquiries through an emphasis on broad and open measurements; quantitative inquiries with their precise and formalised measurements are more limited in terms of exploring beyond the focus of the study. Explorability can be an attribute of some artefact designs but more often than not exploration is not a key requirement when creating novel artefact designs.

Complexity refers to the extent to which a research design leads to comprehensive, exhaustive, and multi-faceted knowledge contributions. Quantitative inquiries are characterised by a reduction of the inquiry to selected, precisely defined measurements of phenomena, whereas qualitative inquiry, through broader and more open data collection procedures, allow for more manifold findings and knowledge contributions. Complexity in design science research is greatly dependent on the type of artefact but can often be assumed to be a key characteristic of the design, based on the assumption that all simple artefacts have already been discovered and designed.

All of these criteria are usually combined in a favourable way (medium to high, for example) in mixed method designs. Depending on the choice of research methods to be combined in a mixed method strategy, the overall methodology may lean towards the characteristics of either purely qualitative or quantitative inquiries. A valuable mixed method design, therefore, is one that combines “strong”

Fig. 3.2 Qualitative and quantitative methodologies



qualitative characteristics (such as explorability or complexity) with “strong” quantitative characteristics (such as generalisability or deductability), so that the requirements that may not be fully met by one method can be compensated by the other. I have tried to group several research methods within each of the two major strategies of inquiry in a simplistic manner for illustration purposes in Fig. 3.2. Note also the rough thresholds in terms of the extent to which different methods can meet desired characteristics of quantitative or qualitative strategies.

In concluding this chapter, the selection of an appropriate strategy of inquiry to determine the research methodology is critical to the success of any research project and must be driven by the research question as well as the current state of knowledge in the area being studied. When looking at research programs over time, we often find that an emerging problem domain or topic area is typically initially explored through qualitative strategies. This is logical in that the characteristics of explorability and complexity allow researchers to develop a first comprehensive understanding of that area and to identify and define important concepts. As research in a particular domain or area matures over time, we often see the share of studies with quantitative methodologies increasing. This research builds on more precisely defined concepts and explores in a more controllable manner some specific behaviors, processes, events or other phenomena with the view to deduce generalisable findings.

Finally, there is a lot of support for the view that a combination of research methods can often be most effective in achieving a particular research objective.¹ For example, when a subject area is not well understood, qualitative methods may be used to collect observations in an effort to build theory and testable hypotheses. Such theory may then be tested by means of deductive reasoning using quantitative

¹ The authors in [40, 66, 67, 88, 102, 122], for instance, make such an argument.

methods such as surveys and experiments. In some cases, these combinations may comprise the form of doctoral research study, but there are also good theses completed at either end of the qualitative-quantitative spectrum.

Within the realm of qualitative, quantitative, mixed or design science strategies, there are many approaches or *research methods* available. A research method can be viewed as one possible instantiation of a research methodology. Below in Chap. 5 we will introduce and discuss the most prominent research methods as instantiations of the four basic types of research methodologies. Before we address this important topic however, I want to make some statements about literature and its role in the research process.

3.4 The Role of Literature in the Research Process

You should have noted by now that one of the key tasks throughout your Ph.D. journey will be to acquire knowledge in your effort to contribute to knowledge. So far, we have discussed that you need to acquire at least three types of knowledge before you can even start your research:

1. Knowledge about the domain and topic of interest,
2. Knowledge about relevant theories that help you frame questions and phenomena, and
3. Knowledge about relevant research methods that you can apply to develop new knowledge, build innovative artefacts or articulate new questions.

Knowledge in the scholarly field is available predominantly in the form of articles and books. We build a cumulative tradition of knowledge by adding with our own published research to this repository of knowledge. We publish our research in the form of book chapters, books, and journal articles.

This tradition demands that you spend a significant amount of time and effort to acquire and critically appraise this cumulative knowledge base. You will not be able to add significantly to the body of knowledge without having a firm understanding of where that body of knowledge currently stands.

Why do I mention this role of literature at this point in time, in the midst of discussing the research process in terms of its design? Simply because it is already at this stage where a firm understanding of the body of knowledge is required for many of the decisions regarding the *planning* stages of research:

- The literature informs the extent, type, and nature of current organisational problems. This information is viable for the formulation of a research problem.
- The literature informs where gaps of knowledge are surrounding a particular problem. This aids in identifying an important academic research question (i.e., a research question that deserves scholarly attention because its answer will contribute to the body of knowledge).

- The literature informs the extent to which our current theories are able to explain the particularities of the phenomenon or problem, and likewise where they fail to do so adequately.
- The literature informs the strategies and methodologies that have been used in the past to research the phenomena/problem (or related phenomena or problems).
- The literature informs relevant theories that can be used to frame an investigation.
- The literature informs the current body of knowledge in terms of the research methodology available (viz., the processes and guidelines for conducting a particular type of research on the particular problem).

These arguments indicate how critical a firm understanding of the literature is to the design and conduct of a doctoral study. You can also see how the literature may be important in terms of:

- Findings and insights into a specific problem **domain**;
- **Theories** that are available and/or in use to examine the problem or phenomena of interest; and
- The current state of the **methodologies** appropriate and applicable to the study.

This is indicative that the list of readings to be studied is large if not overwhelming. Many authors suggest, as a simple first heuristic, to start identifying 50 papers, and the duplicate this amount after an initial scan and so forth and so on. This tells you something about the complexity and coverage of the Information Systems domain as a research field, but also about the many different ways in which research has been, and can be, conducted – in this area as well as other scholarly disciplines.

It will become clear to you as you go on with your own studies, but let me tell you upfront nonetheless: Do not underestimate how important knowledge acquisition through reading will be for your own studies. Believe me, any “reading list” you may come across will be nowhere near as long as the body of knowledge (i.e., the amount of literature) you will have consumed towards the end of your journey. Therefore, you are strongly encouraged to read straight away from the start of your journey. I am making this point here because I have found that many doctoral students appear to overlook many parts of the literature. In my view, this is because they underestimate how important literature is. The above is my attempt to instil in you the importance of reading as part of a doctoral study.

In consuming the literature, it helps to follow a process of read-think-interpret. Not all literature, or all sections of a particular piece of reading material, will be relevant. Therefore, you need to *read*, *think* about the relevance, and *interpret* the literature based on your assessment of the relevance. Having said that, most students tend to significantly underestimate the relevance of other papers. So, read a little bit too much rather than not enough.

In following a process of read-think-interpret, the following questions can guide you:

- What is the reading’s core contribution with respect to the contemporary practice in your field of inquiry?
- How does it relate to other articles and practices?
- Does it espouse a theoretical or methodological perspective that is useful to study particular phenomena of interest to you? And why would this be – or not be – the case?
- How may it influence your own thinking of the field?
- How do you think has the reading influenced the body of knowledge in the field at the time it was published?

Following these guiding questions should benefit you in your quest to master the current body of knowledge as it pertains to your research. Beyond my suggestions, there is ample guidance available for doing these sorts of literature reviews. I suggest some “readings about reading” below. In the Information Systems discipline, fortunately, there are also excellent web resources dedicated to the literature about theories and methodologies. Let us review these in the following:

In terms of **theories**, scholars at York University have created a wiki with summarised information on theories widely used in information systems research. The wiki is available at http://www.fsc.yorku.ca/york/istheory/wiki/index.php/Main_Page. Here, you can explore theories by name, type of application, details about the theory, some examples of papers using the theory, and links to related information.

In terms of **methodologies**, similarly, IS scholars have crafted online resources as a starting point to read, learn, and apply different research methodologies. At present, the most prominent ones are:

- The *AIS World Section on Qualitative Research in Information Systems*, which is available at <http://www.qual.auckland.ac.nz/>. This online resource provides qualitative researchers in Information Systems – and those wanting to know how to do qualitative research – with useful information on the conduct, evaluation and publication of qualitative research.
- The *AIS World Section on Quantitative, Positivist Research Methods in Information Systems*, which is available at <http://dstraub.cis.gsu.edu:88/quant/default.asp>. This online resource attempts to address the needs of quantitative, positivist researchers in Information Systems and caters for both seasoned veterans and those just beginning to learn to use these methods.
- The *AIS World Section on Design Research in Information Systems*, which is available at <http://home.aisnet.org/displaycommon.cfm?an=1&subarticlenbr=279>. Similarly to the other two resources, the intent of the page is to provide design science researchers in Information Systems, as well as others interested in design research, with useful information on understanding, conducting, evaluating, and publishing design science research.

Literature about **problem domains** varies depending on the type, nature, and maturity of the domain. Still, many problem domains currently have their own outlets of coverage through practitioner communities, blogs, magazines and so forth (think

of the CIO magazine, Business Analyst forums, popular blogs and so forth). It is very useful to explore the range of literature that surrounds different communities of technology practices and read some of them. Often this literature is very valuable, for instance in justifying the relevance and significance of a study. Good examples for such literature include market research reports by institutions such as Gartner, Forrester, WinterResearch and other technology-related research agencies.

As a final hint, be ready and willing to expand your scope of the literature. Many good papers on method, domain, theory or other aspects of the research process have been published in outlets that we do not intuitively associate with the information systems field, but which instead belong to some of its reference disciplines (such as management, accounting, computer science, organizational science, psychology and so forth). These fields have top outlets in which top papers are published, which very often make for a good read. Three good reasons for expanding your literature search outside of your immediate domain are:

- (a) Discovering theories that are popular in other research fields, aside from those that are domain-specific.
- (b) Seeing the way methods are applied in other research disciplines, especially in terms of the guidelines and evaluation criteria available.
- (c) Developing an exposure to the way other scholars in other fields frame, examine, and solve real world problems, in a very general sense. This can be an incredible source of inspiration and can make for a tipping point in your own journey.

3.5 Further Reading

I argued above that finding a research question is an unstructured, domain-specific problem. As such, it is hard to find guidance on this problem in the literature. A good example that can inspire your thinking is Ron Weber's editorial on "The Problem of the Problem" [186]. Other editors and senior scholars in other disciplines and journals have published similar pieces, for instance in the *Academy of Management journal* or in the *European Journal of Information Systems*.

As to creating a pathway for answering a research question, fortunately, there are many books dedicated to research designs. One good example is that of John Creswell [40] who discusses research design choices from the viewpoint of research methods favoured or demanded. Paul Leedy and Jeanne Ormrod's book [104] is a further excellent resource, which also includes criteria for judging the quality of research designs.

Alan Lee [102] has written an excellent paper on research designs that combine what he calls subjective, interpretive, and objective understanding. While his terms are different to mine (exploration, rationalisation and validation) and while his focus is much on the interplay between different research paradigms (interpretivism versus positivism) in use at these levels, much of the argumentation as well as the conclusions from it are similar.

Concerning the selection of a research methodology, by now an ample number of readings are available to summarise different research strategies (such as the 19 strategies discussed by Harry Wolcott [194] or indeed John Creswell's book [40] mentioned above).

Literature regarding literature reviews and search strategies, likewise, are widely available. I have come to appreciate the following resources:

- Jane Webster and Rick Watson's guide for writing a literature review [188].
- The relevant section in Paul Leedy and Jeanne Ormrod's book on research planning and design [104].
- The essays by Harris Cooper [39] and Jack and Sallie Strange [161] on conducting research reviews.
- For inspiration and as prime examples for how literature reviews can be conducted and lead to new knowledge: All articles in the Theory and Review section of the Information Systems discipline's top academic journal, the MIS Quarterly [184].

Chapter 4

Theorising

4.1 What Is Theory?

Chapter 3 above took us through the steps of designing a research inquiry. It is the *planning* stage of the research. This chapter and Chap. 5 below now address two key challenges in *executing* the research, by discussing **theory** and **method**. Let's start by looking at theory.

Early on in this book, we stated that scientific knowledge is our collection of theories built, derived, and tested using the scientific method. We also stated that theories represent our current suggested explanations for what our world looks like and how things in it behave and function.

So why are theories important? For one thing, theories are at the core of any research process. As a scholar, it is our job to develop theories, to evaluate them, to reject them where required or to revise, extend, or modify them. Because theories represent the current accumulation of our body of knowledge, we can only contribute if we somehow work *with* the existing theories in order to compose new ones.

But aside from being the core *outcome* of scientific research, theories are important to the *planning* process in research: to the scholar, theory can provide guidance in terms of where to direct the attention of this or future study. Existing theory can serve as a framework where current, past, as well as future empirical work can be incorporated. From a larger perspective, theories can be the material that integrates sets of individual studies into a larger research program [27].

Finally, theories also have plenty to offer to the *execution* process in research: Theories provide a framework for synthesising, analysing, and integrating empirical findings and observations. Theories can help us analyse empirical data and observations by identifying patterns and themes in the data. They provide suggestions that can be used to explain the findings or observations and to make sense of the data collected. Theory is also the key basis for the derivation of hypotheses that can be examined in empirical work. As such, theory is a key component that adds rigor to the research process [159]. In addition, theory can be of help when we have observations and evidence that are seemingly contradictory, anomalous, or inconsistent.

4.1.1 Definition of Theory

What is theory? Simply put, theories are proposed explanations of empirical natural or social phenomena, constructed in a way consistent with the scientific method.¹ For example, theory in medicine involves trying to understand the causes and nature of health and sickness. This is what we would call an explanatory theory. Anticipating our discussion of types of theory in the section below, we can also find design theories in medicine, which are an attempt to make people healthy. Note here how explanation and design theories are related, but can also be independent: it is possible to research health and sickness without curing specific patients, and it is possible to cure a patient without knowing how the cure worked.

Still, most would argue that theories contain a component of explanation, that is, a sense of logic about the connections among phenomena and a story about why acts, events, structure, and thoughts occur. In that understanding, theory emphasises the nature of causal relationships, identifying what comes first as well as the timing of such events. Thus, a more formal definition would be that theory is a system of constructs and relationships between those constructs that collectively present a logical, systematic, and coherent explanation of a phenomenon of interest [8].

In trying to understand what theory is, it is vastly helpful to consider what theory is not:

- Theory is not *data*: sets of evidence, observations, or arguments do not make up theory. Think of raw materials and the design of a building: We need bricks, mortar and perhaps wooden planks to build a house but these materials themselves are not the house, just as data, evidence, or observations are not theory.
- Theory is not *idiographic*: an explanation of a single situation or phenomenon, in whatever detail, is not theory, as it does not allow for generalising to other situations, events, or phenomena. By extension, therefore, theory is characterised as being *nomothetic* – pertaining to classes of events, behaviours, situations, or phenomena.
- Theory is not *description* or *prediction* only: a mere description or classification of phenomena or other observations does not constitute theory because such descriptions (taxonomies, typologies, classifications and so forth) still operate at the empirical, observational level. Theory delves into underlying processes in order to understand the systematic reasons for a particular occurrence or non-occurrence, it comes with suggested explanatory mechanisms that tell not only *that* and *how* phenomena occur but also as to *why* they occur in the fashion that we observed.
- Theory is not *design*. The construction of an artefact, however novel and useful it may be, in itself is not theory. Just as we may be able to predict future events without understanding why the events will occur, we may be able to construct

¹ Remember, the scientific method is a method of inquiry based on gathering observable, empirical and measurable evidence subject to specific principles of reasoning.

artefacts that operate well without understanding why that would be the case. If, however, design embodies theory (often called a design theory) that informs us with a sense of understanding why this artefact is constructed in a particular way and why it therefore provides the utility or novelty that it provides; then design can be a manifestation of theoretical work.

- Theory is not *self-perpetuating*. Theory is not and should not be an activity that is an end in itself, meaning that it would be important to you not because it will help you to achieve something, but only on the premise that you think that it is important. Instead, theory has implications, which we may have not seen, which may run counter to our common sense, and/or which inform our current and future understanding of the phenomena of interest.
- Theory is not *universal*. I acknowledge that some scholars in the natural sciences (such as physics) believe in or are searching for one universal theory of everything (the current candidate, I believe, is super symmetric string theory). Until that golden nugget of research is found, theories, while striving for comprehensiveness, have their share of limitations in the form of leading assumptions and boundary conditions, which specify the limits to which the theory is held; for example, theories that pertain to large and complex organisations only, rather than all types of organisations.

There are, of course, many other errors in relation to theory that are being made by novice and experienced research alike. Still, equipped with this basic understanding of what theory is not, we can turn to examining the elements that make up theory, to understand the structural components common to all sorts and types of theory.

4.1.2 Building Blocks of Theory

Independent of what theory ultimately is, to which phenomenon it pertains and which goals it strives to reach, several structural components are said to be common across all theory. David Whetten [191] calls these the building blocks of theory and identifies four such blocks, which we will discuss:

1. *What* (constructs),
2. *How* (relationships),
3. *Why* (justifications), and
4. *Who, Where, When* (boundary conditions).

The most fundamental components of theory are the *constructs* of which it is composed. Recall that in Chap. 2 we defined constructs as operationalised concepts, which meant that we attempt to take the abstract meaning of a concept (such as education) and operationalise it to something in the real world that can be measured. Typically, constructs relate to properties of things both tangible and intangible in the real world, so that our theory can make explanations or predictions

about what happens to the thing if the state of a property changes. For example, we may wish to theorise about how user perceptions of an e-commerce website change depending on the way data is represented on that website (as text, as a graphic, or as an animation, for example).

Which constructs compose a theory is a fundamental question. The choice of constructs determines both locus (the domain addressed) and focus (the level of abstraction) of the theory. The number of constructs determines comprehensiveness (how much does the theory account for?) as well as parsimony (what is the most simple account possible?).

Thinking about theoretical constructs also allows us to define at least two key ways in which way we can contribute to theory:

- We can articulate new constructs as the basis for new theory regarding certain previously unknown phenomena or as a new way to look at existing phenomena. Alternatively, as we can articulate new constructs to form part of an existing theory. For example, in recent years scholars have introduced the constructs of ‘habit’ and resulting inertia to explain why in some cases individuals continue to use an old information system rather than accepting a new information system [130]. Note here that the concept of habit is by no means a new one; yet, its operationalisation as a construct within the nomological net (see below) of technology acceptance was a novel addition to theory.
- We can delete constructs from a theory in an attempt to increase parsimony of the account offered by theory. A good example would be the early work around the Technology Acceptance Model, which showed that the effects of the construct “attitude” are fully mediated by the other constructs in the theory, and could therefore be omitted [46].

Typically, constructs can be specified further in terms of their status to the theory. We distinguish *focal* constructs and *ancillary* constructs. Focus constructs are those that are the key components in our theory and determine its locus and focus. There might be further constructs that describe other phenomena or properties of interest and that are associated with the focal constructs in some way perhaps because they moderate or mediate some effects of some constructs on another construct. Note that it is also possible not to have a focal construct per se (perhaps because our theory pertains to constructs in general).

Having identified a set of constructs that describe *what* the theory is about, the next question is *how* the constructs are related to another. In this step, we describe the *relationships* between constructs, or how the changes in the state of one property change the state of another property in that thing or another thing. In this step, we are essentially describing laws of interactions: patterns of how values of a construct change in accordance with the change of values in another construct.

These laws of interactions are typically our attempt to define a sense of causality in a conceptualisation of some phenomena, in that we describe certain patterns of behaviour for the properties captured in our theory. The nature of the specified relationship depends on the purpose of the theory and may be of many types, such as associative, compositional, unidirectional, bidirectional, conditional, or causal.

Again, we can see how we can contribute to theory with a focus on the relationships between constructs:

- We can articulate new laws of interaction amongst existing or new constructs.
- We can delete laws of interactions amongst the constructs of a theory.
- We can re-define the existing laws of interaction amongst constructs in a different way.

In general, the laws of interaction can be specified with varying levels of precision. For instance, some theories merely state that the values of their constructs are associated with one another by showing that high values of one construct are associated with high or low values of another construct, or the existence of one value of a construct signal the existence of a certain value of another construct. In some cases, the functional relationships between constructs can be specified more precisely as when a certain value range of one construct mediates, mitigates, or moderates the value range of another construct or even the relationships between two or more other constructs.

The next step is then to ask the question of *why* – why are the chosen constructs relevant and complete and why are the laws of interactions as specified? This part of theory relates to the so-called *justificatory mechanisms*: the reason for why this theory is a credible account of the phenomenon to which it pertains. An answer to this question is incredibly relevant because the justificatory mechanisms are the key vehicle to lending credence to the particular account that the theory offers, and describe the logic of the key assumptions underlying the theory. In turn, they provide the basis for gauging whether the proposed conceptualisation is reasonable.

Typically, justificatory mechanisms for a theory in information systems are drawn from existing and sound fundamental views of human nature, organisational processes, or societal behaviours. For example, the above-mentioned Technology Acceptance Model builds upon premises by the Theory of Reasoned Action, which describes fundamental human volitional behaviour in terms of the attitude toward that behaviour and the beliefs about the consequences of performing the behaviour. We can see how such a theory of human behaviour can serve as a logical assumption about how people might behave when being confronted with new information technology.

Similar to the above, we can contribute to theory in at least two ways:

- We can articulate new justificatory mechanisms for the constructs or relationships of a new or an existing theory. A good example within the vein of studies on basis of the Technology Acceptance Model is Expectation-Confirmation Theory [19], which demonstrated a different logic based on cognitive-dissonance theory that also explains why people might perceive technology to be useful and are thus inclined to use it.
- We can delete a justificatory mechanism underlying a theory by showing that the assumptions are violated.

The importance of justificatory mechanisms should not be understated. Essentially, they describe the logic of a good theory, upon which the trust is built that

others (other academics, reviewers, practitioners and so forth) believe in our theory. Gradually, in the research process, the logic built upon the key assumptions can be replaced by empirical data that shows that the constructs and relationships indeed behave as expected. Still, a theory will always be used for explaining *why* this is the case. And indeed, an answer to this ominous “why” question is often what separates an empirical study, where scholars can discover qualitatively/quantitatively that certain relationships between certain constructs exist, from a theoretical contribution, where scholars can offer an explanation for why this is the case.

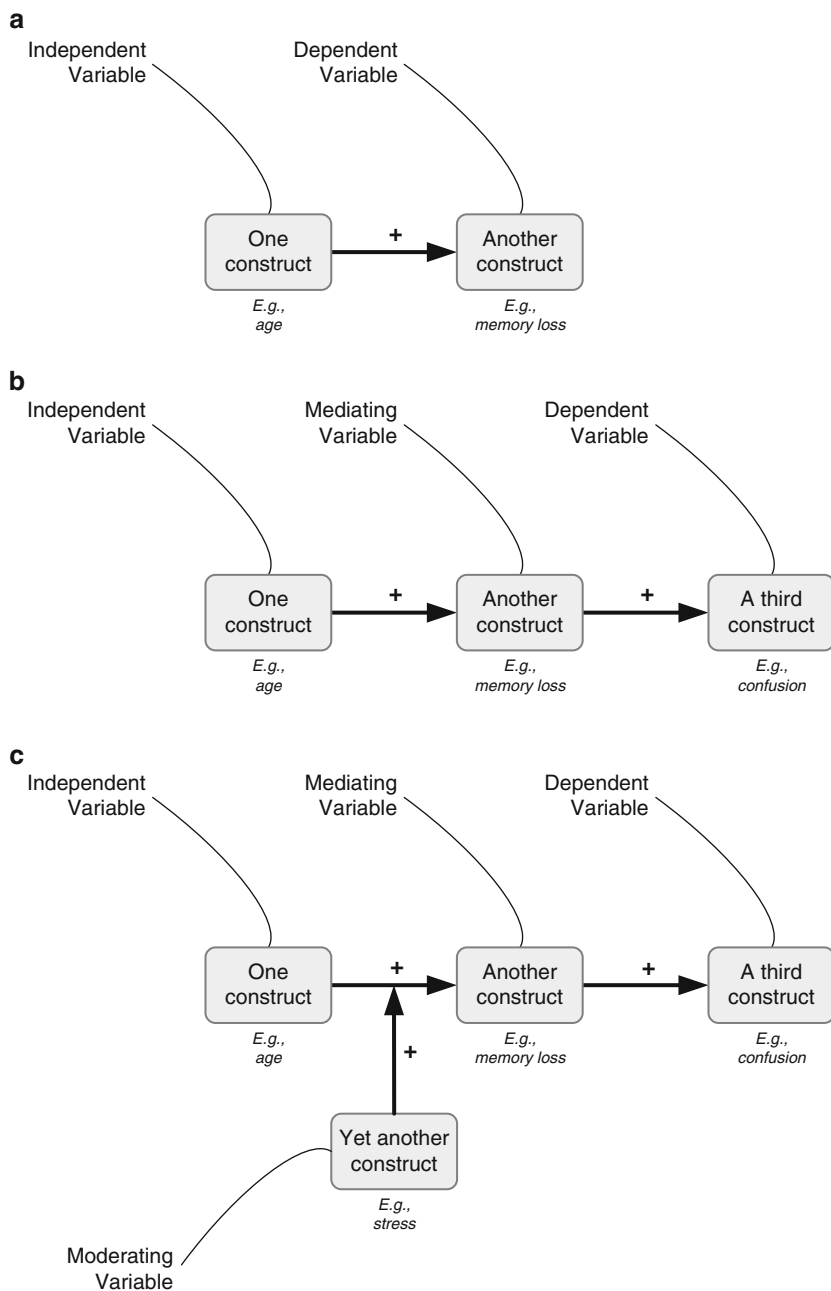
A final component of theory is the set of *boundary conditions*. Boundary conditions describe the circumstances under which the theory is expected to hold. Boundary conditions describe the scope and limitation of a theory. The scope of a theory is specified by the degree of generality of the statements of relationships signified by modal qualifiers such as ‘some’, ‘many’, ‘all’, or ‘never’ and other boundary statements showing the limits of generalisations. These limits can be uncovered by specifying conditions of ‘who?’, ‘when?’, and ‘where?’, all of which place limitations on the propositions articulated through the chosen set of constructs and the laws of interaction between them. These contextual, spatial and/or temporal factors set the boundaries of the generalisability; they define the scope of the theory. The boundary conditions of a theory are specified firstly when the constructs are selected for the theory (e.g., *experienced* computer users as opposed to *all* computer users). Also, they can be specified by considering only certain value ranges of one or more constructs (e.g., computer use during *business hours* as opposed to computer use *at night*). We can contribute to theory in at least two ways through a focus on boundary conditions:

- We can articulate new conditions that specify where a theory will or will not hold.
- We can examine our theory thoroughly in situations that violate some condition of the original theory to explore whether or not the theory will hold.

Often in theory building efforts, boundary conditions are not explicitly considered or explored at first. Typically, we see how work concerning that theory over time adds to our understanding of the boundary conditions. Again perusing the work around the Technology Acceptance Model as an example, over time scholars have explored the boundary conditions under which the premise of the original theory holds; for instance, in situations of mandated versus voluntary technology use [24], in different usage scenarios such as for work or enjoyment purposes [172], or in different cultural contexts [114].

4.1.3 *Nomological Nets*

Equipped with an understanding of constructs, relationships, justificatory mechanisms and boundary conditions, we can further dissect a theory by focusing specifically on the role of the constructs within the theory. Depending on their intended position in the

**Fig. 4.1** Nomological nets

theory, we can classify constructs as *independent*, *dependent*, *mediating*, or *moderating* variables. This classification of constructs is called a *nomological net* of constructs – a representation of the constructs of a theory together with their observable manifestations and the interrelationships among and between these. An example is shown in Fig. 4.1.

One construct in our theory might be affected by another construct if a change in the property values of the latter construct, the *independent variable* in Fig. 4.2, will invoke a change in the property values of the original construct. If so, we denote this construct as the *dependent variable* in our nomological net (see Fig. 4.2a).

It could also be that a relationship between an independent and a dependent variable is intervened upon by a third construct. We call this construct in such a case a *mediating variable*. In the example given in Fig. 4.2b, for instance, we see that the effect of age on confusion is mediated by memory loss. In other words, an increase in age can increase chance of memory loss, which can contribute to more confusion. Mediating variables typically add to the explanatory power of a theory because we can better specify the exact causality between constructs. For example, we realise that age per se does not necessarily lead to confusion, but it might lead to memory loss that in turn can lead to confusion. We can also interpret that confusion is a likely consequence of memory loss, which allows us to extend the conceptualisation of our initial nomological net (in Fig. 4.2a).

Finally, other factors might be present that moderate the strength of the relationship between two or more constructs. We call such constructs *moderating variables*. For example, we might envisage how the positive effect of age on memory loss (a negative consequence, of course, but the relationship between age increase and memory loss increase is positive) might be further strengthened when the individuals are experiencing lots of stress. In turn, stress moderates the relationship because memory loss *also* depends on the stress levels experienced, for instance between two equally old people, the individual that experiences higher stress levels will have higher chance of memory loss.

Nomological nets – the description of constructs, their manifestations, as well as consequences and interactions – are an important tool for researchers to argue the soundness and reasonableness of a theory. A good theory is one that can be embedded and mentally visualised in a net of constructs that specify antecedents (independent variables causing some changes in the values of the constructs) as well as consequences (dependent and mediating variables) and interaction laws (through moderating variables).

4.2 Types of Theory

Having discussed the structural components of theory, let us have a look at different ways that theories have been developed or used in information systems research.

Why are there different types of theory, one might ask? The answer lies in the nature of the information systems discipline as an applied research field. In such

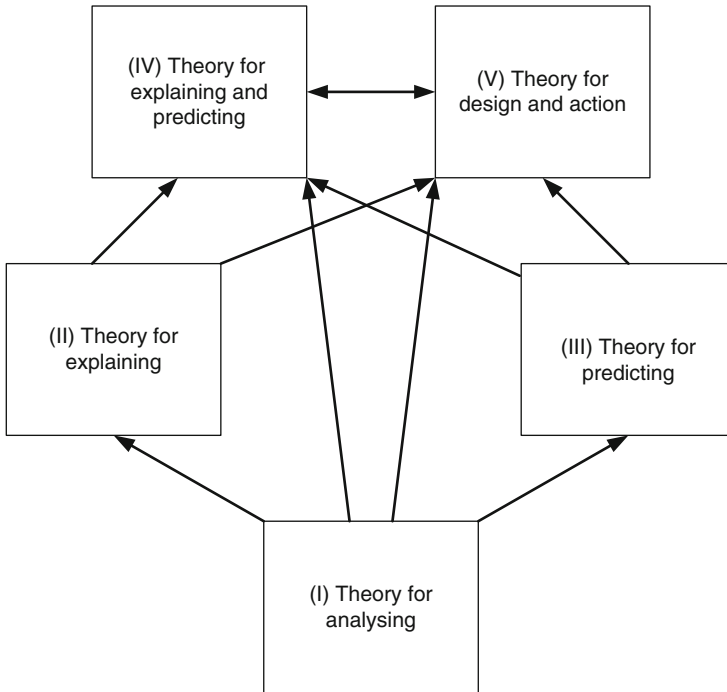


Fig. 4.2 Types of theory [76]

research fields, research usually begins with a problem that is to be solved, some question of interest, or a newly emergent phenomenon that is of relevance. Theory that can then be developed as part of a research process should therefore depend on the nature of the problem, question, or phenomenon being addressed.

