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 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 by the box 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)

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 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 also 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 protective suits (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 box below.

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

DEFINITION: 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 via a 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, on the other hand, 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

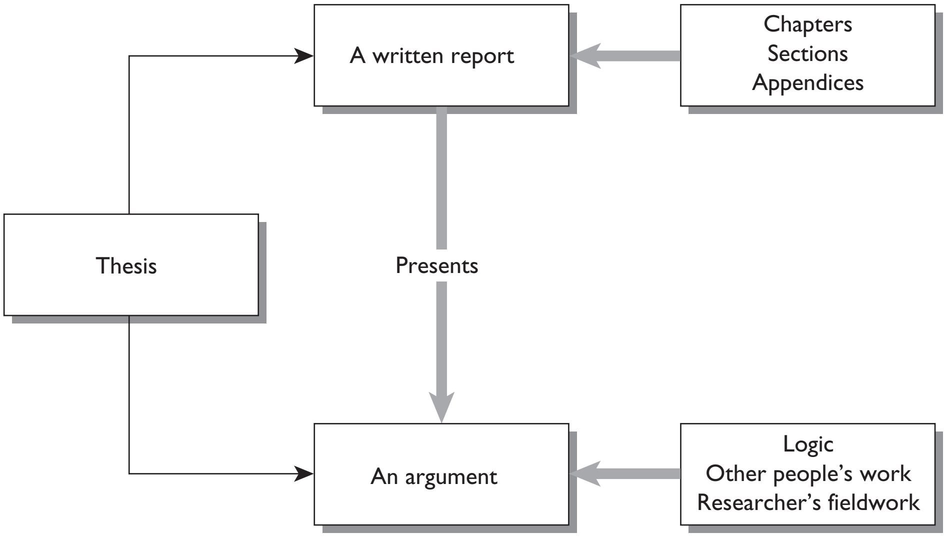

Figure 1.3 A thesis is an argument

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.
- Assessing 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 saw 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 box below 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 use. (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

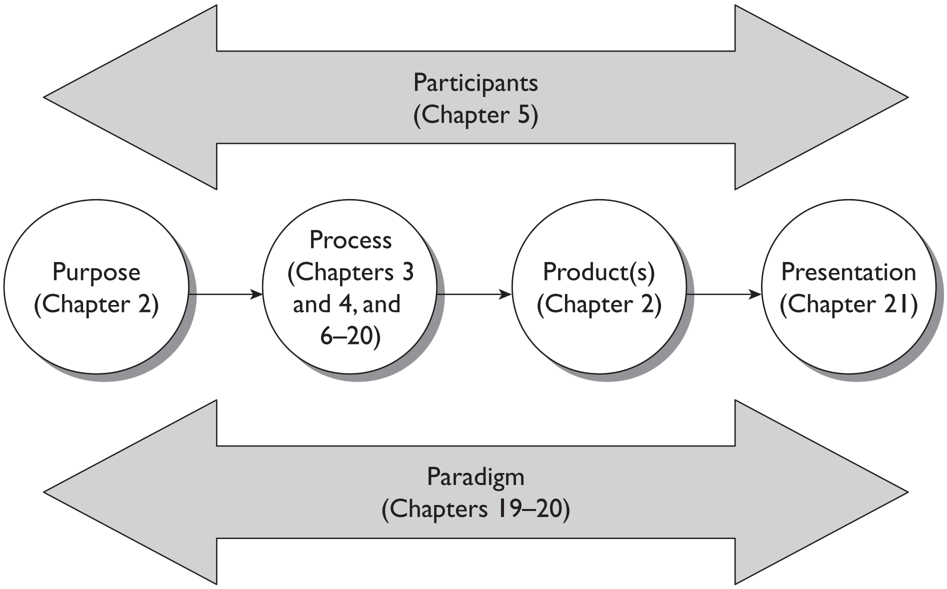

Figure 1.4 The 6Ps of research

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 the 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 (cf. e.g., 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 et al. (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 percent 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.

References

Benbasat, I. & Zmud, R.W. (2003). The identity crisis within the IS discipline: Defining and communicating the discipline's core properties. *MIS Quarterly, 27*(2).

Fitzgerald, B. (2003). Introduction to the special series of papers on, Informing each other: Bridging the gap between researcher and practitioners. *Informing Science,* 6, 13–19.

Hirschheim, R., & Newman, M. (1991). Symbolism and information systems development: Myth, metaphor and magic. *Information Systems Research, 2*(1), 29–62.

Keen, P.G.W. (1991). Keynote address: Relevance and rigor in information systems research. In H.-E. Nissen, H.-K. Klein, & R.A. Hirschheim (Eds.), *Information systems research: Contemporary approaches & emergent traditions* (pp. 27–49). Amsterdam: North-Holland.

Kling, R. (1993). Organizational analysis in computer science. *The Information Society, 9*(2), 71–87.

Kock, N., & Lau, F. (2001). Information systems action research: serving two demanding masters. *Information Technology and People, 14*(1), 6–11.

MISQ. (1999). Issues and opinions section on rigour, relevance and research in IS. *MIS Quarterly, 23*(1), 1–33.

O'Brien, L., Gold, N., & Kontogiannis, K. (Eds.) (2004). *Proceedings eleventh international workshop on software technology and engineering practice. STEP 2003.* Los Alamitos, CA: IEEE Computer Society.

Perry, D., Porter, A., & Votta, L. (2000). Empirical studies of software engineering: A roadmap. In A. Finkelstein (Ed.), *The Future of Software Engineering* (pp. 345–355). New York: ACM Press.

Robey, D., & Markus, L. (1998). Beyond rigour and relevance: producing consumable research about information systems. *Information Resources Management Journal, 11*(1), 7–15.

Senn, J. (1998). The challenge of relating IS research to practice. *Information Resources Management Journal, 11*(1), 23–28.

Tichy, W.F. (1998). Should computer scientists experiment more? *IEEE Computer, 31*(5), 32–40.

Tichy, W.F., Lukowicz, P., Prechelt, L., & Heinz, E.A. (1995). Experimental evaluation in computer science: A quantitative study. *Journal of Systems and Software, 28*(1), 9–18.

Weber, R. (2003). Editor's comments: still desperately seeking the IT artifact. *MIS Quarterly, 27*(2), iii–xi.