RESEARCH METHODS IN CLINICAL PSYCHOLOGY

An Introduction for Students and Practitioners

THIRD EDITION

Chris Barker, Nancy Pistrang and Robert Elliott

WILEY Blackwell

Research Methods in Clinical Psychology

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Contents

Preface to the Third Edition x
About the Companion Website xiii

1 Introduction: The Research Process 1
The Research Process 3

2 Perspectives on Research 5
Philosophical Issues 6
What is Research? 6
What is Science? 12
Social and Political Issues 17
Professional Issues 18
The Intuitive Practitioner 19
The Scientist-Practitioner 20
The Applied Scientist 20
The Local Clinical Scientist 21
The Evidence-Based Practitioner 22
The Clinical Scientist 22
The Practice-Based Evidence Model 23
Comparison of Models 23
Implications for Clinical Training 24
Personal Issues 24
Why Do Clinical Psychologists Do Research? 25
Why Don't Clinical Psychologists Do Research? 26
Weighing up the Pros and Cons of Doing Research 27
Chapter Summary 27
Further Reading 28
Questions for Reflection 28

3 Doing the Groundwork 29
Formulating the Research Questions 30
Choosing the Topic 31

Developing the Questions 32
Hypothesis-testing versus Exploratory Research Questions 33
Some Types of Research Question 34
Literature Review 37
The Proposal 41
Funding 44
The Politics of Research in Applied Settings 45
Access 45
Responding to Doubts 46
Authorship 48
Chapter Summary 48
Further Reading 49
Questions for Reflection 49

4 Foundations of Quantitative Measurement 50
The Process of Measurement 52
Domains of Variables 52
Measuring Psychological Constructs 52
Measurement Sources and Approaches 54
Foundations of Quantitative Methods 54
Positivism 55
Psychometric Theory 58
Definitions 58
Reliability 60
Reliability Statistics 62
Validity 64
Generalizability Theory 67
Item Response Theory 68
Utility 69
Standards for Reliability and Validity 70
Chapter Summary and Conclusions 71
Further Reading 71
Questions for Reflection 72

5 Foundations of Qualitative Methods 73
Historical Background 75
Philosophical Background 76
Phenomenology 77
Social Constructionism 79
Families of Qualitative Approaches 84
Thematic Analysis Approaches 84
Narrative Approaches 87
Language-Based Approaches 88
Ethnographic Approaches 90
Ways of Evaluating Qualitative Studies 91
Conclusion: Choosing and Combining Methods 92

	Chapter Summary	94
	Further Reading	94
	Questions for Reflection	95
6	**Self-Report Methods**	**96**
	Mode of Administration	99
	Open-ended and Closed-ended Questions	100
	Qualitative Self-report Methods	101
	Types of Qualitative Interview	102
	Interview Schedule	103
	Interviewing Style	105
	Quantitative Self-report Methods	109
	Questionnaire Design	110
	Chapter Summary	118
	Further Reading	119
	Questions for Reflection	119
7	**Observation**	**120**
	Qualitative Observation	122
	Participant Observation	122
	Text-based Research	126
	Quantitative Observation	128
	Background	129
	Procedures for Conducting Observations	130
	Reliability and Validity Issues	134
	Chapter Summary	135
	Further Reading	136
	Questions for Reflection	136
8	**Foundations of Design**	**137**
	Nonexperimental Designs	138
	Descriptive Designs	138
	Correlational Designs	139
	Experimental Designs	142
	Cook and Campbell's Validity Analysis	144
	Nonrandomized Designs	146
	Randomized Designs	152
	Conclusion: Choosing a Research Design	159
	Chapter Summary	159
	Further Reading	160
	Questions for Reflection	161
9	**Small-N Designs**	**162**
	Historical Background	163
	Single-case Experimental Designs	165
	Procedure	165

AB Design 166
Reversal (or ABAB) Design 167
Multiple-baseline Design 168
Changing-Criterion Design 169
Data Analysis 169
Generalization 169
Naturalistic Case-study Designs 170
Narrative Case Studies 170
Systematic Case Studies 171
Time-Series Designs 175
Conclusion 175
Chapter Summary 176
Further Reading 176
Questions for Reflection 177

10 The Participants: Sampling and Ethics 178
Sampling 179
The Target Population 181
Bias and Representativeness 182
Sample Size 183
Alternative Approaches to Sampling and Generalizability 185
Summary and Conclusion 187
Ethical Issues 188
Informed Consent 189
Harms and Benefits 191
Privacy and Confidentiality 192
Ethics Self-study Exercise 193
Ethics Committees 194
Chapter Summary 196
Further Reading 196
Questions for Reflection 197

11 Evaluation Research 198
What is Evaluation? 199
The Sociopolitical Context 201
Preparation for Evaluating a Service 203
Aims and Objectives 204
The Impact Model 205
The Target Population 205
Estimating the Extent of the Target Problem in the Target Population 206
Needs Assessment 207
Delivery System Design 208
Monitoring the Process of Service Delivery 209
Coverage and Bias 210
Service Implementation 211
Outcome Evaluation 212
Client Satisfaction Surveys 213

Patient-focused Research and Outcomes Management 213
Cost-effectiveness 214
Chapter Summary 215
Further Reading 216
Questions for Reflection 216

12 Analysis, Interpretation, and Dissemination 217
Qualitative Data Analysis 218
Within-case and Cross-case Analysis 219
Preliminaries to Qualitative Data Analysis 219
Processes in Qualitative Data Analysis 221
Good Practice in Qualitative Analysis 224
Quantitative Data Analysis 224
Data Entry 224
Data Checking 225
Data Reduction 225
Data Exploration 226
Statistical Significance Testing for Answering the Research Questions 227
Analyzing the Strength and Significance of Quantitative Effects 227
Interpretation 232
Contributions to Knowledge: Understanding the Meaning of the Findings 232
Methodological Issues: Strengths and Limitations of the Study 233
Scientific and Practical Implications 235
Dissemination 236
Writing up 237
Publication 238
Authorship Issues 239
Utilization 240
The End 240
Chapter Summary 241
Further Reading 241
Questions For Reflection 242

13 Epilogue 243
Methodological Pluralism 243
Appraising Research 244
Combining Research with Practice 246
Some Images of Research 247
Questions for Reflection 248

References 249
Author Index 277
Subject Index 287

Preface to the Third Edition

The first edition of this book came out more than 20 years ago, and the second more than 10. A lot has gone on during that 20-year time span, both in the book's subject matter and in our own professional lives. When we wrote the first edition, we were junior academics, and the research methods literature was much smaller and easier to master than it is now. We learned an enormous amount in the course of writing that first edition text; as has frequently been observed (originally by the physicist Frank Oppenheimer, according to Wikipedia), the best way to learn something is to teach it. As our careers have progressed, so has the methodological literature, which seems to have outgrown our own capacity (and probably anyone else's) to keep up with it. Such is its volume and complexity that it has seemed as big a task to produce this third edition from the second as it did producing the first from scratch. However, we have once again relished getting to grips with the new ideas ourselves and attempting to communicate them clearly to our readers.

Since the previous edition, there have been major changes in how information is accessed and processed, and in how research is conceptualized and conducted. Some of the most important additions or changes in this edition are systematic review methods and literature-searching methods (see Chapter 3), structured guidelines for appraising the research literature (see Chapters 3 and 8) and for preparing journal articles (see Chapter 8), modern psychometric methods (e.g., item response theory, see Chapter 4), guidance on choosing between different qualitative approaches (see Chapter 5), and the internet as a medium for conducting psychological research (see Chapters 6 and 10).

When we began updating the second edition to produce this one, we initially thought that we would completely revamp the references, as several had endured since the first edition and were written before many of our readers would have been born. We had a general "out with the old, in with the new," "let's clear out the attic" attitude. However, as the writing progressed, it quickly became apparent that many of the old references actually hold up rather well, several being classic papers that all clinical psychologists need to be aware of. So, while we have updated many of the citations, the end result represents what we hope is a judicious mix of ancient and modern.

The choice of title led to some debate among the authors and publishers. The first edition, which was entitled *Research Methods in Clinical and Counseling Psychology*, had its genesis in our teaching on clinical and counseling psychology courses. The second edition, entitled *Research Methods in Clinical Psychology*, focused on clinical psychologists as a primary readership, with counseling, health, educational, and community psychologists also being very much in our minds. The book should really be called something like Research Methods in Clinical Psychology and Allied Professions, but that is too clunky and unfocused. In our time, we have taught research methods to students and professionals in many other allied fields, including health, community, counseling, and educational psychology, psychiatry, speech therapy, and nursing. We want this text to be accessible to all of these audiences and more. We hope that potential readers from other disciplines will judge the book by the content not just the title—we intend it to be useful for not just clinical psychologists, but also for a broad range of mental health disciplines.

We have once again tried to make the text reader-friendly by having frequent bullet-point summaries of the important points in boxes, and a chapter summary and suggested reading at the end of each chapter. In this edition, we have added questions for self-reflection, also at the end of each chapter. Personal preferences are an often unacknowledged influence on the research that one conducts, and the questions for reflection are designed to help readers explore what they think and feel about the various approaches and issues that we have described in each chapter. We have also, as with the last edition, uploaded supplementary material for readers and instructors onto the book's website.

A few matters of grammar and style are worth noting. We have generally preferred vernacular to supposedly purist forms of expression. Thus, following recent trends, we have usually used the colloquial "they" to indicate a single person of unspecified gender, rather than the awkward sounding "he or she." "Data" is treated as a collective noun either in the singular or the plural, as sense dictates, as in common speech. We are fully aware that it is a plural noun in Latin, but like "agenda," also a Latin plural, it is frequently used in the singular in spoken English. We have also not hesitated to boldly split infinitives: the supposed rule prohibiting this practice now seems antiquated.