Theory in this understanding must thus have a *causa finalis*, that is, an ultimate goal that specifies what the theory is for. Shirley Gregor [76], in her essay on the nature of theory, has argued that theory has one or more of the following four aims:

- **Analysis and description:** The theory provides a description of the phenomena of interest, analysis of relationships among those constructs, the degree of generalisability in constructs and relationships and the boundaries within which relationships, and observations hold.
- **Explanation:** The theory provides an explanation of how, why, and when things happened, relying on varying views of causality and methods for argumentation. This explanation will usually be intended to promote greater understanding or insights by others into the phenomena of interest.
- **Prediction:** The theory states what will happen in the future if certain preconditions hold. The degree of certainty in the prediction is expected to be only approximate or probabilistic in the information systems domain.

Table 4.1 Types of theory [76]

Theory type	Description
Analysis	<i>Says what is</i> The theory does not extend beyond analysis and description. No causal relationships among phenomena are specified and no predictions are made
Explanation	<i>Says what is, how, why, when, and where</i> The theory provides explanations but does not aim to predict with any precision
Prediction	<i>Says what is and what will be</i> The theory provides predictions but does not have well-developed justificatory causal explanations
Explanation and prediction	<i>Says what is, how, why, when, where, and what will be</i> Provides predictions and has both testable propositions and causal explanations
Design and action	<i>Says how to do something</i> The theory gives explicit prescriptions (e.g., methods, grammars, principles of form and function) for constructing an artefact

- **Prescription:** A special case of prediction exists where the theory provides a description of the method or structure or both for the construction of an artefact (akin to a recipe). The provision of the recipe implies that the recipe, if acted upon, will cause an artefact of a certain type to come into being.

If we combine the above goals, we can then distinguish five general types of theory that pertain to information systems as a research discipline. Shirley Gregor describes them as summarised in Table 4.1.

Analysis theories are the most basic type of theory; they describe “what is” by classifying specific dimensions or characteristics of phenomena such as individuals, groups, situations, or events. They are needed when nothing or very little is known about the phenomenon in question. We know these theories as taxonomies, typologies, classifications, schemas, frameworks, or even ontologies. A famous example for an analysis theory is the DNA double helix, a model that describes the structure of the genetic instructions used in the development and functioning of all known living organisms. It makes no statements about *why* living organisms function a particular way or *how* the development takes place. Other examples include framework of species and animal classes or indeed Shirley Gregor’s own classification of theory types!

Explanation theories focus primarily on describing how and why some phenomena occur. Such theories also function as models for understanding because they often focus on presenting a specific view or conceptualisation of some real-world phenomenon or domain. Note here that the emphasis lies on explaining some phenomena but not necessarily predicting future phenomena or variations of phenomena. A prime example is given in Robert Yin’s case study textbook [195] – the research on the Cuban Missile Crisis [3]. In this book, an explanatory

model (an explanation theory) is described that offers an account of the confrontation between the Soviet Union, Cuba and the United States in October 1962 through an organisational process and governmental politics model. In this book, the author also demonstrates that the then prevalent explanatory model (mutually assured destruction as a barrier to nuclear war) was unfounded. We can see how this theory can be used to explain a particular event (and the processes that unfolded at that event) – but is not aimed at predicting a second event or related processes.

Prediction theories aim at describing what will be without focusing on why that might be the case. Such theories use a range of independent variables to predict an outcome without including the justificatory mechanisms that would explain the causal connections between dependent and independent variables. This might be because the “internal workings” of the phenomena have not yet been found or because an understanding of the causality is in fact irrelevant to the aim of the theory. As an example, let us turn to Moore’s Law, which predicts that the number of transistors that can be placed inexpensively on an integrated circuit doubles approximately every 2 years. It is interesting to note that this law in general terms also holds (mostly) for other electronic devices and attributes, such as processing speed, memory capacity, sensors, and even the number and size of pixels in digital cameras. The law has been deduced empirically by plotting the graph of the log of the number of components per integrated function against the year from 1959 to 1965 and even further, but no causal model or nomological net is offered that explains why the number of transistors doubles rather than triples or quadruples. A good second example can be found in research on organisational innovation. At present, we know that we can predict organisational innovativeness as a function dependent on organisational size. *Why* organisational size matters to innovativeness is yet unknown.

Explanation and prediction theories attempt to be able to predict as well as explain the underlying causal conditions that lead to a predicted outcome. This type of theory focuses on understanding underlying causal mechanisms as well as on the prediction of a phenomenon. This type of theory is probably most common in the information systems research field, and many good examples can be found. For one thing, the often-cited Technology Acceptance Model is a good example for a model that attempts to predict whether individuals accept new technologies and offers an explanatory account of why that should be the case (acceptance depends on positive beliefs about usefulness and ease of use).

Explanation and prediction theories are often either variance or process theories. Process theories look at the unfolding of events over time and offer an explanation for the unfolding and a prediction regarding future events. Variance theories look at the degree to which one variable can predict changes in another variable and why that would be the case. Good examples include the theory of evolution, which explains and predicts the process of change in all forms of life over generations. The causal conditions identified by the theory are mutations, genetic drift, and natural selection. An example of a variance theory in information systems would be Yair Wand and Ron Weber’s theory of representation [181, 182, 185], which models the desirable properties of information systems at a deep level and predicts consequences when these properties are not met.

Design and action theories, finally, are theories that specify how to do something. These theories give normative, prescriptive rules such as principles of form and function, methods, and techniques together with justificatory theoretical knowledge about how to construct an artefact (for example, a specific type of an information system). Good examples are widespread in many applied research disciplines. Design theory in architecture, for instance, consists of all the knowledge that an architect uses in his/her work, from how to select the best site and the most suitable construction materials, and advice on how to design practical buildings, to designing for ease of maintenance and repair. In software engineering, Erich Gamma's design patterns specify recurring solutions to common problems in software design. We also find examples in education. Take progressive learning theory: it builds on the assumption that humans are social animals, organisms that are highly interactive with other members of their species to the point of having a recognizable and distinct society. Based on this assumption, the theory asserts that humans should learn best in real-life activities with other people. Forthcoming from this assertion progressive learning theory offers prescriptive advice for the construction of teaching materials: teaching materials should not just provide reading and drill, but also real-world experiences and activities that centre on the real life of the students. In information systems research, the focus on design-type research has over recent years led to the formulation of many instances of design theories, such as

- A design theory for tailorable technologies [70],
- A design theory for systems that support emergent knowledge processes [113],
- A design theory for systems that support convergent and divergent thinking [118], or
- A design theory for social recommender systems [5].

Having explained the types of theories, we should briefly discuss interrelationships, logically as well as temporally. Consider Fig. 4.2: analysis theories are required for the development of all other theories because they offer a descriptive account of relevant constructs and attributes for phenomena. Explanation theory as well as prediction theory can provide the basis for the development of theory through explanation and prediction. All the other theory types can inform design theory. Design theory and theory for explanation and prediction can be closely interrelated as the design can be informed by a thorough explanation of how a particular phenomenon works. In the same way, designed artefacts can be examined in terms of the changes in events, processes, and behaviours they induce.

A doctoral thesis may but does not have to include one or many types of theory. Often, doctoral research focuses on developing a theory for explanation, say, together with an attempt to systematically collect evidence in support of that theory. Other theses might offer an analysis theory as a first contribution in emerging domain or about a previously unknown phenomenon. Of course, several contributions have and will be made in the form of design theories and artefacts as instantiations of that theory.

4.3 Theorising as a Process

Above we discussed what theory is, what some common structural components of theories are, and how we can distinguish different types of theory. That should equip us with a good understanding of theory as the outcome (or artefact) of the research process. What we still need to discuss is the process that leads to the generation of theory – the theorising process.

Theorising, in essence, is the application or development of theoretical arguments to make sense of a real account (an observed phenomenon). One of the first characteristics of that process that we need to understand is that theorising is iterative and cyclic in nature, as shown in Fig. 4.3.

Theorising, in some form or other, starts with a set of knowledge (1.) and the assumptions (2.) we can derive from it. Note that I am not specifying what form or structure this knowledge should have. It may or may not be knowledge derived by the scientific method – in fact, it may depend on rigorous data analysis, creative thinking about some observations, inspiration, or simply good luck. Theorising then continues (3.) with formulating tentative propositions (or more formally, hypotheses; or less formally, conjectures) about the phenomenon we are theorising about. This step can be inductive, deductive, or abductive in nature.

Remember that inductive reasoning means moving from a set of specific facts to a general conclusion, and can either be weak or strong. Also, remember that induction is impossible to prove; yet it is an accepted pathway for hypothesis construction because conclusions are offered based on educated predictions.

Deductive reasoning means that we derive arguments as logical consequences from a set of more general premises. That is, we attempt to show that a conclusion necessarily follows from a set of assumptions. This we already discussed above in terms of implications for research design. Theorising may also be done through abductive reasoning. Abduction is a kind of logical inference that arrives at an explanatory hypothesis or proposition through a process that is often called “informed guessing”. Abductive reasoning occurs when an inquirer considers a set of seemingly unrelated facts, and thinks that they are somehow connected. Consider the example of the observation “the lawn is wet”. There might, theoretically, be an unlimited set of potential reasons for why the lawn is wet. It would be unsurprising that the lawn is wet if it had rained the night before. Therefore, by abductive reasoning, the most reasonable theory for why the lawn is wet is that it rained the previous night.

Abductive reasoning means reducing the search space of potential reasons through informed guessing such that the ‘leap of abduction’ is characterised through simplification and economy. The danger is that abductive reasoning offers no means to prove beyond doubt that a proposed reason is indeed a causal factor. It is, however, a useful tool for reducing the solution space to the most likely or economical reason for an observed phenomenon.

Either way is appropriate as long as we arrive at a plausible set of propositions, hopefully well-situated in a nomological net and tightly coupled through justificatory mechanisms; in other words, that the set of propositions has the desired traits of good theory.

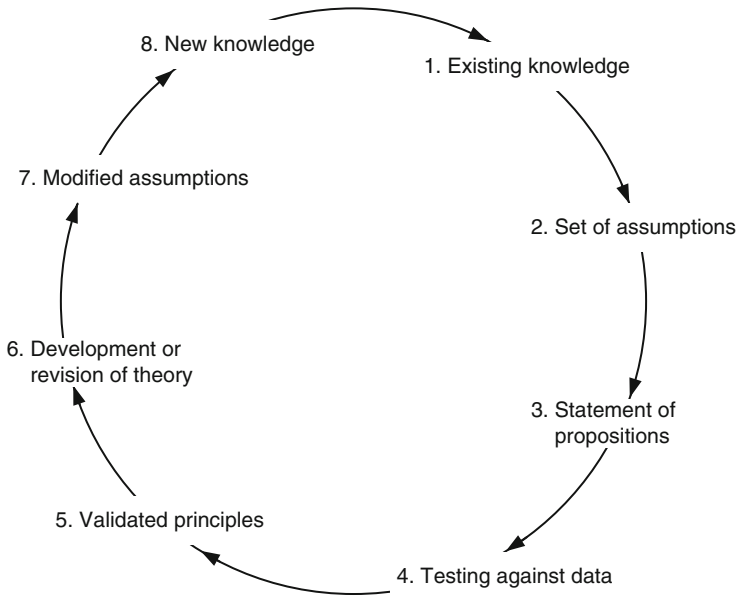


Fig. 4.3 The theorising process

Theorising then should involve some sort of test of the propositions against data (4.). This comprises the validation stage of the research process. In information systems research this typically, but not necessarily, means subjecting the theoretical propositions to careful empirical examination where data is systematically collected and analysed to contrast the observations (the data) against our speculations (the theory in development). Research methods (see Chaps. 5 and 6 below) are the tools used to that end. The outcome of this step is a set of general and validated principles (5.), that is, principles that have been confirmed through testing the theory empirically. These may be the original principles that we speculated in our set of propositions or hypotheses, or they might be adjusted principles drawn from the data. In turn (6.), our theory can be fully developed (when the data confirmed the propositions) or revised (if the data showed that our propositions need to be adjusted). Based on theory we can then modify the set of assumptions by adding the theoretical premises developed to the list of assumptions (7.), and in turn create new knowledge (8.), which can form the set of existing knowledge for a new iteration of the theorising circle.

4.3.1 *An Example*

Let us walk through an example of theorising that explains this cyclic process and further introduces important attributes that relate to this process. I am drawing the

example freely from Stephen Borgatti's adaptation of Charles Lave and James March's textbook on theories in the social sciences (I have adapted the example for my own illustration purposes) [21]. Try to think through each step of the following illustration before proceeding with my suggested theorising outcomes.

We start with an observation.

For example, think about being in college. You're in class, and the guy next to you – who is obviously a football player – says an unbelievably dumb thing in class. So you ask yourself: "Why?"

Let's start theorising about reasons for why the football player guys said an unbelievably dumb thing in class. One of the first ideas for an explanation (a leap of abduction, if you will) that I would come up with is:

- Football players are dumb.

This statement can be considered a theory. It is not a very good one, but it is a start. What would make it better?

One problem with this initial draft of a theory is that it is not very **general** but instead very narrow. We only refer to football players in our theory. Theories that are too narrow and specific are not very interesting, even if they are correct. Instead, generalisation (towards universality even) is a key desired attribute of good theory.

This is not to say that all theorising must inevitably strive for wide generalisation or even universality. The generality of a theory is normally classified as one of three types depending on the breadth of focus of that theory: *Substantive theory* is developed for a very specific area of inquiry. In contrast, *formal theory* is developed for a broad conceptual area. In between these extremes, *mid-range theory* is moderately abstract and has limited scope. Many theories in information systems (such as the Technology Acceptance Model, for example) are mid-range theories. Examples of formal theories include complex system theory, or theories of power and socialisation. The statement "football players are dumb" would be an example of a substantive theory.

Let us explore what a more general theory would look like. For example, we might state:

- Athletes are dumb.

The range of this statement is much more broad than the previous one but is still limited in scope, thus denoting a mid-range theory.

Still, this statement has no sense of **process**, no sense of **explanation**. It says, athletes have this property of being dumb, and that's why they ask dumb questions. It does not offer an account that would explain why they say dumb things. It merely makes a statement.

More precisely, the problem with our statement is that it contains **circularity**. What do we mean when we say that a person is dumb? Practically speaking, it means that they consistently behave stupidly.

The problem with being dumb as a quality is that it cannot be observed or measured directly. It is a latent construct, intangible in nature, like so many other constructs in the social sciences. The only way we can know whether people are

dumb is by examining carefully what they say and do. Yet what we are trying to explain is a dumb thing that they said. So in effect our statement is saying that they say dumb things because they say dumb things. This circularity prevents theories from being **falsifiable**.

We can avoid circularity and the problem of falsifiability by building into our theory a type of **explanatory process** – an account that explains how things unfold to cause a particular phenomenon to occur or behave in a particular way. Every good theory has a sense of process that describes a mechanism by which A makes B happen, like the way the gears in a car transfer the rotation in the engine to a rotation of the tires. Theorising therefore involves imagining an observation as the outcome of a (hidden) process.

What would be an explanatory process that we can build into our athlete theory? Try to develop your own suggestion before continue reading my suggestion:

- To be a good athlete requires lots of practice time; being smart in class also requires study time. Amount of time is limited, so practicing a sport means less studying which means being less smart in class.

The focus of this version of our theory now is a **mechanism**, not an enduring **property** of a class of people (in our case, athletes). You can also probably see how much more thinking had to go into identifying this probably/likely or possible mechanism that we ascribe to explain the behaviour we witness. In fact, I would argue that identifying the explanatory mechanism of theory is probably the hardest part.

Aside from the fact that we moved away from examining how one property begets another property, using mechanisms in a theory also means that we can apply the same reasoning to other people and other situations:

- There is limited time in a day, so when a person engages in a very time-consuming activity, such as athletics, it takes away from other very time-consuming activities, such as studying.

An implication of this version of the theory is therefore that we should now also be able to observe whether good musicians (who also practice many hours a day) also act dumb in class. If we don't find this to be the case, our theory is wrong. This would in fact be a good thing! A good theory is general enough to generate implications for other groups of people and other contexts, all of which serve as potential testing grounds for the theory. That is, the theory is **fertile** because it creates opportunities that allow us to falsify our theory.

We have now conceived a potential explanation of the phenomenon we are studying by imagining the observation as the outcome of a (hidden) process. We stated our theory in a way that it is general, has a sense of process, is falsifiable and is fertile. We may call it the "Limited Time Theory". Of course, our Limited Time Theory is but one possible explanation for the observation we started with. Theorising also involves moving forward and not only supporting our theory

through data but also **ruling out alternative theories**. For example, through the same process as above we may have ended up with different theories such as the following two accounts:

- The “Excellence Theory”: everyone has a need to excel in one area. Achieving excellence in any one area is enough to satisfy this need. Football players satisfy their need for accomplishment through football, so they are not motivated to be smart in class.
- The “Jealousy Theory”: we are jealous of others’ success. When we are jealous, we subconsciously lower our evaluation of that person’s performance in other areas. So we *think* football players ask dumb questions.

Note how both these rival theories are also general and fertile and generate implications for other groups of people, such as musicians or beauty queens.

To rule out alternative theories – or to choose between rival theories – we can utilise the fact that our theories are fertile and non-circular. This is because our theories can be applied to scenarios in which they are expected to hold but which would violate the implications of some (or all) of the alternative theories.

If a theory is specific enough, a situation can be plugged into the theory in order to discover what outcome would present. The idea, then, is to collect a set of situations which when applied to different theories would result in different predictions or expectations.

Consider, for example, how football players should behave (or appear to behave) in class out of season. Will they still be asking dumb questions? According to the first theory (“Limited Time Theory”), football players should not ask dumb questions out of season, because there is plenty of time to study. But according to the second theory (“Excellence Theory”), members of the football team should continue to ask dumb questions because they are still football players and still getting recognition, so they still don’t feel the need to excel academically. The third theory (“Jealousy Theory”) would also yield the expectation of continued dumb questions, because we are still jealous, and jealousy is not dependent on seasons.

In turn, studying football player behaviour out of season should help to distinguish between the first theory and the other two, no matter how the data turn out. If the football players appear smart out of season, then the Excellence and Jealousy theories are wrong and we can rule them out. If the football players appear dumb, then our original Limited Time theory is wrong. In that case, however, we still don’t know whether Excellence or Jealousy is the better explanatory account for our observation.

What we can do in such a situation is to conceive another scenario by which we can distinguish our theories. For example, consider athletes who do not look like athletes because they are not unusually big (like football), tall (like basketball) or fat (like sumo wrestling). Would those athletes appear to ask dumb questions? The Limited Time theory will again clearly say “yes” because practice time required for these sports is unaffected by physique. The Excellence theory will also say “yes”

Table 4.2 Expectations generated by each theory for the two situations

Question	Limited time theory	Excellence theory	Jealousy theory
Do football players ask dumb questions out of season?	No	Yes	Yes
Do athletes who do not look like athletes ask dumb questions?	Yes	Yes	No

because even if people can’t recognise them on the street, they are still fulfilling their need to do one thing really well so they will not feel the need to excel in class. The Jealousy theory, however, would say “no” (for most people anyway), because if the athlete is not recognizable as such (by virtue or being tall, big or fat), we would not realise the fact that we are in the presence of an athlete.

In essence, what we have done is we conjured situations in which we have different expectations as per the propositions of our theory, and we conjured sufficient situations to determine a verdict about the theories (see Table 4.2).

You should see how this set of expectations would allow us to go and precisely collect data that, upon analysis, should allow us to rule out two theories in favour of one remaining theory. In other words, we have created testable hypotheses that allow us to determine the validity of our theory.

Of course, this example is simplistic in nature, but you should still be able to learn about important principles of theorising from it. You should also have noted how in many cases we made a set of **assumptions** on which our theory is based. For one thing, we are assuming that there is a time (out of season) when football players are not consumed by the sport – which in some cases might not be true. The jealousy theory also builds on an assumption, namely that we ascribe negative characteristics to people that look like they have high social status.

4.3.2 Practical Suggestions to Theorising

Let us end this chapter by reviewing some general suggestions about the act of theorising. Most importantly, the suggestion is not to underestimate the significance of theorising in scholarly work. Many of the editors of prominent journals keep stressing the point that, typically, reviewers of scholarly work expect your method to be sound and rigorous, your research plan and execution is effective and appropriate. This means that you will not get a paper accepted because you executed the survey or case study method well. This is simply expected of you. Instead, papers are being inspected primarily for novelty and theoretical contribution. Detmar Straub [165], the current Editor-in-Chief of the MIS Quarterly, chose these words:

Theory is King and it is in the evaluation of the theoretical contribution that most reviewers become convinced, or not.

Having recognised the importance of this task in the research process, let us review some of the tips I have received over the years from other colleagues and senior scholars:

First, theories should be *well argued*. Theorising is a lot about building trust in your account of a phenomenon. The fact that we go out and test our theory against empirical data does not change the fact that the theory itself should be built on solid arguments and logical conclusions. Therefore, theorising should be data-inspired or theoretical/logical arguments-inspired.

Second, theorising should be *insightful*. Through this work, we should come to develop a new perspective on a new or existing phenomenon that we didn't have before. Theorising becomes striking when we instantly know that we have not read something like this before, at least not from this perspective.

Third, theorising should *challenge existing beliefs* and offer a set of new beliefs. Our theoretical account should be characterised by novelty – a new lens for looking upon our world, through which we actually see the world differently. The case of the Cuban Missile Crisis that I used above as an example for explanation theory shows how the organisational process and governmental politics model of the theory challenged the then prevalent theory, which stated that the Cold War did not eventuate because the mutually assured destruction power through nuclear weapons acted as a barrier to nuclear war.

Fourth, related to the point above, theorising should have (surprising) *implications that make sense and are intuitively logical*. A good theory is often characterised by its sense of “obviousness”: the feeling that the consequences of the tenets of the theory are “just” what is truly happening in the world – which is what the theory aspired to do!

In aspiring to meet these four key attributes of good theorising, there are several tips that might be useful:

Don't hide but instead focus on unique findings. This means that when we you set out to collect and examine data and you find something inconsistent with prior research, chances are that these findings can lead to a new account of that phenomenon – and in turn to new theory. Often, we have the tendency of ensuring that findings are in line with current theories at the time – and propositions and hypotheses that are not supported by the data are a bad thing. The opposite might be the case: if your data clearly shows findings that you cannot explain through the existing body of knowledge, then surely there is something to theorise about.

Throughout the theorising process, *use easy yet convincing examples*. Some have even conflated theorising with storytelling [58, 60]. The key argument is that theory development can well be argued using appropriate yet simple examples that may be described in narrative prose. The above case of the dumb athlete is an example.

Be aware of and familiar with reference theories. Reference theories are existing theoretical accounts from research disciplines related to the domain of inquiry. For example, if you study organisational adoption of social media technology, a reference discipline could be organisational psychology: the study of work with and within global organisations. Reference theories are also often formal theories

that could provide a framework for identifying the key types of constructs that are probably relevant to the phenomenon studied. Being able to relate to such theories allows you to build your theory but also to contrast your account to others in an effort to rule out rivalry theories.

Iterate between theory and data. Build, read, apply, evaluate, reiterate, build, read, apply, evaluate, and so forth. Now, this suggestion assumes that there is some sort of close connection between rationalisation and exploration (and perhaps even validation, as per Fig. 4.3). This might not always be the case and it might not always be required, but let's assume this is true for a moment. Theorising is often a close cyclic and iterative interaction between the idea that is forming in your head and the data you have collected (or are collecting) about the phenomena you are studying. A new development in the theory might induce you to re-analyse the data or to go and collect more data, and by applying the findings from your data you may revise parts of the theory, or extend them. Often, in this process, reference to other theories is interwoven in and so forth.

So, how do you know that you are finishing with theorising, that you have good theory? I think you can feel that you are on a good way if you have answer to the following questions:

- Is your account insightful, challenging, perhaps surprising, and – importantly – does it seem to make sense?
- Does it connect disconnected or disconnect connected phenomena in a new way?
- Is your account (your arguments) testable (falsifiable)?
- Do you have convincing evidence to support your account?
- Is your account as parsimonious as it can be?
- Are the arguments logical?
- What can you say about the boundary conditions of the theory?
- What are implications of your theory?

4.4 Further Reading

Theorising is a phenomenon of interest to many scholars, which results in a large variety of literature being available about theory. I would argue there are many essays and conceptual articles that are 'must-reads', and which include the following:

- Shirley Gregor's essay on the nature of theory in information systems research [76]
- The debate between Eisenhard, Dyer and Wilkins about theory building [58–60]
- The attempts at theory definition by Wacker [176], Weber [186, 187], van de Ven, Weick [189, 190] and Whetten [191].

There are also textbooks available on theorising. A simple introduction is Paul Reynolds' primer on theory construction [141]. Another good read is Robert Dubin's "Theory Building" [57], as well as some of the processes that have been developed on basis of his account, e.g., [86].

Chapter 5

Research Methods

Information systems research as a social science is complex, diverse, and pluralistic, meaning that it can take many forms of inquiry, theory, and outcomes. The way information systems research is conducted as well as the goals, theory and assumptions of the research can vary significantly. This is probably most evident in the choices related to the selection of an appropriate research methodology, as we discussed in Sect. 3.3 above.

With all this diversity, it is still possible to identify the most popular forms of information systems research. Generally speaking, the most popular methods in social science are either exclusively quantitative or qualitative, with only a small fraction of mixed method studies and, increasingly, studies that rely on design science as a research method. Given that the latter is a quite recent method to emerge, most reviews of the literature focus on the other three research methods. Take Fig. 5.1, for example, which shows the use of qualitative, quantitative and mixed method research published in six of the field's top journals (*MIS Quarterly*, *European Journal of Information Systems*, *Journal of Information Technology*, *Information Systems Research*, *Journal of Management Information Systems* and *Information & Organization*), as reported in the study by Wen Shin Chen and Rudy Hirschheim [35]. The study shows specifically that quantitative methods dominate (roughly 60% of all published papers), followed by qualitative method papers (roughly 30% on average), and mixed method papers (10%). Note that the timeframe of the study was 1991–2001; however, other literature reviews, e.g., [34, 68, 107], report similar statistics.

In this chapter, we will first address in detail quantitative and qualitative research methods and secondarily design and mixed methods. The focus will be on key guiding principles that characterise each of these. There are other strands of research that do not easily fit the standard methods discussed here. Examples include structuralist or postmodernist research. Also, research of a conceptual nature – non-empirical research that emphasises ideas and concepts – does not fall within the quantitative/qualitative divide. Most researchers however conduct their studies using either qualitative or quantitative methods, or both.

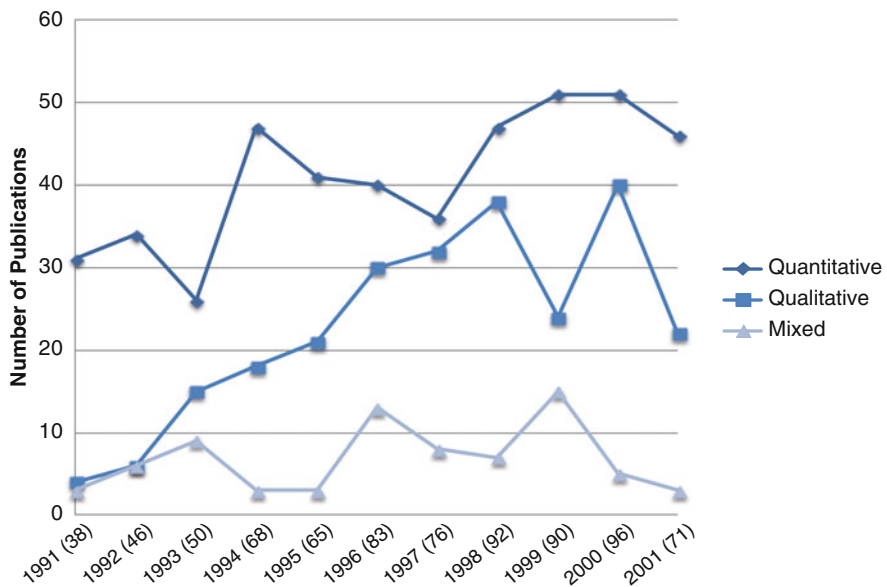


Fig. 5.1 Trend of research methods used in publications in selected top IS journals [35]

5.1 Quantitative Methods

Quantitative methods describe a set of techniques to answer research questions (for example, about the interaction of humans and information technologies) with an emphasis on *quantitative data*.

There are two cornerstones common to such research methods. The first cornerstone is the emphasis on quantitative data. The second cornerstone is an emphasis on positivist philosophy. Regarding the first cornerstone, quantitative methods tend to specialise in “quantities”, in the sense that numbers are used to represent values and levels of theoretical constructs and the interpretation of the numbers is viewed as strong scientific evidence of how a phenomenon works. Often, the presence of numeric data is so predominant in quantitative methods that people understand statistical tools, techniques, and packages to be an essential element of quantitative methods. While this is often true, quantitative methods do not necessarily involve statistical examination of numbers. For now, let us simply assume that quantitative methods focus on how you can do research with an emphasis on quantitative data collected as scientific evidence.

This emphasis on numerical analysis is also key to the second cornerstone of quantitative methodologies: a positivist philosophy of research that is guided by realist and objectivist ontology and an empiricist epistemology. Together, these assumptions of quantitative methods shape the idea that theories can be proposed that can be falsified by comparing the theory to carefully collected empirical data. More precisely, realist ontology presupposes a reality that is objective, simple,

and fixed. This posits that reality exists independent of human consciousness and perception and rests on order consisting of strict, natural laws. Objectivist ontology describes reality and truth existing objectively, discoverable and adequately measurable. Quantitative research methods thus allow us to uncover this reality and its hidden truths, and bring them to awareness. An empiricist epistemology supposes that knowledge comes through experience mediated through the senses. Hence, observation and experience of empirical facts form the basis of knowledge. On the basis of these principles, research using quantitative methods sets out to study events in the real world, learn about them and their relationships so that general laws can be discovered, explained, and documented.

5.1.1 *The Focus on Measurement*

Due to its focus on quantities that are collected to measure the state of some variable in real-world domains, quantitative methods are very focused on the process of measurement. This is because measurement provides the fundamental connection between empirical observation and mathematical expression of quantitative relationships. Measurement is, very simply, the most important thing that a quantitative researcher can do to ensure that the results of the study can be trusted.

Consider Fig. 5.2, which describes in simplified form the process of quantitative research. Typically, such research starts with developing a theory that offers a hopefully insightful and novel conceptualisation of some important real world phenomena. In attempting to falsify the theory or to collect evidence in support of that theory, operationalisations are sought and data is collected from a real world domain that are supposedly measuring variables that relate to the operationalised conceptualisation of our theory.

