1 Introduction – The Purpose of this Book

In this chapter you will learn about:

- the aims of this book;
- the information systems and computing disciplines;
- what we mean by 'research';
- the 6Ps of research;
- the structure and content of this book.

Aims of this Book

This book is about how to do research in the information systems (usually shortened to IS) and computing disciplines. Its main aim is to help you to undertake a research project, whether you are a research student aiming for an MPhil or a PhD, or a student on a taught course at masters or senior undergraduate level, or a member of academic staff who is uncertain about how to do some research. The book also aims to help you analyse and evaluate research undertaken by others, so that you will come to know what constitutes 'good' (or 'valid' or 'trustworthy' or 'high quality') research, and so that you will be able to assess whether studies carried out by others provide you with the evidence you need.

The IS and Computing Disciplines

IS as a discipline is concerned with the development and use of information systems by individuals, groups, organizations and society, where usually those information systems involve the use of computers. Other degree titles for IS include Business

Computing, Management Information Systems and Informatics, and IS students and lecturers are found in business schools, social sciences and computing departments, as well as in IS departments. IS is particularly concerned with the real-world social and organizational context in which information systems are developed and used.

The computing discipline includes computer science, software engineering, information technology (IT), web development, computer games, computer animation and multimedia. Like IS, it is concerned with the development and use of computer-based products, but it tends to concentrate primarily on the technological rather than the social aspects.

IS and computing have developed as separate disciplines, with independent academic communities and discrete bodies of literature. However, since they are both concerned with the analysis, development and use of computer-based products there is a large area of overlap between them, so it is appropriate to address them together in this one book.

There are many books on how to do research, but they are aimed at researchers in the natural or social sciences or business studies. Very few are aimed at research in IS and computing. This book helps to fill that gap by concentrating on:

- the kinds of research questions addressed in IS and computing;
- the research approaches used in IS and computing;
- examples of previous research from the IS and computing literature.

Evidence-based Practice

IS has a long tradition of carrying out field research (often called *empirical research)* in order to find out what happens when information systems are requested, developed and used by people. This book will help new IS researchers to do such research, and to analyse and evaluate the work of other IS researchers. The findings of IS research can be used as evidence to support the effective development and use of information systems – that is, evidence-based practice in IS.

Until recently, almost all computing research was concerned only with developing computer-based products (for example, data processing systems, websites, artificial intelligence robots, computer games and digital art) and the methods we use to build such products. This book will help new computing researchers carry out such *design and creation* research, and to analyse and evaluate the work of other computing researchers.

Some computing researchers, software engineering researchers in particular, have begun to realize the necessity to go *beyond* designing and creating new computer-based products, to find out what happens when their products are implemented in the real world (often called the *empirical assessment of systems*). There have been many examples of computer-based products with which the developers were happy, but they failed in some way when put into use. Computing researchers are realizing they need to know *why* that happens. Often computing research has offered new technical products, but they have not been taken up and used. Again, some computing researchers want to know why. They have also realized the need to find out what happens when the methods they propose are put into use in the real world. The computing literature, especially that for software engineering, contains many proposals for new ways of designing and developing computer-based systems, for example, information hiding, design patterns, UML (Unified Modelling Language) and agile development methods such as eXtreme Programming (XP). However, surprisingly (and worryingly) little work has so far been done to find out whether and how these methods work in practice. Similarly, consultants and teachers tell systems developers and computer artists about how they ought to develop computer-based products, but few of these prescriptions have been validated – they often amount to little more than someone's opinion.

In short, as illustrated below, little work has been done to find the *evidence* that validates ideas about appropriate technical products and methods and links the theory to practice.

A lack of evidence

> There are plenty of computer science theories that haven't been tested. For instance, functional programming, object-oriented programming, and formal methods are all thought to improve programmer productivity, program quality, or both. It is surprising that none of these obviously important claims have ever been tested systematically, even though they are all 30 years old and a lot of effort has gone into developing programming languages and formal techniques. (Tichy, 1998, p. 33)

There is now, therefore, increased attention by some in computing to the empirical assessment and evaluation of computer products and development processes, so that we can have *evidence-based computing*.

This means that when people suggest how to develop systems in better ways, or how to get computers to do new things, we should know that:

- there is proper evidence to support these proposals;
- the ideas are based on more than the opinion of someone in an academic ivory tower, or some well-paid 'consultant'.

We also want to know whether these proposals are practical in the pressurized world of real-life systems development or games production and in situations when they are implemented against a background of office politics. And vice versa: by gathering evidence about what happens in real life, academics (and consultants) can refine their theories about how computer-based systems development should be done or computer products be used.

This book will help computing researchers to do studies that produce the evidence in support of their computer-based products and methods, and to analyse and evaluate the evidence offered by other researchers.

The Internet and Research

The Internet and World Wide Web are becoming increasingly important in our modern world and offer exciting possibilities for both new research topics and new research approaches. Naturally, these are of particular interest to IS or computing researchers. This book therefore includes:

- some possible research topics *about* the Internet or web;
- how the Internet can be used as a tool *within* a research approach (for example, Internet-based surveys, and Internet 'interviewing');
- how Internet-based research is different from other types of research;
- examples of previous research studies that have investigated the Internet or World Wide Web, or used them to support the research process.

What is Research?

So what do we mean by 'research'? Perhaps the word conjures up an image of people in lab coats (and egg-shaped heads?) designing new washing machines or watching liquids boiling in glass cylinders, or trying to insert a gene from a fish into a tomato. All these people are doing research, but research is more than the preserve of an elite group of white-coated individuals. In fact we all do research every day – research is a particular kind of everyday thinking.

Let's use an example. Supposing you come out to your car one day and discover you have a flat tyre. You need to get the punctured tyre repaired, or buy a new one. How will you tackle this problem? Perhaps you will fetch the telephone directory, find tyre suppliers, phone some and find out the price of a new tyre. If the price is affordable you take your car in and have a new tyre fitted. Simple! However, if the price is more than you really can or want to pay, you might take your punctured tyre to one or two repairers and find out whether it can be repaired and how much that will cost. You then consider all your options and

Research task	Everyday thinking
Identify a problem	How can I deal with my punctured tyre?
Gather data	Obtain prices of new tyres
Analyse the data	What is the cheapest?
Interpret the data	That's more than I want to pay. I need more information.
Gather more data	Is it reparable? Obtain prices for tyre repair.
Analyse the data	Can it be repaired? What is the lowest cost? How does the cost compare with a new tyre?
Interpret the data	Repairing it is possible. Repair will cost 20% of a new tyre. Repair rather than replace means I can still afford to go out on Friday night.
Draw conclusion	I will get it repaired at Tyres-U-Like

Figure 1.1 A piece of research – dealing with a puncture

decide: new tyre, repaired tyre or do nothing and hope to get away with it. You have carried out a piece of research. As Figure 1.1 shows, you identified a problem, gathered some data (or evidence) to help you address the problem, analysed the data, interpreted it, decided to gather more data, analysed and interpreted that and drew a conclusion.

So doing research is a type of thinking we do most days. It means *creating some new knowledge*: initially you did not know what to do about your punctured tyre; now you do. However, in everyday thinking we often take shortcuts and use poor or incomplete data. For example, we draw conclusions about what people are like from our first impressions. Doing *good* research means that we do not jump to conclusions but carefully find sufficient and appropriate sources of data, properly record, analyse and interpret that data, draw well-founded conclusions based on the evidence, and present the findings in an acceptable way in a report, thesis, conference presentation or journal article (Figure 1.2).

Returning to our tyre puncture example, you probably would not bother to contact every tyre fitter listed in the telephone directory – life is too short! Nor would you

Everyday thinking is often characterized by:	Good academic research is characterized by:
• Poor data • Incomplete data • Hasty thinking	• Sufficient data sources • Appropriate data sources • Accurately recorded • Properly analysed • No hidden assumptions • Conclusions well-founded • Properly presented As judged by the *users* of the research

Figure 1.2 Everyday thinking versus good research

adopt a very systematic approach, for example by telephoning every third fitter in the directory. You would telephone two or three, and that would do. You might never know that the 29th fitter in a list of 30 has the cheapest tyres of all, but that would not matter, you have gathered enough information to let you deal with the problem *to your own satisfaction*. And that gives us the key to further defining what we mean by research: research is creating new knowledge, *to the satisfaction of the user(s) of the research*.

Systems developers recognize that they could design a perfect computer system, completely bug-free and meeting all of the requirements specification, but the system would still be a failure if it did not satisfy the end-users of the system. Similarly, researchers must satisfy their end-users. This book is concerned with academic research, that is, the 'users' of the research will be members of the academic community – lecturers, other researchers and students. For an undergraduate research project, these academics might just be in your own university, such as your project supervisor. You should ask yourself, 'What can staff and students here learn from my research project report that they would not learn from a standard textbook?' For MPhil and PhD students, and anyone wanting to publish research papers, your users are academics across the world. Academic end-users are usually interested not just in the end product, the research findings, but also in how these were achieved, that is, the *process* of the research should exhibit the characteristics of good research shown in Figure 1.2. Then they will be satisfied that the knowledge you have created can be added to the sum total of knowledge about IS or computing. So now we can define research as shown in the following box.