As with previous editions, we have tried to make this one relevant both to North American and to British readers. We are a transatlantic authorship team (one Brit, one American, and one who is both), although we are all currently working in the United Kingdom. Due to limitations in our abilities and experience, we have restricted most of our examples to the English-speaking world. However, we have taught research methods in other countries, and have had some instructive correspondence with our Asian, African, and Australian readers, so we hope that the book can be useful to readers outside of North America and the British Isles.

The first two authors are fortunate to work at University College London (UCL) in London's Bloomsbury district, which is probably the best place on the planet for library access. For this book, we have relied on three excellent libraries – the UCL library, the University of London Research Library, and the British Library – which are all within easy walking distance. UCL has provided us with an outstanding selection of electronic journals, the University of London Research Library has a superb reference collection of psychology books for browsing, and the British Library is a magnificent public resource capable of supplying our every bibliographic want. Long may these institutions flourish!

Revising this book has also brought home once more what an excellent research methods education we three all received in our graduate school days at the University of California, Los Angeles. We were exposed to the full gamut of methodological options, by first-rate statistics and measurement instructors in the Psychology Department and innovative qualitative researchers in Sociology. This book is a tribute to all of our own instructors and mentors.

We are grateful to our many academic friends and colleagues—both past and present—in our own universities and our wider scientific circles, for inspiring us, keeping us up to date, and challenging us. We would also like to thank the following for their help with preparing the current edition. Several colleagues gave us suggestions or generously commented on chapter drafts: John Cape, Kate Cheney, James Coyne, Ravi Das, Allen Dyer, Peter Fonagy, Andy Fugard, Vyv Huddy, Zoe Huntley, Narinder Kapur, John King, Henry Potts, Tony Roth, James Schuurmans-Stekhove, and Francine Wood. Special thanks to Will Mandy for looking at several chapters at short notice. Marie Brown capably assisted with the library research, efficiently chasing up some of the more obscure references, and road-tested several parts of the text. Rachel Schön kindly assisted with the indexing. Shamil Wanigaratne and Sue Salas have been encouraging and supportive readers over three editions (and three countries). Our thanks to the team at Wileys: Andrew McAleer, who first encouraged us to undertake this rewrite, Karen Shield, our project editor, Amy Minshull, the editorial assistant, Nivedha Gopathy, the project manager, and Stephen Curtis, our eagle-eyed copy-editor. Thanks also to those who helped with previous editions: John Cape, Lorna Champion, Linda Clare, Michael Coombs, Neil Devlin, Jerry Goodman, Les Greenberg, Dick Hallam, Connie Hammen, Wendy Hudlass, Maria Koutantji, David Rennie, Laura Rice, Joe Schwartz, Pam Smith, and Mark Williams with the first edition, and Anna Barker, Chris Brewin, John Cape, Kate Cheney, Pasco Fearon, Dick Hallam, David Shapiro, Jonathan Smith, Lesley Valerio, and Vivian Ward with the second. And, finally, many thanks to all of our students, past and present, for their engagement with our teaching and supervision, and for continuing to keep us on our toes.

Even though we have benefited enormously from the advice and scrutiny of our colleagues and students, the responsibility for any residual errors remains our own. The process of preparing this edition has unearthed some minor mistakes in the previous one, and doubtless others still lurk herein. If you spot something wrong, please let us know, and we will post a correction on the book's website. We appreciate any feedback, positive, negative, or neutral, from our readers. We hope that this book will prove a useful resource in your own consumption or production of research, or in simply appreciating what a complex business it all is.

About the Companion Website

The companion website for the book, at www.wiley.com/go/barker provides supplementary material for readers, both students and instructors. For each chapter there are PowerPoint slides, questions for reflection, internet resources, and more.

1

Introduction: The Research Process

KEY POINTS IN THIS CHAPTER

- Research tells a story.
- Research raises questions as well as answering them.
- There is a vigorous debate within psychology about what constitutes legitimate research.
- This text takes a stance of methodological pluralism: of fitting the research method to the research question.
- The research process can be divided into four main stages: groundwork, measurement, design, and analysis/interpretation.

Research tells a story. Ideally, it resembles a detective story, which begins with a mystery and ends with its resolution. Researchers have a problem that they want to investigate; the story will reach its happy ending if they find a solution to that problem.

In practice, however, things aren't quite that simple, and the actual picture is closer to an adventure story, with many unexpected twists and turns. Often, the resolution of a research project is uncertain: it doesn't answer your initial research question, rather it tells you that you were asking the wrong question in the first place, or that the way that you went about answering it was misconceived. You struggle with discouragement and frustration; perhaps you come out of it feeling lucky to have survived the thing with your health and relationships (mostly) intact. So, if you enjoy research and are determined to make a contribution, you organize a sequel, in which you try out a

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better question with a better designed study, and so it goes on. Another way of putting it is that there are stories within stories, or a continuing series of stories. Each individual research project tells one story, the series of projects conducted by a researcher or a research team forms a larger story, and the development of the whole research area a yet larger story. And this progression continues up to the level of the history of science and ideas over the centuries.

Another way that things are not so simple is that not all researchers agree on what constitutes a legitimate story. The situation in psychology is analogous to developments in literature. On the one hand is the traditional research story, rather like a Victorian novel, which has a clear beginning, middle, and end, and is expected to provide a more or less faithful reflection of reality. On the other hand, in this modern and postmodern age, we encounter narratives that do not follow an orderly chronological sequence or tie up neatly at the end. Furthermore, they may not claim to represent, or may even reject the idea of, reality.

These developments in literature and psychology reflect general intellectual developments during the last century, which have ramifications across many branches of European and English-speaking culture, both artistic and scientific. Our own field of interest, psychology in general and clinical psychology in particular, has been going through a vigorous debate about the nature of research – that is, which of these narratives we can call research and which are something else. Scholars from various corners of the discipline of psychology (e.g., Carlson, 1972; Driver-Linn, 2003; Gergen, 2001; Rogers, 1985; Sarbin, 1986) have questioned the validity and usefulness of psychology's version of the traditional story, which has been called "received-view" or "old-paradigm" research: essentially a quantitative, hypothetico-deductive approach, which relies on linear causal models. These and other critics call for the traditional approach to be replaced, or at least supplemented, by a more qualitative, discovery-oriented, nonlinear approach to research.

This debate, as Kimble (1984) pointed out, is a contemporary manifestation of William James's (1907) distinction between tough-minded and tender-minded ways of thinking, which is itself a translation into psychological terms of the old debate in philosophy over empiricism (Aristotle) versus rationalism (Plato). However, it is simplistic to view this debate as two-sided, with researchers being either in one camp or the other. It is better viewed as reflecting multiple underlying attitudes, for example, preferences for quantitative versus qualitative methods, attitudes towards exploratory versus confirmatory research questions, experimental control versus real-world relevance, and so on (Kimble, 1984).

One consequence of the lack of consensus about acceptable approaches to research is that people who are doing research for the first time may experience considerable anxiety – rather like the existential anxiety that accompanies a loss of meaning (Yalom, 1980). Undertaking a research project without being clear about what standards are to be used to evaluate it is an unsettling experience. Furthermore, there is a political dimension, since people in powerful positions in the academic world – journal editors, grant reviewers, and university professors – often adhere to the more traditional models.

This anxiety is exacerbated because the rules are not always made explicit, which may make beginning researchers feel, like Alice in Wonderland, that they are in a strange country with mysterious and arbitrary rules that are continually being changed. Researchers are constantly reminded, in various ways, to behave themselves properly in accordance with these scientific rules; as the Red Queen said to Alice, "Look up, speak nicely and don't twiddle your fingers all the time!" This experience can be understandably off-putting for people trying to enter the research wonderland for the first time.

We will reconsider these issues in Chapters 2, 4, and 5, which address the conceptual underpinnings of research. However, it is worth stating at the outset that our own stance is one of methodological pluralism. We don't think that any single approach to research (or, indeed, that psychological research itself) has all the answers; thus, we believe that researchers need to have at their disposal a range of methods, appropriate to the problems being investigated. We have considerable sympathy with the critics of the received view, but are not convinced that the consequence of accepting their criticisms is to abandon traditional quantitative methods, or even research in general. Indeed, we feel that to do so would be a disaster for psychology and for society. Fortunately, we see increasing signs that it is possible to articulate a synthesis of the old- and new-paradigm traditions, that there are general principles common to rigorous research within whatever paradigm, and that it is possible to lay out an overall framework which organizes different approaches to research and clarifies the ways in which they can complement one another. Learning to do psychological research is partly a process of learning disciplined enquiry according to these principles within this general framework.

At the same time, there are rules of good practice specific to each type of research. We will base our methodological pluralism on a principle of appropriate methodologies (by analogy to the catch phrase "appropriate technology" in the economics of development). By this, we mean that the methods used should flow out of the research questions asked. Different questions lend themselves to different methods. To resume our literary analogy, like the different literary genres (mystery, romance, science fiction, autobiography, etc.), we can think of different research genres, such as survey research, randomized clinical trials, systematic case studies, and in-depth qualitative interview studies. Each of these research genres has different stories to tell and different rules of good practice.

We will attempt to clarify these general principles and specific rules of good practice, so that you will be in a better position to appreciate other people's research. We hope that this will help you feel less intimidated about the prospect of conducting your own research. Also, there is value in making the rules of research explicit, so that one can challenge them more effectively, and thus contribute to the debate about how psychological research should be conducted.

Research is demanding: it does require clear and rigorous thought, as well as perseverance and stamina, but it is also fascinating and exciting, and, we hope, beneficial to the public that psychologists ultimately profess to serve.