Figure 5.2 also displays two key challenges in research using quantitative methods. Moving from left (theory) to right (data collection), the first issue is that of *shared meaning*. If researchers fail to ensure shared meaning between their theoretical constructs and their operationalisations in measurement variables, an inherent limit will be placed on their ability to measure empirically the constructs about which they theorised. Then, while taking steps to obtain accurate measurements (the connection between real world domain and operationalisation) can reduce the likelihood of problems on the right side of Fig. 5.2 affecting the data (accuracy of measurement), but even if complete accuracy is obtained the measurements will still not reflect the construct theorised because of the lack of shared meaning. As a simple example, consider the scenario that your research is about individuals' affections when working with information technology and the behavioural consequences of such affections. An issue of shared meaning could occur if, for instance, you are attempting to measure "compassion". How do you know that you are measuring "compassion" and not, say, "empathy", which is a construct that has a similar meaning?

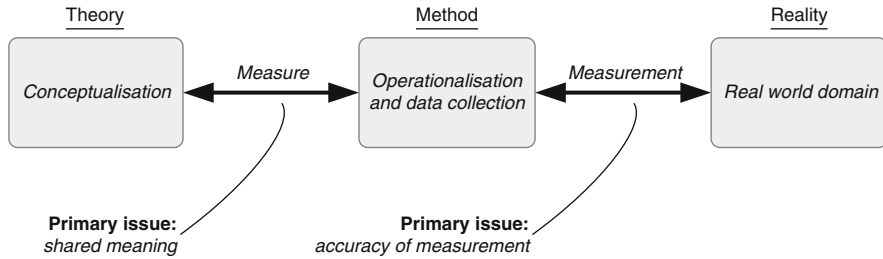


Fig. 5.2 The essential process of quantitative research, following [28]

Likewise, problems manifest if *accuracy of measurement* is not maintained. No matter how sophisticatedly researchers explore and analyse their data, they cannot have faith that their conclusions are valid (and thus reflect reality) unless they can faithfully demonstrate that the accuracy of their data.

To avoid these problems, we need to realise that not just “any measurement” will do in quantitative research. Instead, we must take steps to meet two key requirements to avoid problems of shared meaning and accuracy:

1. Our measurement variables must indeed measure the theoretical construct that we wanted to measure (in all its complexity if needed). This step concerns the **validity** of measurement.
2. Our measurement variables must indeed measure the theoretical construct consistently and precisely. This step concerns the **reliability** of measurement.

Validity and Reliability are called the psychometric properties of measurement variables, and describe the benchmarks against which the adequacy and accuracy (and ultimately the quality) of our quantitative method procedures are evaluated in scientific research.

Concerning the psychometric properties of measurement, four situations can occur, as shown in Fig. 5.3.

The worst situation is clearly when our measures are neither reliable nor valid (the left-most image in Fig. 5.3). In such a case, we cannot trust that the measurement variables of our operationalisation have shared meaning with the construct we set out to measure and we also cannot trust the accuracy of our data. A measurement can be reliable but not valid, if it is measuring something very consistently but is consistently off. A good example is that of a mis-calibrated weighing scale (image one that puts another 10 kg on your actual weight). The measurements you will receive are consistent but they don’t reflect your actual weight. Another type of problem exists when a measurement is valid but not reliable. This could be a measurement that is approximating the ‘true’ score of a construct but repeated measurements will yield inconsistent results (like an arrow that is scattered around the target).

Let us take a closer look at reliability and validity, and ways to ensure these two key requirements of quantitative research.

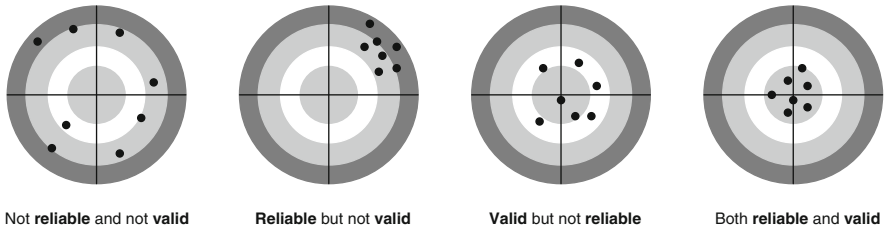


Fig. 5.3 Psychometric properties of measurement

5.1.2 Reliability

Reliability describes the extent to which a variable or set of variables is consistent in what it is intended to measure. If multiple measurements are taken, the reliable measurements will all be very consistent in their values. Reliable measurements approach a true, but unknown “score” of a construct.

Reliability implies that the operations of a study can be repeated in equal settings with the same results. Consider our example of weighing a person. An unreliable way of measuring your weight would be to ask others to guess your weight. Most likely, you will get different answers from different persons (and perhaps even different answers from the same person if asked repeatedly!). A more reliable way, therefore, would be to use a scale. Unless your weight actually changes in the times between stepping repeatedly on to the scale, the scale should consistently give you the same results. Note again, however, how a mis-calibrated scale would still give consistent (but inaccurate) results. This example shows how reliability ensures consistency but not necessarily accuracy of measurement.

Sources of reliability problems often stem from a reliance on subjective observations and data collections. All types of observations one can make as part of an empirical study inevitably carry subjective bias because we can only observe phenomena before the background of our own history, knowledge, presuppositions, and interpretations at that time. This is why often in quantitative research observations by the researcher or other subjects is replaced by more “objective” techniques such as questionnaires, or the use of other more factual means of data. Other sources of reliability problems stem from poorly worded questions that are imprecise or ambiguous, or, simply, by asking respondents questions that they are either unqualified to answer, unfamiliar with, predisposed to a particular type of answer or uncomfortable to answer.

Because even the most careful wording of questions in a survey or the reliance on non-subjective data does not necessarily guarantee that the measurements are indeed reliable, one precondition of quantitative methods is that instruments of measurement must still be tested for reliability. There are many ways in which reliability can be tested, and there is ample literature discussing reliability tests. A very good read is the article by Straub et al. [166] which introduces and discusses a wide range of reliability tests, including internal consistency (also known as

Cronbach's alpha), composite reliability, split-half reliability, test-retest reliability, alternate forms of reliability, inter-rater reliability, and uni-dimensional reliability. This article discusses each of these in detail, and so these topics do not bear repeating here. Instead I merely stress again how the demonstration of reliable measurements is a fundamental precondition to any quantitative study – and very simply the study results will not be trusted (and thus the conclusions foregone) if the measurements are found not to be consistent and reliable.

5.1.3 *Validity*

Validity describes whether the data collected really measure what the researcher set out to measure. Valid measurements represent the essence or content upon which the construct is focused. For instance, recall the challenge of measuring “compassion” – a question of validity is to demonstrate that indeed your measurements are focusing on compassion and not on empathy or other related constructs.

There are different types of validity that are important to identify. Some of them relate to the issue of shared meaning (Fig. 5.2) and others to the issue of accuracy. In turn, there are theoretical assessments of validity (for face and content validity), which assess how well an operationalisation fits the conceptual definition of the relevant theoretical construct; and empirical assessments of validity (for convergent, discriminant, as well as concurrent and predictive validity), which assess how well a measurement behaves in correspondence to the theoretical predictions. Note that both theoretical and empirical assessments of validity are key to ensuring validity of study results. Let us now take a look at the important notions of validity. Important assessment techniques are discussed amply in the literature [22, 69, 109, 163, 166], and will thus not be discussed in great detail here.

- **Face validity** refers to whether an indicator seems to be a reasonable measure of its underlying construct (“on its face”). Face validity, you can say, appeals to the sensemaking nature of a measure. For example, is annual salary a good measure of job satisfaction? Hopefully you agree that it is not. Face validity is often assessed by perusing a panel of experts who are well-positioned to rate a set of measurement items on how well they fit the relevant conceptual definition of a construct.
- **Content validity** refers to how well a set of measurement items matches with the relevant content domain of a theoretical construct. The key question of content validity is whether the instrumentation (questionnaire items, for example) pulls in a representative manner all of the ways that could be used to measure the content of a given construct. Content validity is important because researchers have many choices in creating means of measuring a construct. Did they choose wisely so that the measures they use capture the essence of the construct? They could, of course, err on the side of inclusion or exclusion. If they include measures that do not represent the construct well, measurement error results. If they omit measures, the error is one of exclusion. As with face validity, assessments may include an expert panel that peruse a rating scheme and/or a qualitative assessment technique called Q-Sort [117].

- **Construct validity** is an issue of operationalisation or measurement between constructs. The concern is that instrument items selected for a given construct are, when considered together and compared to other latent constructs, a reasonable operationalisation of the construct. Maybe some of the questionnaire items, the wording in the interview script, or the task descriptions in an experiment are ambiguous and are giving the participants the impression that they mean something different from what was intended. This is a construct validity issue. Two aspects of construct validity are important: **convergent validity** indicates the ‘closeness’ of a measure to its theorised construct and is evidenced when items thought to reflect a construct converge. **Discriminant validity** indicates the extent to which the measurement items posited to reflect or “make up” a construct differ from those that are not believed to make up the construct. There are numerous ways to assess construct validity. Typically, one tries to establish statistically that items that are meant to converge (measure the same constructs) have similar scores whilst also being dissimilar to scores of measures that are meant to measure other constructs. This could be done by comparing item correlations and looking for high correlations between items of one construct and low correlations between those items and items associated with other constructs. More sophisticated tests include exploratory factor analysis or principal component analysis, statistical tests that assess whether items “load” appropriately on higher-order factors. These we can then assume to reflect the theoretical constructs.
- **Other types of validity** also exist that apply to some aspects of quantitative methods in information systems research. These include **statistical conclusion validity**, where we assess the appropriate use of statistics to infer whether the presumed independent and dependent variables covary as predicted, **manipulation validity** (in experiments), where we assess whether an experimental group (but not the control group) is faithfully manipulated, **external validity**, where we assess the extent to which the study findings can be generalised, **predictive validity**, where we assess the degree to which a measure successfully predicts a future outcome that is theoretically expected, or **ecological validity**, where we assess the degree to which a study setting approximates faithfully a real-life situation.

5.1.4 Measurement Development

Establishing measurement reliability and validity is a demanding and resource-intensive task. This is by no means an “optional” task or one where shortcuts can be allowed. The key message bears repeating: any inferences or conclusions drawn from unreliable or invalid measures are meaningless. Of course, this is not to say that in the history of information systems research, measurement validation has always and consistently received the attention it deserves. Many studies have pointed out the measurement validation flaws in published research (see, for

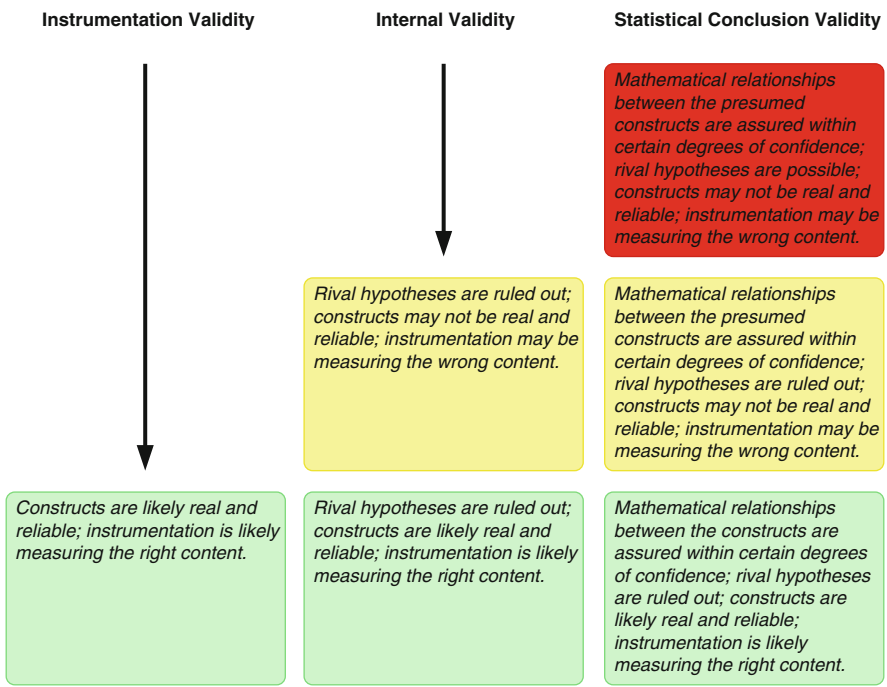


Fig. 5.4 Validation decision tree [166]

example [22]). The brighter side of this situation is that there is also continuous research being done on measurements to update our knowledge of challenges, approaches and solutions.

One of the best-known tools to guide researchers in this area is the validation decision tree by Detmar Straub, and which is shown in Fig. 5.4. It shows how to prioritise the assessment of measurement (here called instrumentation validity) as opposed to other validities such as internal validity and statistical conclusion validity.

In attempting to develop measurements that pass all tests of reliability and validity several guidelines exist. Information systems research to date has mostly relied on methodologies for measurement instrument development that build on the work by Gilbert Churchill Jr. in the field of marketing [36]. Yet, in the actual instantiation and implementation of his guidelines, many different attempts have been used, relying on a varied and disparate set of techniques. Table 5.1 reviews some of the measurement instrument development procedures reported in information systems research, and describes the extent to which these procedures involved users during measurement instrument development. In Table 5.1, I review some of these and note the different forms of theoretical and empirical assessments used in these procedures.

Table 5.1 shows how a wide variety of procedures, as well as a mixed and sometimes limited extent of empirical assessments, have been used in the development

Table 5.1 Measurement instrument procedures in use [137]

Reference	Assessments used	Required sample for empirical assessments
[9]	Literature review Pre-test interviews Ranking test Questionnaire	Organisational managers
[45]	Literature review Pre-test interviews Index card sorting test Field survey	Students End users
[117]	Literature review Own category test Index card sorting test Pilot test Field survey	Academic staff Faculty users End users
[149]	Literature review Pilot test Field survey	IS Executives
[73]	Pre-test Interviews Field survey	End users
[162]	Literature review Questionnaire Sorting test Ranking test Field survey	Expert panel End users
[183]	Literature review Field survey	End users
Procedure in this book	Literature review Own category test Ranking exercise Index card sorting test Pre-test Pilot test Field survey	Expert panel Practitioner panel Students End users

of measurement instruments. In the following, I will advance a procedural model that consolidates some of these approaches, and extends these in terms of the incorporation of empirical assessments at various stages of the procedure. This procedural model is proposed for use by researchers who wish to create new measurement instruments for conceptually defined theory constructs. It is not concerned with developing theory (which is discussed in Chap. 4 above); rather it applies to the stage of the research where such theory exists and is sought to be empirically tested. In other words, the procedural model described below requires the existence of a well-defined theoretical domain and the existence of well-specified theoretical constructs.

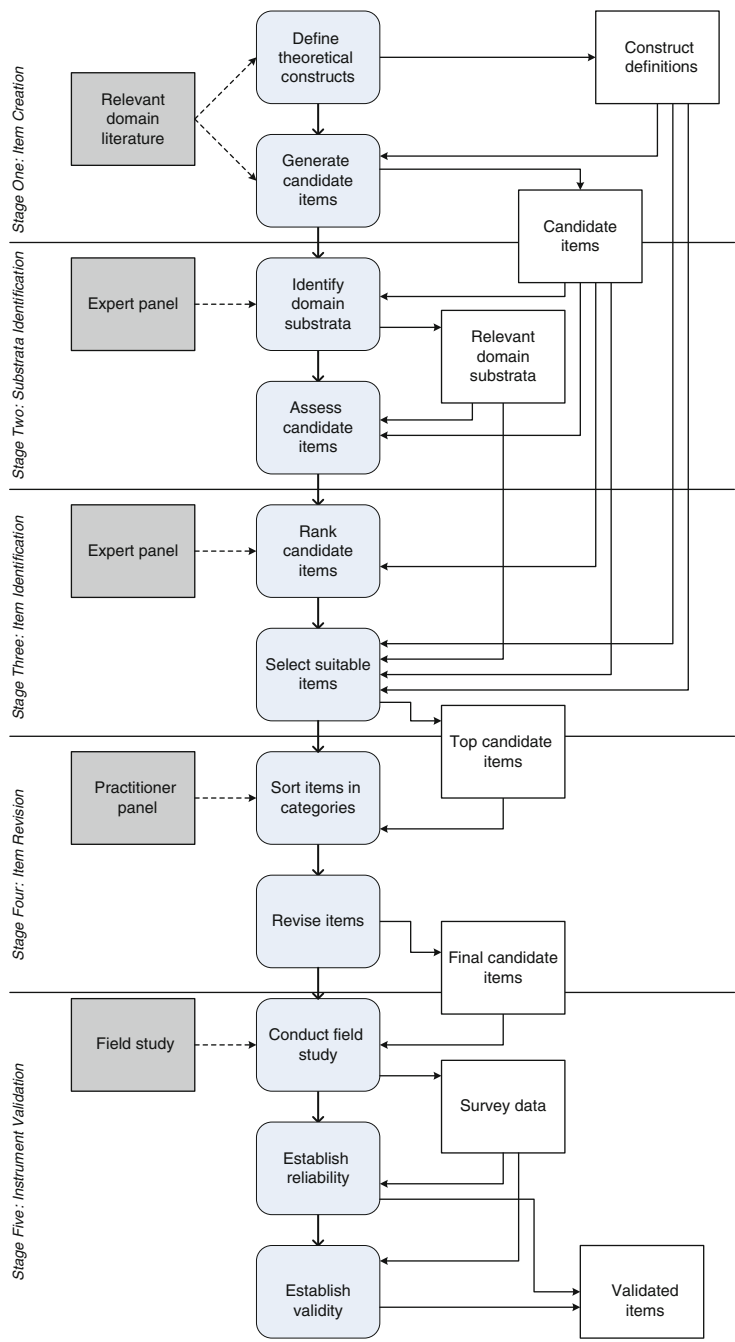


Fig. 5.5 Measurement instrument development procedure [137]

I have found this procedural model helpful specifically in demonstrating content validity of the measurement instrument, because it prescribes various techniques and tests to establish content validity within the design of the measurement instrument from the outset.

Figure 5.5 shows the procedural model. This model describes in five stages the different tasks to be performed (grey rounded boxes), related inputs and outputs (white rectangles), and the relevant literature or the source of empirical data where applicable (dark grey rectangles).

The first stage of the procedural model is *item creation*, which is concerned with specifying the theoretical constructs for which measurement items are to be developed and to derive pools of candidate items for each construct. This task can be carried out through an analysis of the relevant literature. The next stage is *substrata identification*, the purpose of which is to sort the candidate items into meaningful separate domain sub categories to display construct, convergent and discriminant validity. This task can be carried out with the help of a panel study with experts in the selected domain of study, which provides input to the sorting task. The third stage is *item identification*, the purpose of which is to identify from the pool of candidate items a revised set of items that show good potential for high content validity. This task can also be carried out by means of an expert panel study, which provides input to the ranking task. The fourth stage is *item revision*, the purpose of which is to re-specify and further improve the set of candidate items as well as to get an initial indication of reliability and validity. This task can be carried out through a panel study with a sample of the final study population, to obtain input from a sample representative of the target research population. The last stage is *instrument validation*, which is concerned with obtaining statistical evidence for reliability and validity of the developed measurement items. This task can be fulfilled by performing any field-study research method (such as a study or experiment) that provides a sufficiently large number of responses from the target population of the respective study.

More details about the procedure including illustrations and applications are available in the literature [135, 137, 138]. As I noted above, there are also constantly updated procedural models being published. A recent example (December 2011) are the guidelines by Scott MacKenzie and his co-authors [109].

5.1.5 Quantitative Method Procedures

Research using quantitative methods often follows a linear process model:

1. Generation of models, theories and hypotheses
2. The development of instruments and methods for measurement
3. Collection of empirical data, sometimes through experimental control and manipulation of variables
4. Statistical modelling and/or other analysis of data
5. Evaluation of results

Quantitative methods typically presume the existence of a theoretical model with well-specified hypotheses. This means that quantitative methods are typically confirmatory in nature as opposed to exploratory. This is not to say that exploratory research is impossible to conduct via quantitative methods but rather that it is more often used to confirm a previously developed and well-specified theory.

The procedure model in this instance covers many types of techniques that can be applied in quantitative methods, such as surveys, laboratory experiments, field experiments,¹ or other types of field studies such as cases, focus groups, archival analysis, and so forth. Of these, surveys and laboratory experiments have traditionally been the most dominant techniques in use in information systems research, so we will focus on these two techniques here.

5.1.6 Survey Research

Surveys are non-experimental research methods that do not involve controlling or manipulating independent variables (that is, they do not contain a “treatment”). A survey is a means of gathering information about the characteristics, actions, perceptions, attitudes, or opinions of a large group of units of observations (such as individuals, groups or organisations), referred to as a population. Surveys thus involve collecting data about a large number of units of observation from a random sample of subjects in field settings through techniques such as mail questionnaires, online questionnaires, telephone interviews, or less frequently through structured interviews or published statistics. The resulting data is analysed, typically by using statistical techniques or other quantitative approaches.

The survey method is preferable in research contexts when the central questions of interest about the phenomena are “what is happening” and “how and why” “is it happening?” and when control of the independent and dependent variables is not possible or not desirable.

Survey research in general can be used for at least three purposes, these being exploration, description, or explanation. The purpose of survey research in *exploration* is to become more familiar with a phenomenon or topic of interest. It focuses on eliciting important constructs and the best ways for measuring these. It may also be used to uncover and present new opportunities and dimensions about a population of interest. The purpose of survey research in *description* is to find out about the situations, events, attitudes, opinions, processes, or behaviours that are occurring in a population. Thereby, descriptive surveys ascertain facts and do not test theory. In contrast, the purpose of survey research in *explanation* is to test theory and hypothetical causal relations between theoretical constructs. Explanatory surveys

¹ The difference between a laboratory experiment and a field experiment is that the former is conducted in an artificial setting (the “lab”) where control over the experiment conditions is easier to maintain and the field experiment is conducted in the actual real-world setting (the “field”).

Table 5.2 Advantages and disadvantages of the survey method (Based on [123])

Advantages	Disadvantages
Surveys are easy to administer and simple to score and code.	Surveys are just a snapshot of behaviour at one place and time.
Surveys determine the values and relations of variables and constructs.	One must be careful about assuming they are valid in different contexts. In particular, different cultures may produce different results.
Responses can be generalised to other members of the population studied and often to other similar populations.	Surveys do not provide as rich or ‘thick’ description of a situation as a case study.
Surveys can be reused easily, and provide an objective way of comparing responses over different groups, times, and places.	Surveys do not provide as strong evidence for causality between surveyed constructs as a well designed experiment.
Surveys can be used to predict behaviour.	Surveys are often susceptible to low response rates, which can diminish the generalisability of the results.
Specific theoretical propositions can be tested in an objective fashion.	
Surveys can help confirm and quantify the findings of qualitative research.	

ask about the relations between variables often on the basis of theoretically grounded expectations about how and why the variables ought to be related. Typically, the theory behind survey research involves some elements of cause and effect in that not only assumptions are made about relationships between variables but also about the directionality of these relationships. Sometimes, explanatory survey research extends not only to uncovering the existence of a causal link but also to asking why the link exists.

Similar to any other research method, there are certain scenarios in which surveys are more or less appropriate. Table 5.2 lists common strengths and weaknesses.

A number of suggestions are available to maintain rigor and quality of survey research. Essentially, good survey research should follow the following guidelines:

- *Report the approach used to randomise or select samples.* A researcher should include all relevant details of the sampling procedure (for instance, whether the survey used simple random sampling, clustering, stratification or other forms of selecting sub-groups of the population). By describing the randomisation or selection procedure, a researcher permits readers to decide for themselves about the validity and representativeness of the sample frame and ultimately the survey results.
- *Report a profile of the sample frame.* For a sample to adequately represent the population, the sampling frame should exhibit characteristics of all members of the target population. Reporting the boundaries of the sample frame is the minimum that a researcher should provide to indicate how the sample frame is adequately corresponding to the target population.

- *Report the characteristics of respondents.* Describing the characteristics of the respondents permits readers to appreciate whether particular characteristics are representative of the sample.
- *Append the whole or part of the questionnaire.* By reproducing the questionnaire, the researcher provides readers with the exact wording of questions.
- *Establish the validity and reliability of the survey instrument.* IS researchers must validate their research instruments. Confirmatory empirical findings will be strengthened when instrument validation precedes both internal and statistical conclusion validity.
- *Perform an instrument pre-test.* A pre-test is a tryout, and its purpose is to help produce a survey form that is more usable and reliable. Pre-testing helps refine the instrument and ensure executability of the survey.
- *Report on response rate.* Response rate and non-response error are amongst the most critical issues in survey research. Obtaining a large sample would be meaningless without consideration of the response rate, which should therefore be a minimal requirement.

The general procedure for survey research is shown in Fig. 5.6. The procedure distinguishes six stages of survey research: model development, scale development, instrument development, instrument testing, survey administration, and data analysis.

Model development is the exercise of theorising anticipated effects between the phenomena under observation in a well-specified theoretical model. Explanatory survey research requires such a model. Without a model, we could not develop measurements because what we would be required to measure would be unknown.

Measurement development is concerned with developing measurement arrays (one or more measurement items, for example) that can be used as an operationalisation of the theoretical constructs to be measured empirically. The objective of this stage is to devise these measurement arrays in such a way that they are reliable and valid.

Instrument development is the step of converting measurement inventories into complete survey instruments. An important design consideration in the conduct of survey research is the form of the data collection instrument. Traditionally, mail surveys have been the predominant means of data collection. Recently, however, web-based surveys have been proliferating at a rapid rate due to the advent of the Internet. Especially the potential savings in time, costs, and effort have uplifted electronic surveys. It would appear that, most often, the advantages of web-based surveys outweigh their potential disadvantages (such as dangers of fraud, viruses, issues of response rate calculation, potential response bias). A web-based instrument can assist in reducing distribution and processing complexity and costs (for distributing the survey via mail and arranging prepaid return envelopes for different countries, for example). It simplifies data entry, transfer, cleansing, and codification phases as all data is automatically extracted in computer-readable format and automatically stored in a database. Additionally, a web-based instrument reduces item completion mistakes by utilising input validation checks available through

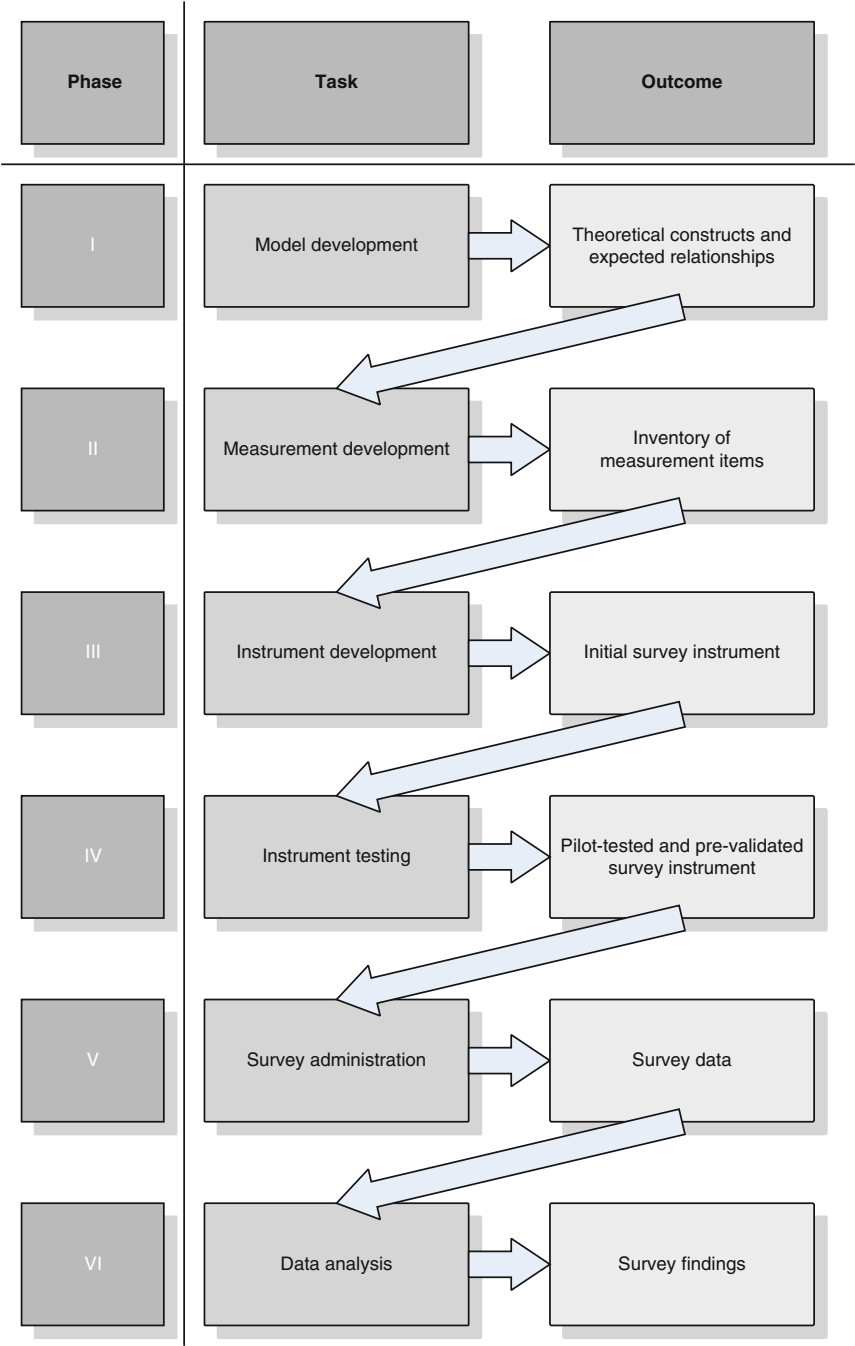


Fig. 5.6 Survey research procedure

web scripts. Furthermore, notwithstanding the initial efforts of designing and implementing a web-based survey, the marginal costs are low in comparison to traditional techniques. Response times are minutes rather than days, mailing and transfer costs are marginal, and data entry is done automatically.

There are a number of further design elements that need to be taken into consideration when designing survey instruments [48, 64, 143]. These guidelines relate to implementation and general layout, instructions and help, and wording.

Instrument testing is the step of trying out the survey prior to administering it to the target population of interest, to identify and rectify problems up-front. For instance, these tests can:

1. Provide further details of the respondents that were not identified before,
2. Establish if the questionnaire is easy to follow,
3. Establish if there is sufficient space for all responses,
4. Establish how much time it takes on average to fill the survey, and
5. Contribute to identifying ways to increase response rates.