DEFINITION: Research

Research is the creation of new knowledge, using an appropriate process, to the satisfaction of the users of the research.

So, as Figure 1.2 indicates, academic researchers want to see *sufficient* data sources, and an *appropriate* process, with the findings *properly* presented. But what do we mean by 'sufficient', 'appropriate' or 'properly'? For some academic disciplines these are well-defined. However, for IS and computing there are several different academic communities, each with different ideas, or philosophies, about the kinds of research questions to ask and the process by which to answer them. These can partly be categorized by the sub-disciplines within IS and computing: for example *computer science* concentrates on technical development and researchers would mostly use design and creation activities, whereas *management information systems* researchers might use a survey carried out by questionnaire to answer research questions about how business executives use IT in their organizations. The different philosophies about research can also vary from country to country. For example, the majority of IS research in the USA is probably based on a '*positivist*' philosophy about how to do research, which believes there is a single reality or truth. IS research in Europe, conversely, often has an '*interpretivist*' philosophy, which believes that there are multiple versions of reality, so multiple truths. The philosophy underlying the choice of a research question and the process of answering it can even depend on an individual: our own views about the nature of the world we live in and therefore about how we might investigate it. This book will therefore explain these different philosophies of research (see especially Chapters 19 and 20). The fact that there is more than one school of thought in IS and computing about what constitutes good research makes research in these disciplines both challenging and interesting.

Let's Have an Argument!

PhD students are expected to produce a thesis. But 'thesis' has two meanings. One refers to the report itself, anything from 100 to 500 or more pages, bound together. The other meaning of thesis is 'an argument to be proposed or maintained'. Such an argument is made in the written report and might also have to be defended at a viva: a face-to-face meeting with the examiners.

In fact, all research consists of assembling and defending an argument. Researchers are essentially making an argument to their users that they have indeed created some new knowledge – see Figure 1.3. They build this argument by a combination of logic (for example, carrying out and reporting the research process in a logical and structured way), drawing upon other people's work (for example, surveying what has previously been argued and citing as evidence what others have written in the literature) and by carrying out their own fieldwork (that is, generating, analysing and interpreting data

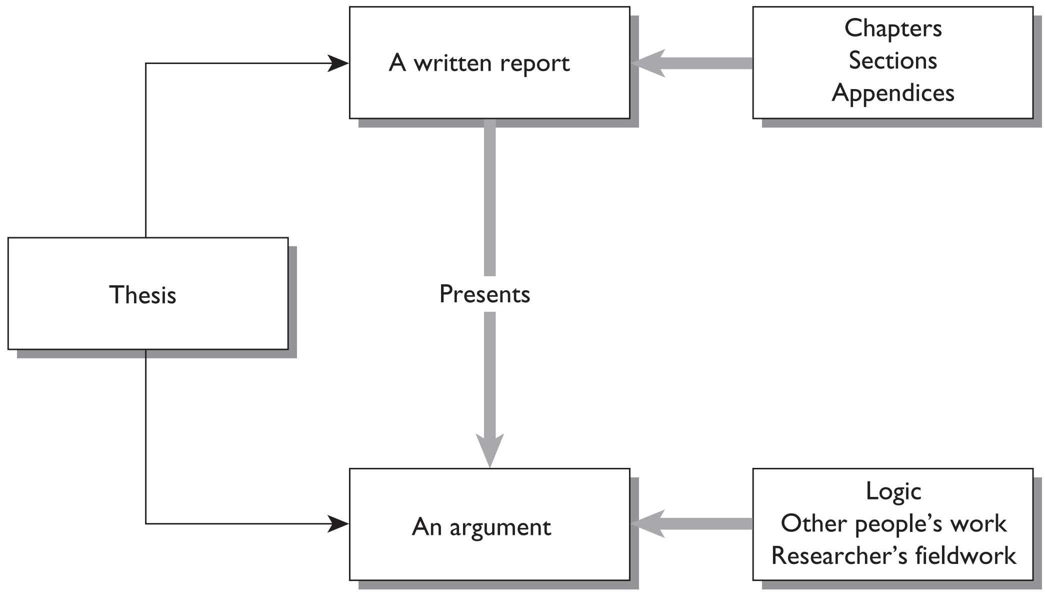

Figure 1.3 A thesis is an argument

and drawing conclusions). This book will help you to build such an argument, to the satisfaction of academic researchers.

Evaluating Research

This book will also show you how to analyse and evaluate the work of other researchers, by providing you with 'Evaluation Guides'. By using these guides you should be able to judge whether the researchers have met the requirements of academic research in your discipline, and whether they have provided the evidence to support their claims. Being able to evaluate the research of others is important because:

- You might want to cite the work of others in your own research – but not if the others' research has been poorly executed.
- Your assessment of the research of others and finding flaws or incompleteness helps you think about and improve your own research.
- Politicians and journalists frequently report research findings as if they must be true because they have been produced by scientists or other academic researchers. As a thoughtful citizen you should be able to assess for yourself whether the findings are based on good research, and not just take someone else's word for it.

'I Just Want to Develop a Computer-based System'

Some readers, perhaps particularly those in the computing disciplines, might by now be asking, 'What's all this got to do with me? I just want to develop a computer system/ build a website/create a computer animation [*delete as appropriate*] and write a report about it, not collect and analyse data or make arguments.'

The response is that designing and creating any kind of computer-based product is still a form of research, which requires finding or generating data, analysing it and drawing conclusions. The research question is, 'Is it possible to develop a computer-based product to do X?' In order to define that question fully (for example, What is X? Why is it important to have a computer system to do X?) and then to answer it, you will have to:

- gather data about the computer-based product required (for example, interviewing people, examining company documents, or studying the cultural or genre conventions for a particular kind of animation);
- generate your own data (for example, system models, storyboards, character sketches) to document how and why you designed and implemented the product;
- test the computer-based product and obtain user or viewer feedback, which will involve more data generation and analysis (for example, testing logs, user questionnaires, observation of audience reactions).

Ultimately, you will have to convince the readers of your report (that is, argue) that you went about the design and development tasks in a systematic way, finding, generating and analysing appropriate data, so that you could draw conclusions about whether or not you could indeed develop a computer system to do X.

Of course, some systems development is fairly trivial, for example, using a PC-based software package to build a database to keep track of your music collection. For it to satisfy academics as a valid piece of research, the computer-based product must contribute something new, for example, the system includes some new functions not previously automated using IT, or its design is based on a new theory or algorithm, or the system exhibits some new artistic ideas. Viewing the design and creation of computer systems as research is discussed more fully in Chapter 8.

As explained above, increasingly researchers who build computer-based products are also being encouraged to evaluate their products in use. Effectively, a new research question is being added to design and creation projects: 'What happens when the computer-based product is used in practice/viewed by an audience?' To answer that question, researchers have to use one or more of the other strategies and data generation methods described in this book.

Rigour and Relevance in Research

It is generally believed that IS and computing research should be both 'rigorous' and 'relevant'. However, from time to time IS and computing academics write papers or engage in rather anguished discussions about whether their research is, or can ever be, both rigorous and relevant. Part of the problem is that most researchers do not provide definitions of rigour or relevance, relying instead on supposed common intuition. So what *do* these terms mean?

Rigour encompasses both *systematic conduct* and *validity*. *Systematic conduct* means that the research tasks are undertaken in a rational fashion, with logical relationships between them; they are not random acts or beliefs somehow cobbled together. This book therefore explains how research can be conducted systematically. If a piece of research has *validity*, it means that an appropriate process has been used, the findings do indeed come from the data and they do answer the research question(s). As we noted above, different communities have different ideas about appropriate research questions and processes so, not surprisingly, they also have different ideas about what constitutes 'valid' research in their eyes. This book therefore explains how different research philosophies define 'valid' or 'sound' research.

Relevance is defined as 'being pertinent, having direct bearing'. However, few researchers explain *relevance to whom*. The common assumption is relevance to 'practitioners', but again these are left undefined. 'Practitioners' can be people working in businesses, from chief executives and managing directors, through personnel managers or finance managers to data processing clerks – all use IT themselves or are interested in its use to benefit their organization. 'Practitioners' also includes systems developers, computer artists and digital archivists, that is, those who plan, analyse, design, implement and maintain computer-based products, and who are interested in what kinds of systems to build and appropriate methods for developing and maintaining them. IS and computing research is also potentially relevant to other kinds of practitioners, for example, teachers using IT to aid student learning, or community workers helping disadvantaged communities to use IT to overcome barriers such as geographical or psychological isolation. Since computers and other information and communication technologies are now so pervasive, in the developed world at least, much IS and computing research has the potential to be relevant to *someone*.