The Research Process

This book is structured around a simple chronological framework, which we call the *research process:* that is, the sequence of steps that researchers go through during a project. The steps can be grouped into four major stages. Like all such frameworks, it is idealized, in that the stages are not always distinct and may interact with each other. However, we find it a useful way of thinking about how research is conducted, both one's own and other people's.

1. *Groundwork* (Chapter 3). This stage involves both scientific issues – choosing the topic, reviewing the literature, specifying the conceptual model, formulating the research questions – and also practical issues – resolving organizational, political, financial, or ethical problems. Sometimes researchers give the groundwork short shrift, being anxious to get on with the business of running the project itself. However, we will argue that devoting careful thought at this stage repays itself with interest during the course of the project.
2. *Measurement* (Chapters 4 to 7). Having formulated the research questions, the next step is to decide how to measure the psychological constructs of interest. We are here using the term "measurement" in its broadest sense, to encompass qualitative as well as quantitative approaches to data collection.
3. *Design* (Chapters 8 to 11). Research design issues concern when and from whom the data will be collected. For example: Who will the participants be? Will there be an experimental design with a control group? How many pre- and post-assessments will there be? What ethical concerns need to be addressed? These design issues can usually be considered independently of measurement issues.

   The research questions, measurement procedures, and design together constitute the *research protocol,* the blueprint for the study. Having gone through these first three stages, researchers will usually conduct a small pilot study, whose results may cause them to rethink the protocol and possibly to conduct further pilots. Eventually the protocol is finalized; the last stage then consists of implementing it.
4. *Analysis, interpretation, and dissemination* (Chapter 12). The data are collected, analyzed, interpreted, written up, possibly published, and, let us hope, acted upon.

These stages in the research process constitute our framework for the book. However, we will also examine some key philosophical, professional, and political issues that are central to thinking about the whole research enterprise (Chapters 2, 4, and 5). Although following these arguments is not necessary for learning purely technical research skills, it is important to understand the wider context in which research is being conducted, as doing so will lead to more focused, coherent, and ultimately useful research programs. It is also important to keep in mind that doing research is much more than the exercise of a set of techniques; carrying out research involves imagination and empathy, problem-solving skills and critical thinking, and ethical reflection and social responsibility.

The first part of this background material is given in the next chapter, which analyzes the meaning of some of the terms we have so far left undefined, such as "research" itself. We will also discuss why anyone might want to engage in research at all.

2

Perspectives on Research

KEY POINTS IN THIS CHAPTER

- Psychological research is situated within philosophical, professional, personal, and political contexts.
- The process of psychological research is similar to that of open-minded enquiry in everyday life.
- Several philosophers have attempted to characterize the essence of scientific progress: Popper, Kuhn, and Feyerabend are central figures.
- Social and political forces shape the development of science.
- The scientist-practitioner model is a central part of clinical psychology's professional ideology, but there is often a gap between rhetoric and reality.
- Practicing clinical psychologists may choose to do research, or not to, for a variety of reasons.

This chapter examines some important background issues, in order to give a sense of the context in which research is conducted. These cover the "three P's": the philosophical framework (i.e., the underlying set of assumptions about the research process), the professional context (i.e., how research fits in to clinical psychology's professional identity), and also the personal context (i.e., each individual researcher's own attitudes towards research). In the background there is also the fourth P, the political context.

Understanding these contextual issues is helpful both in reading other people's research and also in conducting your own. It helps make sense of other people's research if you understand the framework within which it was conducted. If you are doing research yourself, it follows that the more you are aware of your assumptions, the more

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you are able to make informed choices about what methods to use, rather than following available examples blindly (Elliott, 2008). This is similar to clinical work, where clients who have greater insight into their motivating forces are generally better able to live freer and more productive lives, and therapists who are able to step outside of their own perspective are better able to understand and help their clients (Rogers, 1975). However, again as in clinical work, making decisions can be hard work as you become aware of the multiple possibilities of action instead of making automatic choices.

The chapter has three sections, covering philosophical, professional, and personal issues. Political issues are touched on in all three sections.

PHILOSOPHICAL ISSUES

This section examines what is meant by two key terms: research and science. However, we need to start out with a couple of disclaimers. First, several of the ideas are complex and require philosophical expertise to appraise them properly. We do not possess such expertise, nor do we expect the great majority of our readers to. Second, grappling with difficult issues such as the nature of reality at this early stage can be heavy going. As is the case in all philosophy, there are more questions than answers. We attempt to give an overview of some interesting contemporary issues; it is not necessary to follow them in detail in order to conduct or critique research. However, having a broad grasp of them will help you understand (perhaps more clearly than the researchers themselves do) what a piece of research is attempting to achieve.

Philosophical issues that relate more specifically to psychological measurement (namely discussion of the positivist, phenomenological, and social constructionist positions) are covered in Chapters 4 and 5.

What is Research?

- Conducting research is essentially a circular activity (see Figure 2.1).
- Research requires psychological flexibility and open-mindedness.
- Research is not the only way to acquire psychological understanding: literature, life experience, and supervised clinical work are also important.
- The main reason for following rigorous research methods is to minimize bias and reduce errors in drawing conclusions.
- A rudimentary understanding of epistemology (the theory of knowledge) helps to elucidate some basic procedures and distinct stances towards research (e.g., critical realism and constructionism).

As Figure 2.1 suggests, the research process is a potentially everlasting circle. Our human propensity to understand ourselves and the world that we live in has been noted since ancient times. Plato had Socrates say (in the *Apology,* 38) that "the unexamined life is not worth living." Some writers, for instance, Cook and Campbell (1979),

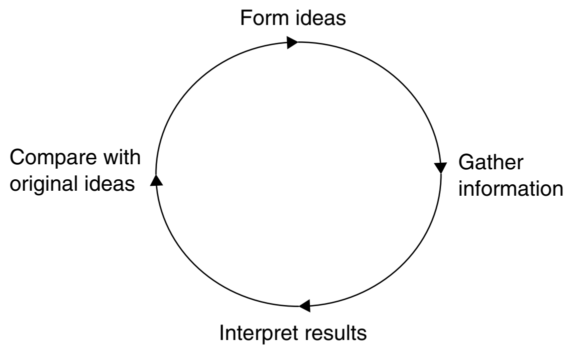

Figure 2.1 The research cycle

consider that the psychological roots of research have evolutionary significance: that there is survival value in our attempts to understand the world and ourselves.

Note that this circular model does not attempt to explain where we get our ideas from in the first place. There is a long-standing debate in philosophy and developmental psychology, which we will sidestep for the moment, about whether acquiring knowledge of the world is possible without some previous understanding. Our emphasis is on how educated adults discover and test ideas.

Research demands a degree of psychological flexibility, that is, an ability to modify one's ideas if they are not supported by the evidence. It may be helpful to view various sorts of disruptions in the circular model as corresponding to various maladaptive psychological styles. For instance, a refusal to interact with the world at all, elaborating theories without ever testing them against the "real world" (i.e., never moving down off the first stage of our circular model), is a solipsistic stance of building dream castles with no basis in reality – a stance captured in the epithet used to describe out-of-touch academics: "the ivory tower." This refusal to gather information also characterizes someone who is overconfident in the value of their ideas, and does not see any need to put them to any kind of empirical test. (Politicians often seem to fall into this category, with the result that many aspects of our society, such as education, the penal system, and health care, are largely determined by ideology rather than evidence.)

Problems in the lowest quadrant of the circle include biases in analyzing or interpreting the data: allowing what you want to get from a research project to distort how you report what actually happened. Our data are always influenced to some extent by our values and preconceptions; after all, these determine what we choose to study in the first place, what we count as data, what we select as important to report from amongst our findings, and inevitably the conclusions we draw about the world from our research. Indeed, Bayes's theorem holds that drawing inferences from research to the world is impossible without taking prior assumptions into account (Dienes, 2011). In extreme cases, however, researchers' personal circumstances or ideological commitments may lead them to ignore or suppress unwanted findings, or even to fabricate results (Pashler & Wagenmakers, 2012). While extreme cases of scientific dishonesty are probably rare, each of us is subject to self-deception, which may lead to distorting our results in subtle ways, the most common of which is simply dismissing our own or other people's results that don't fit our preconceptions.

Similar problems exist in the final step of the circular model: the refusal to modify one's ideas, because one dismisses or distorts the evidence, which characterizes a rigid, dogmatic stance. This can be seen in people who cling to various kinds of orthodoxies and fundamentalist beliefs in the face of contrary evidence. (Politicians often seem to fall into this category too!)

While passions and personal feuds make science more interesting, and have always helped drive it forward, we believe that curiosity and an inquiring, open-minded research attitude is one aspect of good psychological functioning. It is similar to Jahoda's (1958) concept of "adequate perception of reality" as one criterion for positive mental health.

Thus far, our characterization of research applies to everyday life as much as to organized science. We all do research informally; it is one way that we form our mental representations of the world. This is what Reason and Rowan (1981) call "naive enquiry." George Kelly (1955) elaborated the metaphor of the person as a scientist into an entire theory of personality: that people are continually building and testing their set of "personal constructs." However, cognitive and social scientists have also shown that people display pervasive biases in the way that they process information (Fiske & Taylor, 2013; Kahneman, 2011; Nisbett & Ross, 1980). The fundamental reason for the development of rigorous research methods is to attempt to minimize biases in drawing conclusions from evidence.