Three objectives are typically pursued when doing pre-tests of survey instruments:

- Evaluate the authenticity of the questions,
- Evaluate the survey interface and layout, and
- Establish validity and reliability of the survey instrument.

Pre-tests are inherently different from *pilot tests*. Pre-tests are carried out to test some or all aspects of the instrument for any type of difficulty that may not be visible from reading through the survey. During pre-tests, survey respondents should be observed during questionnaire completion and then engaged in follow-up interviews. The objective of this stage is to establish face validity and to improve content validity of the survey instrument.

Pilot tests include the administration of an entire survey to a small, convenient sample group that is close in characteristics to the sample targeted with the final survey. The objective of a pilot test is to test the revisions made after the pre-test of the instrument and most notably to test the survey instrument as well as the developed measurement items with a larger sample that more closely represents the target population. At this stage, initial indications of validity and reliability of measurements can be obtained to increase trust in the eventual final results.

Survey administration concerns the actual roll-out of the survey to the target population. A number of challenges exist. The most prevalent challenge relates to establishing a sufficient representation of the survey sample; in other words, the establishment of a sufficient response rate. An ideal survey research procedure would involve selecting a random sample from a relevant population. In a typical study, however, random sampling is often sacrificed, implicitly or explicitly, for the sake of study feasibility, costs, and resource commitment.

Data analysis finally concerns the examination of the collected survey data through appropriate quantitative data analysis techniques. Data analysis can take the form of simple descriptive statistics or of more sophisticated statistical inferences. Statistical data analysis techniques include univariate analysis (such

as analysis of single-variable distributions), bivariate analysis, and more generally multivariate analysis. Univariate or bivariate analyses belong to the so-called class of *first generation data analysis* methods, which includes techniques such as regression models or analysis of covariance (ANCOVA). They are characterised by their shared limitation of only being able to analyse one layer of linkages between independent and dependent variables at a time.

Multivariate analysis refers broadly to all statistical methods that simultaneously analyse multiple measurements on each individual or object under investigation. As such, many multivariate techniques are extensions of univariate and bivariate analysis.

Structural equation modelling (SEM) techniques such as LISREL [90] or Partial Least Squares (PLS) [78] belong to the class of so-called *second generation data analysis* methods. These methods are widely acknowledged to be state-of-the-art for high quality statistical analysis in survey research. SEM has been widely used in social science research for the causal modelling of complex, multivariate data sets in which the researcher gathers multiple measures of proposed constructs. In recent years SEM has become increasingly popular amongst researchers for purposes such as instrument validation and the testing of linkages between constructs.

In general terms, SEM is a statistical method for testing and estimating causal relationships using a combination of statistical data and qualitative causal assumptions. It encourages confirmatory rather than exploratory analysis. SEM usually requires one or more hypotheses, which it represents as a model, operationalises by means of measurement items, and then tests statistically. The causal assumptions embedded in the model often have falsifiable implications that can be tested against the data. While the initial hypotheses often require model adjustment in light of empirical evidence, SEM is rarely used for exploration only.

SEM grows out of, and serves purposes similar to, multiple regression. However, in contrast to first generation data analysis methods, SEM permits complicated variable relationships to be expressed through hierarchical or non-hierarchical, recursive or non-recursive structural equations. Thereby, SEM can not only be used to assess the *structural model* – the assumed causation amongst a set of multiple dependent and independent constructs – but within the same analysis the *measurement model* – the loadings of observed items or measurements on their expected latent variables or constructs. In other words, SEM allows the researcher to examine reliability and validity of the measurements together with the examination of the hypotheses contained in the proposed theoretical model.

5.1.7 *Experimental Research*

Observational research methods, such as surveys or case studies, rely on data sampling – the process of selecting units from a population of interest and observe or measure variables of interest without attempting to influence the responses. Such data, however, is often not suitable for gauging cause and effect relationships due to potential confounding factors that may exist beyond the data that is collected.

Experimental studies are those that are specifically intended to examine cause and effect relationships. This is done by deliberately imposing some *treatment* to one group of respondents (the treatment group) but not to another group (the control group) while also maintaining control over other potential confounding factors in order to observe responses. A treatment is something that an experimenter administers to the subjects (also known as experimental units) so that the experimenter can observe a response. The treatment is thus how the independent variable is operationalised or realised into data. A typical way this is done is to divide the subjects into groups randomly where each group is being “treated” differently so that the differences in these treatments result in differences in responses across these groups as we hypothesise.

The different treatments constitute the different levels or values of our independent variable.

Experiments are called true experimental designs if participants are randomly assigned to treatment or control groups. In quasi-experimental designs, random assignment is not followed.

The primary strength of experimental research over other methods is the emphasis on internal validity due to the availability of means to isolate, control and examine specific variables (the cause) and the consequence they provoke on other variables (the effect). Its primary disadvantage is often a lack of ecological validity because the desire to isolate and control variables typically comes at the expense of realism of the setting. Also real world domains are often much more complex than the reduced set of variables that are being examined in an experiment.

Experimental research is often considered the gold standard in research as it is one of the most rigorous forms of collecting and analysing data, but it is also one of the most difficult research methods. This is because experimental research relies on very strong theory to guide construct definition, hypothesis specification, treatment design, and analysis. Any design error in experiments renders all results invalid. Moreover, experiments without strong theory tend to be ad hoc, possibly illogical, and meaningless because you essentially find some mathematical connections between measures without being able to offer a justificatory mechanism for the connection (“you can’t tell me why you got these results”). The most pertinent danger in experiments is a flaw in the design to rule out rivaling hypotheses (potential alternative theories that contradict the suggested theory). Probably the second biggest problem is the inappropriate design of treatment and task.

We should discuss the most important basic concepts in experiments:

The **treatment** describes an experimental stimulus that is provided to some participants but not to others. The treatment is considered successful if the responses from the treatment group upon reception of the treatment are different as expected from the responses from the control group that did not receive the treatment. **Treatment manipulation** concerns the control for the cause in cause-effect relationships by identifying the appropriate type and number of stimulus levels (provision versus non-provision, low/medium/high levels of stimulus and so forth). Experimental designs typically involve a phase prior to treatment manipulation, called *pre-test measures*, and most often a phase after treatment manipulation, called *post-test measures*.

Controls are the mechanisms employed to ensure that the different responses observed are indeed due to the treatments and not other confounding factors. Thus, in experiments, sources of potential bias and confounding influences that could prevent the actual effect of the treatment from being observed need to be identified and controlled for. For example, in medicine, since many patients are confident that a treatment will positively affect them, they react to a control treatment that actually has no physical affect at all such as a sugar pill. This phenomenon is called the placebo effect. For this reason, it is common to include control, or placebo, groups in medical experiments to evaluate the difference between the placebo effect (no treatment or zero-treatment) and the actual effect of the treatment.

Randomisation is the process of selecting a sample from a population in such a way that any personal characteristics or predispositions do not interfere with the treatment or any response to the treatment. One key bias that can creep into experiments is the effect of subjects' differences in terms of experience, risk adversity, and knowledge among others. A critical aspect of experimental design is therefore ensuring that either such bias is accounted for during data analysis (by administering appropriate controls) or designing the experiment so that such biases are evenly distributed across the groups (and thus cancel each other out). One way to achieve this is by matched allocation of subjects to different groups thereby ensuring that there are equal numbers of risk-takers and risk-averse individuals in both groups for example. However, this is expensive and not possible most of the time as there could be a vast multitude of personal characteristics that could result in bias, and it is impossible to match subjects or all these characteristics. Random assignment of participants to experimental groups is thus most often employed to ensure that the experimental groups are similar in their characteristics.

Experimental designs can vary. Figures 5.7 and 5.8 describes the most popular design types in basic design notation, as either true experimental designs or quasi-experimental designs.

The simplest form of a true experimental design is a *two-group design* involving one treatment group and one control group, and possibly pre- and/or post-tests. Typically, in such cases the effects of a treatment can be analysed using simple analysis of variance tests between the treatment and the control groups, where you typically expect a difference in the response levels in the treatment group and you examine whether this difference is statistically significant.

More sophisticated true experimental designs include *covariance designs* where measures of dependent variables can be influenced by extraneous variables called covariates. Covariates are variables not of central interest to the theory but recognised as potential source of bias. It is therefore important to control for their effect on the dependent variable in order to identify the true effect of a treatment.

Other studies require *factorial designs* that involve manipulation of two independent variables (treatments). Each independent variable then denotes a factor and each manipulation describes a factor level. For instance, you can imagine a 2×2 factorial design where one independent variable is a drug dose with the levels high and low and the other independent variable is a medical treatment that is either

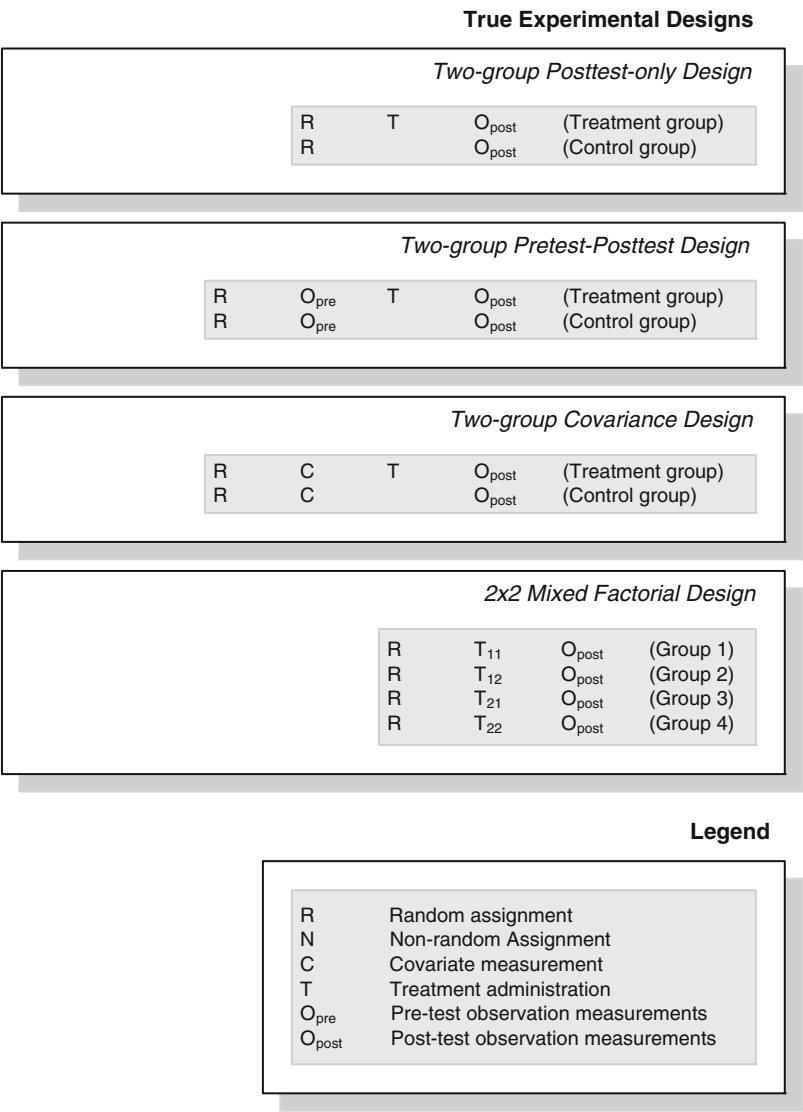


Fig. 5.7 True experimental design types

absent or present. Correspondingly, in such a design we would have $2 \times 2 = 4$ treatment groups (high \times absent, high \times present, low \times absent and low \times present). Factorial designs have higher sample size requirements because we need to ensure that we receive sufficient responses for each treatment group (“cell”) to allow for meaningful data analysis (to achieve stable results with significance levels). Figure 5.7 shows frequently employed true experimental designs.

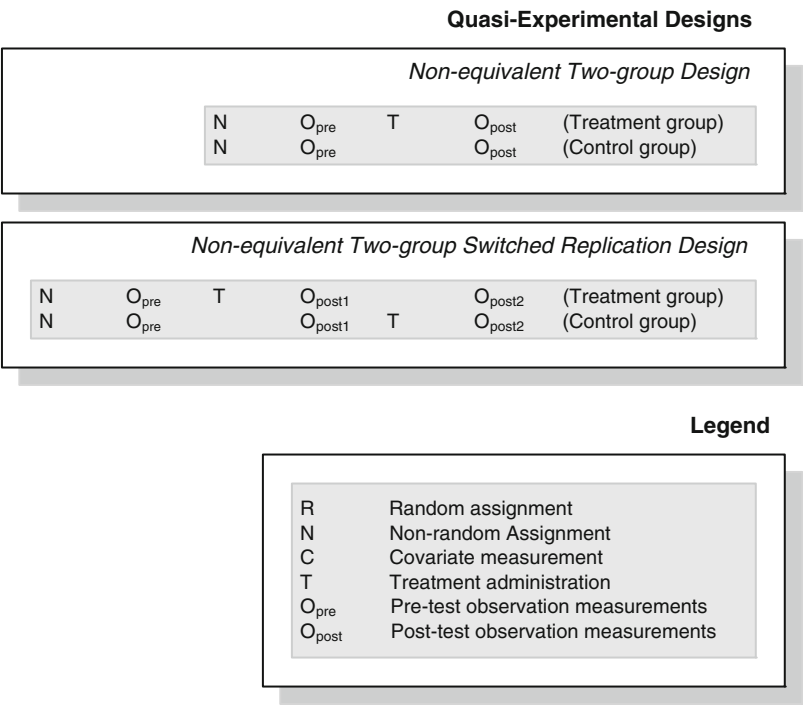


Fig. 5.8 Quasi-experimental design types

Quasi-experimental designs (see Fig. 5.8) are similar to true experimental designs, with the difference being that they lack random assignment of subjects to groups. In effect, one group (say, the treatment group) may differ from another group in key characteristics; for example, a post-graduate class possesses higher levels of domain knowledge than an under-graduate class. Quasi-experimental designs often suffer from increased selection bias, in turn diminishing internal validity. Still, sometimes a research design demands the deliberate assignment to an experimental group (for instance to explicitly test the effect of an intervention on under-performing students versus well-performing students). The most common forms are *non-equivalent groups design* – the alternative to a two-group pretest-posttest design, and *non-equivalent switched replication design*, in which an essential experimental treatment is “replicated” by switching the treatment and control group in two subsequent iterations of the experiment.

Data analysis concerns the examination of quantitative data in a number of ways. *Descriptive analysis* refers to describing, aggregating, and presenting the constructs of interests or the associations between the constructs to describe, for example, the population from where the data originated, the range of response levels obtained, and so forth. *Inferential analysis* refers to the statistical testing of hypotheses – the suspected cause and effect relationships – to ascertain whether the theory receives support from the data within certain degrees of confidence, typically

described through significance levels. Most of these analyses are nowadays conducted through statistical software packages such as SPSS, SAS, or mathematical programming environments such as R or Mathematica. For any quantitative researcher, a good knowledge of these tools is essential.

Data analysis relies on data that is coded appropriately. *Data coding* refers to converting raw data into appropriate numerical formats. For instance, if we ask people about their perceived satisfaction with a new computer program, we might later code the responses received into different levels of satisfaction (low, medium, high), which can also be expressed numerically as 1,2,3. Coding is an essential exercise to prepare data for analysis and requires much thought as to the type of conversion (into ratio scales, numerical scales, binary scales, ordinal scales or others). The type of conversion determines the type of analysis technique that can be applied to the coded data. Data coding might sometimes involve interpretation, to rate the innovativeness of improvement ideas written in plain text on a scale from low to high, for example. In such cases, data coding is subject to bias concerning validity and reliability of the coding. Often, therefore, such coding requires multiple coders so that the reliability can be examined by comparing how much congruence in coding was achieved across the coding team.

With coded data, multiple analyses can be conducted. *Univariate analyses* concern the examination of one variable by itself, to identify properties such as frequency, distribution, dispersion, or central tendency. Classic statistics involve mean, median, variance, or standard deviation.

Bivariate analyses concern the relationships between two variables. For example, we may examine the correlation between two numerical variables to identify the changes in one variable when the other variable levels increase or decrease. An example would be the correlation between salary increases and job satisfaction. A positive correlation would indicate that job satisfaction increases when pay levels go up. It is important to note here that correlation does *not* imply causation. A correlation between two variables merely confirms that the changes in variable levels behave in particular way upon changing another; but it cannot make a statement about which factor causes the change in variables (it is not unidirectional).

Inferential analyses involve estimation of general linear models (GLM) or two-group comparisons to examine hypotheses statistically. A common technique relies on the Student *t*-test that examines whether the mean of a dependent variable differs significantly between two groups; for example, whether one group has a statistically significant difference in mean from the other group. This is essentially done by computing the probabilistic estimates of population means within a certain confidence interval (typically 95% or higher) for both groups. To compute this, we compare the sample means and standard deviations of the two groups to identify the *t*-statistic. For this statistic, we can then look up the confidence interval in a so-called *t*-test table that describes the probabilities (p-values) associated with the computed *t*-statistic (an example can be found at the following URL: http://www.stattools.net/tTest_Tab.php). Most current statistical packages compute the relevant probability value straightaway. Other inferential analyses involve data analysis techniques that allow for estimation of main and interaction effects of covariates.

Table 5.3 Criteria for choosing inferential analysis techniques [40]

Number of independent variables	Number of dependent variables	Number of control variables	Type of variables	Distribution of variable scores	Statistical test
1	1	0	Categorical/ Continuous	Normal	<i>t</i> -Test
1 or more	1	0	Categorical/ Continuous	Normal	Analysis of variance
1 or more	1	1	Categorical/ Continuous	Normal	Analysis of covariance
1	1	0	Categorical/ Continuous	Non-normal	Mann–Whitney <i>U</i> test
1	1	0	Categorical/ Categorical	Non-normal	Chi-square
1	1	0	Continuous/ Continuous	Normal	Person product moment correlation
2 or more	1	0	Continuous/ Continuous	Normal	Multiple regression
1	1 or more	0	Categorical/ Categorical	Non-normal	Spearman rank-order correlation

The choice of the correct inferential analysis technique is dependent on the chosen experimental design, the number of independent and dependent (and control) variables, the data coding and the distribution of the data received. This is because statistical tests all come with a set of assumptions and preconditions about the data to which they can be applied. We do not want to diverge into this important issue here, but still, a simplistic overview is provided in Table 5.3.

Perils of experimental research definitely include the reliance on very strong and precisely formulated theory. Without such theory, experimental results are often in vain, ad hoc, spurious, and meaningless. Second, experimental research relies on measurement instruments with excellent validity and reliability. Unfortunately, due to the multitude of potential treatments that one may envisage, it is often difficult to demonstrate conclusively how a particular treatment manipulation was valid. Third, many experiments employ inappropriate or irrelevant dependent variables, inappropriate or unavailable experimental controls and/or non-equivalent stimuli between treatment groups. In fact, the greatest danger is to forget appropriate control measures to be able to rule out alternative hypotheses (consider the example we used in the theorising section in Chap. 4). The design of an appropriate treatment especially is a key challenge for researchers since the treatment is the key element in the experimental method and deserves utmost attention and careful development, both during pre- and pilot-testing. In many cases, it is demanded that researchers maintain a discussion of the *threats to validity* that pertain to their experiment. Very good examples of such threats are provided by John Creswell [40] and William Shadish et al. [150].

5.2 Qualitative Methods

Qualitative methods are designed to assist researchers in understanding phenomena *in context*. Quantitative research, with its focus on measurement, has the tendency to isolate specific aspects of phenomena by measuring these and only these through dedicated instruments. This specific focus comes with a lack of consideration of the wider setting in which phenomena occur. Qualitative methods, therefore, have been developed in the social sciences to enable researchers to study social and cultural phenomena. Examples include case study research, action research, and grounded theory, amongst others.

The prime example of qualitative methods is the study of people and the social and cultural contexts in which they live, operate and behave. Because why people make decisions and act they way they do is often highly contextual, qualitative research methods are designed to explore this very context, in order to offer explanations for why the phenomena occurred the way they do.

To understand qualitative research it is vastly helpful to compare it to quantitative methods. The simplest distinction between the two is that quantitative methods focus on *numbers*, and qualitative methods focus on *text*, most importantly text that captures records of what people have said, done, believed or experienced about a particular phenomenon, topic, or event.

Qualitative methods are strategies of empirical inquiry that investigate phenomena within a real-life context. They are helpful especially when the boundaries between phenomena and context are not apparent, or when you want to study a particular phenomenon in depth. They are well suited for exploratory research where a phenomenon is not yet fully understood, not well researched, or still emerging. They are also ideal for studying social, cultural, or political aspects of a phenomenon (for example, related to information technology in use).

Compared to quantitative research, qualitative methods employ different philosophical assumptions, techniques for data collection, and techniques for analysis and interpretation. In the following, I have consolidated a number of characteristics of qualitative methods:

- **Natural setting:** Qualitative research is performed in the field, studying a phenomenon in the context in which it occurs.
- **Researchers as a key instrument:** Qualitative researchers collect data and information themselves (rather than through an ‘objective’ instrument), often through face-to-face interactions, observing behaviours, studying documents, or interviewing participants.
- **Multiple sources of data:** Qualitative researchers typically gather a variety of data of different sorts, from interviews to documents to observations and so forth.
- **Inductive analysis:** Qualitative methods emphasise bottom-up analysis of data and the build-up of patterns, themes, and concepts into increasingly abstract units from the data.

- **Focus on emergent meaning:** Qualitative methods focus on uncovering or learning the meaning of behaviours, opinions, or views that participants have about a phenomenon.
- **Evolutionary design:** Qualitative methods, while following a general procedure, typically follow an evolutionary research process in which a research plan, a theory, data collection, or analysis can unfold and change over time as the research progresses.
- **Interpretive:** Qualitative methods often consist of interpretive research in which researchers develop interpretations of the data they collect and analyse, often in the form of suggested conceptualisations of theories about the phenomenon of study. Interpretations are inherently subjective, historically and contextually bound to the researcher developing and expressing the interpretation.
- **Holistic and contextual:** Qualitative methods are designed to assist researchers in developing a comprehensive, detailed picture of complex phenomena. This typically means shedding light onto a phenomenon from multiple perspectives, developing a larger picture and paying attention to various aspects of the phenomenon without isolating or reducing to one or few dedicated variables.

Qualitative methods are often also called **interpretive research methods** because of the typical focus on the development of interpretations of data. This approach is grounded in a social-constructionist view of reality that stresses an ontology prescribing that phenomena are bound within and shaped by their social and historical context and cannot be isolated from that context. Epistemologically, this research builds on the view that the best way to study social reality is through subjective interpretations within the socio-historic context. Remember, quantitative methods build on the view that reality is relatively independent of the socio-historic context and can thus be isolated and studied objectively.

Because of the emphasis on non-numerical data (“text”), qualitative research is not as amenable to quantitative analyses such as statistics and instead primarily relies on other analysis techniques such as ethnography, content analysis, or discourse analysis. Another key difference between qualitative and quantitative methods is the purpose of sampling. Quantitative methods rely on *random sampling* where cases are selected randomly from a wider population. Qualitative methods by contrast rely on *purposive sampling*, where cases are selected because they possess certain properties of interest.

Qualitative methods have distinct advantages in exploratory research because they can possibly uncover complex, multifaceted, or even hidden phenomena and can lead to a more comprehensive, multi-perspective view. They are used for theory building purposes because of their exploratory nature and because they can faithfully be applied to domains or phenomena where little knowledge or theory and hence constructs and measurements exist. As with any other research method, qualitative research also has weaknesses. A major disadvantage of qualitative research methods is the difficulty in generalising to a larger population. Moreover, qualitative methods require the appropriate skills and experience because the

interpretive nature of such research makes it strongly susceptible to subjective interpretation bias. Qualitative methods often also have issues with reliability because study processes are often so contextualised to one case that they cannot faithfully be replicated in other cases.

5.2.1 *Data Collection Techniques*

Qualitative methods employ a variety of techniques to gather data. The most prominent form is certainly **interviewing**, either face-to-face, one-to-many (in focus groups) or via telephone/conferencing. Interviews can be descriptive, exploratory, or explanatory. *Descriptive interviews* are used to provide a rich description of a phenomenon as perceived by individuals. This way, subjective understanding can be generated. Focus is typically given to the development and exploitation of multiple individual perspectives regarding the phenomenon to arrive at a comprehensive, multi-faceted description or conceptualisation. This conceptualisation can then generate interpretive understanding. *Exploratory interviews* are typically used to define questions, propose new theory constructs, and/or build new theories. Usually, propositions or hypotheses are generated based on the observed relationships or on the interpretive understanding generated. *Explanatory interviews*, on the other hand, are performed in causal studies, for instance to determine whether presumed and/or postulated relationships and causal links between concepts or constructs do in fact occur and are perceived as such in real-life settings.

Interviews boast the advantage of being targeted (focus is directly on the selected topic) and insightful (they can provide causal inferences as perceived by interviewees). There are, however, also weaknesses and challenges associated with interviewing. Among these, the challenges of reflexivity (the interviewee responds with what the interviewer would like to hear), inaccuracy (poor recall of answers), artificiality (the researcher is typically a stranger to the interviewee), and response and system bias due to poorly constructed questions stand out.

Interviewing typically makes use of more or less formally structured protocols depending on the purpose of the interview. Most commonly, interviews are of a semi-structured nature. In these, respondents are being asked about the topics of the study following a pre-defined interview structure (a protocol). The interview progresses flexibly as new questions can be brought up during the interview as a result of what the interviewee says. Hence, the interview follows a conversational form that allows for follow-up questions and bidirectional discussions about the topic (or other topics and links that emerge during the interview). Semi-structured interviews usually start with questions that are more general, or with topics. These questions are typically formulated ahead of the interview. Yet, the possible relationships between the questions, potentially related topics and issues become

the basis for more specific questions (which are typically not pre-formulated). This approach allows both the interviewer and the person being interviewed the flexibility to probe for details or discuss issues if necessary or beneficial. Semi-structured interviewing is thus guided only in the sense that some form of interview protocol provides a framework for the interview. To that end, semi-structured interviews exhibit a number of benefits over other interviewing approaches:

- They are less intrusive to those being interviewed because semi-structured interviews encourage two-way communication. For instance, those being interviewed can ask questions of the interviewer.
- They can be used to confirm what is already known whilst at the same time providing the opportunity for learning. Often the information obtained from semi-structured interviews will provide not just answers but also the reasons for the answers.
- When individuals are interviewed personally and in a conversational rather than structured manner they may more easily discuss sensitive issues.

A second key technique is **observation**. *Direct observation* involves the researcher as a passive and neutral bystander that is not involved in the phenomenon of interest (“looking over the shoulder”), whilst *participant observation* includes the researcher as an active participant (“influencing the phenomenon”). In some studies, some events or actions are being directly observed while others are also being influenced. One of the key challenges in observation stems from the fact that we typically study unfamiliar contexts, requiring us to go through a period of enculturation in which we learn to become accustomed to the context that we are studying.

A third key technique is **documentation** where all sorts of internal and external documents relating to the unit of observation are gathered and used as a data source. These documents can be structured (spread sheets) semi-structured (emails, reports, policy documents, websites) or unstructured (music, video, other media). The documentation may also be personal, private, or public, which may impact access, use, or reporting of the data.

These and other data collection techniques (for example, the collection of media files or videos) also relate to a fundamental principle of qualitative research, that of **triangulation** of data. Triangulation literally means doing more than just one thing. Data triangulation refers to perusing, and relating, multiple sources of evidence about a particular phenomenon or topic. Through triangulation of data, researchers can gain a more nuanced picture of the situation, and increase reliability and validity of their findings. For example, researchers might triangulate interview data with data published in an organisational report or with observational data. An excellent example of data triangulation is described by Lynne Markus’ study of email use by managers [112] where she shows aptly how survey data corroborated her findings from interviews and document analysis based on the content of email messages. As her and other examples show, the use of triangulation assists researchers in increasing the robustness of results; findings can be strengthened through the cross-validation achieved when different kinds and sources of data

converge and are found to be congruent (or even when explanation is developed to account for divergence).

5.2.2 *Data Analysis Techniques*

In quantitative research, data collection and analysis are typically separate, sequential stages. This distinction is often not meaningful or productive in qualitative research, where data analysis and collection can be highly interwoven or even dependent on another. Think of analysing a set of interviews and developing a theoretical model from the interviews. You might realise at that stage that you need to follow-up with the interviews or other sources of data to further explore one emerging concept in the theory or to explore its associations with other constructs and so forth. The subsequent analysis of the then newly collected data might in turn further modify the emerging conceptualisation and so on and so forth.

One of the key attributes of the data analysis stage in qualitative research is the sheer amount of data that needs to be analysed, typically without a clear understanding of which parts are relevant or not to the final outcome, or why.

There are many techniques for analysing qualitative data. Similar to the chapter on quantitative methods, I will only provide some brief introductions to some of the more popular techniques.

- **Coding:** Coding is probably the most commonly employed, popular, and vastly useful techniques for reducing qualitative data to meaningful information. Coding refers to assigning tags or labels as units of meaning to pieces or chunks of data collected – words, phrases, paragraphs, or entire documents. Through coding, we can categorise or organise data. Often, coding is employed to organise data around concepts, key ideas or themes that we identify in the data. We see here that coding already is analysis – in fact, it is already interpretation of the data.

There are many approaches to coding, but the most common coding techniques were introduced by Anselm Strauss and Juliet Corbin [168] as open, axial, and selective. *Open coding* is a process aimed at uncovering and naming concepts from within data. Concepts may be grouped to higher-level categories to reduce the number of uncovered concepts on a higher level of conceptual abstraction. *Axial coding* involves organising categories and/or concepts into causal relationships, for example, to distinguish conditions from actions and interactions, and consequences. Third, *selective coding* might then be used to identify from one to a few central categories and then systematically and logically relate all other categories to these central categories. This is done by selectively sampling the available data to validate or refine categories or relationships.

- **Memoing:** Memoing is typically used right after/together with data collection and is effectively a subjective commentary or reflection about what was happening at the time or place of the data collection. Memos can be a summary of what was done, what was happening, or how something was achieved.

Memos can also be a commentary describing a ‘hunch’ or initial idea about possible interpretations about the data. Memos can be useful tool to guide the further research process, and can guide the identification of concepts and themes.