Some IS and computing researchers might regard as their users only other academics. Sometimes this is criticized as academics engaging in 'navel-gazing'. But often much research initially seems understandable and relevant only to a few other academics, only later does its relevance to others become apparent – as the following section indicates.

“ It was said that …

I think there is a world wide market for maybe five computers. (Thomas Watson, Chairman IBM, 1943)

This telephone has too many shortcomings to be seriously considered as a means of communication. The device is inherently of no value to us. (Western Union internal memo, 1876)

But what is it good for? (Engineer at the Advanced Computing Systems Division of IBM, 1968, commenting on the microchip)

There is no reason why anyone would want a computer in their home. (Ken Olson, president, chairman and founder of Digital Equipment Corporation, 1977)

Computers in the future may weigh no more than 1.5 tons. (*Popular Mechanics*, 1949)
Source: www.ideasmerchant.com/go/useful/facts-quotes.htm ”

Other potential users of research are students, like many of the readers of this book. By examining how someone else has carried out a piece of research and the findings that emerged, students can identify their own research questions and plan their research activities, and generally learn about research. Many of the exercises in this book therefore involve you examining actual published research papers.

The 6Ps of Research

The aspects of research covered in this book can be categorized as the 6Ps: *purpose*, *products*, *process*, *participants*, *paradigm* and *presentation* (see Figure 1.4). All of these aspects need to be considered in any research project.

- **Purpose**: the reason for doing the research, the topic of interest, why it is important or useful to study this, the specific research question(s) asked and the objectives set. Research without a purpose is unlikely to be good research.
- **Products**: the outcomes of research, especially your contribution to knowledge about your subject area. Your contribution can be an answer to your original research question(s) but can also include unexpected findings. For example, you and the academic community might learn something about a particular research strategy as a result of your research. Your thesis, dissertation, conference paper or journal article is also a product of your research. For those research projects that

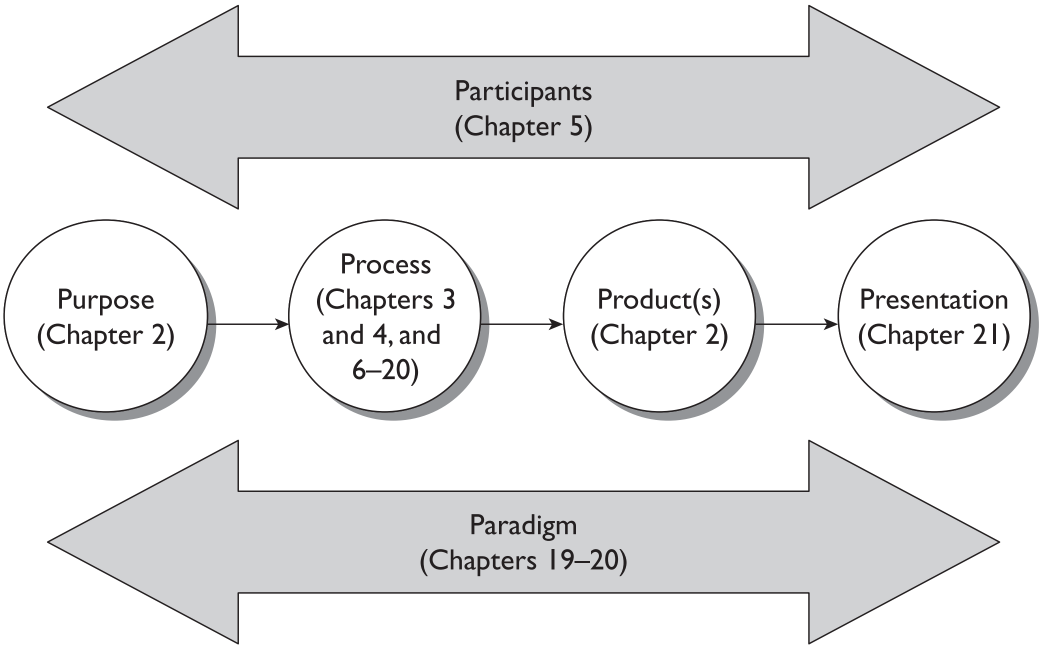

Figure 1.4 The 6Ps of research

involve design and creation, a new computer-based product or new development method could also be a product of your research.

- **Process**: the sequence of activities undertaken in any research project. The process involves identifying one or more research topics, establishing a conceptual framework (the way you choose to think about your research topic), the selection and use of a research strategy and data generation methods, the analysis of data and the drawing of conclusions, including recognizing any limitations in your own research. As explained already, the process should be carried out systematically if the research is to be accepted as rigorous.
- **Participants**: these include those whom you directly involve in your research, for example by interviewing them or observing them, and also those who are indirectly involved, such as the editors to whom you submit a research paper. It is important that you deal with all these people legally and ethically, that is, you do not do anything that might annoy them or cause them harm (physically, mentally or socially). You yourself as a researcher are also a research participant. As we shall see later, for some types of research, researchers are expected to be objective and remain largely unseen in the reporting of their research, whereas in other types of research the researchers are open about their feelings and how their presence influenced the other participants and the research situation.

- **Paradigm:** a pattern or model or shared way of thinking. Managers sometimes talk of the need for a 'paradigm shift' to mean that a new way of thinking is required. In computing, we talk about programming language paradigms, for example, a group of languages that share a set of characteristics, such as the object-oriented paradigm (for example, Smalltalk and C++). Here we are concerned with the philosophical paradigms of *research*. Any piece of research will have an underlying paradigm. We have noted already that different academic communities and individuals have different ideas about the kinds of research questions to ask and the process by which to answer them because they have different views about the nature of the world we live in and therefore about how we might investigate it. These different views stem from different philosophical paradigms. We shall look at three such paradigms: 'positivism', interpretivism' and 'critical research' – each will be explained later.
- **Presentation:** the means by which the research is disseminated and explained to others. For example, it may be written up in a paper or thesis, or a conference paper is presented to an audience of conference delegates, or a computer-based product is demonstrated to clients, users or examiners. It is important that the presentation is carried out professionally – otherwise your audience might assume your whole research project was not undertaken in a professional manner.

Structure of this Book

This book is structured according to the 6Ps (see Figure 1.4). Chapter 2 discusses the *purpose* of research, including generating possible research questions, and some possible *products*, or outcomes, of research. Chapter 3 gives an overview of the research *process* and Chapter 4 discusses the nature of Internet research. Chapter 5 explains the *participants* and ethics of research. The bulk of the book (Chapters 6–18) then covers the research *process* in detail, including a literature review, a chapter on each of six research strategies (surveys, design and creation, experiments, case studies, action research and ethnography), one on each of four data generation methods (interviews, observation, questionnaires and documents) and two on data analysis techniques (quantitative and qualitative). Chapters 19 and 20 discuss the philosophical *paradigms* of research (positivism, interpretivism and critical research). Chapter 21 covers writing up your research and research *presentations*.

At the end of each chapter there are some practical exercises. These will help you both to reflect upon the ideas discussed in the chapter and also to practise reading, analysing and evaluating pieces of research. Each chapter also includes suggestions for further reading that will direct you to other work explaining the ideas discussed in the chapter, and to examples of research in IS and computing where these ideas have been used.

PRACTICAL WORK

1 Find several definitions of 'research' from dictionaries, books on research methods and from friends, family or colleagues, and then compare them to the one given in this chapter. What conclusions can you draw about the concept of 'research'?
2 The section 'Evidence-based Practice' pointed to the lack of evidence for many of the ideas about appropriate methods and products in computing. Study Hirschheim and Newman (1991). This paper is now quite old – consider whether the myths, metaphors and magic rituals the authors identify still exist today. What other myths, metaphors and magic rituals might be observed in your branch of IS or computing? Consider whether they might make suitable research topics.

FURTHER READING

The nature of IS research was discussed in *MIS Quarterly* by Weber (2003) and Benbasat and Zmud (2003). Subsequent issues of *MIS Quarterly* contain papers by other authors agreeing or disagreeing with these views. Debates about rigour and relevance have occurred regularly in the IS literature (Keen, 1991; Kock & Lau, 2001; MISQ, 1999; Robey & Markus, 1998; Senn, 1998) and on ISWorld (www.isworld.org), an online forum for IS researchers and lecturers. 'Relevance to whom?' is discussed in the editor's introduction and in the papers that follow in the special issue of *Informing Science* on bridging the gap between researcher and practitioners (Fitzgerald, 2003).

The nature of computing research, software engineering research in particular, and the need for empirical assessment of computer systems is discussed by Perry, Porter and Votta (2000). This paper would work just as well if the words 'information systems' or 'computing' replaced 'software engineering' throughout. Several papers in O'Brien, Gold and Kontogiannis (2004) review the kinds of empirical evidence that *could* be used in software engineering and lessons that can be learnt from other disciplines. Kling (1993) argues that computer scientists should pay more attention to the application domains of computer products. Tichy, Lukowicz, Prechelt and Heinz (1995) report that 40–50 per cent of computer science and software engineering papers include no empirical validation of proposed designs (of systems, algorithms or models) and argue that this is a serious weakness of computer science research.