Finally, we should make it clear at the outset that we do not see research as being the only, or even an especially privileged, route to knowledge. One can learn much of relevance to psychology from the works of Shakespeare, Tolstoy, George Eliot, or James Joyce (to name a few of our own favorites). Great works of art or literature will often have a ring of truth that will immediately resonate with the viewer or reader. Furthermore, everyday life experiences also help build a knowledge base. In Morrow-Bradley and Elliott's (1986) survey of sources of psychotherapeutic knowledge, therapists reported that they learned most from experience with their clients, followed by theoretical or practical writings, being a client themselves, supervision, and practical workshops. Research presentations and research reports were ranked first by only 10% of the sample of practicing therapists (in contrast to experience with clients, which was ranked first by 48%).

However, the strength of formal research is that it is a systematic way of looking at the world and of describing its regularities, and it provides knowledge that can allow us to decide between conflicting claims to truth that may be put forward by rival proponents. New approaches to treatment are constantly being developed, and usually the person who develops the therapy will offer some preliminary evidence for its effectiveness. One example of a therapy that has gained widespread attention is multisystemic therapy (MST) for adolescent conduct disorders (Henggeler, Melton, & Smith, 1992). However, it has also attracted controversy about the quality of its supporting evidence (Littell, 2006), which has mostly been produced by the model's proponents. Until several rigorous studies have been conducted by researchers without a theoretical allegiance to the model, we will not be able to properly evaluate its effectiveness and mechanisms of action.

Furthermore, because research is a shared, public activity, it has a crucial role in contributing to the development of theory and professional knowledge. Interactions with clients, conversations with fellow professionals, and personal growth experiences are all useful ways of educating oneself individually, but research, theoretical writings, and published case reports are public documents and therefore contribute to the development of the profession as a whole.

We will explore such professional issues more fully in the next section, and then, in the final section, discuss why individual psychologists might (or might not) want to do research. However, before we can do this, we need to examine the meaning of some of our core terminology in greater depth.

Definition of "Research"

The Oxford English Dictionary's definition of "research" serves as a good working definition. It is: "A search or investigation directed to the discovery of some fact by careful consideration or study of a subject; a course of critical or scientific enquiry." Five aspects of this definition are noteworthy.

First, the definition stresses the *methodical* aspect of research, that research is careful and disciplined. It is a craft that requires considerable dedication and attention to detail. There is also, however, a chance element to research: not all discoveries are necessarily planned and serendipity often enters in (Merbaum & Lowe, 1982). The classic example of an accidental scientific discovery is Fleming's isolation of penicillin, when he noticed that some mold in a dish stopped the growth of bacteria he was attempting to cultivate. However, to take advantage of a chance discovery, the researcher must have the knowledge and insight to appreciate its significance, and then the persistence to follow it up. As Louis Pasteur, the microbiologist who invented the rabies vaccination is reputed to have said, "In the fields of observation, chance favors only the mind that is prepared" (O'Brien & Bartlett, 2012).

Second, the definition specifies a *critical or detached* attitude. This attitude is an important feature of the clinical psychology discipline. Clinical psychologists are trained to question the basis of professional practice, for example, "What's going on here?"; "How do you know that?"; "What's the evidence for that assertion?" This skeptical attitude does not always endear them to their colleagues from other mental health disciplines: it can at times lapse into rigid adherence to a narrow form of scientific practice (e.g., large randomized clinical trials), and may contribute to the common perception of psychologists as standing at one step removed from the other professionals in a team or service.

Third, the definition does not specify the method of research, suggesting the value of both *rational and empirical* investigation. While rational or conceptual research is sometimes denigrated in psychology as "speculation" or "armchair philosophizing," it is essential in other disciplines, especially the humanities, and is the method of choice in mathematics (the "queen of the sciences") and theoretical physics, both of which proceed from axioms to deductions. Psychology is primarily an empirical science, concerned with systematically gathering data, which are then used, in ways we will discuss below, to develop and test its theories. However, there is also an important role for conceptual research, to formulate theories, to explicate underlying principles, and to identify the assumptions underlying research (Slife & Williams, 1995). This issue of research method relates back to the centuries-old philosophical debate between rationalists and empiricists over the sources of human knowledge (Russell, 1961).

Fourth, the definition states that research is a process of *discovery*. This raises the distinction between exploratory research, which sets out to find something new, and confirmatory research, which sets out to evaluate existing theory (see Chapter 3). Philosophers of science make a similar distinction between the context of discovery and the context of justification of a particular finding (Reichenbach, 1938). We include both exploratory and confirmatory approaches under the definition of research, and see both as equally valid and useful.

Finally, the definition says that research is directed towards the discovery of *facts*. The Oxford English Dictionary defines a fact as "something that has really occurred or is the case." However, this definition begs some difficult philosophical questions about how we come to know what is true, and requires some consideration of the philosophical basis of truth and knowledge.

Epistemology

The theory of knowledge is known as *epistemology;* it is the area of philosophy devoted to describing how we come to know things or believe them to be true or real. In fact, when psychologists talk about validity and reliability, in either quantitative psychometrics (see Chapter 4) or qualitative research (see Chapter 5), they are talking in epistemological terms. According to Hamlyn (1970; see also Packer & Addison, 1989), there are four fundamental epistemological positions, or criteria of truth:

1. The *correspondence theory* of truth, the basis of realist philosophies, holds that a belief is true if it matches reality.
2. *Coherence theory,* the basis of rationalist philosophies, holds that a belief is true if it is internally consistent or logically non-contradictory.
3. The *pragmatist or utilitarian criterion* holds that a belief is true if it is useful or produces practical benefits.
4. The *consensus criterion,* the basis of sociological theories of knowledge (see below), holds that a belief is true if it is shared by a group of people.

None of these theories is completely adequate: all have serious logical flaws. For example, correspondence theory involves an infinite regress, because reality must be measured validly before the degree of correspondence can be assessed. (This is referred to as the criterion problem in measurement.) Furthermore, counterinstances of each of the other three criteria can readily be imagined (e.g., an elegant, coherent theory which has no bearing on reality; a false belief which nevertheless proves useful; and a false consensus or collective delusion). On the other hand, all four theories have some value, as practical, but fallible guidelines (Anderson, Hughes, & Sharrock, 1986), suggesting the importance of a pluralist epistemology. Optimally, one would attempt to realize all four truth criteria in one's research (cf. Elliott, Fischer, & Rennie, 1999).

Realism and Constructionism

Physical scientists often implicitly work from a *realist* position, which is based on a correspondence theory of truth. Realism posits that there is a real world out there, independent of whoever may be observing it (Bhaskar, 1975). Thus the rocks of the moon have a geological composition that is, at least in principle, discoverable: that some people may believe the moon to be made of green cheese is irrelevant. Within this realist framework, the task of the scientist is to understand as accurately as possible the properties of the real world. Scientists themselves might say that they are trying to understand Nature.

For most of the past 100 years, psychologists have also emphasized a correspondence theory of truth, although in the latter half of the 20th century this evolved into a *critical realist* position (Cook & Campbell, 1979). This assumes that there exists a real world out there that has regularities. However, we can never know it with certainty: all our understandings are essentially tentative. The critical realist position emphasizes the replicability of research: that other researchers should be able to repeat your work and get approximately the same results, or in more technical language, that knowledge should be "intersubjectively testable" (Cook & Campbell, 1979; Popper, 1959). This means that researchers must be explicit about how they collected their data and drew their conclusions, so that other researchers can evaluate their conclusions or replicate the study themselves. Beyond this, it suggests that researchers should approach the same topic using different methods, with complementary strengths and weaknesses, a strategy of "triangulation" (Creswell, 2009; Tashakkori & Teddlie, 2009), a term taken from geometry and surveying. Thus, critical realists go beyond correspondence theory to include consensus and coherence truth criteria.

In the last two decades of the 20th century, various challenges to realist and critical realist philosophies emerged. These approaches emphasize either coherence or consensus theories of truth and try to eliminate correspondence criteria. The major current alternative to the critical realist position can be found in the various forms of *constructionism* and *constructivism*, some of which overlap considerably with *postmodernism* (Gergen, 2001; Guba & Lincoln, 1989; Neimeyer, 1993) and with *narrative* approaches (Bruner, 1991; Riessman, 2008). These are fairly imprecise terms, but they share a common stance of dispensing with the assumption of an objective reality and instead studying people's interpretations or stories (see Chapter 5 for further discussion). Postmodernists are impatient with what they call "grand theory"; instead they present a more multifaceted, fractured world view, some taking the extreme point of view that there are no true and false stories, only different stories. The central problem with such radical constructionist or postmodernist views is that not all constructions or stories are equally interesting, consistent, replicable, shared, useful, or even accurate. That smoking causes lung cancer or that poverty reduces one's quality of life, though not unassailable propositions, seem to describe important consistencies in the world.

Social constructionists emphasize the social construction of reality and see the research setting as a specialized form of social interaction, a situation for eliciting and studying people's stories. They argue that researchers are not detached observers, but actively play a part in what they are studying and how they make sense of it (McGrath & Johnson, 2003). Thus, the collection, analysis, and interpretation of data involve processes of active construction. A related point is the interdependence of the knower and the known, which is emphasized by constructivists, like Piaget (1970), Vygotsky (1978), and Bruner (1987). That is to say, in coming to know a thing, both the state of our knowledge and the thing itself may be changed; what we call facts are a joint construction of the things themselves and our knowing process. For example, the process of interviewing a client about her reactions to a recent therapy session may change both the way that the interviewer understands the process of therapy, and the way that the client feels about the session, her therapist, or herself.

Pure and Applied Research

There are many ways to classify research, for example, according to content, setting, population, or method. One important distinction is between basic academic research and applied (including evaluation) research. Although often presented as a dichotomy, the two positions are better thought of as two ends of a continuum (Milne, 1987; Patton, 2002).