- **Critical incidents:** Critical incidents analysis involves identifying and examining series of ‘events’ or ‘states’ and the transition in between (for instance in chronological order). Such incidents can then be used to develop a storyline or to speculate about temporal or logical relationships between things, events, or actions that occurred.
- **Content analysis:** Content analysis is concerned with the semantic analysis of a body of text, to uncover the presence of dominant concepts. In general, content analysis approaches fall into two categories: conceptual and relational. In conceptual analysis, text material is examined for the presence, frequency, and centrality of concepts. Such concepts can represent words, phrases, or constructs that are more complex. Relational analysis, on the other hand, tabulates not only the frequency of concepts in the body of text, but also the co-occurrence of concepts, thereby examining how concepts (pre-defined or emergent) are related to each other within the documents. These relationships may be based on contextual proximity, cognitive mapping, or underlying emotion, to name just a few. Content analysis is performed through involvement of trained human analysts who tag a corpus of text with pre-defined or emerging codes, thereby introducing a source of bias to the coding process. More recently, however, computational approaches have become available that facilitate more efficient and objective exploration of the content of large bodies of text. Several such approaches have been developed, including hyperspace analogue to language [26], latent semantic analysis [100] and data mining tools such as Leximancer [155].
- **Discourse analysis:** Discourse analysis looks at the structure and unfolding of a communication (such as a conversation, argument, or debate). Discourse analysis is concerned with language-in-use, and can be used to examine the use, or evolution, of phrases, terms, metaphors, or allegories. Different sub-forms of discourse analysis include social semiotics, conversation analysis, or post-structuralist analysis.

These are but a few categories of the techniques that can be used in qualitative data analysis. Each set of techniques has a particular focus and a set of advantages and disadvantages. It is hard to declare ahead of time the appropriate use of any one approach, because it will depend on the philosophical stance of the researchers, the stage of the research process, the research method chosen, the type of data collected – and also on personal interest and the availability of good advice!

Some analyses can make use of qualitative data analysis software such as QSR Nvivo or Leximancer. These tools are designed to assist with analyses such as coding, cross-referencing, creation and management of typologies, and identify sources of evidence or even content analysis of unstructured corpora of text. Be reminded that such tools can be a valuable aid especially when analysing large volumes of qualitative data, but they remain only that – a tool. Qualitative research

builds on interpretive analysis – an inherently subjective activity that relies on the researcher him-/herself. Another potential limitation is the possibility of becoming bogged down in detailed data analysis within a tool and losing the “big picture”.

Before we get to different types of qualitative methods, let us quickly discuss *rigor in qualitative research*.

Many of the mechanisms and analysis to display **rigor** in quantitative research (such as demonstrating validity and reliability of measurements through dedicated statistics such as Cronbach’s Alpha, cross-loadings, goodness-of-fit measures, etc.) do not apply to non-numerical data. Also, qualitative methods rely on interpretive analysis and thus are a more subjective form of research. Still, this is not to say that rigor does not apply to qualitative methods, or that rigor in these methods cannot be achieved, but rather that the means of ensuring and demonstrating rigor are different. While construct validity or statistical conclusion validity do not apply, several other guidelines have been applied that cater more to the subjective and interpretive nature of qualitative methods (see for instance [106, 119–121, 151, 179, 180]). Several key principles are common across these guidelines:

- *Dependability (aka reliability)*: Dependability concerns whether individuals other than the researchers, upon considering the same observations or data, would reach the same or similar conclusions. If dependability can be demonstrated, it is similar to reliability in that it is demonstrated that measures provide consistently similar results.
- *Credibility (aka internal validity)*: Credibility of findings concerns whether the researcher has been able to provide sufficient substantiated evidence for the interpretations offered in qualitative data analysis (this relates to the internal validity of the research results). Credibility can be achieved through triangulation, maintaining a chain of evidence, and keeping clear notes regarding any decision made throughout the research process.
- *Confirmability (aka measurement validity)*: confirmability is a principle that postulates that qualitative research findings can be independently verified by outsiders in a position to confirm the findings (typically participants). This is usually done by reviewing interview summaries, conclusions, or other inferences drawn from qualitative data.
- *Transferability (aka external validity)*: Transferability concerns whether and how much the findings from a study can be generalised to other settings, domains, or cases. Very detailed and rich descriptions of the research context should be provided such that others can assess the extent to which the context characteristics match those of other fields of research.

We will now explore briefly the most common research methods under the qualitative paradigm, specifically *case study*, *action research*, and *grounded theory*. There are other methods such as ethnography, phenomenology or hermeneutics, but these remain few and, frankly, I am not in the best position to give any advice on them.

5.2.3 Case Study

Case study research is the most popular form of qualitative methods as well as the most well-established and published approach to research in information systems research and other social sciences, particularly business and management. The top journals provide ample examples of excellent cases and the types of studies conducted on these cases.

The case study method is a method involving intensive research on a phenomenon (a case) within its natural setting (one or more case sites) over a period of time. A case study is commonly used to investigate a contemporary phenomenon within its real-life context, especially when the boundaries between phenomenon and context are not clearly evident.

Case study methods are designed for distinctive situations where there are many more variables of interest than data points and as a result case studies rely on multiple sources of evidence, where data needs to converge in a triangulating fashion. This is why case studies typically utilise multiple means of data collection (documentation, observations, interviews and/or secondary data). Case studies are used both for confirmatory purposes (theory testing) and for exploratory purposes (theory building).

The main strengths of the case study method are that researchers:

- Can study information systems-related phenomena in their natural setting,
- Can learn about the state of the art and generate theory from practice,
- Are enabled to understand the nature and complexity of processes, events, actions and behaviours taking place, and
- Can gain valuable insights into new, emerging topics.

Case study methods also have limitations, with the most significant ones including problems of controlled deduction (a lack of adequate evidence to support evidences beyond any doubt), problems of replicability due to the highly contextualised nature of inquiry, and problems of control mechanisms to account for rival explanations or potentially confounding factors.

Aside from weaknesses inherent in case study research, in my experience the case study method is sometimes regrettably regarded as an “easy” research method – mostly because it does not require the student or scholar to learn about quantitative methods, statistics, or complex instrument development procedures. Nothing could be further from the truth. All famous and accomplished case study researchers stress the point that case studies are incredibly complex and difficult to do well. As my colleagues have aptly said ([85], p. 134):

Most people feel they can prepare a case study, and nearly all of us believe we can understand one. Since neither view is well founded, the case study receives a good deal of approbation it does not deserve.

The challenges of case study research lie in its reliance of getting access to a case site (typically an organisation). This means much more than being allowed to interview one or more stakeholders! Research often uncovers things that do not

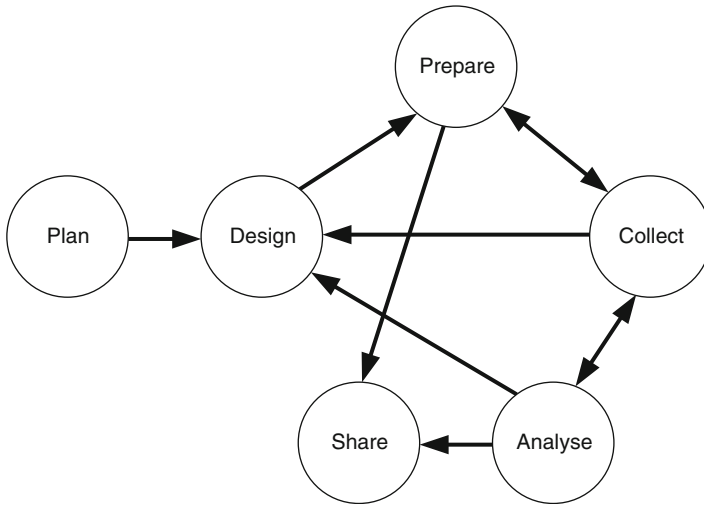


Fig. 5.9 Case research procedure [195]

go or bode well in an organisation, and access may be restricted due to the use of confidential or commercial information – or simply because organisation do not like bad PR (in case you ever wondered, see how many “failure factor” theories you can find in the literature in comparison to “success factor models”). Another challenge is the lack of control over what is going on. The phenomenon being studied might be feeble, or the importance or perception of the research problem might change over time, sometimes even before the research can be concluded. Furthermore, case study methods build on vast amounts of largely unstructured data, the analysis of which is often a tremendous challenge especially for young, inexperienced scholars. Finally due to the quantity of data, cases, and inferences, it can be complex and challenging to write up a case study – especially in journal articles that often impose word or page limitations to the authors.

The research process for case studies varies depending on the case, the phenomenon, or other restrictions, but generally speaking, case studies follow a linear and iterative process as shown in Fig. 5.9.

Planning refers to identifying the research questions and other rationale for doing a case study. The case study method is a preferred method to answer research questions of a “how” and “why” nature (for example, how or why a particular technology works or does not work in an organisation). Other criteria as well might influence whether or not to use a case study method as opposed to other methods, such as the need to exert control over events or behaviours. If there is a need for more control, the case study is not as appropriate as an experiment. On the other hand, if the focus of the study is on a contemporary event the case study method may be better suited. Planning also refers to appreciating the relative strengths and weaknesses of the various research methods.

Table 5.4 Classification of case study designs with examples

Dimension	Variants	Examples in IS research
Research objective	Description	[7, 84]
	Exploration	[10, 126]
	Explanation	[111, 146]
Research epistemology	Positivism	[44, 151]
	Interpretivism	[96, 126]
Research method	Qualitative	[160, 178]
	Qualitative and Quantitative	[61, 92]
Case design	Single	[18, 146]
	Multiple	[4, 140]
Unit of analysis	Holistic	[142]
	Embedded	[10, 11]

Designing refers to defining the unit of analysis, the number and types of cases to be studied, and the potential use of theory or propositions to guide the study. Case study design also involves the development of research procedures, protocols, and other means utilised to maintain the quality and rigor of the research and to demonstrate reliability and validity of data as well as findings.

There is great variety in the design of case studies. Table 5.4 gives five dimensions alongside which case study designs can be classified.

One of the most important design decisions is to identify the appropriate type of case study design and the corresponding unit(s) of analysis. Robert Yin [195] illustrates four types of designs for case studies, namely single-case (holistic) designs (Type 1), single-case (embedded) designs (Type 2), multiple-case (holistic) designs (Type 3) and multiple-case (embedded) designs (Type 4) (see Fig. 5.10).

A *single case study* is preferable when the researcher wants to identify new and previously unchallenged phenomena or issues. It is most often used to represent a unique or extreme case (for instance, in a prevalently revelatory manner). *Multiple cases* are desirable when it is the researcher’s intention to build or test a theory. Multiple cases strengthen the results by replicating the pattern-matching ability and increasing confidence in the robustness of the results. In other words, a multiple-case study is deemed best to enable an appropriate level of generalisation of the findings and to eliminate single-case bias. Embedded design means that there is more than one unit of analysis in a study of one or several cases related to the same object of investigation. Case studies that investigate a phenomenon on a more global level are labelled holistic. The holistic design is advantageous either when no logical subunits can be identified or when the theory itself is of a holistic nature. An embedded design, on the other hand, would allow a researcher to define an appropriate set of subunits and thereby add to the sensitivity of the investigation.

In designing case study procedures, attention is given to demonstrating essential criteria of rigor, viz., dependability, credibility, confirmability, and transferability. Typical components of case study procedures include the definition of case study and interview protocols and the setup of a case evidence database where all records and data can be stored and managed.

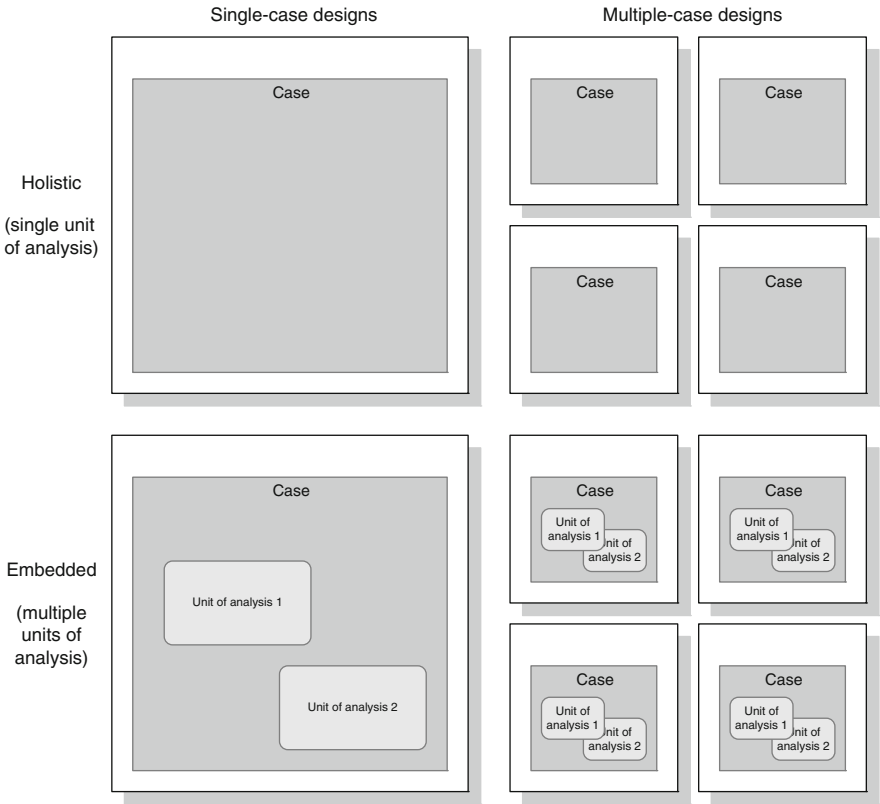


Fig. 5.10 Case study design types [195]

Preparing involves taking the necessary steps to conduct high-quality data collection. This involves especially honing the data collection skills of the researchers, most notably in interviewing and observation. Case study protocols are being further developed, revised, and finalised at this stage, and data collection procedures should be trained and pilot tested. Often, this stage involves a pilot test to verify that procedures are applicable and appropriate.

Collecting means executing the case study protocol(s) and gathering data, preferably from multiple sources. An important principle in this phase includes the maintenance of a chain of evidence – that is, a process that describes how case study questions link to protocol, citations, or other forms of evidentiary sources, elements in the case study database, and ultimately inferences and conclusions as described in a case study report. A case study database is an important tool in this phase for organising and documenting the data collected. Interviews, as a key source of evidence, should be recorded and transcribed. One of the key pieces of advice that I offer to students involved in data collection at a case site is that it is important to gather and maintain complete and precise records of data

(that is, of everything that was seen, heard, said, observed, etcetera). Data analysis might occur a long period of time later, and often researchers need to revisit their collected data at a much later point of time. Therefore, maintaining a close record of as much data as possible is a key component to good case study research.

Analysing consists of examining, categorising, coding, tabulating, testing or otherwise combining and studying the evidence collected to draw empirically based inferences and other conclusions. At this stage qualitative, and perhaps also quantitative techniques for data analysis are employed. Strategies for data analysis include reflecting on theoretical propositions as to how they fit the data (in situations where the case study is used to test or confirm a theory), and examining rival explanations. Refer to the above description of qualitative data analysis techniques for more suggestions for data analysis.

Finally, **sharing** refers to bringing case study results and findings to a close by identifying and addressing relevant audiences and providing them with the findings through appropriate reporting or presentation. This stage is importantly not only for the final write-up (for a thesis or a scientific article) but also to share findings and conclusions for confirmation by case study participants.

Like all other research methods, case studies need evaluation to assess the contribution offered to general knowledge in the area of study by the research. Because quantitative criteria such as statistical power, sample size, or goodness-of-fit do not apply, typically the following of guidelines are offered to reflect on the quality of case studies [120]:

1. The case study must be interesting.
2. The case study must display sufficient evidence.
3. The case study should be complete.
4. The case study must consider alternative perspectives.
5. The case study should be written in an engaging manner.
6. The case study must contribute to knowledge.

5.2.4 Action Research

Action research is an interactive method of inquiry that aims to contribute both to the practical concerns of people in an immediate problem context and to the goals of social science by joint collaboration within a mutually acceptable ethical framework. It builds upon the idea of introducing changes or other sorts of interventions into a context and studying the effects of those actions. Importantly, the researcher acts as the agent of change or intervention (for example, in a role as consultant or organisational member) while also examining how these actions influence the phenomenon at hand. In effect, action research attempts to solve current practical problems while simultaneously expanding scientific knowledge. It involves research that produces immediate practical as well as academic outcomes and requires various degrees of integration between the researcher and the subject of

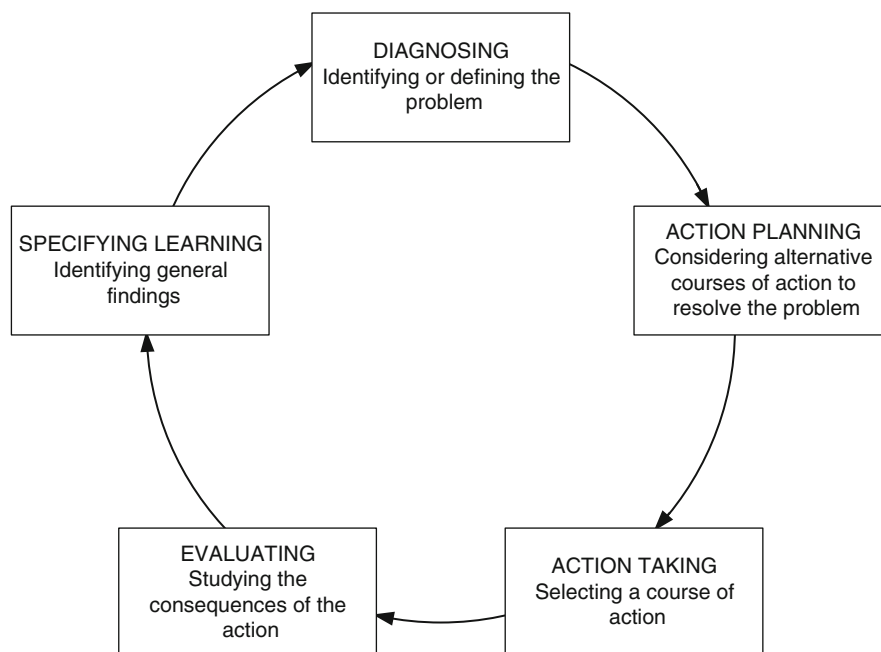


Fig. 5.11 Action research cycle [169]

the research. It can also serve as a means of recording what happened during the process of an event involving change. The distinctive feature of action research – the deliberate introduction of interventions by the researcher – makes it uniquely different from other research methods that typically try *not* to interfere with the subject of study.

The action research process, as described by Gerald Susman and Roger Evered [169], is of cyclic nature and comprises five stage (see Fig. 5.11).

Diagnosing refers to identifying and defining the problem in context. **Action planning** involves specifying the organisational actions that should be taken to relieve, mitigate, or rectify these problems. The researcher at this stage employs a theoretical framework that should explain why and how the planned actions will bring forth the desired change. **Action taking** then implements the selected course of action. **Evaluating** is the analysis of the actions and its consequences and considers whether the planned actions achieved their intended effects. The last phase, **specifying learning**, describes what was learnt throughout the process, applies this knowledge within the organisation or other problem context, and also communicates it to the scientific community.

Typically, this action research cycle is traversed at least twice so that learning from the first iteration can be implemented in the action planning, action taking, and evaluating phases of the second iteration.

Action research, whilst also perusing interviews, documents, archival analysis and other data collection techniques, relies much on participant observation, where the researcher not merely observes, but also find a role within the group observed from which to participate in some manner.

Several key principles guide the process of action research:

- Establish the research infrastructure including formalising the involvement of the organisation and the responsibilities of the researcher and practitioners in a formal research agreement or contract that also specifies necessary legal and/or ethical arrangements.
- Develop statements that describe the issue or problem to be addressed within a theoretical framework.
- Plan and use methodological data collection methods and measurement techniques prior to taking action.
- Avoid dominating the diagnosis and action planning phases by allowing subject learning to improve idiographic usefulness of any theory.
- Undertake iterative cycles and record repetitive planning and action cycles. Report both action successes and failures as failures may provide more insights into learning and positive results.
- Make restrained generalisations and use synchronic reliability to achieve consistent observations within the same time period.
- Explain what happened in a factual and neutral manner. This means going through the cycle steps carefully, describing how the project arose, what was intended, the outcomes (intended and unintended, expected and unexpected) and their impact. Support the story by providing evidence of data recorded (such as organisational documents, feedback, performance metrics and the like).
- Distinguish facts from value judgements and explicitly identify the researcher's inferences and interpretations.
- Make sense of what happened. Provide an analysis of what went on but also describe how it made sense as the story unfolds. Report assumptions and how they were tested, especially if the assumptions were private.
- Explain how the project is contributing to theory or usable knowledge. Be able to demonstrate relevance of the project to the uninvolved reader.

One of the main advantages of action research is the opportunity to contribute to both academic knowledge and to solving a real-world problem. Thus, this method combines relevance and rigor in research. This advantage, however, is also its biggest disadvantage, for doing *action* and *research* together is a challenging act for anyone, let alone an inexperienced scholar. One of the key related challenges is assuming a position of a value-neutral, independent observer to the extent that it allows for critical reflection and analysis, while at the same time maintaining a role as an influencer and intervener.

Action research is evaluated based on contributions made to practice (the 'action') as well as the contributions made to theory (the 'research'). These contributions can be demonstrated through statements from an organisational sponsor about the change achieved, and by a clear discussion of the study outcomes and what was learned in light of the theories pertinent to the domain.

5.2.5 *Grounded Theory*

Grounded theory is a type of qualitative research that relies on inductive generation (building) of theory based on (“grounded in”) qualitative data systematically collected and analysed about a phenomenon. The grounded theory approach essentially attempts to explore for, and develop, generalised formulations about the basic features of a phenomenon while simultaneously grounding the account in empirical observations or data. One of the key advantages – and challenges – of the grounded theory approach is that it is applicable to research domains that are new or emergent and may yet lack substantive theory.

The grounded theory method follows two primary principles. First, the process of theory building is highly iterative, during which theory and data are constantly compared. This process is referred to as *constant comparative analysis*. Second, grounded theory builds upon *theoretical sampling* as a process of data collection and analysis that is driven by concepts that emerge from the study and appear to be of relevance to the nascent theory. Four main characteristics of grounded theory have been identified by Cathy Urquhart and her colleagues [171]:

1. The main purpose of the grounded theory method is theory building, not testing.
2. Prior domain knowledge should not lead to pre-conceived hypotheses or conjectures about the research that the research then seeks to falsify or verify.
3. The research process involves the constant endeavour to jointly collect and compare data, and to constantly contrast new data to any emerging concepts and constructs of the theory being built.
4. All kinds of data are applicable, and are selected by the researcher through theoretical sampling.

Grounded theory can be used as a strategy of research in itself (with the clear objective of theory generation), but it can also be used as a data coding technique that is characterised by independence from theory. This is because its main purpose is to develop new concepts and theories of phenomena firmly grounded in empirical evidence.

There are two main strands of grounded theory. Grounded theory was first introduced as a general qualitative research method by Barney Glaser and Anselm Strauss [72]. Anselm Strauss and Juliet Corbin [168] then revised this general understanding and provided detailed procedures and techniques for the analysis of data, producing the “Straussian” approach to grounded theory. Glaser disagreed with Strauss on two fundamental issues, namely the different coding stages and the use of a coding paradigm that has been proposed. Thus, differences between the two strands of grounded theory include the role of induction, deduction, and verification as well as coding procedures and the generated theories. Both the “Glaserian” and the “Straussian” are in use today, and the debate continues about the relative merits and shortcomings between the two.

In pursuing grounded theory, the research process typically starts with collecting data, followed by the initial stage of data analysis through *open coding*. The codes that

are generated at this stage are descriptive in nature, and identify, name, and categorise phenomena emerging from the data. Open coding is normally accompanied by constant comparison of codes in a search for similarities and differences.

Depending on which version of grounded theory is followed, open coding is followed typically by *axial* or *selective coding*. The objective of this stage is to refine the emerging constructs and develop tentative explanations between the unearthed descriptive categories. Often, this stage involves relating constructs or categories to others by coding for evidence from the data that suggests the existence, nature, and strength of such a relationship.

The third phase is called *theoretical coding* and concerns the development of a substantive theory that includes inferences or tentative conjectures, propositions or hypotheses. Within, explicit causal or correlational linkages are specified based on the interpretive development of the constructs. This step typically involves data examination with a specific focus on some aspect of the theory being developed.

The grounded theory method includes an excellent set of guidelines for data analysis and for ensuring rigor of theory generation procedures. Still, the method describes a process, not an outcome. What I mean is that successful research is not solely dependent on the faithful application of the method, but also involves critical and creative thinking. A faithful execution of the grounded theory method does not guarantee a novel theory or a substantial contribution to knowledge.

Advantages of grounded theory certainly include its tight and early immersion into data analysis – unlike, say, quantitative research where data analysis is typically conducted at a much later stage of the research process. It encourages systematic and detailed data analysis and the literature provides ample guidelines for conducting these steps.

The main disadvantage of grounded theory also lies in the detailed and systematic bottom-up analysis of data. It is very easy to get bogged down in data analysis on a very low level of detail, which makes it difficult to integrate to higher levels of abstraction. The process and the grounded theory to be developed are dependent on both excellent and rich data – collected typically before knowing what to look for, and creative and critical thinking ability brought forward by the researcher – a skill not easily learned or taught. In fact, a widely held belief is that grounded theory is a particularly challenging method especially for early career researchers.

5.3 Mixed Methods and Design Science Methods

Aside from quantitative and qualitative research methods, we should devote some attention to two other prominent strands of inquiry in use in information systems research. Both have emerged in response to some of the criticisms aimed at either quantitative or qualitative methods, or both. We will firstly examine mixed methods and then explore briefly the design science method.

5.3.1 *Mixed Methods*

Mixed methods research is a type of inquiry that features combinations of both qualitative and quantitative methods for data collection and analysis in either sequential or concurrent fashion (“numbers and words”). Mixed method research emerged for two reasons: first, it was developed as a response to the “paradigm wars” between qualitative and quantitative researchers and second, it provided a pragmatic way of using the strengths of a pluralistic approach to research that combined the strengths of both qualitative and quantitative methods. Mixed methods designs incorporate techniques from both the quantitative and qualitative research traditions yet typically combine them in unique ways to answer research questions that could not be answered in other ways.

Mixed method research is said to encourage stronger inferences, to provide a greater diversity of divergent views and to enable researchers to simultaneously answer confirmatory and exploratory questions, verifying and generating theory at the same time.

There are a number of typologies for mixed method designs and they address several different dimensions of research:

- *Weighing*, that is, deciding whether to give the quantitative and qualitative components of a mixed study equal status or to give one paradigm the dominant status;
- *Timing* of the qualitative and quantitative phases – sequential or concurrent;
- *Mixing*, which can form a continuum from mono-method to fully mixed methods; and
- *Placing*, that is, deciding where mixing should occur (in the research questions, methods of data collection, research methods, during data analysis, or data interpretation).

There are five major rationales for conducting mixed method research [75]. Any of these five reasons justify employing a mixed method research design. **Triangulation** means you are seeking convergence and corroboration of results from different methods and designs studying the same phenomenon. **Complementarity** refers to seeking elaboration, enhancement, illustration, and clarification of the results from one method with results from the other method. **Initiation** means attempting to discover paradoxes and contradictions that lead to a re-framing of the research questions. **Development** concerns using the findings from one method to help inform the other method, such that its usage plan is developmental in nature. Finally, **expansion** is a rationale that occurs if you are seeking to expand the breadth and range of research by using different methods for different inquiry components.

The processes of data collection and analysis depend on the type of methods being employed, and in general follow the guidelines for the techniques chosen. Still, a number of particularities exist in mixed method research that warrants attention:

- **Data transformation:** especially in concurrent mixed method research – employing qualitative and quantitative methods in parallel means that data

Table 5.5 Strengths and weaknesses of mixed method research [89]

Strengths	Weaknesses
Words, pictures, and narrative can be used to add meaning to numbers	It can be difficult for a single researcher to carry out both qualitative and quantitative research, especially if two or more approaches are to be used concurrently
Numbers can be used to add precision to words, pictures, and narrative	The researcher has to learn about multiple methods and approaches and understand how to mix them appropriately
The research can benefit from both quantitative and qualitative method strengths	Methodological purists contend that one should always work within either a qualitative or a quantitative paradigm
The researcher can generate as well as rigorously test a grounded theory	Mixed method research is typically more expensive than mono method research
Mixed method research can answer a broader and more complete range of research questions because the re-searcher is not confined to a single method or approach	Mixed method research is typically more time consuming than mono method research
A researcher can use the strengths of an additional method to overcome the weaknesses in another method by using both in a research study	Some of the details of mixed research remain to be worked out fully by research methodologists (e.g., problems of paradigm mixing, how to qualitatively analyse quantitative data, or how to interpret conflicting results)
Mixed method research can be used to provide stronger evidence for a conclusion through convergence and corroboration of findings	Mixed method research may require a research team
Mixed method research can add insights and understanding that might be missed when only a single method is used	
Mixed method research can be used to increase the generalisability of the results	

needs to be transformed to suit the different analysis techniques. For example, qualitative data (e.g., codes) need to be quantified (e.g., by counting frequency of occurrence in text). Alternatively, quantitative data may need to be qualified to enable meaningful comparison.

- **Data correlation:** data collected with different methods require comparison with a view to identifying outliers (e.g., in a quantitative survey) that require follow-up analysis with qualitative data.
- **Data comparison:** one essential component in mixed method is constant comparison for purposes of triangulation. Often this is achieved through a data matrix that combines a quantitative axis and a qualitative axis. The objective is to identify similarities as well as differences – which would then require further investigation to examine the cause of difference.
- **Legitimation:** A key activity is the description of the steps undertaken to ensure the validity of quantitative data as well as accuracy of qualitative findings.

Table 5.5 provides a summary of the strengths and weaknesses of mixed method research.

5.3.2 *Design Science Methods*

Over recent decades, methods ascribed to a *design science* attracted interest in the information systems community, especially because of an article published by Alan Hevner and colleagues in the MIS Quarterly in 2004 [83]. The main motivation behind the (re-) emergence of design science as a research paradigm is said to stem from a desire to complement the mainstream behavioural orientation of information systems research with more design-oriented research.

Design science research has been defined ([82], p. 5) as

a research paradigm in which a designer answers questions relevant to human problems via the creation of innovative artefacts, thereby contributing new knowledge to the body of scientific evidence. The designed artefacts are both useful and fundamental in understanding that problem.

The fundamental principle of design science research is therefore that knowledge and understanding of a design problem and its solution are acquired in the building and application of an artefact. The term artefact is central to design science research and is used to describe something that is artificial, or constructed by humans, as opposed to something that occurs naturally. In design science as a research activity, the interest is on artefacts that improve upon existing solutions to a problem or perhaps provide a first solution to an important problem. Typically, at least five types of artefacts are differentiated:

- **Constructs** (vocabulary and symbols)
- **Models** (abstractions and representations)
- **Methods** (algorithms and practices)
- **Instantiations** (implemented and prototype systems)
- **Design theories** (improved models of design, or design processes)

In examining how such artefacts can be developed, design science research can be placed within a framework bounded by the practical environment and the available knowledge base at that point in time. This framework is shown in Fig. 5.12.