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2 The Purpose and Products of Research

In this chapter you will learn about:

- some of the reasons why people do research;
- some possible products, or outcomes, of research;
- how to look for ideas for research topics.

Chapter 1 explained that we could think of research in terms of 6Ps: *purpose*, *products*, *process*, *participants*, *paradigm* and *presentation* (see Figure 1.4). In this chapter we'll look at two of those 6Ps: the *purpose* and the *products*. The *purpose* of the research incorporates the reason for doing it, the topic of interest, why it is important or useful to study this, the specific research question(s) asked and the objectives set. The *products* of research are the outcomes, especially your contribution to knowledge in your subject area. Let's start with the *purpose*.

Reasons for Doing Research

Why do people do research? You might be embarking upon a piece of research because you have to – you must carry out a piece of research in order to pass your course and get a qualification, or doing research is part of your contract. But doing research because you have to is probably not a sufficient reason to keep you motivated when the going gets difficult. So let's consider some of the other reasons why people do research. You may recognize that at least one of these applies to you. Knowing the reason(s) you choose to do research will help you find a topic that interests you, and also give you a reason to keep going.

To add to the body of knowledge

Some researchers want to add to what is known about their specialist subject, and it does not really matter whether the new knowledge has any practical application. For example, right now someone might be studying patterns of Sunday School attendance in 18th-century England or 19th-century Virginia. Knowing about this is probably not going to affect the current government's educational policy, or suggest new uses for computers. But exploring and finding out the answers to questions is part of what makes us human, so it is natural that some should simply want to explore a subject in depth and find out things we did not know before.

All PhD students are expected to add to the body of knowledge, but for only a few of them is it the *only* reason for doing research. Similarly, undergraduate students try to add to the body of knowledge within their own institution, but that is not normally their only motivation. Usually there are other reasons too, as explained below.

To solve a problem

Many researchers in IS and computing want to solve a problem, for example, 'Is it possible to develop a computer-based product to do X?' If they are developing a computer product to do something in a new way, then it is research. For example:

- Is it possible to develop a website that is based upon the very latest marketing theories to help the ABC company's marketing strategy?
- Is it possible to develop a computer-based tool to support online discussions for teaching purposes?
- Is it possible to effectively render the texture of an animal's hair in a virtual zoo?

Often the computer system developed is of use to someone else (for example, company managers, teachers), but sometimes the problem is seen as just a personal, technical challenge. For example, Linus Torvalds wanted to build a terminal emulation system on his newly acquired 386 computer at home so that he could dial up and use remotely the university's Unix computer (Torvalds & Diamond, 2001). However, for research that is based on addressing a personal, technical challenge it is usually expected that at the end you should be able to discuss how your work could be of relevance to others. (As Torvalds added more features to his terminal emulator, it evolved into the Linux operating system, which is now used by millions worldwide.)

To find out what happens

What happens when a new computer system is put into real-world use? For example, when email systems are provided within an organization, are paper mailing systems abandoned altogether? Is the new email system ignored by some people? What factors might explain what happens? The computing literature is full of papers that describe prototype systems. What happens when these systems are turned into real-life systems and used by non-technical people? Finding out what happens is one of the key areas of interest for IS researchers, who are concerned with the interaction between the social and technical spheres when computer systems are designed and used. As explained in Chapter 1, for computing researchers, the empirical assessment of software systems in use is also becoming increasingly important. Similarly, there is increasing attention, especially in software engineering, to the empirical assessment and evaluation of software *processes*, for example, what happens when a company tries to use a particular development approach such as structured methods or UML? Does the approach live up to the claims made for it by academics or commercial vendors?

To find the evidence to inform practice

How *do* we develop computer systems? How *should* we? Can we come up with better ways? Many methods have been proposed over the years, for example, structured methods, agile methods, formal methods. Similarly, many computer-based tools for developers are marketed: analyst workbenches, project management tools, and so on. Is one approach or tool better than another? In which circumstances should we use each approach or tool? Systems developers are sometimes accused of always seizing upon the latest hyped idea or 'silver bullet'. Good research could provide the evidence that would inform developers about which new ideas are worth taking up and which are not. Similarly, business managers are sometimes accused of blindly following the latest management fad and buying software applications because everyone else is, recent examples being, perhaps, ERP (enterprise resource planning) and CRM (customer relationship management) systems. Good IS research could give managers the evidence about the enablers or barriers to successful adoption of such computer applications.

To develop a greater understanding of people and their world

We might want to find out about people and their world out of simple curiosity (nosiness?). But our findings might also have practical applications. For example, if we know about how people use PCs in the home, that might help companies who want to design software for home PCs. For example:

- Should they make a television more like a computer, or a computer more like a television?
- Do home users feel comfortable with the desktop metaphor currently used to structure the user interface on office PCs, or would they prefer a user interface that looked more like some aspect of their home life?

- What kinds of website advertising are effective for home-based web surfers?
- How does the advent of Internet shopping affect family relationships in the home? Stereotypes suggest women do the weekly grocery shopping, but men are the main technology users, so who does the shopping from an online supermarket? What implications might that have for how supermarkets design their online shopping websites?

> “ **Feminists' joke**
>
> If men liked shopping, they'd call it 'research'! ”

To predict, plan and control

Computer-based systems can provide tools to help people predict, plan and control. A lot of work is going on at the moment, for example, to predict the amount and effect of global warming. Evidence is being gathered about the rate of climate change to help us determine what might happen (for example, a rise in sea levels), and how soon, so that we can take action to cope with or even alter the predicted course of events. Computer systems can be built to help with modelling weather patterns and simulating possible scenarios. In the computing world, organizations and politicians are carrying out research to try to predict the number of people that will regularly go online in, say, 5 years' time – so that they can make plans for manufacturing and selling computers and providing the necessary IT infrastructure. Computer-based information systems help many organizations to plan for the future. For example, an accounting and budgeting system helps a company monitor and manage its cashflow, or an admissions-handling system in a university provides information to the people planning timetables and accommodation. Researchers could look at what systems are needed to help with prediction, planning and controlling, develop such systems, or investigate their use.

To contribute to other people's well-being

Some people do research to help make life better for other people. For example, can we research and design better user interfaces so some people, such as older people or disabled people, do not have to struggle when using a computer? Can we design a computer system that makes life easier for people in some way, for example, a system to help people who are frightened of spiders overcome their fear by 'handling' them in a virtual world?

To contribute to personal needs

Some people see doing research, especially studying for a PhD, as a personal challenge. They want to see if they can meet that challenge. Others do it because they think it will help them in their future career. And for women, becoming a 'Doctor' does finally resolve the Mrs/Miss/Ms issue!

To test or disprove a theory

As we shall see later, testing and disproving a theory is the main approach of scientific research. In IS and computing, there are many theories that could be tested. For example, many parents worry that computer games are harmful to the development of their children because of the use of violence, blood and gore, and because they keep children sitting down inside instead of getting exercise by running around outside. Alternatively, others argue that computer games are beneficial because they hone perception and motor skills and reaction times. Can we test these theories?

To come up with a better way

Sometimes a research question appears to have been answered, but another researcher wants to suggest a better answer. For example, a better algorithm, a better explanation, a better method … . Of course, initially a different way may not be accepted by others, as Parnas found – see the box below.

CASE STUDY
Getting a new idea accepted

In 1972, Parnas proposed a new way of decomposing systems into modules, which eventually became known as 'information hiding', and submitted a paper to the *Communications of the ACM*. A reviewer wrote: 'Obviously Parnas does not know what he is talking about because nobody does it that way.' Parnas responded that an idea claiming to be new should not be rejected just because it was new, and the paper was published (Parnas, 1972, 2003).

To understand another person's point of view

A researcher might join a group of people to find out about their views and ways of seeing the world. The group might know they are being researched, or they might not even know a researcher is among them. For example, you could work on an IT help-desk and find out about how your co-workers view the end-users who call in with problems. Are they seen as customers, whose needs are paramount, or are they seen as the enemy?

To create more interest in the researcher

Some researchers do research because it makes them look good and they get invited to give keynote speeches at conferences or to appear on television and radio shows to display their authority and knowledge. But let's be honest: if you are embarking on your first piece of research, it is unlikely that it will make you a media star.

There are probably many more reasons for doing research, but those listed here are enough to show that different people have different motives. What motivates you? What all these reasons have in common is that the research they stimulate leads to the creation of some new knowledge.