Basic (or pure) research addresses the generation and testing of theory. What are the underlying processes that help us understand the regularities in nature? Basic research emphasizes processes common to most people. Because clinical psychology is an applied discipline, basic research is rare, but examples of research toward the basic end of the spectrum include the relative contributions of relationship versus technique factors in therapy outcome in general, and the neuropsychological mechanisms involved in recalling traumatic memories.

Applied research addresses practical questions, for example, whether a particular intervention works for a particular client group. At the far applied end of the spectrum is *action research* (Patton, 2002), carried out to address a particular local problem, such as the high dropout rate at a local psychotherapyservice. *Evaluation research* also resides near the applied end of the spectrum, as it primarily addresses the general needs or outcomes of a particular agency or service, but may have a broader relevance. Evaluation is often motivated by pragmatic concerns, such as the need to maintain funding for a particular service. Although the methods used in pure and applied research overlap considerably, we will address some issues particular to evaluation research in Chapter 11.

In actual practice, pure and applied research blend into each other. As the above examples of pure research demonstrate, there is often an element of application in clinical research: that is what makes it clinical. Many examples of clinical research lie on the middle ground. For instance, psychotherapy outcome research addresses questions of both theory and application. Since we see the pure/applied distinction as a continuum rather than a dichotomy, we adhere to a definition of research that encompasses the full spectrum, and can even be extended to clinical practice (a point we take up later in this chapter).

What is Science?

We have used the word “science” up to now without questioning its meaning. Yet there is a lively debate about what science consists of, a debate that goes to the heart of some enduring controversies within psychology and related fields. It addresses the question of how knowledge is acquired and which methods of research are “scientific” (and therefore respectable). In a much-used example, how can we distinguish between legitimate science and voodoo or astrology? Or is such a distinction only a social construction? Closer to home, in what sense is psychoanalysis a science? Or, indeed, psychology in general?

Key points:

- There is a lively debate within psychology about which methods are scientific and which are not.
- Philosophers of science have attempted to define the unique characteristics of science.
- Induction is the process of deriving theories from careful observations. The central problem with induction is the theory-dependence of observation.
- Deduction is the process of making testable predictions from theories. It is the basis of the hypothetico-deductive model of science.
- Popper proposed that good scientific theories should be testable and therefore potentially falsifiable.
- Kuhn analyzed the historical progression of scientific thought in terms of his concepts of paradigms and scientific revolutions.
- The sociology of knowledge examines the role of social and political forces in the development of scientific thought.

The literature on this area is enormous: philosophy of science is an entire academic discipline in itself. Here we briefly review some central ideas. Since much undergraduate psychology education is implicitly based on a traditional view of science, it is important for psychologists to know about the positions presented here and in Chapters 4 and 5, in order to understand the context of the traditional view and to be aware of its alternatives.

Induction

An initial, common-sense way of attempting to characterize science is that it is based on careful observation, from which theories are then formulated. The derivation of theory from observation is known as *induction*, that is, going from the particular to the general. Astronomy is the classic example: astronomers gaze at the heavens, record what they see, and then try to spot the general pattern underlying their observations. Kepler's 17th-century laws of planetary motion were derived in such a way, using the accumulated data of his predecessor, Tycho Brahe. Within psychology, clinical observation also uses induction. For example, the psychoanalyst carefully observes a number of patients within the analytic setting, and then attempts to formulate his or her impressions into a theory. This was the basis of Freud's methods when he enunciated psychoanalytic theory at the beginning of the 20th century.

Unfortunately, there are two insuperable problems with induction as a guiding principle of science (Chalmers, 2013). The first is that it is impossible to have pure observations: what we observe and how we observe it are, implicitly or explicitly, based on theory. This phenomenon is known as the *theory-dependence of observation*. For example, a psychoanalyst, a Skinnerian behaviorist, and a lay person will notice very different things in a videotape of a therapy session. The second problem is that there is no logical basis for the principle of induction. Because something has been observed to happen on ten occasions, it does not necessarily follow that it will happen on the eleventh. This means that theories can never be conclusively verified, only temporarily corroborated by scientific evidence, resulting in probabilistic rather than necessary truths. The philosopher, Karl Popper, who was a contemporary of Freud and Adler in 1920s Vienna, expressed this point of view forcefully. It is worth giving an extended quotation, which is of enduring relevance to psychologists:

> I found that those of my friends who were admirers of Marx, Freud, and Adler, were impressed by a number of points common to these theories, and especially by their apparent explanatory power. These theories appeared to be able to explain practically everything that happened within the fields to which they referred …
>
> The most characteristic element in this situation seemed to me the incessant stream of confirmations, of observations which 'verified' the theories in question; and this point was constantly emphasized by their adherents. … The Freudian analysts emphasized that their theories were constantly verified by their "clinical observations." As for Adler, I was much impressed by a personal experience. Once, in 1919, I reported to him a case which to me did not seem particularly Adlerian, but which he found no difficulty in analyzing in terms of his theory of inferiority feelings, although he had not even seen the child. Slightly shocked, I asked him how he could be so sure. "Because of my thousand fold experience," he replied; whereupon I could not help saying: "And with this new case, I suppose, your experience has become thousand-and-one fold."
>
> What I had in mind was that his previous observations may not have been much sounder than this new one; that each in its turn had been interpreted in the light of "previous experience," and at the same time counted as additional confirmation … I could not think of any human behavior which could not be interpreted in terms of either theory. It was precisely this fact – that they always fitted, that they were always confirmed – which in the eyes of their admirers constituted the strongest argument in favor of these theories. It began to dawn on me that this apparent strength was in fact their weakness. (Popper, 1963: 34–35, reproduced by permission)

This quotation illustrates several important issues: (1) the limits of a verificationist approach (i.e., the approach taken by Adler of supporting his theory by looking for confirming instances) – good theories should be potentially capable of disconfirmation; (2) the problems of post-hoc explanation (it is easy to fit a theory to facts after the event); (3) the theory-dependence of observation (e.g., Adlerians tend to interpret everything in terms of inferiority complexes); and, finally, (4) the temptation for scientists to jump to conclusions without careful data gathering – Adler might have been more convincing if he had actually seen the child in question.

However, despite these major problems with induction, we are not suggesting that it be abandoned altogether, rather that it be conducted within a rigorous framework and complemented by other approaches, such as deduction and falsification. We will return to this in several subsequent chapters, especially in the section on systematic case studies in Chapter 9.

Deduction and Falsification

Having rejected the principle of induction as a sole, secure foundation for science, Popper attempted to turn the problem on its head: he looked at solutions based on deduction rather than induction, on falsification rather than verification. *Deduction* is the converse of induction: it means going from the theory to a testable prediction, known as a hypothesis. This approach to research, which is the traditional scientific approach within psychology, is known as the *hypothetico-deductive method*.

Popper's landmark volume, *The Logic of Scientific Discovery* (1959), set out to establish a demarcation between science and non-science (or "pseudo-science"). His central criterion was that a science must be able to formulate hypotheses that are capable of *refutation* or, in his preferred terminology, *falsification*. For example, Newtonian physics generates the proposition that a ball thrown up in the air will come down to land again. If tennis balls started shooting out into space, the theory would have a lot of explaining to do. In a more technical example, Newtonian physics also generates the proposition that light travels in a straight line. Although this proposition seems almost self-evident, it was ultimately falsified in a spectacular way by Eddington's expedition to observe a solar eclipse in Africa in order to test a deduction from Einstein's theory of relativity that light will bend in the presence of a gravitational field.

In psychology, such unequivocal falsifications of theoretically derived predictions are less common. One area where they can be found is in neuropsychological case studies of patients with acquired brain damage. The presence of certain patterns of dysfunction in a single case can be used to refute general theories of mental structure (Shallice, 1988).

As an example of a non-falsifiable theory, consider this statement, by the painter Mondrian: "The positive and the negative break up oneness, they are the cause of all unhappiness. The union of the positive and negative is happiness" (quoted by Wilson, 1990: 144). This certainly appears to be some sort of psychological theory, but it is not clear to what extent it could generate falsifiable propositions, and thus what could be done to test its validity. According to Popper, a statement that cannot be falsified is unscientific (though it is not necessarily meaningless – religion and poetry may have meaning, but they are not falsifiable).

For Popper, good science is characterized by a series of bold conjectures, which will be ultimately falsified. This approach is encapsulated in the title of one his books, *Conjectures and Refutations* (1963). A good or productive theory is one that provides the basis for a large number of falsifiable propositions. A bad theory, or an unscientific one, is incapable of falsification. However, all theories must be considered to be tentative; it is impossible to know the world exactly. Every theory in its time will be falsified and replaced by another (as Newtonian mechanics were supplanted by Einstein's theory of relativity).

The falsifiability criterion places those fields which rely exclusively on post-hoc explanatory methods outside the boundaries of science. In particular, Popper (1963) used falsifiability to rule out psychoanalysis and Marxism, fashionable theories in Popper's Vienna of the 1920s, which he explicitly pointed to as his main targets. On the other hand, operant behavioral approaches, with their philosophy of "prediction and control" (Skinner, 1953), would be included as scientific.

This version of falsificationism has a number of problems. The main one is that no theory ever completely accounts for all the known findings. Inconsistencies always exist, but the theory may well be retained in spite of them, as they could be explained in other ways than the falsification of the theory, for example, measurement or design errors in the studies. Refutation is never cut and dried: there is always scope to deny that it has occurred. One historical example is in the debate over the effectiveness of psychotherapy. In a landmark paper, Eysenck (1952) claimed that psychotherapy showed no benefit above that of spontaneous remission, sparking years of controversy. This hypothesis seemed to have been finally laid to rest by Smith and Glass's (1977) pioneering meta-analysis. However, Eysenck responded by dismissing the whole meta-analysis procedure, labeling it "an exercise in mega-silliness" (Eysenck, 1978, p. 517), continuing to deny that his hypothesis had been refuted.