The *environment* defines the problem space in which the phenomena of interest reside. In information systems research, the environment is comprised of at least people, organisational structures, and technologies and thereby establishes the relevance for design science research. The *knowledge base* provides the materials from and through which design science research is accomplished. That is, prior research and results from reference disciplines provide foundational theories, frameworks, instruments, constructs, models, methods, and instantiations that can be used in the design phase. The knowledge base therefore assists the design science

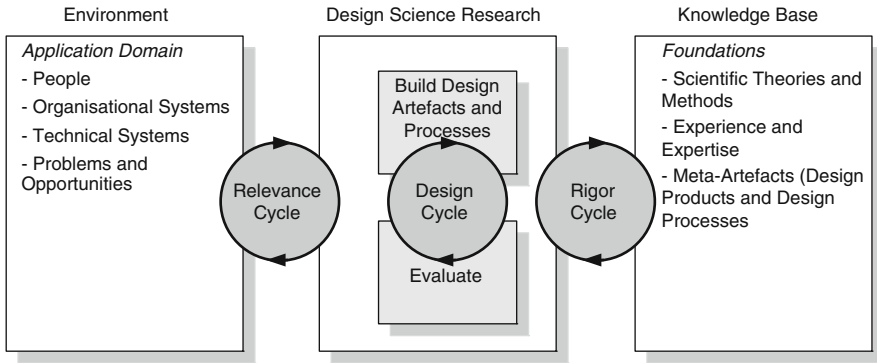


Fig. 5.12 Design science research framework [81]

in achieving rigor. *Design science research* is comprised of activities related to building and evaluating artefacts designed to meet the identified business needs.

The *relevance cycle* bridges the contextual environment of the research project with the design science activities. The *rigor cycle* connects the design science activities with the knowledge base of scientific foundations, experience, and expertise that informs the research project. The central *design cycle* iterates between the core activities of building and evaluating the design artefacts and processes of the research. Alan Hevner [81] posits that these three cycles must be present and clearly identifiable in a design science research project.

One of the current debates about design science concerns the actual procedure by which design science research is to be executed [77, 81, 87, 124]. Alan Hevner and his colleagues identified seven high-level guidelines that give guidance in the form of key principles to which design science research adheres. These are summarised in Table 5.6.

Currently, there is an ongoing debate on whether or not these seven guidelines are appropriate, or if they are rather misleading. Proponents argue the generic yet procedural nature of these guidelines, bringing structure into the wicked nature of design science. Critics lament that these guidelines are often interpreted in an overly mechanical and procedural way whilst overseeing that they specify outcome requirements but actually no procedural guidance for how each guidelines should be implemented.

Whatever one's personal views on the topic may be, I would recommend young academics and scholarly students to consider these and other guidelines [77, 81, 87, 124] within a critical reflection process. In the end, design science is predominantly focused on the artefact and not primarily on the execution of one or another set of procedural steps. You may meet all of the requirements spelled out in Table 5.6 without ever reaching the goal of design science, and you may design a great novel artefact without executing these steps.

Indeed, the key evaluation criterion for design science – and one of its key challenges – is the notion of *demonstrated utility* that the design artefact provides.

Table 5.6 Hevner et al.'s design science research guidelines [83]

Guideline	Description
Guideline 1: design as an artefact	Design-science research must produce a viable artefact in the form of a construct, a model, a method, or an instantiation
Guideline 2: problem relevance	The objective of design-science research is to develop technology-based solutions to important and relevant business problems
Guideline 3: design evaluation	The utility, quality, and efficacy of a design artefact must be rigorously demonstrated via well-executed evaluation methods
Guideline 4: research contributions	Effective design-science research must provide clear and verifiable contributions in the areas of the design artefact, design foundations, and/or design methodologies
Guideline 5: research rigor	Design-science research relies upon the application of rigorous methods in both the construction and evaluation of the design artefact
Guideline 6: design as a search process	The search for an effective artefact requires utilising available means to reach desired ends while satisfying laws in the problem environment
Guideline 7: communication of research	Design-science research must be presented effectively both to technology-oriented as well as management-oriented audiences

Demonstrated utility means specifically that utility is illustrated in such a way that utility is improved beyond the current state of utility. This definition also implies three key criteria that should be met:

- (a) Novelty of the demonstrated utility of an artefact;
- (b) A positive difference of the utility of an artefact in comparison to existing work; and
- (c) A thorough evaluation that provides decisive evidence for superior utility of an artefact.

The definition of utility can vary. Utility can be a performance metric that defines the extent of improvement of the novel artefact over an existing solution, but may also be defined differently. For example, utility may also be interpreted by end users, or in terms of efficacy, efficiency, effectiveness, or other criteria. The vagueness of the utility concept as an ultimate concept thus brings forward issues in defining it adequately and identifying the most appropriate means for evaluation. Typically, at this stage, some sort of empirical study is carried out, by means of simulation, case study, focus group, survey, or experiment.

Another key challenge of design science research can be epitomised in the question “when is a design a research contribution?” One source of confusion to novice design-science researchers in particular is to understand the subtle difference between conducting design as a science versus practicing routine design. The answer lies in the contribution to academic knowledge that is inherent in the design – both as a process and as an outcome.

A third challenge lies in the role of theory and design science. One can argue that the goal of design science is all about efficacy and utility; there is no theory being sought. Hence, theory can be regarded as unimportant to such endeavours. One may

also argue, however, that as researchers we can attempt to define the anatomy of what constitutes a good design theory [77]. In that view, an artefact is seen as an instantiation (and thus a demonstration) of the theory, which then becomes the fundamental component of the design research.

5.4 Further Reading

This book is not meant to discuss research methods extensively but rather to provide an introduction to basic concepts, processes and caveats. You are encouraged to peruse the excellent literature available on research methods. For example, a must-read for everyone is Anol Bhattacharjee's *free* textbook on social science research, available in the global text project (<http://globaltext.terry.uga.edu/books>).

Concerning quantitative methods, the bible for survey research is Floyd Flower's textbook [65], and the bible for experimental research is written by Thomas Cook and Donald Campbell [150]. I also find inspiration in the quantitative side to research from books such as Freakonomics [105] or Fermat's Last Theorem [153]. A wonderful introduction to behavioural experimentation is Lauren Slater's book "*Opening Skinner's Box: Great Psychological Experiments of the Twentieth Century*" [154].

Importantly, there are many useful and important additions to the content of this book in terms of processes and challenges of quantitative methods. For example, the computer sciences have an extensive tradition in discussing not only notions but also threats to validity. Claes Wohlin's book on Experimental Software Engineering [193] illustrates, exemplifies and discusses many of the most important threats to validity, such as lack of representativeness of independent variable, pretest sensitisation to treatments or lack of sensitivity of dependent variables.

Another important debate in the realm of quantitative research is the ongoing discussion on reflective versus formative measurement development. I have abstained from this discussion in this book because I feel that it would have been misleading in terms of understanding critical issues of operationalisation and measurement, but also because this debate is still ongoing with unclear outcomes as to when and how reflective versus formative measurements should be used, or whether and how we can gauge the validity and reliability of these measures. Still, this methodological discussion is an important one and is likely to affect all quantitative researchers in their efforts. Several viewpoints pertaining to this debate are available [1, 31, 53–55, 94, 128] and I encourage you to study these and forthcoming papers.

Concerning qualitative methods, I noted above that a brilliant webpage about quantitative methods is maintained by Detmar Straub at <http://dstraub.cis.gsu.edu:88/quant/default.asp> and is especially useful because of the reading list it provides.

Similarly, the webpage <http://www.qual.auckland.ac.nz/>, maintained by Michael Myers is an excellent resource for qualitative researchers. An excellent introduction

to qualitative research is Michael Myers' book on qualitative research in business and management, as well as [120] as well as Norman Denzin and Yvonne Lincoln's textbook on qualitative research [52].

Literature on different qualitative research methods is also available. The bible for case study research is Robert Yin's case study textbook [195], filled with guidelines and many examples from social science research. There are also several journal articles published in information systems research journals that specify recommendations and critiques for the case study method, e.g., [16, 30, 44, 56, 59, 101, 151, 156, 179].

An excellent introduction to the action research method is the article by Gerald Susman and Roger Evered [169]. For a more in-depth look at the method, the collection of articles in the book by Stephen Kemmis and Robin McTaggart [93] is very useful.

In information systems research, a good place to start would be the articles by Richard Baskerville and Trevor Wood-Harper [13, 14]. Also, the MIS Quarterly's special issue on Action Research published in 2004 [12].

The key literature on grounded theory build on the seminal books by Anselm Strauss [167, 168] and Barney Glaser [72]. Another excellent book is the Sage Handbook on Grounded Theory [25].

For mixed method research, I recommend the book on research design by John Creswell [40] as an introductory read as well as a number of very good mixed method methodological debates [89, 116, 145]. Abbas Tashakkori and Charles Teddlie's "Handbook of Mixed Methods in Social and Behavioral Research" [170] is seminal for this type of research. Examples for mixed method research in information systems research involve the work by Guy Gable [66], the work by Jose Esteves and Joan Pastor [61], or the study by Kevin Crowston and Michael Myers [42].

Alan Hevner and Samir Chatterjee recently produced an edited volume on design science research that covers a lot of topic, methodologies, examples and guidelines in one book [82]. There are also a variety of methodological essays available that provide guidelines for this type of research [29, 71, 77, 81, 83, 87, 110, 115, 129, 148]. There are several excellent examples that demonstrate good design science research in information systems, for instance [5, 70, 113, 118]. Some of you might also be interested in the foundations of design science research in Herbert Simon's "The Sciences of the Artificial" [152].

Part 3

Publishing Research

Chapter 6

Writing IS Research Articles

In this chapter, we will examine some guidelines and advice that relate to *reporting* of research conducted. Publication is an essential scholarly activity because it informs academic and other communities about lines of investigation, (and their outcomes) related to relevant phenomenon being studied.

Publishing research is so important because it is a scholar's main vehicle to add to the body of knowledge – which, as we discussed much earlier in this book, is basically a huge library of written texts. Publications are also typically the only way we can build a manifestation of a research process – or of ourselves! Papers are “the study”, and often papers also represent “the scholar”. Over the course of your career, you will likely meet many scholars whose papers you have read. I have always found this to be a very nice as well as stimulating experience, to identify the person behind a paper.

Before we continue with advice on publishing research, we should note some important characteristics related to this process. First of all, at the stage of writing, we normally assume that we have actually completed the relevant research, that is, we have examined a phenomenon of interest, we have theorised about it, we have collected and analysed data to test our theory. Moreover, importantly, we assume we have a research outcome to report on!

Now, this comment is an important one. Research reports disclose information about research outcomes and the related processes in cases where the research uncovered findings worth reporting on. One of the rather sad characteristics of information systems research as a science (and it holds for virtually all other fields of research as well) is that we only get to see those reports on successful research – those where the research led to novel findings and contributions to knowledge, and where the research reports have been peer-reviewed and accepted by the community for publication. What we do *not* get to see are those research endeavours that were not successful – simply because in virtually all cases these will never be published! Extrapolating only from my own experiences, the quantity of such studies and research projects, I believe, is very, very high.

Why am I telling you all this? I have seen many cases of inexperienced (as well as more experienced) scholars that attempted to produce research reports about

projects that, bluntly put, were not successful (enough) to warrant publication. I think a common misapprehension exists that “we have done this study so it should be published”. Very often, I find students perform case studies, surveys or experiments that, for a variety of reasons, did not yield good findings (or valid results), but the researchers involved still push to try to get a paper published.

Eszter Hargittai, in her book “Research Confidential” [80] suggests that scholars in the social sciences (such as Information Systems) may be unaware of such and other problems in part because academics do not share stories of what didn’t work on their projects, and how to deal with particular challenges, especially in empirical research projects.

Why is this situation a problem? First, publishing is about reporting *results*. This means that it requires a mindset of telling “what you have found” more so than reporting “what you have done”. It implies that the results are worth publishing and it implies that the authors focus their effort on describing these results instead of focusing on what they did towards that end (independent of whether or not they reached that goal).

Second, the problem is that writing a good research article is a very challenging task, consumes a great deal of time and dedication, and is something most of us have never really received any education in. With all other commitments as an academic, therefore, we need to manage writing as an activity in the sense that we should only write papers that are worth writing. And experience in publishing also tells us that papers about “unsuccessful” research (whatever that may mean) are incredibly difficult to publish – to the extent that virtually none will appear in the good publication outlets. There are exceptions, of course, but in general, one is much better off not trying to publish for the sake of publishing.

Having said this, I concede that staying away from trying to publish is very hard to do. Many a time have I completed a study, examined the results, found them not to be strong or convincing enough – and still spent a great deal of time and effort trying to get a report published. Guess what happened? Of course, I failed in successfully publishing such a study. However, it can take quite some commitment and harsh reflection to “let go” and realise that research can involve trial and error and you simply might not get it right all the time.

So, with all these problems related to writing, why do we bother in the first place? Well, aside from the fact that a report is the representation of a research process (and often of the researchers themselves), and aside from a report being the key element in the accumulation of a body of knowledge, publishing is probably *the* key performance metric for any academic – be it a student, an early career researcher, or a tenured faculty member.

Recall that the purpose of an academic is to produce **and** disseminate research results. Your publications advertise your skills, areas of interest, as well as your expertise. They are a demonstration of how, and how well, you do research. Therefore, the **number** (“how many”) and **quality** (“how good”) of your publications will determine your success in research.

As an example, when you finish your PhD and start your first academic position (typically, at an Assistant Professor or Lecturer level), you will be evaluated based

on the number of publications you have written in different journals. For example, to achieve tenure in some of the ‘top’ information systems research schools up to eight or more “top” journal papers are expected within 5–6 years after PhD graduation [50]. There are publicly available ranking lists of universities as well as authors (e.g., <http://www.vvenkatesh.com/ISranking/>), and tools such as *Harzing’s Publish or Perish* (www.harzing.com/pop.htm) allow anyone to examine and compare the publication productivity of scholars in all fields.

However, not only are publications important for academic staff members of a university, but also for the aspiring research student. Student publications are an indication of research training success, and are often a good indicator of the quality of the student’s thesis. Simply put, theses that build on a set of successful publications are hard to reject by an examiner because the parts of the study have already been subjected to review (for the papers) – and apparently found to be acceptable.

In addition, publications advertise the strengths and areas of interest of the student, the research group, or the university as a whole. As a side note, publications also generate direct income from the federal government to the university, which is then used to support the researchers and students in their research. Publications matter. In fact, an academic’s life is often epitomised in the saying that lends the name to Anne Harzing’s citation tracking tool: “Publish or perish!”

Therefore, we will now discuss some essential elements of research publication processes, particularly strategy, writing as a process, and handling the feedback received on publications.

6.1 Strategising

Because of the importance of publishing in research, writing should be considered a strategic activity, and should receive some dedicated management and, indeed, strategising.

While publications matter to an academic’s success, the reality is that in fact few publications matter. Amanda Spink, one of the most widely published and cited researchers in information science, once quoted the following statistics:

- 50% of papers are never cited.
- 20% of papers are highly cited (often early papers in an emerging research area because they tend to dominate the research agenda for years to come).
- Only 5% of papers populate the research front – these are the very highly cited papers.

These statistics point to the importance of citations. *Citations* are an indication of how many scholars have read an article and used it in their own line of argumentation and thus refer to the paper in their own papers. Typically, it is assumed that those papers that are cited often are “good” papers – otherwise other scholars wouldn’t refer to them. There are obviously exceptions to this rule,

Table 6.1 Qualities of papers and studies

Good papers	Good research
Are purposive	Is purposive
Have clearly defined goals	Has clearly defined goals
Contain a reporting of procedures (including flaws) that is complete and honest	Follows a replicable procedure
Are written such that objectivity is clearly evident	Is conducted such that objectivity is clearly evident
Describe the use of appropriate analytical techniques	Uses appropriate analytical techniques
Present conclusions that are limited to those clearly justified by the data or facts	Provides conclusions that are limited to those clearly justified by the data or facts

for instance, when a paper contains flaws that other scholars attempt to address and overcome in their papers. Amanda Spink’s main argument seems to be that in the publishing field, as in any other field, the top end is sparsely populated and hard to reach.

So, how do we get there? The most important rule when strategising about publishing is that **good papers build on good research**, and good publication outlets (the “good” journals) only ever publish good research. Consider Table 6.1.

Do you notice the similarities? You can only write good papers when you do good research. Let’s repeat that: **You can only write good papers when you do good research**. In other words, if your research does not meet the quality criteria (validity and reliability in quantitative methods) you will not be able to publish the research in a good journal. More importantly, it also means that you can only write good papers if you have good results. The most beautifully and carefully executed experiment will not result in a good paper if you did not get good (that is, valid, reliable and significant) results from it.

Having stated this universal principle, we must note that we can also revert the statement above to its opposite to learn about a second key fact about publishing: **you can easily waste good research by not writing about it well enough**. Daryl Bem ([15], p. 205) has put this brilliantly:

I believe that the difference between the manuscripts accepted and the top 15 to 20% of those rejected is frequently the difference between good and less good writing. Moral: Do not expect journal reviewers to discern your brilliance through the smog of polluted writing. Revise your manuscript. Polish it. Proofread it. Then submit it.

With the importance of good research and good writing established, let us turn to important facts of the publishing game. First, we will look at the publication process, to build an initial understanding.

6.1.1 The Publishing Process

Scholarly publications, typically contributions in books, journals, or conference proceedings, only “count” as publications if they are *peer-reviewed*. Peer review

(also known as refereeing) is the process of subjecting an author's scholarly work, research, or ideas to the scrutiny of others who are experts in the same field, before a paper describing this work is published in a publication outlet such as a journal or book. The work may be accepted, considered acceptable with revisions, or rejected. Peer review requires a community of experts in a given and frequently narrowly defined field who are qualified and able to perform impartial review. Impartial review, especially of work in less narrowly defined or inter-disciplinary fields, is often difficult to accomplish; also, the significance (good or bad) of an idea may never be widely appreciated among its contemporaries. Typically, peer review is performed in a single- or double-blind format, meaning that the identities of the reviewers and/or the authors are anonymised or hidden in the review process so as to avoid bias.

The publication process typically works as follows: Upon submission of a proposed publication, an editor of that outlet (say, book or journal) sends advance copies of an author's work to researchers who are experts in the field (known as "referees" or "reviewers") normally by e-mail or through a web-based manuscript processing system. Usually there are two or three referees for a given article, sometimes more (I received five reviews on a manuscript once).

These referees each return an evaluation of the work to the editor, noting weaknesses or problems along with suggestions for improvement. Typically, most of the referees' comments are eventually seen by the author. The editor, usually familiar with the field of the manuscript although typically not in as much depth as the specialist referees, then evaluates the referees' comments, adds their own opinion of the manuscript, and the context of the scope of the journal or level of the book and readership, before passing a decision back to the author(s), usually with the referees' comments.

Referees' evaluations usually include an explicit recommendation of what to do with the manuscript, often chosen from options provided by the outlet. Most recommendations are along the lines of the following:

- To unconditionally accept the manuscript for publication,
- To accept it conditional to the event that its authors improve it in certain ways,
- To reject it, but encourage revision and invite resubmission, or
- To reject it outright.

During this process, the role of the referees is advisory, and the editor is typically under no formal obligation to accept the opinions of the referees. Furthermore, in scientific publication, the referees do not act as a group, do not communicate with each other, and typically are not aware of each other's identities or evaluations.

This process as I have sketched it is quite common across different outlets, be it journals, edited proceedings, or scholarly books. Typically, the outlets also provide public information about their editorial and review processes, for instance they inform prospective authors about how they handle incoming manuscripts and how they make decisions about them.

We should also note here that different types of outlets have different levels of credentials associated with them. In other words, be aware that many publication

outlets exist and that publication outlets differ *widely* in quality. In information systems research, bluntly put, what matters are *journal articles*, that is, contributions that appear as articles in scholarly journals such as the MIS Quarterly, Information Systems Research, or others. There are many hundreds of these journals being published by a wide range of publishers, with some being considered “good” and some not. To assist scholars in making a decision which journals to target with their work, there are ranking lists available that evaluate different journals based on metrics such as impact factor, readership, and other criteria.

At present, typically, the following six journals are considered the elite journals relevant to information systems research. They are called the “Top-Six Basket of Journals”:

- Management Information Systems Quarterly
- Information Systems Research
- Journal of the Association for Information Systems
- European Journal of Information Systems
- Journal of Management Information Systems
- Information Systems Journal

There are, of course, many other journals (and many other good ones, too). Typically, schools, universities, domain areas, or countries have their own ranking lists that indicate the quality of journals (e.g., as A-level, B-level or C-level journals). In Australia, for example, a ranking exercise was conducted in 2010 as part of the “Excellence in Research in Australia” initiative (see <http://lamp.infosys.deakin.edu.au/era/?page=fordet10&selfor=0806>). Although widely disputed and certain to be changed over time, this ranking list serves as a useful orientation guide to learning about the most esteemed outlets (at least in Australia) and gives some indication about how challenging it will be to publish in these journals.

Sometimes research is published as a *referred book chapter* in an edited book volume. You will typically find that scholars write fewer chapters than journal articles, simply because these publications are not considered as important. At some universities, they don’t count at all as publications (as part of promotion or tenure decisions, that is).

A third outlet for information systems researchers is *conference proceedings*. Conference proceedings typically include copies of papers that are being presented at the conference. They are usually shorter than journal articles and often describe research that is still ongoing or not fully completed.

You will find that some scholars and some universities recognise papers in conference proceedings in tenure or promotion decisions. Generally speaking, however, *conference proceedings do not matter*. This means that in most countries (and certainly in the best universities), conference proceedings (except in certain fields such as Computer Science) cannot be used to build an argument about the publication productivity or success of a scholar. This is because at the majority of conferences, the review process is significantly shortened and far less rigorous than for a good journal (partially because of the deadlines involved with organising a conference for a particular date). Also, conferences have been incepted to give academics an

opportunity to present their *ongoing* research to the community, to harvest opinions and feedback and new ideas about how they continue their research in the best possible way. Therefore, typically, conferences are meant to be one stepping-stone in the research and publication process – and not the end goal.

Therefore, the best rule for any academic in Information Systems is that you need to publish in journals, not conferences! In fact, publications at conferences may count against you. I know of the case of a colleague who was rejected for tenure because of the ratio of conference papers versus journal papers published. Because my colleague had published around 50 conference papers together with around five or so (good) journal papers, the committee felt that the researcher had a tendency “not to see studies through to the end”, as indicated by presenting the work at an intermediate state at a conference without publishing the final results in a credible journal.

6.1.2 Key Publishing Decisions

When selecting a suitable target outlet for an academic paper, you need to keep a couple of important things in mind: First, the academic publishing process is a “*one shot option*”: Once you published a result, you can’t publish it again. This means that a paper that contains some results or ideas, once published with copyright will be the only place to publish that result. You cannot write two papers on the same finding. This (mal-) practice is called double-dipping (and we return to it in Chap. 7 below).

Second, the academic publishing process is an *open publishing process*: Once you publish an idea, it is out there with your name attached to it. This can be good or bad for a reputation, depending on the idea and the quality of the research conducted. And believe me, I can easily name one or two papers I wish I had never written. . .

Third, you need to consider that research *writing is a skill* that is typically not part of the curriculum for your doctoral degree (or any other degree you might have pursued). Good research writing takes time and practice. The advice that I give students is to write early and to write often. Publish papers from your Honours or Masters-level research, or identify some areas of your work that are completed, and start writing. Target a conference (initially), then a reasonably good journal, and over time aim higher and higher. The reason behind this advice is the fact that at the end of your studies, you will need to produce a thesis – a document as a manifestation of your work, your research, and its outcomes. Such a thesis ranges from 100 to over 400 pages – it is a book essentially. Now, writing a book is not an easy endeavour, especially if you have not written a single scientific paper before! Writing papers is the best practice for writing a good thesis. It is as simple as that.

Table 6.2 Paper-based research and research-based papers

Papers-based research	Research-based papers
You want to write a paper, so you start doing some research	You did some research, and you seek to publish a paper about your findings
Often deadline-driven	Often results-driven
Works relatively well for conference papers	Often “too much” for a conference paper
Typically does not work well for (good) journal papers	Works well for journal papers
Does rarely work for a thesis	Works well for a thesis

Fourth, distinguish ‘research-based papers’ from ‘papers-based research’. I often find, especially with young academics, that they plan to write because a deadline for a particular conference or journal issue submission is approaching, and they are thinking, “Let’s produce something for this outlet.” I call this *papers-based research* and differentiate it from *research-based papers* (Table 6.2).

Papers-based research, I believe, is highly problematic because it is deadline-driven. We tend to learn of a particular deadline about 6–12, sometimes 18 months in advance. I am not saying it is impossible but most good and substantial research consumes much more time than that.

Papers are the publication of the outcome of a study, alongside with details about the research process. That is, they are driven by the availability of hopefully novel and insightful results. In other words, you can write the paper once the research is finished, typically making for a much stronger paper because the contribution is better developed. This type of approach is aided by and catered towards scientific journals, because these typically do not have submission deadlines, which means that you can submit a manuscript at any time when it is ready.

Finally, one of the most useful tools I have employed is a *publication matrix* that maintains an overview of your publication work in progress. Table 6.3 gives an example. Such matrices assist you in planning your research and publication program and in monitoring progress, tasks, and responsibilities. At the latest after completing your doctorate, you will have multiple projects running at the same time, and multiple papers in draft, in review or in publication as well. The matrix can also assist in recognising how different studies, and papers, together form a portfolio of research. Finally, such a planning document prevents you from over-committing to projects and papers, simply when the matrix becomes too big for anyone to work on.

What you also see from Table 6.3 is that not all papers are targeted or under consideration only at the top journals (the A* journals). This is quite common indeed and reflects recognition that not all portions of work have the quality and the potential of becoming publishable in a top journal. This recognition demands that you are objective and honest about your own work. In other words, don’t waste your time on research that takes too long and will not be accepted anywhere “good” and also don’t submit a paper to an outlet when you know you won’t make it.

Table 6.3 Publication matrix

Topic	Team	Target	Ranking	Status	Task	Responsibility	Deadline
BPMS acceptance	Stephan, Hajor, myself	ITM	B	Under first round review			12/10/2011
Gateway constructs	Myself	EJIS	A*	Under first round review			27/09/2011
Model understanding as learning process	Myself, Hajo, Sander	JMIS	A*	Rejected	Revise and resubmit to different journal	Myself	4/01/2012
Green transformations	Stefan, myself, Jan	MISQ	A*	Major revisions	Revise and resubmit	Stefan	9/01/2012
Collaborative technology impact	Myself, Jan, Christopher	JIT	A*	Under first round review			28/11/2011
Sustainability transformation	Jan, Axel, Christoph, Stefan, myself	MISQE	A	Rejected	Revise and resubmit to different journal	Stefan	29/09/2011
Routing symbol design	Kathrin, myself, Jan	DSS	A*	Under first round review			30/11/2011
Modelling expertise	Jan, myself, Hajo	ISR	A*	To be written	Finalise and submit	Jan	15/06/2011
BPM culture	Theresa, Jan, myself	EJIS	A*	Under first round review			22/11/2011
Modelling preferences	Kathrin, myself	Inf. sys	A*	To be written	Finalise and submit	Myself	15/01/2012

6.1.3 Co-authorship

Collaboration is a key element of scientific work, and thus, co-authored papers are the norm rather than the exception in scholarly publications. Collaboration might mean working together with your supervisor, but it can also mean collaborating with other (junior or senior) academics on topics of mutual interest.

Co-authoring papers is a preferred strategy for several reasons. For one thing, working only by yourself requires you not only to do all the work yourself but also that you need to be strong in all aspects of the research lifecycle, from problem identification, idea generation, theorising, research methods, data analysis, and writing. Because of the amount of work required, you will also not be able to progress on as many projects and papers concurrently as colleagues that collaborate.

In contrast, collaboration could mean synergising based on mutual domain expertise, complementing strengths you possess (e.g., theorising) with those of others (e.g., data analysis), or simply to share the workload of doing or writing about research.

Of course, it goes without saying that collaboration success much depends on the type of people you work with. There is a good reason why several well-known scholars co-author so many papers – they are obviously good in what they are doing and are hence preferred targets for co-authorship. Aside from searching for and working with good people, I find two other aspects to be important: *complementarity of skills* and *alignment of working styles*. With complementarity I refer to synergy benefits that stem from working with people that provide strengths complementary and not redundant to your own. In turn, if you are an expert in some area (say, quantitative data analyses), this might also make you a perfect collaborator for someone with a need for such expertise. Regarding alignment, over time I have come to realise that my successful collaboration were those with people with similar work styles (I would call my style independent and document-centric) and who I enjoy working and being with.

Co-authoring papers typically means sharing the work amongst the collaborators. Many tasks are involved in the research lifecycle and sharing can occur at any stage. I believe that sharing any of the following tasks should be a good indication for co-authorship:

- Having an original idea
- Designing a study
- Organising data collection
- Collecting data
- Analysing data
- Writing a paper
- Driving a revision

When you find yourself in a situation where the above tasks are distributed amongst several people, then you should consider co-authorship scenarios. My rule of thumb for these situations goes as follows:

Authorship of a research output is defined as substantial participation in a research output, including:

- Conception and design, or analysis and interpretation of data, or
- Drafting an article or revising it critically for important intellectual content.

Therefore, authorship of a research output should not be claimed where participation is solely in the acquisition of funding or the collection of data. Similarly, in my view, general supervision of a research group or management of a research project does not constitute authorship. Note here that these are my views and I am well aware that there are different practices in some institutions.

In determining your role in co-authorship, remember that good papers build on good research, and you can contribute in either or both of these areas. Examine honestly your individual strengths and capabilities to determine your role:

- Are you an original thinker? Are you capable of extending your field? Having new ideas?
- Are you an expert in research conduct? Do you know methods? Approaches? Analysis techniques?
- Do you like to write? Are you good at writing?

In most collaboration situations you find that different people have different, dedicated roles. In fact, this will work much better than, for instance, having four expert analysts but no one who is a skilled writer.

One of the key decisions to be made is the order of authors to be placed on a published paper. This is because in most circumstances, papers get ascribed to the first author, and the first author receives most of the credit for the publication. Typical practices when it comes to putting the authors' names on a paper include ensuring that the name of the principal contributor appears first and subsequent names in order of decreasing contribution. However, in some scientific fields the most highly credited author is the one whose name appears [first/last]. In some institutions, project leaders are always included, and in some areas of social science, scholars tend to place the authors' names in alphabetical order regardless of the amount of effort that was contributed. Still, in information systems research globally, authors are named in recognition of their contribution.