> “ **The rewards of scientists**
>
> The reward of the young scientist is the emotional thrill of being the first person in the history of the world to see something or to understand something. Nothing can compare with that experience … . The reward of the old scientist is the sense of having seen a vague sketch grow into a masterly landscape. Not a finished picture, of course; a picture that is still growing in scope and detail with the application of new techniques and new skills. The old scientist cannot claim that the masterpiece is his own work. He may have roughed out part of the design, laid on a few strokes, but he has learned to accept the discoveries of others with the same delight that he experienced his own when he was young. (Cecilia Payne-Gaposchkin (astronomer). Excerpt from acceptance speech and memorial lecture for the Russell Prize, 1977.) ”

Possible Products – the Outcomes of Research

So what types of knowledge can be created? There are many types of knowledge. Even given the same research question as a starting point, two different researchers could produce different kinds of knowledge as the outcomes or *products* of their research. Let's consider some of those different types of knowledge outcomes.

A new or improved product

This is the focus of some IS research and much computing research. Indeed, some computing departments expect that a new IT product is always one outcome of a research student's work. It might be a computer application designed to meet a functional specification, or, in a multimedia or computer animation department, it might be an example of digital art. In other cases the 'product' is not necessarily a computer application, it could also be a computing-related process, for example, a new or improved method for developing a particular kind of computer application.

A new theory

Computers and computer-based information systems are still comparatively new in our world and are still evolving. We don't yet always know how to use them efficiently and effectively. Nor do we know much about the interaction between individuals, groups and societies, and computing technology. As new computing technologies are invented, new uses of computers and information systems become possible, raising more questions about efficient and effective use. There is room, therefore for many more theories about how to analyse, build and use computer-based products.

A re-interpretation of an existing theory

Can an existing theory be applied in a new context? This has been a fruitful approach to research in IS and computing. For example, taking theories from psychology or economics or education, and applying them in an IS or computing context, for example, to the development of user interfaces, management information systems or computer-aided learning. The researcher explores whether and how the theory can be incorporated into a new design. Sometimes a theory might need to be re-appraised because it cannot be found to apply in a computing context. For example, retailing theories about consumer behaviour might not apply to e-commerce sites (see the box below).

CASE STUDY
Developing a new theory and re-interpreting existing theories

John was researching how online supermarkets could stimulate impulse buying by their customers. However, he was alarmed to find very little about impulse buying in the literature on e-commerce. He told his supervisor that he was worried this would mean a lower mark because his literature review would not be as large as he had hoped.

His supervisor, on the other hand, explained to him that this was an excellent opportunity to *improve* his research outcomes. He could use the detailed literature on impulse buying for supermarkets in the physical world and examine how far these theories could be used in the virtual world. He could also carry out field research into existing successful online shopping sites, such as Amazon, and analyse the techniques they used to stimulate impulse buying. Then he could combine the findings from his literature review and his field research to propose a new theory about impulse buying online.

New or improved research tool or technique

In IS research, there has been increasing attention paid to newer research tools and techniques from the social sciences (for example, action research, see Chapter 11, or qualitative data analysis, see Chapter 18), although conventional approaches such as surveys and experiments still predominate, especially in the USA. Very little computing research, conversely, has yet adopted *any* of the newer approaches. There is scope therefore for IS and computing researchers to adopt new research approaches and see if they prove useful. This book discusses some of the newer approaches. However, as a new researcher, you should discuss with your supervisor the acceptability in your department or discipline of your proposed strategy and the risks involved in adopting a non-conventional method. You might decide to leave such a risky course for when you are more established.

A new or improved model or perspective

A research outcome might be to suggest that we look at something in a new way. For example, Chen (1976) proposed that we should view the world in terms of entities and relationships, leading to the entity-relationship diagrams used in database design. Similarly, Morgan (1986) argued that most organizational analysis saw organizations as machines, whereas they could also be viewed as organisms, brains, psychic prisons and so on.

An in-depth study of a particular situation

Since computers and information systems are comparatively new, and since we continue to find new uses for them, there are many opportunities to study them in depth in contexts that have not been studied before. For example, a case study of a particular systems development department that decides to move from its existing method to an approach using UML; or an investigation of a company's website: its original conception, its evolution, its outcomes, and whether what occurred was consistent with the theory found in the literature.

An exploration of a topic, area or field

A literature-based survey of the state of knowledge in a particular area can be one outcome of research. However, a straightforward description is usually not sufficient. The researcher would normally be expected to make a further contribution by ordering the material, picking out themes, comparing and contrasting different views, and identifying areas of controversy or requiring investigation. Such a survey is often one of the first outcomes of an MPhil/PhD student's work.

A critical analysis

An example of a critical analysis might be an examination of a systems development method: its features, its omissions, the claims made for it and whether they are borne out in practice. Another example might be a critical analysis of government policy concerning the development of a national IT infrastructure and its implications for the digital divide still found in most societies.

Sometimes, two or more examples are critically analysed and compared. For example, during the 2001 foot and mouth disease outbreak in the UK, a study analysed and evaluated how different local governments used their websites to inform, communicate and deliberate with those affected (Oates, 2002).

Summary of types of research products

Davis and Parker (1997, p. 64) summarize the different types of contributions to knowledge as:

- new or improved evidence;
- new or improved methodology;
- new or improved analysis;
- new or improved concepts or theories.

To this we can add:

- new or improved computer-based product.

Unanticipated outcomes

Although you may be able to say in advance the kind of outcomes you expect, sometimes there are unexpected outcomes too. For example:

- A research project used an ethnographical approach (see Chapter 12) to investigate the use of IT in a business organization. In addition to anticipated outcomes about the ways IT was incorporated into the organization's culture, new knowledge was also created about the research strategy of ethnography.
- A research project designed and created a software tool to support tutors dealing with large numbers of students. During the evaluation stage, observations about how the class and tutors interacted with the software led to new knowledge about effective user interface design.

When you write up your research, remember to look for new knowledge that you had not anticipated creating.

Remember, too, a thesis, dissertation, conference paper or journal article is also a product of your research – usually these are the means by which your knowledge outcomes are disseminated to the wider academic community.

Finding and Choosing Research Topics

We've now seen that there are many different reasons for doing research, and many different possible outcomes. But how do we get started? That is, how do we find research topics?

Sometimes they emerge out of personal circumstances and opportunities. For example, a disabled person might be frustrated by the poor accessibility of many websites, and then realize that she could turn accessibility into a research topic, asking questions such as:

- What percentage of websites meets the published accessibility guidelines?
- How aware are web developers of accessibility issues?
- What factors in a company support or militate against accessibility being considered from the outset of a web development project?

Most researchers try to be aware of unexpected, serendipitous opportunities for research ideas, such as a conversation at a party, an interesting magazine article or a news event.

However, if an idea for research does not suddenly present itself to you, you will have to follow a more structured approach to look for ideas. You will need to look at what others have done, or suggested, and at what seems to be happening in the world, both in your research community and in the wider world.

Sources of research ideas

Sources for research ideas include:

- Suggestions from staff in your department. In some departments, staff circulate suggested research topics or project ideas.
- Past research students' work. You could ask your supervisor to suggest some recent good examples of student dissertations or theses. Often they discuss a topic in general and a number of research questions, before explaining that they will focus on just one aspect. What they choose *not* to cover could still be available for you to investigate.
- Recent conference and journal papers, especially the sections (often found near the end of a paper) discussing where further research is needed.
- Current events reported in the media. Sometimes a phenomenon is noticed in popular culture before it is taken up by academics. For example, Internet gambling or the use of text messaging by teenagers.

- Needs expressed by potential clients. You might know someone who asks you to use your IT skills to develop an IT product for their organization. Be careful though, to make sure you can identify an academic *research* aspect to such work, and that it does not just call for the use of your technical abilities.
- Calls for conference papers or special issues of journals on a particular theme. These calls can tell you what topics are currently 'hot' in the academic world, and often they list the kinds of research questions that need to be addressed.
- People making assumptions or assertions with little supporting evidence. Could you carry out research to find evidence to support or refute them? For example:

  - 'Organizations that don't go online will perish.'
  - 'Computer-based teaching means the end of universities as we know them.'
  - 'Everyone knows that object-oriented is much better than structured analysis.'
  - 'Open source software is the future.'

Brainstorming and clustering

It is often helpful to discuss ideas with a group of colleagues, perhaps using brainstorming and clustering techniques. Give the group some background information on yourself:

- your interests;
- what subjects you liked or disliked studying;
- your strengths and weaknesses;
- what might motivate you to do research.

Using a whiteboard, blackboard or just a large piece of paper, all the group members write down quickly keywords, ideas and suggestions – as many as they can, in any order, and as quickly as possible. Write down any idea that occurs, regardless of whether it sounds unfeasible or downright silly. When you have run out of suggestions (and only then), examine the ideas more closely. Can some of them be grouped together because they are related in some way? Does that clustering suggest a topic area or research question? Which ideas or clustered sets of ideas could be turned into potential research projects? Which of these possibilities excite you?