Abduction

One recent development is the revival of interest in Charles Peirce's (1965) notion of *abduction* (an awkward term, which has nothing to do with its usual sense of kidnapping), picked up by Haig (2005), Rennie (2012), and Stiles (2009). According to Peirce, abduction is a logical process that scientists use when faced with a surprising finding: they search with their imagination for possible explanations. This corresponds to Popper's (1963) "bold conjecture" formulation, but it is more thoroughly worked out logically and is related to the processes of induction ("fact gathering" in Stiles's (2009) formulation) and deduction (checking the theory for logical consistency and deriving implications to be tested for). The ideas are complex, but the key point is that working scientists use a combination of processes, cycling between careful data collection, creative leaps, and logical or statistical inference.

Paradigms and Scientific Revolutions

A central problem arising from Popper's work is to explain how one theory is replaced by another. Since there are always unexplained or contradictory observations within a scientific field, what determines when one theory is rejected and replaced by another? This issue was the point of departure for the work of Thomas Kuhn, one of the central figures of 20th-century philosophy of science. In *The Structure of Scientific Revolutions* (1970), he applied the tools of historical analysis to address these questions.

Kuhn proposed the concept of a *paradigm*, that is, the central body of ideas within which the great majority of scientists is working at any given time. The paradigm determines what phenomena scientists consider important and the methods that they use to make their observations. Scientists working within a paradigm are said to be doing *normal science*: they are elaborating theories rather than attempting to refute them. Eventually, the accumulated deficiencies of a paradigm lead to its overthrow and replacement by another paradigm, in what Kuhn labels a *scientific revolution*. For example, the replacement of the ancient Greek cosmology of Aristotle and Ptolemy (that the earth was the center of the universe) by Copernican theory (that the earth moves around the sun) in the 1500s was a scientific revolution.

The concept of a paradigm is central to Kuhn's work. It fits well with the physical sciences, but there is much debate about how well it can be applied in the social sciences (Driver-Linn, 2003; Lambie, 1991). Is there a guiding paradigm in clinical psychology? Or are there multiple paradigms, indicating that we are still in what Kuhn referred to as a *pre-paradigmatic* state? Arguably, cognitive-behavioral, psychodynamic, and humanistic approaches may be considered as concurrent competing paradigms, although this is perhaps overlooking the fundamental overlap between these approaches.

Kuhn's views and their relationship to those of Popper were hotly debated when they first appeared (Lakatos & Musgrave, 1970). Lakatos accused Kuhn of propounding a "mob psychology" (Lakatos, 1970, p. 178) of scientific revolutions, saying that his system contained no criteria for considering one paradigm an advance on another, and thus no sense in which scientific understanding could be said to be progressing.

Feyerabend's (1975) pragmatic-anarchistic view takes this to an extreme. Under a slogan of "anything goes" as long as it's useful for carrying knowledge forward, Feyerabend appears to be claiming that different theories are "incommensurable" and that there are therefore no clear grounds for preferring one to another. So the anarchistic view would accord astrology, voodoo, and Babylonian astronomy the same scientific status as quantum mechanics or relativity (Chalmers, 2013). This viewpoint is pithily summed up in a rhyming couplet by the late poet and musician Moondog (n.d): "What I say of science here, I say without condition/ That science is the latest and the greatest superstition" (Louis Hardin, Managarm; reproduced by permission).

It seems as though the views of Popper and of Kuhn are themselves "incommensurable" in that they are each using different concepts to discuss somewhat different phenomena. Popper takes a logical approach, Kuhn a historical one. While trying to avoid the danger of falling into a relativist, "anything goes" position ourselves, we contend that much of value can be taken from both writers.

From Popper, researchers can take the central admonition of making bold theories that lead to clear and risky predictions, and being ready to give these theories up in the face of contradictory evidence. Popper urges researchers to put their thoughts into clear and precise language. As an example, Rogers's (1957) seminal paper on the necessary and sufficient conditions of therapeutic personality change is written with admirable clarity, and makes bold hypotheses about the central mechanisms of therapeutic change.

Kuhn also encourages taking intellectual risks, though from a different standpoint. By clearly delineating the constrictions of "normal science," he provides an implicit critique, helping researchers to be aware of and to question the assumptions of the paradigm within which they work, and to ask whether that paradigm is worth challenging. His work also leads scientists to look ahead to the next paradigm revolution and to ask whether their work will have any enduring value.

Finally, the methodological pluralist stance that informs this book owes something to the spirit that animates Feyerabend's writing. We agree with his stress on the value of diversity and the dangers of scientific conformity. We do, however, strongly disagree with his rejection of the canons of scientific method. As we hope to show, it is possible to articulate criteria to evaluate work conducted within the very different scientific traditions in clinical psychology.

Social and Political Issues

As Kuhn (1970) illustrates, science is not conducted in a cultural and political vacuum. It is carried out by scientists working within a particular scientific, professional, and cultural community at a specific moment in history. Sociologists of knowledge (e.g., Berger & Luckmann, 1966) and social constructionists (e.g., Gergen, 1985) look at how social factors influence the development of thought and the role of consensus in science. For example, what is seen as abnormal behavior varies from culture to culture, and within cultures over time.

Sociological and historical methods can be applied to examine science itself, to look at how socioeconomic and political forces shape the kind of science that is practiced within a given culture (Chalmers, 1990; Schwartz, 1992): how one set of ideas gains prominence over another. These analyses have often been carried out within a Marxist framework, which examines the influence of class interests on scientific thought (Albury & Schwartz, 1982). For example, genetic explanations of individual differences in IQ scores fit in well with racist and fascist ideologies, and some of the impetus behind the development of IQ tests probably arose from such a background (Rose, Kamin, & Lewontin, 1984; Rust & Golombok, 2008).

An example within clinical psychology is the debate about “evidence-based practice” or “empirically supported treatments” – the attempt to produce a list of therapies that have been systematically researched and found to be beneficial. Proponents of this project argue that it is an essential attempt to summarize the state of scientific research on the psychological therapies, and it will ultimately benefit clients. Its opponents argue that it is driven by the needs of the U.S. managed care industry or the U.K. National Health Service to have short-term treatments to save money, and by factions within clinical psychology itself that are seeking to advance their own favored orientations at the expense of other approaches (Elliott, 1998).

Rigid rules for what is and is not science sometimes serve political purposes (e.g., fighting for limited funds from government or universities), and may have the unfortunate consequence of restricting healthy diversity in studying complex clinical phenomena. On the other hand, psychology in general now seems more secure as a discipline, and as a consequence it seems that more psychologists are now freer to work within a broader definition of science and to use a wider range of methods.

One other important source of sociopolitical influence on scientific activity stems from the fact that research is conducted within an organized professional context. The official pronouncements of the clinical psychology profession have stressed the value of conducting research and have also sought to prescribe what type of research is regarded as legitimate. The various ways that this is expressed are examined next.

PROFESSIONAL ISSUES

It now seems almost uniformly accepted that research should be part of clinical psychologists’ training and practice. How did this idea arise? What is the relationship between clinical practice and research? Several models of how practitioners might produce or consume research have been proposed, among them the scientist-practitioner model and the applied-scientist model. It is also worth considering, as a kind of baseline, a model of a psychologist who does not use research, which we have labeled the intuitive practitioner model.

There are several models of how practitioners might produce or consume research:

- The intuitive practitioner, who conducts clinical work on the basis of personal intuition and knowledge from sources other than research.
- The scientist-practitioner, who is competent as both a researcher and a practitioner.
- The applied scientist, who conducts clinical work as a form of applied research.
- The local clinical scientist, who applies a range of research methods and critical thinking skills to solve local problems in clinical settings.
- The evidence-based practitioner, who systematically searches the literature to obtain the best evidence on which to base clinical decisions.
- The clinical scientist, who draws on general psychology to produce research on clinical problems for the evidence-based practitioner to use.
- The practice-based evidence model, in which clinicians generate evidence about the effectiveness of clinical services using their own routinely collected data.

The Intuitive Practitioner

The intuitive practitioner is a term we use to describe the therapist who does not conduct research and does not consciously use research findings in their clinical work. Intuitive practitioners conduct clinical work mostly on the basis of knowledge drawn from personal experience, supervision, or reading clinical case reports; they are often highly skilled but are sometimes unable to articulate their implicit knowledge. If they do research at all, it is in the form of clinical observation and narrative case studies. This model overlaps with the concept of the reflective practitioner (Schön, 1987); however, the main point here is the intuitive practitioner's relative neglect of research evidence, which may arise from a questioning of the value of scientific, as opposed to clinical, knowledge (e.g., Miller, 2004). Most clinical psychologists have probably encountered examples of skilled colleagues or supervisors who fit this description.

It is hard to estimate what proportion of psychologists comes under this heading. It has often been observed that many psychological therapists do not conduct research or even consume it (Morrow-Bradley & Elliott, 1986). A much-cited statistic from surveys of U.S. clinicians is that the modal number of publications among practicing clinical psychologists is zero. However, this statistic (the mode) gives a misleading impression of the overall pattern, as the majority of respondents to the most recent survey had published at least one paper (Norcross, Karpiak, & Santoro, 2005). Furthermore, psychologists are often involved in research that does not reach the publication stage. Nevertheless, this statistic is a salutary reminder to academically oriented clinical psychologists, who have been at the forefront of articulating the more mainstream models of research and practice, that, despite all their earnest pronouncements, research and research findings are generally not very salient in practicing psychologists' minds.