My own experiences suggest that in most collaboration situations, there will always be one individual clearly identifiable that is the driver of the project. He or she will be the one that pushes the project forward, takes over critical tasks, consolidates and reviews contributions from the collaborators and overall making sure the project and/or paper stays on track. In such scenarios, the driver will most likely (or should) become the leading author on a paper. If such a case cannot be identified, co-authorship and collaboration roles are best discussed upfront: Who will "own" the research? Who will own the data? Who will drive the publication(s)?

To that end, maintaining open lines of communication throughout the research process is vital. You should talk openly and frankly and as a team, you have a responsibility to create a communication environment without fear of reprisal, demotion, or other punishment. The most important parts of this process are:

- Voicing one's investment,
- Creating transparency about publication strategies,
- Mutually recognising each other's goals,
- Building flexibility into the process, and
- Establishing commonly accepted criteria for making these basic decisions

6.1.4 The Pre-submission Lifecycle

There are several aspects to consider way before you actually submit a manuscript for consideration for publication. One of the most important decisions in the pre-submission stage is the **target outlet**. Where do you want to publish?

The key recommendation here is to choose a format or journal that targets your specific audience and to target the highest impact venue possible. However, be realistic in doing so – do not waste time submitting to journals that will not publish your work. Often, this decision is hard to make for students or junior researchers without much experience. Thus, consult with your supervisor or senior colleagues that have experiences with different outlets. In some cases, you may be constrained to lower tier venues owing to factors such as size of manuscript (some outlets have word or page limits that may or may not gel well with the type of study conducted), required speed of publication (some journal publishing processes take years, and some ideas may deserve expedited publication), or the availability of special volumes or proceedings.

Points to consider in making target outlet decisions include:

- What journals did you read in doing the research? Typically, journals have more or less explicit favourite domains of interest where they often publish papers. You can typically tell from your own references list to see which journals tend to favour the work that your work builds upon.
- How well do you know the outlet? Check the written descriptions of target areas on the conference or journal's web site, and on occasion in the editor's regular introduction. Look at other articles published in the outlet and identify their type and style of work. If in doubt about the appropriateness, email the editor an abstract and ask whether it is appropriate for that journal/conference.
- Did you follow the guidelines? All outlets provide submission guidelines or requirements. Follow format requirements very closely for conferences and somewhat more loosely for journals (this is because journals typically have an editing and typesetting process after an acceptance decision has been made). Identify the word or page limit. It is very common that manuscripts are rejected straightaway for being too long.

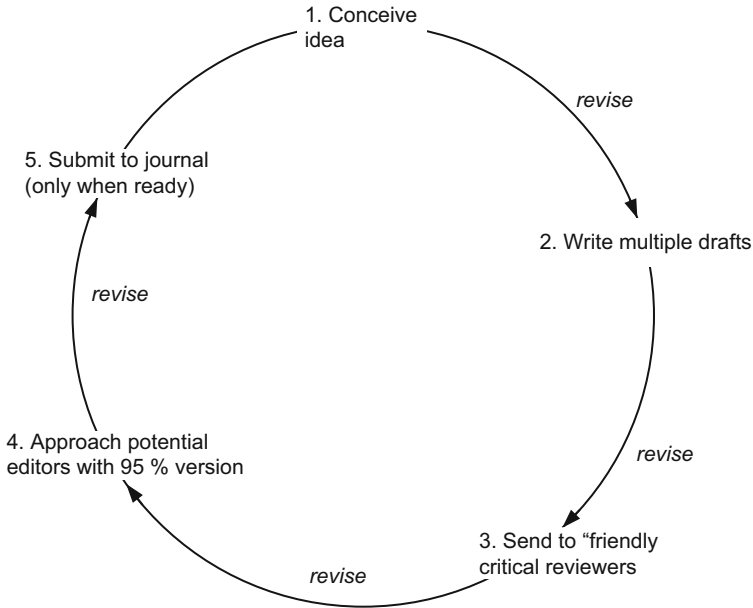


Fig. 6.1 Manuscript pre-submission lifecycle

- What is the audience and pool of reviewers? Who reads the journal and who publishes in the journal? Typically, outlets provide information about their editors and referees. You can check on them to identify their own areas of expertise, favoured theories, and methods. Another good rule of thumb is to identify regular authors in the journal. Outlets tend to recruit referees from the pool of contributors.
- How low should you go? Remember that academic publishing is a one-shot option. You should aim as high as possible. If you get rejected at a venue, you can subsequently aim at a lower target with more relaxed requirements for rigor or quality. If you aim too low, you will never know whether you had a chance at a really good journal. Moreover, because quality of journal publications is so much more important than quantity of journal publications, aiming too low can backfire on your career.

Having identified a suitable target, the next phase consists of carefully threading through the pre-submission lifecycle shown in Fig. 6.1.

The point highlighted by the diagram in Fig. 6.1 is one about revising, honing the writing, and doing it again and again. As I mentioned earlier, good writing will never be enough for getting a paper accepted but it is more than enough to get one rejected! When a paper is ungrammatical, filled with sloppy errors, badly organised, and awkwardly phrased, it only serves to put readers off.

Revising the writing of a manuscript is difficult for several reasons. First, it is difficult to edit your own writing. You will not notice ambiguities and logical gaps because you know what you meant to say and you understand the omitted steps. One strategy for overcoming this difficulty is to put your manuscript aside for a while and then return to it later when it has become less familiar (step 2 in Fig. 6.1).

Because this typically is not enough, you should approach a friendly but critical colleague for review. The best readers are those who have themselves published in the journals you target, but who are unfamiliar with the subject of your article.

If your colleagues (or anyone else, really) find something unclear, do not argue with them. They are right: if they don't get your meaning you didn't convey the meaning appropriately. In truth, your writing is unclear. Their suggestions for revisions in the areas that are unclear may be wrong, but the point about clarity remains. In removing the unclear areas work on the manuscript, don't verbally clarify to the friend. They are not your enemy – but also not your target.

A helpful practice is to obtain pre-submission feedback from within the outlet. Most journal editors will be kind enough to give you early feedback prior to formally submitting the manuscript. They can comment on coarse flaws as well as suitability of the manuscript to the journal. Of course, they will not make any statement about the likelihood of publication success.

In my experience, it can take up to a year after the research work has been concluded before a manuscript is submitted. I am not saying it needs to take this long and I certainly don't want to put you off. I merely want to draw your attention to how critical a polished manuscript is and how important is the effort that goes into it well before submission. The key principle, as others have called it, is to avoid “hot off the press” manuscript submissions – simply because a quick writing exercise increases the likelihood of errors in presenting your research work. Moreover, having a paper rejected not because the research wasn't good enough but instead because of the way in which the research was represented in the manuscript is a terrible experience.

Once all the iterations of the pre-submission lifecycle are complete, hopefully, you have a draft that has the best of chances of successfully traversing the reviewing process. You can feel assured if you have worked by the following guidelines:

1. Writing is a process and it takes time. Dedicate resources to this task. Include it in your time and project management and make it a priority.
2. Writing is a skill that needs to be developed and honed. Therefore, do it as often and as intensively as possible! Learn to write conference papers, then go on to write journal papers, then learn to write books, and thus gradually increase your ability and efficacy in writing.
3. Review papers to learn how to publish in good journals. You can gain so much from reading the best and the worst in writing. The best are those published, but you can learn a great deal from working on manuscripts that may never be published – and understanding why.
4. Write, revise, give it to someone to read, put in the drawer for a month, and then start over again!

6.2 Structure and Contents

Concerning how to write a manuscript, there is actually a wonderful solution available. Very simply, the best suggestion is to commonly help yourself by finding a good published paper that documents research similar to that that you are doing and use it as a template to help you structure your own paper. This advice is easy to follow and incredibly valuable. Reviewers, editors, and readers alike value familiarity in the “plot” of a paper, the same way that novels, movies or other stories regularly follow a similar genetic structure.

The type of structure your manuscript requires will vary depending on the nature of the field you are publishing in (Information Systems versus Computer Science versus Management), and on the type of research that you are reporting on (case study versus experiment versus design science). Still, the general writing process should look as follows:

1. Develop a structure.
2. Revise the structure.
3. Start populating the sections.

This typically is not sequential. Indeed, I find the first paragraph of the Introduction is the hardest part of a paper to write. Similarly, the “methods” section (at least in quantitative studies) is the easiest and can be started even when the research is unfinished.

4. Revise the sections.
5. Revise the paper/thesis.
6. Put it in a drawer, take it out, read it again, revise it again (remember?).
7. Pre-submit to a friendly reviewer, pre-submit to an editor, and finally actually submit the paper.

While papers vary somewhat across methodologies and fields (for example, some fields may use different headings or sections), you will find that most ‘regular’ research papers in information systems research adhere to the following structure shown in Table 6.4 that we will discuss below. In Table 6.4, I included a number of examples from some of my papers to show how this generic structure can be instantiated in papers. You can see how the actual paper structures often very closely resemble the generic structure shown in the left-most column. In fact, I would go as far as stating that the best option is to replicate as close as possible the “standard” paper structure instead of creatively inventing new structures for academic papers. You will realise very early and very often that innovative structures are not well appreciated in the academic community, most notably because they distract from the content.

Table 6.4 Paper structure with examples

	Example I	Example II	Example III
Generic paper structure	[139], published in the <i>MIS Quarterly</i>	[134], published in <i>Information & Management</i>	[133], published in the <i>European Journal of Information Systems</i>
1. Introduction	1. Introduction	1. Introduction	1. Introduction
2. Theoretical background/ literature review	2. Theory	2. Background and research models	2. Background
3. Research model (where appropriate)	3. Proposition development		3. Theory and hypotheses
4. Procedure/ approach/ methodology/ research design	4. Research method	3. Research method 4. Operationalisation and validation	4. Method 5. Operationalisation and validation
5. Results	5. Scale validation 6. Results	5. Data analysis and results	6. Data analysis and results
6. Discussion of results	7. Discussion (including limitations and implications)		7. Discussion
7. Implications – for research and practice		6. Conclusions (including contributions, limitations and implications)	8. Future research 9. Practical implications
8. Conclusions	8. Conclusions		10. Conclusions

6.2.1 Introduction

Ideally, your introduction should be planned, if not partly written, before the research commences. This is because the introduction should spell out motivation, justification, and approach – all of which are important elements of the research design (consider Chap. 3 above). Still, I find that I often significantly rework an introduction after the rest of the paper is spelled out. It is easier to motivate and specify the content of a paper once you have a firm and fixed understanding of what that content actually is.

The introduction is hard to write mostly because it is not the quantity but the quality of an introduction that matters. An introduction section needs to be brief. More precisely, it should contain three parts, and three parts only:

- 1. **Motivation:** What is your problem, and why is it a problem?
- 2. **Specification:** what is your specific research question/research outcome? What is your research approach (in very general terms)?
- 3. **Outline:** how will the paper tell us about what you did?

Throughout these three parts, the introduction must define the problem the research will address and explicitly state your hypotheses (where appropriate). It

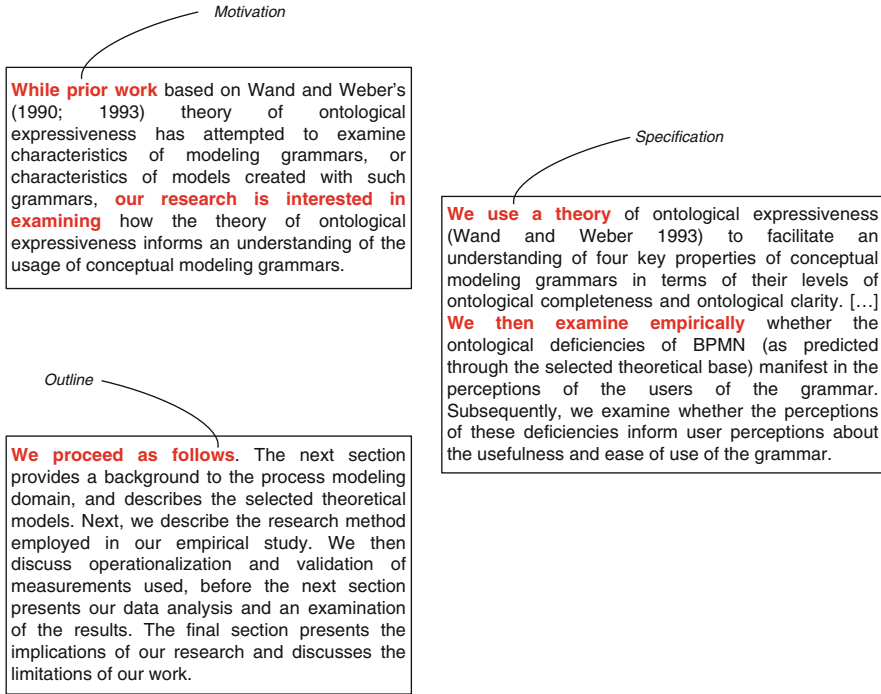


Fig. 6.2 Excerpts from paper introductions

must place the study in the context of previous research but only to the extent that the reader needs to know to understand the present work. That means, the introduction must define the current state of knowledge and thus the gap in knowledge. It must convey enough information to identify your research approach and state (briefly) why your results are significant.

Figure 6.2 gives three examples for motivation, specification, and outline (highlighted red), by showing excerpts from some of my papers [134, 136, 139]. Independent from what the papers or the reported studies are about, note how in the examples, I use only one or two key phrases to achieve three objectives: the studies are distinguished from other work in the field, how the general approach to the study was carried out, and how the paper unfolds.

6.2.2 Background

The background section must provide what is required to facilitate an understanding of your research processes and findings. That is, it must discuss relevant theories that are the foundation for your study, relevant concepts or terms that you use, and relevant previous methods/algorithms/findings/arguments that your work builds on. A good piece of advice is to concentrate only on literature

- That led you to investigate the problem,
- That puts your findings into context, and/or
- That sets up your argument.

Importantly, be reminded what a background section *is not*:

- A recap of all other works in the area,
- Criticism of all other works in the area, or
- A collection of term definitions.

I should note here two important differences between background sections in works that are more mainstream ‘behavioural-oriented’ research in information systems, and the more ‘design-oriented’ research in information systems:

In behavioural-oriented research, the background section typically sets the scene for the method and results to come, typically after the introduction section. For instance, it introduces the theoretical lens through which the empirical results will later be interpreted.

In design-oriented research (that is published in computer science-favoured outlets), the background section (most often called “related work”) often contrasts the method/algorithm/implementation to other works. In such cases, it is often placed after the details about the design work are given, typically before the conclusions section. For instance, it compares features of a prototype to similar system prototypes that exist.

In writing a background section, follow these pieces of advice:

- Organise the section according to topics
- Discuss, don’t just list related work
- Don’t exaggerate differences and instead explain how your work complements others’ work.
- Don’t be afraid if your work contradicts previous work.
- Highlight areas in which your work builds on others.
- Most importantly, keep it concise and precise.

6.2.3 Research Model

The research model (or hypothesis/theory development) section is a special section in that it typically applies only to empirical (and most notably quantitative papers). In such research, it provides an overview of the factors being studied in an empirical setting, such as the independent and dependent factors in an experiment, the important theoretical constructs and their relationships in a survey, or the set of hypotheses or propositions to be tested, explored or falsified in a case study.

The section should be organised by first providing an overview of the conceptual or research model that is being developed. Where appropriate, subheadings should reflect the model. Key references should be worked into the discussions of key constructs, and, importantly, each hypothesis should be preceded by a strong argument as to why it should hold (to build a justificatory mechanism for the theory, see Chap. 4).

The central part of this section is the development of hypotheses. Keep several things in mind: A hypothesis must contain a justified theoretical argument for why you expect a certain phenomenon to occur (or not). Simply put, there is no such thing as a self-evident hypothesis or a hypothesis that you explore “because you can”.

Connect your hypotheses to existing research. Include references to key literature in building your argument – and this literature should have been discussed in your background section.

Keep your hypotheses simple but precise. They should specify two (or more) key constructs and the relationship between them. Identify dependent and independent variables and the direction of expected relationships. Operationalise the variables in a hypothesis in such a way that they can clearly be mapped to data variables (and values) that you have collected and that will be discussed in the “research method” section that follows.

In specifying a hypothesis, do not use words like “prove”. Instead use words like “suggest” or “support” (because hypotheses are never proven, only supported).

The following excerpt gives you an example for the reasoning that led to the development of a hypothesis, that is, the justificatory mechanism that led to a hypothesis:

Model interpretation occurs in two stages [196]: perceptual processing (seeing), and cognitive processing (understanding). The perceptual processing stage is extremely important due to a potential computational offloading effect of graphical information in a diagrammatic model: model elements that can be perceptually discriminated reduce the processing burden of the cognitive system because the parsing of model elements to different semantic components is performed by the perceptual sensors [197]. **Several studies suggest that perceptual processing largely explains differences in the effectiveness of the subsequent cognitive processes** [198, 199].

These considerations suggest that **syntactical effects** (the perceptual discriminability effect of an explicit visual syntax of gateway constructs) **will dominate semantic effects** (the reduced ontological clarity of the process model due to the presence of visual gateway constructs). **This is because** the perceptual discriminability effect of gateway construct occurs before the ontological clarity effect occurs.

6.2.4 Research Method

In this section, you should explicitly state how the research was done. Ideally, the objective of the section is to describe the research carried out in such a way that anyone should be able to repeat your work based on your description – to meet the requirement of replicability. Key elements in this section pertain to the selection and justification of a *research strategy*, the identification of *materials*, *case sites*, *scope of survey*, appropriate *samples*, *participant selection* and all other decisions pertinent to the research design. Other key elements include the detailed and precise description of the *analytical measurements* or *data collection techniques* employed as well as the *data analysis techniques* in use – both in qualitative and quantitative studies.

I said above that in most quantitative studies, this section is rather easy to write. This is because instrument development and site/participant selection typically follows similar guidelines and criteria. In consequence, you can write this section easily by following a favourite example of yours. In addition, you should try to avoid writing up this section in a very unique or dissimilar way – most reviewers are accustomed to a particular style of reporting and expect certain content and structure.

6.2.5 Results

This section again is easy to write, and quite methodical – check published papers that use a method similar to yours.

Technically, the results section “drives” a paper, in that it describes the evidence gathered to test or evaluate the study hypotheses or propositions. Hence, be sure that your results are set out in such a way that they serve as a test for your hypotheses.

The important thing to remember about a results section is that it only contains a *description* of findings. That is, it should contain just factual result reporting (typically written in past tense), including appropriate statistical tests or other analyses – but no discussion or interpretation of the findings. In other words, this section is about telling the reader “this is what we found” but not “this is what that means”.

Writing, statistics, and graphs should be presented as simply as possible. Try to make figure legends self-contained so that reference to the text is unnecessary. Do not present irrelevant data to which the discussion that ensues will not refer.

6.2.6 Discussion

This section is where the paper becomes interesting. In fact, most readers tend to skim through an introduction, glimpse at some graphs and tables in the results section – and read the discussion section in detail. It is where we interpret the data gathered and the findings reported above. This is where we make sense of the results, by exploring what they indicate, how they relate to our theoretical expectations, and how they inform us.

A discussion section should begin by *summarising the main findings*. This should be done in one, or at most two, short sentences. Then, *interpret the findings*: What do the results actually mean? Why did you get the results that you obtained? What did they tell you in light of the theory you set out to evaluate? To that end, focus on three key activities: *explaining* the results, *abstracting* the results, and *theorising* about the results.

One key mistake often made is not to confine the discussion to the results, rather going beyond the data. In such papers, interpretation and explanation is replaced by speculation – which in its essence is arbitrary, and subjective, and thus not preferred in scientific work.

A good discussion section also contains **reflection**. Reflection means relating the findings back to the initial problem the paper set out to do. This should come early in a discussion section.

Additionally, you can interpret the findings against related studies published in the literature. Do your results confirm existing theories? Falsify them? Do they serve as supporting or contrarian arguments to claims or arguments made in the literature? This step is essential to show how your research adds to, complements, or clarifies the current body of knowledge. You may be able to explain inconsistencies found in prior work through the findings you have uncovered. Or you can perhaps improve a theoretical explanation of some phenomenon based on the results obtained.

Finally, be realistic as well as cautious. Do not over-interpret the results. Often, findings are tentative and suggestive rather than conclusive in nature – due to limitations inherent in the data collection or analysis.

6.2.7 *Implications*

Research should inform future studies and the actual practice in the domain being studied. Therefore, good journals demand specific and explicit implications for ongoing research and practice.

Implications for practice detail how the study findings change or otherwise impact the way that important stakeholders work in actual practice. Often, the findings from a study inform a framework, or help form a set of guidelines for certain practices surrounding a particular phenomenon, such as a piece of technology being investigated. It is good practice to identify the type of stakeholders being the primary addressees of the implications. Are your implications addressed at managers, executive, end users, or customers?

Implications for research are not to be confused with ‘future research’ that is often a component in conference papers. The latter is a mere description of how the research presented at the conference will be continued after the presentation, to give an indication as to the type of findings that can be expected in the future. Journal papers (or books, typically) describe completed research. These manuscripts should detail what we have found through execution of the study reported that can guide future studies or other scholars. Examples include a new measurement instrument that was developed for the study and is now available to other researchers. It could be a new theory that was developed and is now awaiting further exploration, application, or evaluation. Another set of implications could relate to potential

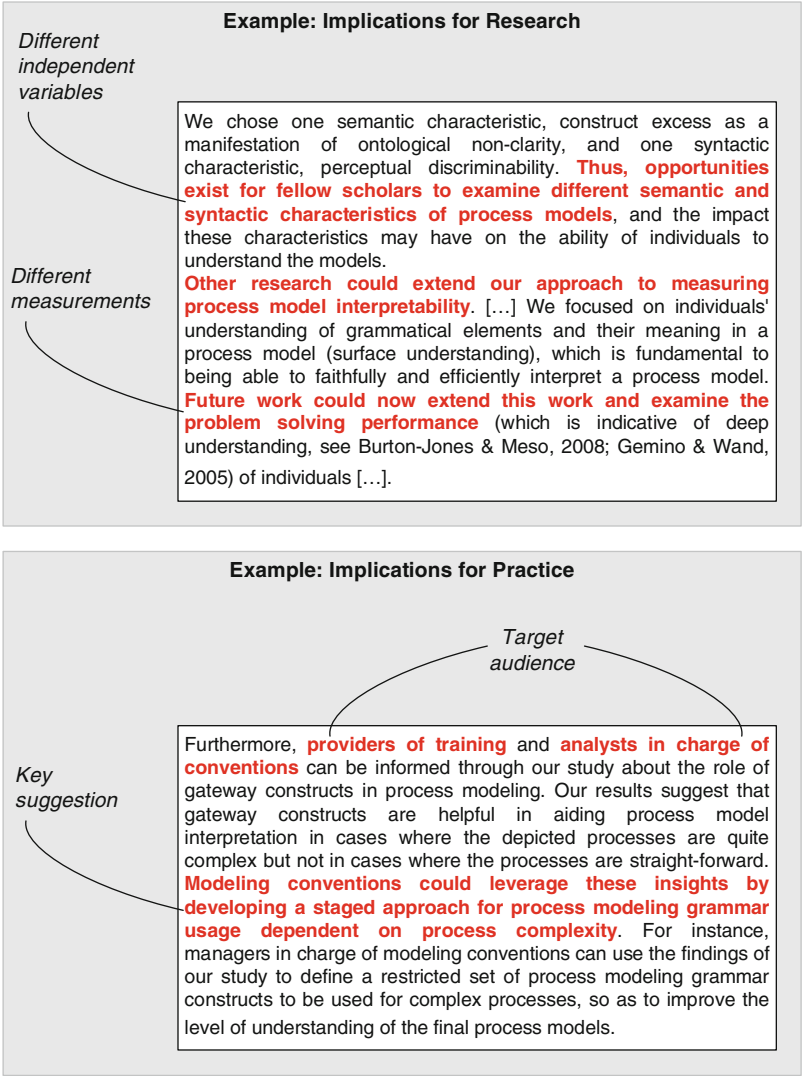


Fig. 6.3 Examples for implications for research and practice

extensions of the study that might explore, for example, important boundary conditions of a theory being developed, or the investigation of some inconsistencies uncovered in collected data.

Figure 6.3 gives two examples for writing Implications sections, with some relevant key arguments being highlighted red. The top part shows an excerpt for an “Implications for Research” section, while the bottom part shows an excerpt for “Implications for Practice”.

6.2.8 Conclusions

The conclusions section provides the closing frame to a paper. Very importantly, this section is short! It recaps very briefly the key contributions of the paper, without going into detail. It provides a reflection about the research presented in the paper, *without* introducing new material or findings.

In this section, identify the key findings of your paper *and* relate them back to the introduction. If this is not possible, either rewrite the introduction or the conclusions. Highlight the specific significance of the work and how it provides a way forward for academia and practice.

Again, I would like to present an example for a conclusions section that I believe encapsulates the above principles of *summary*, *recap*, and *reflection*. Note how the whole section covers only four statements (and some would argue this to be a lengthy conclusion). The example is taken from [136]:

In this study, we contribute to process modelling research by providing a theoretical and empirical analysis of the ontological deficiencies of the BPMN grammar. **Our study provides theoretical and empirical evidence** on the strengths and weaknesses of using BPMN, and it also discusses a range of contextual factors that impact the use of BPMN. These **findings assist an understanding** about capabilities and issues in the use of BPMN, and, in turn, they can guide executive decision makers in their investment decisions when choosing to implement a process modelling initiative in an organisation. In summation, our study has uncovered a rich and contextualised understanding of usage behaviour associated with process modelling grammars. It has provided also further evidence for the utility of the ontological theory of grammar expressiveness to aid our understanding of process modelling practice.

6.2.9 Abstract

Finally, papers typically contain an abstract – somewhere between 100 and 300 words long – that serves as an informative summary of the paper and is used to entice people to read the full paper (“pull your audience in”) but also to summarise the main insights (“give your audience the necessary and sufficient information”).

You must make your abstract informative – the reality is that more people will read the abstract than any other part of the paper, and many people will read no further.

The short length of an abstract requires you to write clearly and concisely. What you should include in the abstract is

- A statement of the problem being addressed,
- A statement of the approach/methods,
- A synopsis of the results,
- Your key interpretations of the results, and
- The major implications of the results and conclusions.

6.3 Handling Reviews and Revisions

6.3.1 *Understanding Reviews*

Peer review is a very important process in any academic profession. It ensures that certain quality standards are met, and that the quality of research and publication work being done in the profession is of a high standard. When that standard is not met, reviewers or peers are entitled to reject the work outright, or to ask for further research or work on its presentation in a manuscript to be completed before a decision can be made. Typically, this means a second version (a revision) will be reviewed once again. This process means that nothing average or passable makes it through. If such work is accepted and becomes common practice, standards slip and the profession as a whole suffers.

I described above how submissions are nowadays commonly done by electronic upload (but hard copy submissions still occur). An editor will typically pre-screen a paper to gauge the general suitability of the manuscript for the outlet, and to quickly pre-assess the viability of a manuscript for traversing the review process. If a paper is in much misalignment with the key focus of the outlet (for instance a chemistry submission in a management journal), or of very bad quality, the editor can desk-reject a paper without even sending it out for review. This stage is done to reduce the significant workload of reviewers, who often (at least in information systems research) are performing their duties voluntarily – a significant commitment of time and effort.

After this stage, as described, the manuscript will generally be sent to two or more reviewers. They are asked about various (and varying) characteristics of the manuscript, such as significance of contribution, quality of the methods, data analysis, reasoning, results and conclusions, whether it is up-to date with the literature, whether it is well written, etcetera.

The time passed until a review is received varies from outlet to outlet. Good journals tend to provide reviews and decisions within 2–4 months after submission. You will find that “lower” journals are also characterised by a review process that is not so efficient (or effective). I had cases of reviews being provided within a couple of weeks, all the way up to waiting for 14 months for reviews. I am yet to discover a pattern – I’ve had very quick positive reviews and outright rejections. I have waited for a year or more to learn about acceptance of a paper – and I received a rejection more than a year later. Luckily, most outlets nowadays employ manuscript submission systems such as ScholarOne, which allow authors, reviewers, and editors alike to monitor the status and progress on a paper.

The outcomes of the review process can be generally be classified into five types:

1. **Reject:** Your manuscript is denied publication. The outlet will not consider this manuscript or any reworked version of it. This is obviously the most disappointing outcome – and a final one at that.

2. **Revise and resubmit.** The manuscript is denied publication, but the editors and reviewers feel that a significantly reworked manuscript (which basically means, scrap the manuscript and start writing all over again) may be submitted to the journal again as a new submission. This resubmission is most often handled by a new editor and a fresh set of reviewers.
3. **Major revisions required.** The editors and reviewers see potential in the research conducted, however, the research as presented in the manuscript cannot be published as is, and major revisions are required. These revisions can relate to writing (introduction, motivation, results presentation, discussion) or also to the research (different analysis, collection of additional data, (re-) consideration of theory).
4. **Minor revisions required:** The editors and reviewers feel that the manuscript is quite strong already but have some comments, issues, and suggestions for further improving the manuscript. Typically, a manuscript is well underway towards publication with such a review outcome.
5. **Acceptance or conditional acceptance:** This is good news, meaning that the editor decides that a paper can be published as-is, or with very minor changes that do not require further review.

The five above outcomes are not only ordered from worst to best, but also probably by frequency of occurrence. Paper acceptance rates are very low, around 10% of submissions for the best journals (which means that 90 out of 100 submissions get rejected). I am no different as are virtually all of my colleagues. Many, many papers are rejected. Virtually all reviews on an initial (so called “first-round”) submission demand major revisions or resubmission (if they are not rejected). In fact, a common joke is that (a) there is no acceptance in the first round, and (b) a minor revision is like an acceptance and thus rule (a) applies.

I found that the best attitude towards review is the “we are in the game unless they kick us out”. This means, a revise and resubmit or a major revision is good news. You are still in the game. They have *not* decided that your manuscript cannot be published. With this attitude, seemingly harsh reviews (and major revisions are harsh) are much easier to handle. It is still up to you to improve the manuscript so that it can be published.

One of the common myths in the publishing process is that reviewers want to reject your paper. This is simply not true. Good journals are good because they carefully select very good reviewers. In fact, they build their esteem on the reviewing process. In my experience, good journals have good reviewers that provide good reviews. Good reviews are characterised by clear and precise criticism accompanied by constructive advice. Such reviewers want to see their advice taken seriously, and rejections based on good reviews are probably the best thing ever to happen.

6.3.2 *Managing Revisions*

The key rule to handling reviews is not to be precious. Your objective is to get published. Pride or arrogance will not get you there. If reviewers and editors find flaws in your work then there are flaws in your work that you need to rectify. It is not their fault; it is yours. I hear many comments of the nature “this guy clearly didn’t understand what I was doing”. It’s not his fault. It’s yours. You did not communicate it well enough for him to understand.