Selecting a topic

Having found some potential research topics, you have to choose one. This involves identifying the topic that best meets the two criteria of enjoyability (for you) and feasibility (as a piece of academic research).

Ask yourself, 'Will I enjoy working on that topic?' If the answer is, 'Not really', then find another topic. There are times when many researchers become bored with particular aspects of their research (for example, chasing up reference details, or negotiating access), but if the overall topic does not interest you then it is unlikely that you will do good research.

Try to express each potential topic as a single research question. Now break down what you would need to do to address that question, expressing it as series of objectives you would have to meet. This requires looking at the other chapters in this book to find out, for example, the kind of research strategy and data generation methods you might use and the sort of techniques you would need for data analysis. Examine each of the objectives and ask yourself whether you think doing the work to meet that objective would be both enjoyable and feasible. Only you can really know what you're likely to find enjoyable – although your friends and family might be able to advise you, based on their knowledge of you. To help you think about the feasibility, ask yourself the following questions:

- **Is the research likely to offer something new for your target users?** The previous section discussed the kinds of new knowledge that can be the outcomes of research.
- **Does the topic have 'symmetry of potential outcomes'?** This means, will your results be of value, whatever they are? For example, you might ask a question concerning whether help-desk staff suffer more from stress than other types of IS or computing staff. If they do, this would be interesting and worth further investigation. But if they do not, the result is not particularly interesting. A better research topic, with useful outcomes whatever they are, would be to investigate what factors contribute to feelings of stress in an IS or computing department.
- **Will your research still contribute something to knowledge, even if you do not complete all of the technical product in the time available?** This is an important issue for those doing design and creation research, where a computer-based product is an expected outcome. You need to design your research project to make sure you still have a contribution to knowledge even if the computer-based product is unfinished. For example, your models and design rationale might, on their own, offer something new.
- **Is there a theory (or set of ideas) that will help you structure your approach, at least in the beginning?** Usually there is some theory in the literature that can be used, even if you have to look outside your own discipline. Of course, one of your research outcomes might turn out to be a critique of the theory, or suggestions for how it needs to be modified or extended. If there is no pre-existing theory, then at least you know your research topic is something new, but it could be difficult working out where to start.

- **Is the research and its outcomes likely to be of sufficient scope to meet your course requirements?** The scope expected of a 3–4 week research project carried out as part of a taught course is clearly much less than that expected of a PhD carried out over 3 years or longer.
- **Can the research be carried out in the time available?** Even though PhD students have a long period of time available, they can still be caught out by not allowing enough time for such things as gaining access to a particular research context and writing the thesis. Note that funding bodies are becoming much more prescriptive on the maximum time allowed for PhDs.
- **Does the research topic fit in with your own motivations, strengths and weaknesses, likes and dislikes?** For example, it would be foolish for a student with poor programming skills to take on a highly technical project, just as someone with poor interpersonal skills would be ill-advised to take on research involving a lot of interviewing.
- **Does the research meet your own learning objectives?** That is, do you really want to know the answer to your research question, or will the topic help you to acquire some skills that you want?
- **Do you have the necessary resources?** For example, hardware, software, library resources, money for travel expenses, access to interviewees.
- **Can you approach the topic without too much bias?** Scientific research is expected to be objective, the researcher's own feelings and views should play no part. In other types of research, it is accepted that the researcher's own feelings cannot be ignored, but they must not be allowed to 'force' the research into a particular conclusion. A simple test is to ask yourself, 'Do I know the answer to my research question already?' If you think you do, then you should choose another topic since you are unlikely to be sufficiently open-minded during the research.
- **Will the research be safe and ethical?** You should be able to carry it out without breaking the law, without causing yourself or anyone else discomfort or harm (social, emotional or physical), and within the ethical guidelines agreed by academic researchers (these guidelines are explained in Chapter 5).

Most research students feel uncertain and anxious until they have settled upon a research question and had it approved. However, the good news is that you are not expected to decide in Week 1, and you do not have to do it alone. This book will help you to decide the kind of research you want to do. You should also use your supervisor. A supervisor is there to help you think through possible ideas and decide about their feasibility and whether they would meet your course requirements, so do not wait to see him or her until you know exactly what you want to do!

'I Write Therefore I Think'

As soon as you begin to have ideas about possible research questions and their associated objectives, write them down. Also write down your thoughts about whether you think doing the work to meet each objective would be both enjoyable and feasible, and from that draw your conclusions about whether this is a viable topic for you.

The main reason for writing all this down, rather than just thinking it through in your head, is that most researchers find that the act of writing is not just record-making; it helps them to clarify their ideas, to discover what they really think. It is therefore good practice to write up your thoughts, problems, insights, plans, emerging analysis and interpretations throughout your research project. Additional benefits of writing down as much as possible include:

- You can show your current thoughts to your supervisor, who will find it much easier to understand something written down than something expressed verbally, often hidden within a sequence of 'Erm's' and 'Well, you see, it's sort of …'.
- You will be able to include some of it in your project proposal, dissertation or thesis.
- The more you acquire the habit of writing up ideas, rationales and arguments, the easier you will find producing your final report, dissertation or thesis.

Evaluating the Purpose and Products of Research

Start to study research papers with a critical eye. Even though you might not yet understand the theory, research approach or findings, you can evaluate the research's purpose and products, and how well the researchers have explained them. Use the 'Evaluation Guide' below to help you.

EVALUATION GUIDE: PURPOSE AND PRODUCTS

1. How do the researchers describe the purpose of their research? Who are the potential users of it?
2. What reasons (if any) do the authors give as their motivation for this research?
3. Which of the reasons listed in this chapter do you think *might* apply?
4. What were the outcomes (*products*) of the research? Classify them by type as in this chapter.
5. How well do you think the authors have explained and justified their purpose and products?

PRACTICAL WORK

1 Practise brainstorming for possible research topics. Imagine you have 4 months to plan, carry out and write up a research project, and you have decided it should be something to do with IT and students. In a group of between two and six, do the following:

   a On paper or a white board, write 'IT' and 'Students'. Brainstorm and write down concepts related to IT, students, or 'IT and students': single words, short phrases or questions. (Remember students have home and social lives, as well as their university-based lives, you could take 'students' to be at school or university, or 'lifelong learners', and IT could cover digital communications, such as mobile phones, as well as computers.) Every suggestion should be written down, however silly it sounds. Continue until you have filled the page/board or have no more ideas (15 minutes to 1 hour).
   b Look through the list, cluster related ideas together, remove those that do not seem to lead anywhere, and identify two or three potential research topics concerning IT and students.
   c Turn each of these research topics into a research question and a series of objectives. Try to come up with questions that will always have an interesting and fulsome answer, for example avoid: Question: 'Do most university students have their own PC at home?' Answer: 'No'.
   d Decide what knowledge outcomes might emerge from each research topic, and who might be interested in the research findings (for example, politicians, salespeople, historians, parents, lecturers or …).
   e Assess the feasibility of each topic, using the suggestions given in this chapter.

2 Start to practise analysing and evaluating research. Study two research papers – look in academic IS or computing journals or ask your supervisor to suggest some. Answer the questions in the 'Evaluation Guide' above, using the material in this chapter to help you.

FURTHER READING

The knowledge outcomes were based on suggestions in Cryer (1996), an excellent guide for researchers in any discipline. Other general research guides include Davis and Parker (1997), which concentrates on the US model of a PhD, and Phillips and Pugh (1994), which mostly relates to the UK model.

Calls for conference papers and special issues of journal papers can be found in online bulletin boards and newsletters serving academics. For example, see the archives of ISWorld (www.isworld.org) and of Design Research News, a digital newsletter of the Design Research Society (www.designresearchsociety.org).

Alter and Dennis (2002), two researchers in the IS field, propose a framework for selecting research topics, and reflect on how they chose their own topics. Broy and Denert (2002) provide the proceedings of a fascinating conference in 2001, where many of the 'software pioneers' gathered together. These pioneers are some of the key contributors to computer science and software engineering, for example, Boehm, Chen and Parnas. Using book and DVD format, the proceedings contain both the original seminal works and a discussion by the authors on how they developed their groundbreaking ideas, how they were received, what has happened since, and how they now view those ideas. Browsing these proceedings will provide illustrations of how ideas for research emerge in a variety of ways and how often the significant contribution to knowledge is not realized until some time later.

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3 Overview of the Research Process

In this chapter you will learn about:

- the process of doing research;
- the components that make up the research process;
- some models of the research process.

So far we've looked at the *purpose* of research (the reasons people do research) and the *products* of research (outcomes from research projects). In this chapter we'll start to look at another of the 6Ps of research – the *process*. The majority of chapters in this book are concerned with the research process. This chapter gives an overview of it, and later chapters go into greater detail.

- **Question**: You've got a research question that interests you, you're keen to get started, so what should you do next?
- **Answer**: You start to plan the sequence of activities you need to perform that will:
  - take you from your initial research question to an answer, or set of answers;
  - enable you to present your evidence and conclusions to an academic audience and argue convincingly that you have created some new knowledge.