The Scientist-Practitioner

Since its inception, psychology has been a university-based discipline. It originally emerged out of philosophy in the 19th century and was later aligned with the natural sciences in order to give it increased respectability in the academic world. The profession of clinical psychology started life in the first decades of the 20th century and was initially concerned with "mental testing" as an aid to selection and diagnosis; it was only after World War II that its role expanded to include treatment (Humphreys, 1996). However, during its transition from university to clinic, the profession sought to retain its academic roots, in that the distinctive role of the psychologist was seen to lie in his or her academic, scientific, or scholarly viewpoint. As we have mentioned, this academic viewpoint may lead to tensions with other colleagues in multidisciplinary clinical teams.

This scientific role has received somewhat different emphases in the United States and the United Kingdom. In the United States it is known as the *scientist-practitioner* (or Boulder) model, in the United Kingdom the *applied scientist* model.

The post-war expansion of U.S. clinical psychology, especially in the Veterans Administration Hospitals, led to an upgrading of training from the Masters to the Doctoral level, and to an examination of what such training should consist of (Hayes, Barlow, & Nelson-Gray, 1999). The consensus view at the time was expressed in a conference at Boulder, Colorado, in 1949, and became known, naturally enough, as the *Boulder model.* The field was then in its infancy, its knowledge base was tiny, and there was a great need for placing the profession on a firm scientific footing, in order to know whether its procedures worked. The conference concluded that clinical psychologists should be able to function both as scientists and practitioners, capable of conducting research as well as clinical work. A quotation from an article slightly prior to the Boulder conference gives the flavor:

> Participants [in doctoral training programs] should receive training in three functions: diagnosis, research, and therapy, with the special contributions of the psychologist as research worker emphasized throughout. (American Psychological Association, 1947: 549)

Thus the scientist-practitioner model emphasizes research and practice as separate, parallel activities. Clinical psychologists are seen as both productive researchers and productive clinicians.

The main limitation of the scientist-practitioner model is that it is hard to put into practice. It demands a high level of skill and motivation in two distinct areas – research and practice – and clinicians who are equally comfortable in both of these areas are rare. Furthermore, the pressures of many clinical psychologists' jobs make it hard to find the time and resources to do research.

The Applied Scientist

In the United Kingdom, the applied scientist model took a slightly different emphasis to the American scientist-practitioner model: less on research and clinical work as separate activities, more on the systematic integration of scientific method into clinical work. Monte Shapiro, one of the founders of British clinical psychology, set out the three aspects of the applied scientist role (Shapiro, 1967, 1985):

1. Applying the findings of general psychology to the area of mental health.
2. Only using methods of assessment that have been scientifically validated.
3. Doing clinical work within the framework of the scientific method, by forming hypotheses about the nature and determinants of the client's problems and collecting data to test these hypotheses.

Thus, in Shapiro's applied scientist model, research and practice are not dichotomized but integrated. This approach is also manifested in the behavioral tradition of single case experimental designs (see Chapter 9, this volume, and Hayes et al., 1999).

In sum, the applied scientist is principally a clinician; the scientist-practitioner is both a clinician and a researcher.

The limitations of the applied scientist approach are that, like the scientist- practitioner model, it can be hard to put into practice. It works better within some types of therapy than others (e.g., it is hard to fit psychodynamically oriented therapies into this approach). Also, the intensive collection of session-by-session data can be burdensome for both therapist and client, although simplified versions of the Personal Questionnaire, one of Shapiro's main data collection methods, have been developed (e.g., Elliott, Wagner, Sales, Rodgers, Alves, & Café, 2015).

The Local Clinical Scientist

Stricker and Trierweiler (1995; Trierweiler & Stricker, 1998) have put forward the *local clinical scientist* model, a more flexible version of the applied scientist model. Their formulation is worth quoting here:

> The local clinical scientist is a critical investigator who uses scientific research and methods, general scholarship, and personal and professional experience to develop plausible and communicable formulations of local phenomena. This investigator draws on scientific theory and research, general world knowledge, acute observational skills, and an open, skeptical stance toward the problem to conduct this inquiry. (Trierweiler & Stricker, 1998, pp. 24–25)

Thus, in their formulation, Trierweiler and Stricker emphasize both quantitative and qualitative methods of inquiry, as well as critical thinking skills, all adapted to the local needs and culture of the particular agency within which the psychologist is working. Their emphasis is firmly on understanding local phenomena, as opposed to producing generalizable knowledge. For them, the local clinical scientist is anthropologist, detective, and experimentalist, all rolled into one. While this is clearly a tall order, it can be understood as an aspirational goal.

The scientist-practitioner, applied scientist, and local clinical scientist models all try to encompass research and practice within the same person. At their best, these models all set up a creative tension between practice and research; at their worst, they set up an impossible ideal leading to role strain and, ultimately, cynicism. The final three models, presented next, attempt to resolve the tension by shifting the balance toward either practice or research.

The Evidence-Based Practitioner

On the one hand, practitioners can be regarded as consumers rather than producers of research. The notion of basing one's practice on the systematic use of research findings has been part of clinical psychology's ethos since its inception. In the late 1990s it found renewed currency within the medical profession. Sackett, Richardson, Rosenberg, and Haynes's (1997) book, *Evidence-Based Medicine,* has been influential in articulating how this might work for individual doctors in practice.

Evidence-based medicine is defined as "the conscientious, explicit, and judicious use of current best evidence in making decisions about individual patients" (Sackett et al., 1997, p. 2). Thus, when faced with a difficult clinical decision, whether about diagnosis or treatment, doctors are urged to consult the research literature for an answer. Sackett et al.'s book gives rules for how to judge good research. Evidence from well-conducted randomized controlled trials and meta-analyses are regarded as being especially valuable.

Within clinical psychology, there is a parallel, contemporary movement to identify "empirically supported treatments," which we mentioned earlier in this chapter. This movement has grown out of the understandable need for healthcare purchasers (usually insurance companies in the United States; the National Health Service in the United Kingdom) to be reassured that they are paying for the most cost-effective care. However, there is considerable controversy about whether it is desirable, or even possible, to specify preferred treatments in this way, and particularly about the methods and standards used to designate certain therapies as "efficacious" or not (Kazdin, 2008, Westen, Novotny, & Thompson-Brenner, 2004).

The evidence-based practitioner model also leaves aside the question of who will produce new research findings, an issue addressed by the final two models.

The Clinical Scientist

At the opposite extreme from the intuitive practitioner is the *clinical scientist* model (not to be confused with the *local* clinical scientist model described above), which has recently emerged in North America (McFall, 2006). In this model, clinical psychologists are first and foremost researchers, usually with substantial background in one or more areas of experimental psychology, who study clinical phenomena but may not be involved in delivering clinical services at all. This is actually not a new model, but has only recently been distinguished from the traditional scientist-practitioner model. In a way, because of its emphasis on general psychology, this model is the opposite of the local clinical scientist. Clinical scientists are portrayed as producers of research that evidence-based practitioners can consume.

This overall division of labor sounds logical, but has several potential drawbacks, all of which stem from the loss of the creative tension between research and practice. First, clinical scientists may be too divorced from clinical practice to produce research that is meaningful to practitioners. Second, in this model, practitioners may be relegated to the role of passive consumers or even technicians. Third, the actual relation between research and practice is more often the opposite of that portrayed in the clinical scientist or evidence-based practitioner model: innovations are more likely to emerge out of clinical practice than out of research (Stiles, 1992). Researchers are thus more likely to be consumers of practice (e.g., subjecting clinical hypotheses to rigorous tests) than the other way around.

The Practice-Based Evidence Model

Finally, the most recently developed approach is the practice-based evidence model, which emphasizes the value of conducting research in working clinical settings. Practitioners gather data on their routine clinical practice, which is used to generate evidence about how various interventions are working in the actual clinical environment. The model has grown out of a dissatisfaction with the limitations of the randomized designs that are relied on by the clinical scientists and evidence-based practitioner models, which, as we will see in Chapter 8, emphasize internal validity (the ability to infer causality) over external validity (the potential generalizability of the findings).

The book *Practice-Based Evidence* by Barkham, Hardy, and Mellor-Clark (2010) gives a useful compendium of research methods that might be adopted. Two commonly used approaches are "*case tracking*", which uses frequently administered standardized measures to monitor client progress, and "*benchmarking*", in which the outcomes from the service in question are compared with good practice from other services.

Comparison of Models

The models described above each have a different orientation towards research, and emphasize different types of research (see Table 2.1).

Producing versus Consuming Research

The models can be ranked in terms of how much they regard the practitioner as a producer and as a consumer of research. As we have noted, the scientist-practitioner model assumes that the clinician will be producing research (as well as consuming it), whereas the evidence-based practitioner model emphasizes only the use of research. The applied scientist and local clinical scientist models take a middle position, focusing on doing research within a clinical context. The intuitive practitioner does

Table 2.1 Characteristics of professional models

Model	*Orientation to research*	*Research emphasized*
Intuitive practitioner	Nonconsumer or indirect consumer	Narrative case studies
Scientist-practitioner	Producer and consumer	Basic and applied
Applied scientist	Integrated with clinical work	Applied small-N
Local clinical scientist	Integrated with clinical work	Evaluation and action
Evidence-based practitioner	Consumer	Controlled trials
Clinical scientist	Producer	Controlled trials
Practice-based evidence	Integrated with clinical work	Case tracking

not produce or consume research, except in the form of case studies. Clinical scientists produce research as their main function.

Type of Research

The scientist-practitioner model places no restriction upon the type of research that psychologists are expected to conduct. The applied scientist model, as its name implies, emphasizes applied research, often single case research or at least research using small sample sizes (see Chapter 9), while the local clinical scientist model emphasizes evaluation and action research. The evidence-based practitioner and clinical scientist models give preference to high-quality randomized controlled trials and meta-analyses.