If you do get a rejection, determine why. The editor and reviewers will give you plenty of reasons. In considering the future of the paper, attempt to fix the flaws identified in the rejection. Do not resubmit the same flawed manuscript somewhere else. This would be a waste of time and effort – if the research has flaws reviewers will spot it.

If you are asked for revisions (major or minor), be aware that handling these is a challenge that I often find harder than the initial writing of the manuscript. Very often, you get plenty of reviews – probably upward of tens of pages of detailed comments from editors and several reviewers. My highlight so far, I think, were 27 single-spaced pages of review comments in a first round that was labelled a major revision. The revision took 10 months, and led to two more rounds of reviews and revision – but the paper got published!

In managing revisions, I learned the following over the years:

1. **Put the review aside for a while.** Given that virtually all initial reviews are critical (and you will very rarely be accepted straightaway – it only happened to me once), you will probably initially have some strong negative reactions to a review. The comments sound unfairly harsh, virtually impossible to achieve and it’s a mountain of work to be done. In this state of mind, your revisions will be affected by your emotions. Therefore, put the reviews aside, and read them again later (after several days). Believe me, they will look more manageable.
2. **Read the reviews.** Make sure that you truly and fully understand each and every comment. Read the reviews several times. Do not start revising, or editing, or responding as yet. You need to develop a good grasp on each comment as well as the overall tenet that the reviews provide. This is important to develop a strategy that will allow you to handle each comment but also all comments together.
3. **Tabularise the reviews.** A very simple yet utterly effective tool is to create a table for each of the editors and reviewers and copy and paste each “block” of review comments into a separate row. Have three columns, one for the comment, one for the response, and one for notes and discussion within the research team. I start tabularising early (while reading), so I can better dissect the different issues. After several rounds of reading, I then start jotting down thoughts and strategies about the comments, and marking them as “quick fixes” (easy to handle) and “major pieces of work”. I often also indicate the type of revision required: (a) change the research, (b) change the presentation of research, or (c) a suggestion that cannot be handled given the research conducted. Over time, the responses column can start to be populated until the table is complete.

4. **Revise the paper.** At some stage or another, changes will have to be made to the document. Depending on the type of revision (whether minor or major), the changes will be more, or less, ranging from changing/inserting/deleting paragraphs/figures/tables of some sections, to writing a completely new manuscript. The rule here is that you must not be afraid to make big changes to a manuscript. A major revision should always look like a major revision. Some scholars suggest completely re-writing a paper from scratch for major revisions – and in my experience this can often be necessary. Several of my papers look vastly different from the initial manuscript that was submitted.
5. **Write a response letter.** Typically, editors and reviewers demand point-by-point responses to all comments made. This is because it makes it easier for them to trace the changes to a manuscript, especially if many revisions were required. You can write the response letter by perusing the revision table (now only including the comments and response column). Start with a pre-amble that expresses your gratitude towards the editor and reviewers for considering your manuscript and investing their time to give you feedback and assisting you to improve your work. You should also outline the strategy you pursued in your revision, that is, you should summarise the main changes.

In the response letter, I find that typically two strategies can be used – the intensive or the extensive response. In *extensive responses*, you develop key arguments for or against the point made in the comments. You delineate the change strategies you considered, and which worked (or not). You include quotes from the revised paper to show where/how you made a revision. As you can see, the extensive response strategy is a very detailed account of your revision process. Some reviewers appreciate this. Others, I found, are disinclined because these responses tend to get lengthy (my personal record is 84 pages of responses) and they also need to consider the manuscript! By contrast, *intensive responses* are short, pointed, and precise. State in few phrases your verdict about the comment and how you handled it.

In your response letter, make sure you included pointers (page numbers) to where changes were made in the manuscript. This will make it easier for the reviewers to judge their comments and the related responses and reviews.

In concluding this section, I want to highlight that in my experience the task of revision is more challenging the better the journal. Much effort is required to successfully traverse revisions, and success is never guaranteed. Do not forget, a major (or even a minor) revision never guarantees eventual acceptance. Like many other colleagues, I have had papers rejected after four rounds of reviews.

Having said that, I can still say that the first round of reviews is the most critical stage. At that stage, reviewers and editors either see promise in your paper or they don't. If they do, you stand a chance of turning the paper around in such a way that the promise is fulfilled.

As a final piece of advice, ensure that your tone in the revision and response also remains polite. No one is attacking you personally, or criticising you personally. Firing arguments at the reviewers will not change their opinion or their impression

of you or your work. This is not to say that there will never be instances where you disagree with the reviewer (which is fine), or where the suggested changes are impossible (which is also fine). In such cases you can – politely – argue against a reviewer, or clearly state why you will not pursue the change requested.

Still, with all the hurdles and challenges of reviews and revisions, it will be an utterly gratifying and exciting experience when you realise how a manuscript progresses and improves over the rounds of reviews until publication. In all my published papers the final versions were greatly improved compared to the initial manuscripts. Moreover, the gratification that comes from this experience is not limited to the authors alone. Having been involved in the publishing process also as reviewer or responsible editor, I can honestly say that it is an immensely enjoyable experience to see a paper that you reviewed or edited get better over time and finally become published.

6.4 Further Reading

There are many very useful articles available giving advice on how to write a paper. Below I give some sources about different elements related to academic writing.

In his book on academic success, Viswanath Venkatesh [174] has a dedicated chapter on paper writing (Chap. 4) that makes for a valuable read (no wonder, given his incredible success in terms of paper publishing). Another good source is Antoinette Wilkinson's guide to scientific writing [192]. Also, the book by Ad Lagendijk [99] is a valuable source, especially in terms of specific suggestions for developing statements that show strong and clear argumentation.

The following sources contain good advice for different publication-related challenges:

- Writing about theory: [191], [187]
- Publishing tips from an editor's perspective: [17, 144, 147, 164, 165]
- Studies of publication practices: [108]
- Tips about paper reviewing: [43, 103]
- Tips about manuscript writing: [15, 62, 157, 158]
- Tips about reviewing literature: [184, 188]

Finally, a good source of inspiration about how to handle reviews in a revision is to learn from the best. The MIS Quarterly once published an article [91] about the use of online wikis in the academic profession. As part of this research, they decided – together with the editor and reviewers assigned to their manuscript – to conduct the reviewing process based on wikis. Being a wiki, the information about the manuscript versions, reviewer comments and author responses is publicly available at <https://www.socialtext.net/misq5040/index.cgi>. Aside from being an interesting study in itself, this case also presents a good example to learn from about the nature, tone, and content of reviews and how they can be handled. The related paper is due to be published in the MIS Quarterly in 2012 [79].

Chapter 7

Ethical Considerations in Research

7.1 The Role of Ethics in Research

In this part of the book, I want to draw your attention to ethical considerations as they pertain to research in information systems. Ethics form a branch of philosophy that seeks to address questions about morality; that is, about concepts such as good and bad, right and wrong, justice, and virtue. Ethics define the principles of right and wrong conduct in a community or profession, and can be used by individuals acting as free moral agents to make choices to guide their behaviour. Ethical behaviour is defined as those actions that abide by rules of responsibility, accountability, liability, and due process:

- *Responsibility* means accepting the potential costs, duties, and obligations of one's decisions.
- *Accountability* consists of the mechanisms for accessing responsibility for decisions made and actions taken.
- *Liability* refers to the existence of laws that permit individuals to recover the damages done to them by other actors, systems, or organisations.
- *Due process* requires that laws are known and understood by all, and that individuals can appeal to higher authorities to ensure laws were properly applied.

Like all other communities, science as a profession requires ethical standards as to what is acceptable and unacceptable behaviour in the conduct and publication of research. To illustrate why ethics play a specifically prominent role in research, let us explore a case of scientific misconduct that became known as the Schön scandal.

The Schön scandal concerns German physicist Jan Hendrik Schön who briefly rose to prominence after a series of apparent breakthroughs with semiconductors that were later discovered to be fraudulent. Schön's field of research was condensed matter physics and nanotechnology, and he rose to prominence when he discovered spectacular findings about the use of organic materials in lasers or for the display of superconductivity. He published widely and extensively (at some stage, a new publication every 8 days!) in top journals such as *Nature* or *Science*, and gained

worldwide attention. However, no research group anywhere in the world succeeded in reproducing the results claimed by Schön.

Upon attempting to replicate the experiments, over time, scholars came to note anomalies in the experiments conducted by Schön. For instance, several papers described the exact same “data noise” in experiments using different parameters and settings. Later, examples of the use of duplicated data in experiments were found. This led to a formal investigation during which it was uncovered that Schön had erased all raw-data files from the experiments from his computer. According to Schön the files were erased because his computer had limited hard drive space. In addition, all of his experimental samples had been discarded, or damaged beyond repair.

The investigation report found at least 16 cases of scientific misconduct by Schön. His co-authors were relieved from the allegations. This sparked widespread debate in the scientific community on how the blame for misconduct should be shared among co-authors, particularly when they shared a significant part of the credit.

After the investigation, Schön acknowledged that the data were incorrect in many of these papers and claimed that the substitutions could have occurred by honest mistake. He admitted to having falsified some data and stated he did so to show more convincing evidence for the behaviours he observed. His doctoral degree was revoked. Needless to say, subsequent experiments failed to obtain results similar to Schön’s findings.

If you think such cases of ethically unacceptable research behaviours are rare or in the past, I must tell you that science historically has been hit by cases where data or results have been manipulated or where the researchers conducted their studies in harmful ways. Prominent examples include:

- The Nazi experiments on prisoners during World War II, especially concerning twins, matter transplantation, and modelling of battlefield wounds.
- The Stanford prison experiment in which volunteers were randomly assigned roles as guards or inmates, and which led to outbreaks in sadism and psychological terror.
- The case of German minister Karl-Theodor zu Guttenberg who had his doctoral degree revoked and who resigned from his post as national defence minister in 2011 after it was released that his doctoral thesis contained significant cases of plagiarism.
- The Piltdown man case, where scientists reported the find of hominid remains in Sussex in 1912, which was later proven as a skull of the modern man together with a jawbone from an orang-utan.
- William Summerlin who in 1974 researched tissue rejection and performed experiments that included transplanting fur from a black mouse onto a white mouse. Later it was found the black fur was actually drawn with a black marker pen.
- Vijay Soman and Philip Felig who in 1978 were involved in case where they copied sections of a paper sent to them for review into a “new” manuscript submission of their own, together with additional material later shown to be made up.

These cases are but a few examples that point to one story: research as a profession is susceptible to misbehaviour and misconduct in the same way as any other community of practice. Arguably, the cases above are severe and often resulted in legal actions such as revoking of degrees, stripping of prestigious positions, cancellation of research funding, and in some cases even imprisonment. Other cases of ethical issues may not necessarily lead to legal action yet still bear consequences in terms of professional reputation, standing, or even job security.

We want to focus on the two key research activities in which ethical guidelines should be considered, namely research conduct and research publication.

7.2 Ethical Issues in Conducting Research

The behaviours involved in the research process are subject to ethical considerations (both codified and uncoded), especially those related to empirical data collection and analysis. For example, the majority of universities, especially in the United States and Australia, demand that research projects which involve human subjects must undergo formal approval by an institutional review board. For example, in Australia, research involving the participation of humans must be undertaken in accordance with the National Statement on Ethical Conduct in Human Research. The purpose of the Statement is to promote ethical conduct in all aspects of human research. A research proposal must be submitted for the appropriate level of University ethical review, and the proponent must receive notification of approval before the proposal may commence. While information systems research in most cases does not present potential harm to participants in the same way that biological or medical studies may (for example, by testing the effects of a newly developed drug), the review board will still consider the extent to which participation in a study is voluntary, is not exerting physical or psychological stress or damage to participants, and how the data will be protected in terms of anonymity or confidentiality, as required.

With information systems research being a social science, a key ethical principle in research conduct is the need to be aware of having the responsibility to secure the actual permission and interests of all those involved in the study. We should not misuse any of the information discovered, and there should be a certain moral responsibility maintained towards the participants. We have a duty to protect the rights of people in the study as well as their privacy and sensitivity. The confidentiality of those involved in the observation must be carried out, keeping their anonymity and privacy secure.

In some cases, the principle of **anonymity** (individuals cannot be identified based on the data collected) may run against the purpose of the study (for example, in face-to-face interviews, anonymity is hard to preserve). In such cases, an ethical review arrangement (or the researchers themselves without any guide), must take appropriate actions to ensure that while anonymity cannot be guaranteed, **confidentiality** of the data must be maintained. Confidentiality means that the identity of

a participant cannot be identified from any form of research disclosure (such as a report, paper or presentation).

Typically, studies involve **voluntary participation** by human subjects. Voluntary participation means that people are free to choose whether or not to participate in a study, without any consequence. As a good example, consider a study in which we promise students bonus marks as a reward for participating in an experiment. Students that opt not to partake in a classroom study must still have the opportunity of receiving bonus marks in the same manner as participants (e.g., through an alternative task).

Voluntary participation further means clarifying the rights of a person to withdraw from a study prior to concluding the study. The information rights of participants also involve communication about **potential risks** to subjects. All these details about a study are typically summarised in an information consent form. One of the information consent forms that I use is available at <http://www.bpm.scitech.qut.edu.au/surveys/bpmn/>.

Ethical considerations to research conduct also involve standards for the **storage and analysis of data** (to allow for expedited resolution of cases such as the Schön scandal). Generally speaking, all research data, including primary materials and raw data such as survey questionnaires, measurements, recordings and computer results must be stored in suitable and durable storage facilities. The minimum recommended period for retention of research data and primary materials is 5 years from the date of publication, however this period may vary according to the nature of the research. Key elements to consider are typically ownership (especially in cross-institutional collaboration), storage and backup, privacy and confidentiality, access and re-use.

Regarding data analysis, ethical obligations refer to honest and complete reporting of how data is analysed and reported. Negative or undesired results still require full disclosure, if they run against the premise of the paper or the general research design. Other unethical data analysis behaviours include

- Evaluating hypotheses based on partial, incomplete or improper data analysis,
- Segmenting data for analysis to lend better support for hypotheses, or
- Generating hypotheses ex-post based on results from data analysis.

7.3 Ethical Issues in Writing

Ethical issues in writing most notably relate to challenges of plagiarism, recognition of co-author contributions, honest reporting, and the appropriate use of language.

Plagiarism is the wrongful appropriation, close imitation, or purloining and publication of another author's language, thoughts, ideas, or expressions, and the representation of them as one's own original work. **Recognition of co-author contributions** concerns the appropriate acknowledgment of the labour of collaborators and their substantial contributions to a scholarly work. This also involves the co-called practices

Category One

Items in this category must always be adhered to and disregard for them constitutes a serious ethical breach. Serious breaches can result in your expulsion from academic associations, dismissal from your employment, legal action against you, and potentially fatal damage to your academic reputation.

- ✓ Do not plagiarize
- ✓ Do not fabricate or falsify data, research procedures, or data analysis.

Category Two

Items in this category are recommended ethical behaviour. Flagrant disregard of these or other kinds of professional etiquette, while less serious, can result in damage to your reputation, editorial sanctions, professional embarrassment, legal action, and the ill will of your colleagues.

- ✓ Respect the rights of research subjects, particularly their rights to information privacy, and to being informed about the nature of the research and the types of activities in which they will be asked to engage.
- ✓ Do not make misrepresentations to editors and conference program chairs about the originality of papers you submit to them.
- ✓ Do not abuse the authority and responsibility you have been given as an editor, reviewer or supervisor, and ensure that personal relationships do not interfere with your judgement.
- ✓ Declare any material conflict of interest that might interfere with your ability to be objective and impartial when reviewing submissions, grant applications, software, or undertaking work from outside sources.
- ✓ Do not take or use published data of others without acknowledgement, or unpublished data without both permission and acknowledgement.
- ✓ Acknowledge the substantive contributions of all research participants, whether colleagues or students, according to their intellectual contribution.
- ✓ Do not use other people's unpublished writings, information, ideas, concepts or data that you may see as a result of processes such as peer review without permission of the author.
- ✓ Use archival material only in accordance with the rules of the archival source.

Category Three

Items in this category are considered advice on how to protect yourself from authorship disputes, mis-steps, mistakes, and even legal action.

- ✓ Keep the documentation and data necessary to validate your original authorship for each scholarly work with which you are connected.
- ✓ Do not republish old ideas of your own as if they were a new intellectual contribution.
- ✓ Settle data set ownership issues before data compilation.
- ✓ Consult appropriate colleagues if in doubt.

Fig. 7.1 AIS ethical code for information systems researchers

of *gift authorship*, where individuals are presented with co-authorship recognition without having made substantial contributions (as discussed in Chapter Six, above), or *ghost authorship* where contributors are omitted from the list of authors. **Appropriate use of language** refers to the careful wording of reports such that they are not biased against individuals or communities in terms of gender, race, orientation, culture, or any other characteristics. This involves *specificity* (describing participants' behaviours as dominant and chauvinistic instead of "typically male"), *labelling* (referring to countries of population instead of classes of "Hispanics" or "Asians") and *professional acknowledgments* ("medical practitioner" instead of "female doctor").

The Association for Information Systems, the professional community of academics in the information systems field, has released a professional set of ethical standards expected of persons qualified to be members, in relation to research and publication (see <http://home.aisnet.org/displaycommon.cfm?an=1&subarticlenbr=15>). The code has four primary objectives and its rules of behaviour are shown in Fig. 7.1:

1. It provides guidance for people entering the profession of information systems research as to what plagiarism is, and how to avoid committing it; and it is suitable for use as a resource in postgraduate training.
2. It provides a reference-point for members of the profession of information systems research, including guidance on how to interpret the notion of plagiarism, how to evaluate whether a particular act or omission constitutes plagiarism, and if so how serious an instance of plagiarism it is.
3. It provides a basis for consideration by the AIS Research Conduct Committee of instances of possible scholarly misconduct by a member in relation to research and publication activities.
4. It provides a benchmark, recommended by AIS, against which other organisations such as courts, tribunals, employers, and publishers can consider instances of possible scholarly misconduct by information systems professionals in relation to research and publication activities.

7.4 Further Reading

There are two types of key resources for the consideration of ethics. One is the set of ethical standards set by the professional community, the AIS Code of Conduct, which can be found at <http://home.aisnet.org/displaycommon.cfm?an=1&subarticlenbr=15>. A related journal article explaining the process and outcome of the conduct was published in the MIS Quarterly in 2003 [98]. This article also examines potential reasons for engaging in ethically controversial research behaviours.

Needless to say, the debate about the role, content, and impact of these standards is an ongoing process. Gove Allan and co-authors recently published a study about the adherence of IS researchers to these standards and found that focus and stage of

career influenced individual judgments. Based on their study, they developed a set of recommendations relating to the role of ethical norms [2].

There are several reports about ethical issues that are relevant to information systems research. Ned Kock published “A Case of Academic Plagiarism” in 2001 [97], where he was the victim of scientific misconduct. The *Communications of the Association for Information Systems* included a special issue about the ethical issues relating to self-citation practices in journals [74]. A different view on this topic is offered by Roger Clarke [37] in his analysis of plagiarism in academic research settings.

Roger Clarke also maintains a useful web page related to ethics and information systems, which can be accessed at <http://www.rogerclarke.com/SOS/#ISEthics>.

Chapter 8

Instead of a Conclusion

I would like to say that I am writing these final lines on a drizzly Tuesday afternoon while a sharp wind howls outside and rain is splashing against the window at my office at home. That's not true. It's a Wednesday (for one thing), and the weather in Brisbane is gorgeous and sunny as usual. Finally, I tend to work in the office as at home I encounter too many distractions, especially my beautiful and wonderful partner.

In reflecting on this book, I feel a great deal of joy and relief but also nervous expectation. The thought of explicating and externalising experiences, lessons learned, and personal values and beliefs about research has been with me for several years now, and I am grateful that this book presents the opportunity of sharing these thoughts with others. I wish that my observations, ideas, and advice will be good guidance for you, and I hope that you will not have to venture on the same strenuous path as I did to learn these lessons. The nervous expectations I have mostly come from me wondering whether you will find this book useful, and ultimately, whether your research journey has been aided at least a little bit by the pieces of advice I sought to give.

The aim of the book was to lay a foundation for students and young scholars alike upon which to build their professional academic work. It was meant to set the stage for the paths, endeavours – and most importantly future experiences that are yet to follow. I hope it serves you well in painting a comprehensive initial picture of the life of a researcher, and I hope it inspires you to pursue this profession with great enthusiasm, creativity, and dedication.

If there were just one key outcome I would want you to gain from reading this book, it would be to instil in you a desire to learn more about research, and do more research. Scientific work is so challenging but also unique and rewarding because it is a profession of lifelong learning and constant personal development. If this book provides a gateway for you to expand your interests, and entices you to search for more knowledge, then I will be very happy with the outcome.

While you are on this path, never forget that the time spent on learning and doing research – especially during your doctoral studies – is most likely the most stimulating experience of your life. You are about to develop some new pieces of knowledge that humankind has been lacking to date. How exciting is that!

As is usual in our publications, let me express some notes of caution. This book is coloured by my own individual experiences as a scholar, and is probably a feeble attempt in conceptualising and expressing my own lessons and pieces of fragmented knowledge about what research is about and how one best goes about conducting it. I am very much aware that my own journey towards becoming a better researcher is far from finished; in fact, most of the time I wonder whether I have started at all. You should also be aware of the incomplete state of knowledge and experiences conveyed in this book, and are gentle with me about the quality of my recommendations. They are the results of my own flawed interpretations of my work and that of others, and any errors I have made are my own and no one else's. I do hope that my own journey will help me develop my abilities and capabilities, so that we can look back at this book in years to come and identify many, many things we would do differently. If I don't reach this plateau, it will only mean that I haven't learned a thing.

One saying in our discipline is that you "have to go out and read a lot – and then you have stop reading and start doing!" Now that you have read the final lines of this book, do close it, and go out and start doing, that is, start practicing research.

Most importantly, make sure you enjoy what you do. That was, is, and will always be, the main ingredient to a happy and fulfilling life.

Jan Recker

Brisbane, Australia, March 2012.

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Index

A

Abductive reasoning, 7, 58
Abstract, 135
Abstracting, 132
Abstractions, 9
Acceptance, 137
Accountability, 141
Accuracy of measurement, 68
Action planning, 100
Action research, 99
Action taking, 100
Alignment of working styles, 122
Ambition, 5
Analysing, 99
Analysis, 53, 54
Analytical measurements, 131
Anonymity, 143
Appropriate use of language, 146
Archival analysis, 76
Axial, 103
Axial coding, 92

B

Background section, 130
Bivariate analysis(es), 81, 86
Body of knowledge, 7, 13, 15, 28, 39, 41, 113
Boundary conditions, 47, 50

C

Case design, 97
Case site(s), 95, 131
Case study, 95

Circularity, 59
Citations, 115
Co-authorship, 122–124
Coding, 92
Collaboration, 122
Collecting, 98
Commitment, 5
Complementarity, 104
 of skills, 122
Complexity, 37
Composite reliability, 70
Concept(s), 9, 16, 18, 19, 47
Conceptual analysis, 93
Conclusions section, 135
Concurrent, 70
Conditional acceptance, 137
Conference proceedings, 118
Confidentiality, 143
Confirmability, 94, 97
Confirmatory, 76
Conjectures, 57
Constant comparative analysis, 102
Construct(s), 16, 18, 19, 20, 35, 47, 51, 59,
 69–71, 105
Construct validity, 71
Content analysis, 93
Content validity, 70
Control group, 82
Controllability, 36, 37
Controls, 83
Convergent, 70
Convergent validity, 71
Covariance designs, 83
Credibility, 94, 97

Critical incidents, 93
 Cronbach's alpha, 70
 Cross-sectional, 34

D

Data
 analysis, 80, 85, 131
 coding, 86
 collection techniques, 131
 comparison, 105
 correlation, 105
 transformation, 104–105
 Dedication, 5
 Deductibility, 36, 37
 Deduction, 31
 Deductive reasoning, 7, 58
 Demonstrated utility, 107
 Dependability, 94, 97
 Dependent variable, 51
 Description, 53, 76, 97
 Descriptive analysis, 85
 Descriptive interviews, 90
 Descriptive research, 31, 34
 Design and action, 54, 56
 Design cycle, 107
 Designing, 34, 97
 Design science, 36, 37, 65, 105–109
 Design theories, 105
 Development, 104
 Diagnosing, 100
 Direct observation, 91
 Discourse analysis, 93
 Discriminant validity, 70, 71
 Discussion section, 132
 Documentation, 91
 Due process, 141

E

Ecological validity, 71
 Embedded design, 97
 Emergent meaning, 89
 Environment, 106
 Epistemology, 66, 97
 Equivalent groups design, 85
 Ethical issues, 143–144
 Ethics, 141
 Evaluating, 100
 Evolutionary design, 89
 Expansion, 104
 Experimental research, 81–87
 Explanation, 53, 54, 59, 76, 97, 132

Explanation and prediction, 54, 55
 Explanatory process, 60
 Explanatory research, 34
 Explorability, 37
 Exploration, 32, 76, 97
 Exploratory interviews, 90
 Exploratory research, 31, 34, 74
 External validity, 71, 94

F

Face validity, 70
 Factorial designs, 83
 Falsifiable, 60
 Falsification, 16, 21
 Fertile, 60
 Field, 34
 Field experiments, 76
 First generation data analysis, 81
 Focus groups, 76
 Formal theory, 59

G

Generalisability, 37, 50
 Gift authorship, 146
 Grounded theory, 102

H

Holistic, 97
 Hypothesis(es), 9, 19, 21, 57, 74, 131

I

Implications, 63
 for practice, 133
 for research, 133
 Independence, 16
 Independent variable, 51
 Induction, 31, 32
 Inductive analysis, 88
 Inductive generation, 102
 Inductive reasoning, 7, 57
 Inferential analyses(is), 85, 86
 Information Systems, 3
 Initiation, 104
 Instantiations, 105
 Instrumentation validity, 72
 Instrument development, 78
 Instrument testing, 80
 Instrument validation, 74
 Internal validity, 72, 94

Interpretive research methods, 89
Interpretivism, 97
Inter-rater reliability, 70
Intervention, 99
Interviewing, 90
Item creation, 74
Item identification, 74
Item revision, 74

J

Journal articles, 118
Justifications, 47
Justificatory mechanisms, 49

K

Knowledge base, 106

L

Labelling, 146
Laboratory, 34
Laboratory experiments, 76
Latent semantic analysis, 93
Legitimation, 105
Leximancer, 93
Liability, 141
Literature, 39–42
Longitudinal, 34

M

Major revisions, 137
Manifestation(s), 9, 51
Manipulation validity, 71
Manuscript, 126
Materials, 131
Measurement(s), 14, 16, 18, 67–69, 71, 72, 74
Measurement development, 71–74, 78
Measurement error, 12
Measurement model, 81
Measurement validity, 94
Mediating variable, 51
Memoing, 92
Methodologies, 40, 41
Methods, 105
Mid-range theory, 59
Minor revisions, 137
Mixed method(s), 36, 37, 65, 104
Mixing, 104
Model development, 78
Models, 105
Moderating variables, 51
Multiple-case, 97

Multiple sources of data, 88
Multivariate analysis, 81

N

Natural science(s), 11, 12
Natural setting, 88
Nomological nets, 50–51
Non-equivalent switched replication design, 85

O

Objective, 97
Observation(s), 18, 31, 32, 35, 58
Ontology, 66
Open coding, 92, 103
Operationalisation(s), 9, 18, 20, 21, 35,
36, 67, 70

P

Paper acceptance rate, 137
Papers-based research, 120
Participant observation, 91
Participant selection, 131
Peer-review, 116, 136
Phenomenon (phenomena), 13–14, 19,
28, 35, 60
Pilot tests, 80
Placing, 104
Plagiarism, 144
Planning, 96
Positivism, 97
Potential risks, 144
Precision, 16
Prediction, 53–55
Predictive validity, 70, 71
Preparing, 98
Prescription, 54
Pre-test, 78, 80
Pretest-posttest, 85
Process theories, 55
Professional acknowledgments, 146
Proposition(s), 19, 57, 58
Psychometric properties, 68
Publication matrix, 120
Publishing, 113
Publish or Perish, 115
Purposive sampling, 89

Q

Qualitative methods, 88
Qualitative research, 34, 65, 97
Qualitative strategies, 36, 37

Quantitative methods, 66
 Quantitative research, 34, 37, 65, 97
 Quantitative strategies, 36
 Quasi-experimental designs, 83, 85

R

Randomisation, 83
 Random sampling, 89
 Rationalisation, 32
 Recognition of co-author contributions, 144
 Refereed book chapter, 118
 Reflection, 133
 Reject, 136
 Relational analysis, 93
 Relationship(s), 47, 48
 Relevance cycle, 107
 Reliability, 68, 69, 72, 74, 94
 Repeatability, 37
 Replicability, 16, 131
 Reporting, 113
 Research
 design, 30–35
 methodology, 35–39, 65
 methods, 39, 97, 131–132
 model, 130
 question(s), 25–30, 36
 strategy, 131
 Research-based papers, 120
 Response letter, 139
 Response rate, 78
 Responsibility, 141
 Results section, 132
 Revise and resubmit, 137
 Rigor cycle, 107

S

Sample frame, 77
 Samples, 83, 131
 Sampling procedure, 77
 Scholarly publications, 122
 Scientific evidence, 14
 Scientific inquiry, 11–15
 Scientific knowledge, 13
 Scientific method, 7, 15–18
 Scientific research, 7, 17
 Scientific work, 122
 Second generation data analysis, 81
 Selective coding, 92, 103
 SEM. *See* Structural equation modelling (SEM)
 Semi-structured interviews, 90
 Shared meaning, 67

Sharing, 99
 Single-case, 97
 Social science(s), 11, 12
 Specificity, 146
 Specifying learning, 100
 Split-half reliability, 70
 Statistical conclusion validity, 71, 72
 Statistical modelling, 74
 Storage, 144
 Strategy of inquiry, 36
 Structural equation modelling (SEM), 81
 Structural model, 81
 Structure, 127–135
 Substantive theory, 59
 Substrata identification, 74
 Survey administration, 80
 Surveys, 76, 131

T

Target outlet, 124
 Test-retest reliability, 70
 Theoretical coding, 103
 Theoretical sampling, 102
 Theorising, 57–64, 132
 Theory(ies), 35, 40, 41, 45–52
 Timing, 104
 Transferability, 94, 97
 Treatment, 82
 Treatment manipulation, 82
 Triangulation, 91, 104
 True experimental designs, 83
 Two-group design, 83

U

Understanding, 34
 Uni-dimensional reliability, 70
 Unit of analysis, 97
 Units of observations, 76
 Univariate analysis(es), 80, 86

V

Validation, 32, 58
 Validity, 32, 68, 70, 72, 74
 Variables, 20, 55, 69
 Variance theories, 55
 Voluntary participation, 144

W

Weighing, 104