Remember that it's not enough just to come up with an answer to your research question – gratifying as that might be for you. For academic research, you have to put your findings and the process you followed to the scrutiny of other academics – only if they accept your evidence, process and argument will they be satisfied that you have created something that can be added to the sum total of knowledge in IS or computing.

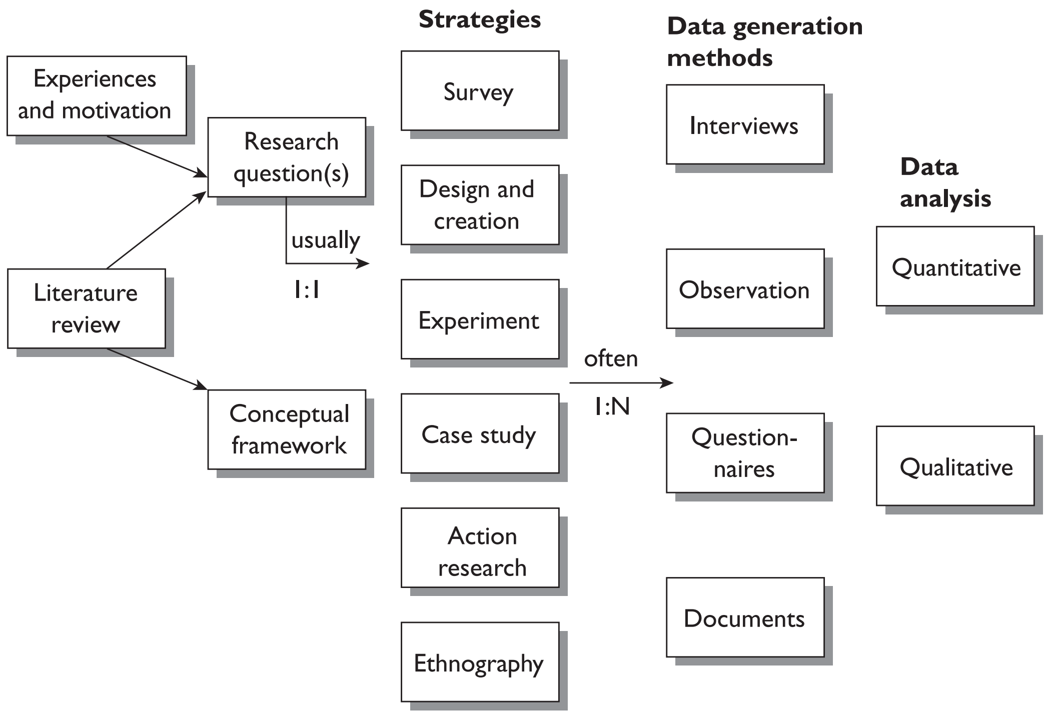

Figure 3.1 Model of the research process

A Model of the Research Process

Figure 3.1 gives an overview of the research process and its components: personal experiences and motivation, literature review, research question, conceptual framework, strategies, data generation methods and quantitative and/or qualitative data analysis. As Figure 1.4 showed, these are all shaped by the participants and underpinned by a philosophical paradigm. At the end of the research process you should have some outcomes, as explained in Chapter 2: knowledge outcomes and, possibly, a new artefact such as a computer-based product or a new development method.

Personal experiences and motivation

As we saw in Chapter 2, people do research for a variety of reasons. You need to think about why *you* are doing research. Thinking about your motivation, as well as your personal experiences, likes, dislikes, strengths, weaknesses and so on will help you think about possible research questions that you could address. Reminding yourself of why you are doing the research can also help keep you going through the difficult, boring or frustrating times – there will be some, guaranteed!

Literature review

The literature review is covered in detail in Chapter 6. All academic researchers have to review the literature in their chosen area of study: the books, journal articles and conference papers that have already been written on the topic and, for digital designers and creators, any related computer artefacts that have previously been produced. By studying the literature you can find out what has been done before, and what topics remain to be addressed. This helps you to decide upon a viable research question that has not already been fully addressed.

You also have to critically evaluate previous work, and look for themes that link different authors' work together. You will be assessed on the thoroughness of the review, your analysis of the literature and your ability to synthesize it into a coherent account that justifies your own research and places it in context. The literature review should help provide the *conceptual framework* for your research (see below).

Research question

To get going on research you need a research question or set of questions. As we saw in the previous chapter, you can find a question by thinking about:

- **Yourself**: what motivates you, the kind of research you would like to do and the kind of knowledge outcome you would like to achieve.
- **What others propose**: suggestions in the literature of where more research is needed, and calls for papers for conferences and journals on a particular topic.

Conceptual framework

A conceptual framework makes explicit how you structure your thinking about your research topic and the process undertaken. It therefore makes clear such things as:

- the different factors that comprise your topic;
- your way of thinking about the topic (for example, via a particular theory such as actor network theory or semiotics, or via a particular technology such as web services);
- your way of tackling your research question(s) (that is, the combination of strategies and methods you adopt – often called your *research methodology*);
- your approach to analysing any generated data (for example, quantitative analysis, which uses mathematics and statistics, or qualitative analysis, which uses thematic approaches);

- your approach to designing and creating any new IT product (for example, the systems development methodology you follow, or the genre you want to use);
- your approach to evaluating your research (for example, whether you will focus on technical accuracy, greater understanding, increased efficiency or aesthetic criteria).

Much of the conceptual framework is derived from, and justified by, a study of the literature.

Strategies

A strategy is your overall approach to answering your research question. In this book we look at six strategies: survey, design and creation, experiment, case study, action research and ethnography.

- **Survey**: focuses on obtaining the same kinds of data from a large group of people (or events), in a standardized and systematic way. You then look for patterns in the data using statistics so that you can generalize to a larger population than the group you targeted. (See Chapter 7.)
- **Design and creation**: focuses on developing new IT products, or *artefacts*. Often the new IT product is a computer-based system, but it can also be some element of the development process such as a new construct, model or method. (See Chapter 8.)
- **Experiment**: focuses on investigating cause and effect relationships, testing hypotheses and seeking to prove or disprove a causal link between a factor and an observed outcome. There is 'before' and 'after' measurement, and all factors that might affect the results are carefully excluded from the study, other than the one factor that is thought will cause the 'after' result. (See Chapter 9.)
- **Case study**: focuses on one instance of the 'thing' that is to be investigated: an organization, a department, an information system, a discussion forum, a systems developer, a development project, a decision and so on. The aim is to obtain a rich, detailed insight into the 'life' of that case and its complex relationships and processes. (See Chapter 10.)
- **Action research**: focuses on research into *action*. The researchers plan to do something in a real-world situation, do it, and then reflect on what happened or was learnt, and then begin another cycle of plan–act–reflect. (See Chapter 11.)
- **Ethnography**: focuses on understanding the culture and ways of seeing of a particular group of people. The researcher spends time in the field, taking part in the life of the people there, rather than being a detached observer. (See Chapter 12.)

Typically one research question has one research strategy. If you think you need more than one strategy, you probably have more than one research question embodied in your plan. This is acceptable (assuming you have the time to use two strategies within your research project), but you should spend time thinking about the different research question that each strategy will address (see the case study below). Often one journal or conference paper will report the use of just one of the research strategies, because of the word number restrictions imposed by journal publishers and conference organizers.

CASE STUDY
Example of two research strategies within one project

Michael wanted to investigate the use of Intranets by people working in Personnel Departments (sometimes called 'Human Resource' Departments). He planned to use two research strategies: a case study followed by a survey. He realized each strategy would help him address a different research question associated with his main topic:

- Case study: What issues in Company X affect the use of Intranets by those working in the Personnel Department?
- Survey: Are the issues found in Company X unique to that company, or found in many other similar companies?

Data generation methods

A data generation method is the means by which you produce empirical (field) data or evidence. Data can be either quantitative or qualitative. *Quantitative data* is numeric data, for example, number of website hits, number of employees, annual turnover, last year's profit. *Qualitative data* is all other types of data: words, images, sounds and so on.

In this book we look at four data generation methods: interviews, observations, questionnaires and documents.

- **Interview**: a particular kind of conversation between people where, at least at the beginning of the interview if not all the way through, the researcher controls both the agenda and the proceedings and will ask most of the questions. One-to-one and group interviews are possible. (See Chapter 13.)
- **Observations**: watching and paying attention to what people actually do, rather than what they report they do. Often involves looking, but it can involve the other senses too: hearing, smelling, touching and tasting. (See Chapter 14.)
- **Questionnaire**: a pre-defined set of questions assembled in a pre-determined order. Respondents are asked to answer the questions, often via multiple choice options, thus providing the researcher with data that can be analysed and interpreted. (See Chapter 15.)
- **Documents**: documents that already exist prior to the research (for example, policy documents, minutes of meetings and job descriptions) and documents that

are made solely for the purposes of the research task (for example, a researcher's logbook or design models). Also includes 'multimedia documents': visual data sources (for example, photographs, diagrams, videos and animations), aural sources (for example, sounds and music) and electronic sources (for example, websites, computer games and electronic bulletin boards). (See Chapter 16.)

Some data generation methods are commonly associated with particular research strategies: an experiment usually uses observations, and a survey often uses questionnaires. However, one research strategy can use more than one data generation method. For example, a researcher using an action research strategy in an organization might interview people, observe what occurs, ask some people to complete a questionnaire and collect documents produced during the action research project.

Using more than one data generation method enables us to look at the phenomenon of interest in different ways. For example, if you are carrying out a case study of how people interact with a web-based system, you could observe people using it, collect documentary data electronically about their movements around the website, interview them about their attitudes and perceptions concerning it, and ask them to complete an evaluation questionnaire. By using more than one method you are likely to produce more data, which could improve the quality of your research, but this may take longer and cost more.

Using more than one data method also allows the findings from one method to be corroborated or questioned by comparing with the data from another method. For example, the people you interview might tell you they found the website easy to use, but your documentary data shows that they frequently had to access the website's help system. If your data shows some consistency across methods, then it can increase confidence in your findings, showing that they are not too closely linked to the particular method you used to generate the data.

The use of more than one data generation method to corroborate findings and enhance their validity is called *method triangulation*. Many types of triangulation are possible in a research project:

- **Method triangulation**: the study uses two or more data generation methods.
- **Strategy triangulation**: the study uses two or more research strategies.
- **Time triangulation**: the study takes place at two or more different points in time.
- **Space triangulation**: the study takes place in two or more different countries or cultures to overcome the parochialism of a study based in just one country or culture.
- **Investigator triangulation**: the study is carried out by two or more researchers who then compare their accounts.
- **Theoretical triangulation**: the study draws on two or more theories rather than one theoretical perspective only.

Triangulation gives researchers multiple modes of 'attack' on their research question. However, researchers differ over whether they should expect triangulation of method or time or space, and so on, to lead to consistency of findings. It depends on their underlying research philosophy (see Chapters 19 and 20 for a detailed explanation). 'Positivists' subscribe to the idea of a single 'truth' or 'reality' and would expect the multiple lines of attack to lead to a consistent set of findings. 'Interpretivists', on the other hand, do not subscribe to the idea of a single reality, believing any notion of 'reality' to be constructed by individuals and groups, so there are multiple realities for people in our world, and different research approaches are likely to lead to different findings. Interview data about recollections of a meeting and company minutes of the same meeting, for example, are two different 'stories', created by different people for different audiences. Interpretivists would not always expect to see convergence in the data they generate using triangulation.

Data analysis

After you have generated some data, by using your chosen research strategy and one or more data generation methods, you need to analyse the data, looking for relationships or themes. *Quantitative data analysis* uses mathematical approaches such as statistics to examine and interpret the data. *Qualitative data analysis* looks for themes and categories within the words people use or the images they create. Both approaches are discussed in Chapters 17 and 18. Note that it *is* possible to apply quantitative data analysis to qualitative data: you could, for example, analyse some interview transcripts (words, qualitative data) and count the number of times a particular phrase is used (quantitative analysis).

You can draw upon your conceptual framework to help you analyse the data. The theory (or theories) in your conceptual framework can get you started by suggesting relationships or themes to look for. However, you should not be too wedded to your conceptual framework so that it blinds you to seeing interesting things in your data. You might discover that the theory in your conceptual framework needs amending, or you might find you have data that does not map onto any existing theory. Some researchers who use qualitative data even argue that you should not analyse it with *any* pre-conceived ideas or theories in mind, you should just analyse the data you have in front of you in its own terms. This approach is known as *grounded theory* and is explained further in Chapter 18.

Figures 1.4 and 3.1 imply an ordered progression from initial thoughts about areas of interest through to a literature review, selection and use of a strategy and data generation methods, through data analysis to final outcomes and presentation. Of course, in real life it's never as straightforward as that. Your plans are likely to change as your research progresses, your research questions evolve over time and you will still be discovering new things in the literature and your data when you are putting together the final presentation of your work. In real life, the process is therefore iterative and you move backwards and forwards between the different components of the model.

Alternative Models of the Research Process

Figure 3.1 gave a model of the research process, but there are other ways of characterizing it. Let's briefly consider two of them.

Conceptualize, operationalize, generalize

This model has three stages. You start off by *conceptualizing* the research topic – you make explicit what the question or problem is and how you propose to address it. This includes the theories from the literature you will use, and the research approach you will adopt (strategies, methods, legal aspects, ethical issues and underlying research paradigm). In the case of design and creation projects, you can also often express your conceptualization via models and diagrams such as storyboards or use-case scenarios.

You then *operationalize* – you put your plans into effect, by following your strategy, generating and analysing the data or developing and testing a computer-based system or other IT artefact.

Finally you *generalize* – you assess what can be learnt from your work (that is, your knowledge outcomes) that has wider applicability than just your single project.

The SDLC analogy

We can also characterize the research process by drawing an analogy with the conventional systems development life cycle (SDLC) used in developing computer-based systems (often called the 'waterfall model' of systems development). Both systems development and research are concerned with creating something new:

- Systems development creates new software.
- Research creates new knowledge.

The SDLC has four main stages: analysis, design, implementation and testing. Each of these stages in the SDLC has an analogy in stages of the research process.

Analysis

- ***SDLC****: Analyse the current system, if one exists, analyse the proposed system and produce a requirements specification or client brief, which normally includes a number of system objectives.*
- **Research**: Analyse the current state of knowledge (the literature review), decide what still needs to be done and develop a research proposal, which normally includes one or more research questions and objectives.

Design

- *SDLC: Design the planned system, initially at a high level (for example, centralized versus distributed system, relational versus object-oriented database), then at a more detailed level (for example, produce storyboards, design a database, write program algorithms).*
- **Research:** Design the planned research approach, initially at a high level (strategy to be used), then at a more detailed level (data generation methods within the chosen strategy).

Implementation

- *SDLC: Follow the design to develop the software (for example, write the program code or produce the webpages).*
- **Research:** Follow the design to do the research (for example, issue a questionnaire and analyse the responses).

Testing

- *SDLC: Evaluate the system to see if it functions as intended and meets the requirements specification or client's brief and is accepted by the users.*
- **Research:** Evaluate the research to see if it has answered the research question and is accepted as valid by the academic research community.

Evaluating the Research Process

Now that you know something of the research process, you can start to analyse and evaluate how well other researchers have described their process. Use the 'Evaluation Guide' below to help you.

EVALUATION GUIDE: RESEARCH PROCESS

1 Do the researchers make clear their research question(s)?
2 Do the researchers explain the theory(ies) they use to conceptualize the research topic?
3 Do the researchers make clear both their strategy and the data generation method(s) within that strategy?
4 Do the researchers indicate their criteria for judging the success or usefulness of their work?
5 Is there a clear process summarized, from the original motivation and literature review through to final outcome(s)? If not, how does that affect your confidence in the research and its reporting?

PRACTICAL WORK

1 Ask your colleagues or supervisor to suggest a journal in your area of interest that is widely read. Analyse one complete volume of it (that is, a year's worth of articles) to see which research strategies and data generation methods are used, and how often. Does one type predominate? Decide whether you would prefer to:

   a follow the strategy and method(s) used by the majority of researchers in your area;
   b follow a less-used research methodology.

   What are the advantages and disadvantages of each approach?
2 This chapter suggested an analogy can be drawn between the conventional systems development life cycle (SDLC) and the research process. Read Parnas and Clements' (1986) paper, entitled 'The rational design process. How and why to fake it.' Consider whether their notion of faking the rational design process destroys the analogy drawn in this chapter between the SDLC and research, or suggests further similarities between them.
3 Gain practise in analysing and evaluating research undertaken by others. Study a piece of research and answer the questions in the 'Evaluation Guide' above, using the material in this chapter to help you.

FURTHER READING

Palvia, Mao, Midha, Pinjani and Salam (2004), and Palvia, Mao, Salam and Soliman (2003) examine papers published between 1993 and 2003 in seven key IS journals (including *Communications of the ACM*, which is also widely read by computing researchers). They look at the relative popularity of different research strategies and data generation methods, and of different research topics. Glass, Ramesh and Vessey (2004) report a similar survey of articles published between 1995 and 1999 in journals for the computer science, software engineering and IS research communities.

Myers and Avison (2002) provide a useful resource for those thinking of researching with qualitative data via case studies, action research or ethnography: they have collected together some of the key texts on such research in IS and reprinted them in one volume. Mingers and Willcocks (2004) and Avgerou, Ciborra and Land (2004) are two edited collections which both provide overviews of a range of social theories and philosophies that have been used as conceptual frameworks in IS research, including actor network theory, structuration theory, phenomenology and hermeneutics.

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