Implications for Clinical Training

With the benefit of hindsight, the scientist-practitioner and the applied scientist models appear as ideals that have not been universally adopted or that may not even be universally desirable (Hayes et al., 1999; McFall, 2006). Many psychologists have called for a reassessment of the role of research in clinical training.

Three different training approaches are currently operating in parallel, at least in the English-speaking world. First, many programs continue to adhere to the traditional scientist-practitioner, Boulder model. This includes many university-based doctoral programs in the United States, which award a PhD, and all training programs in the United Kingdom, which award a Doctorate in Clinical Psychology (DClinPsy). Second, some U.S. programs in "professional schools" of clinical psychology follow the Vail model (Korman, 1976), sometimes known as the practitioner-scholar model, adopting a broader definition of research that can be more easily integrated with practice (Hoshmand & Polkinghorne, 1992; Peterson, 1991). They award the degree Doctor of Psychology (PsyD). Finally, some U.S. PhD programs emphasize clinical science, downplaying the clinical practicum component of traditional programs in favor of rigorous scientific training (McFall, 2006).

It is unclear at present how these different approaches will play out over time. Perhaps there is a need for different training and career routes to suit different personal preferences and abilities. As will be seen in the following section, many clinical psychologists do not see science as being a central part of their professional identity, and therefore it is questionable whether they need to be trained to be producers, rather than consumers, of research. Other individuals may have greater interest or ability in the science domain, and may possibly also lack the personal or interpersonal characteristics needed to become a practitioner. There may be room in the world for a plurality of training models to suit a plurality of roles, providing that it is clear to the general public what kinds of skills each type of psychologist possesses.

PERSONAL ISSUES

Having considered philosophical and professional issues, we now make a transition to the personal level. What motivates the individual clinical psychologist to engage in research – or not to, as the case may be?

- There are several different reasons that individual clinical psychologists might have for being involved, or not being involved, in research.
- Each psychologist will weigh each one differently.
- It is important for clinical psychologists to reflect on where research fits into their own practice.

Why Do Clinical Psychologists Do Research?

We have already mentioned the benefits of conducting research as a systematic way of developing knowledge and theory for the profession and science of clinical psychology. There are also a variety of personal reasons why clinical psychologists may wish to engage in research. Some of the more common ones are:

- *Curiosity.* Research exists to answer questions: it must add something to knowledge at the end, otherwise there is no point in doing it. For many researchers, this is an end in itself: they want to make sense of the world and see research as a way of doing so.
- *Personal satisfaction.* Some psychologists do research purely for the intrinsic satisfaction. They may enjoy the challenge of research, feel a need to maintain their intellectual sharpness (especially in the middle or later stages of their career), value the contact it brings with other colleagues, or simply see research as a break from everyday routine and a way of reducing occupational stress. There is also the satisfaction of seeing one's work in print and of feeling one is shaping the development of one's profession.
- *Professional and social change.* Ideally, research should not just lead to an accumulation of knowledge, but also to some change in professional practice, or social or legal reforms. Karl Marx's epitaph puts this point forcefully: "Philosophers have interpreted the world, the point, however, is to change it." Many clinicians want to know which interventions work and which do not, and to change their practice, or that of their profession, accordingly. Others are disturbed by the manifest inequalities in western societies or the level of human suffering in conflicted or war-torn parts of the world, and want to make social or political changes that will alleviate psychological distress (e.g., Marmot, 2005; Orford, 2008).
- *Competition between professions and theoretical orientations.* Similarly, some people may be drawn to research as a way of advancing their professional field or favored theoretical orientation. Research is a way of legitimizing existing professional practices and of developing new ones. A large part of applied psychology's claim to professional status is that its procedures were legitimized by research. In marketing jargon, one of psychology's "unique selling points" is that its practitioners possess research expertise.
- *Individual career needs.* The career structure of one's employing institution may dictate that in order to advance up the hierarchy one must conduct research. There are research requirements for clinical psychology students wanting to obtain a professional qualification, and a research track record is required for appointment to academic positions in the profession.

- *Institutional demands.* In service settings, there is often pressure from management to conduct evaluation or other forms of applied research. For example, the recent move towards "clinical governance" in the British National Health Service calls for practitioners to systematically monitor their treatment outcomes.

Why Don't Clinical Psychologists Do Research?

Although there are many positive reasons for doing research, psychologists at all levels of experience also voice several reasons to explain why they do not conduct, or draw upon, research (Hayes et al., 1999; Morrow-Bradley & Elliott, 1986):

- *Irrelevance.* Research is seen as not saying anything useful about practice. It is seen as being overconcerned with rigor at the expense of relevance (i.e., journals are filled with rigorous but irrelevant studies). The main source of learning is felt to be clinical practice, rather than research studies.
- *Emphasis on generalities.* There is a tension between the scientific stance, which looks for generalities and lawfulness, and the clinical stance, which stresses human individuality. Most research has been done within the nomothetic tradition, which emphasizes pooling people together to look for commonalities, rather than the idiographic tradition, which emphasizes individual uniqueness (Allport, 1962; see also Chapter 9).
- *Mistaken paradigm.* The positivist paradigm (see Chapter 4), under which much research is conducted, is seen as being reductive and simplistic. This paradigm may be linked with macro-political structures, for instance, feminists have critiqued the patriarchal nature of traditional psychological research and Marxists have critiqued psychology's emphasis on individualism at the expense of collectivism (see Chapter 5).
- *Intrusiveness.* Research is seen as a blunt instrument that crushes the phenomenon under study. Much as zoologists may kill a butterfly to study it, so, for example, the intrusion of research procedures into a therapeutic relationship is felt to damage that relationship. For instance, therapists often fear that the act of audio-recording a session might severely distort the therapy process, for example, by making the client apprehensive about confidentiality issues.
- *Time demands.* Research is time-consuming and often has a low priority compared to service demands. Also, it is often not supported or valued by managers or colleagues.
- *Technical expertise.* Research is seen as requiring considerable technical expertise, with journal editors and other gatekeepers setting prohibitively high standards that discourage beginning researchers.
- *Ethical concerns.* Research in general is felt to dehumanize participants subtly (or not so subtly in some cases) by turning them into "subjects." In addition, there are ethical problems with some psychological studies, for example, those using deception.
- *Bureaucracy.* Conducting a research project usually involves a lot of preliminary paperwork and negotiation, in order to gain approval from the host institution's gatekeepers (see Chapter 3) and to obtain ethical or Institutional Review Board approval (see Chapter 10). This can often deter researchers from conducting studies.

- *Bad training experiences.* For various reasons, during their training many psychologists experience research as a painful, alienating process. For example, some may feel forced to study something they find uninteresting, or may feel inadequate for not being research-oriented. Others fail to receive sufficient direction or support for their research, and therefore find it to be a lonely or unmanageable activity.
- *Being scrutinized.* Research participants can feel scrutinized, which may arouse great anxiety. This may make the conduct of the research project very difficult, particularly in evaluation studies, where the continuation of a service may depend on the findings.
- *Disturbing conclusions.* Research may come up with findings that you do not like. It can lead to a painful re-examination of your cherished ideas if they do not match up to the facts. It may challenge your assumptions and ways of working, and this can be an uncomfortable process.

The last two reasons have to do with the threatening aspects of research. Sometimes, these feelings of threat may not be directly acknowledged, but instead find their expression in the form of some of the other reasons listed. For example, practicing therapists may, understandably, feel sensitive about being scrutinized, and therefore may be reluctant to participate in a project which involves recording their sessions. However, they may argue against the project because of intrusiveness for the client, rather than admitting their own sense of vulnerability.

Weighing up the Pros and Cons of Doing Research

Different individuals will weigh each of the above positive and negative considerations differently. Some concentrate entirely on being a practicing clinician and never conduct research again once their training is completed. Others concentrate on an academic career and do little if any practice. Many, however, take a middle road, combining the two activities in their professional work, although perhaps only consuming research and not conducting it. We hope to show, in the remainder of this book, that doing research need not be a formidable challenge and that it is possible to conduct research even if you work primarily in a service setting. We will also offer practical advice aimed at making your research experience more interesting and less painful.

CHAPTER SUMMARY

In one sense, there is nothing special about doing research, in that the process of psychological research is similar to that of open-minded enquiry in everyday life. However, research is a disciplined and self-conscious activity that can be quite demanding. Clinical psychology considers itself to be scientific, but it is not easy to pin down exactly what science consists of. Several philosophers have attempted to characterize the essence of scientific progress: Popper, Kuhn, and Feyerabend are central figures. Science is a human enterprise, and its development is shaped by social and political forces. The scientist-practitioner model, and the closely related applied scientist model, are a central part of clinical psychology's professional ideology.

However, there is often a gap between the rhetoric and the reality: many clinical psychologists no longer do research once they have qualified. Practicing psychologists may choose to do research, or not to, for a variety of personal reasons. This chapter has attempted to shed light on the research process from three perspectives: philosophical, professional, and personal. It is useful to keep these background issues in mind while planning, conducting, and reading research.

FURTHER READING

The Oxford Companion to Philosophy (Honderich, 1995) is a magnificent resource for anyone needing to know what all those troublesome "-isms" actually mean (and indeed for anyone at all curious about Life, the Universe and Everything). Alan Chalmers, in his book *What is This Thing Called Science?* (2013), explains complex philosophy of science issues with admirable clarity. However, he draws most of his examples from the natural sciences. Slife and Williams (1995) and Proctor and Capaldi (2006) review the range of philosophy of science positions, including alternatives to the traditional views, as applied to psychology. Since Popper and Kuhn are so often referred to, it is worth reading them both in the original. The obvious choice from Kuhn's work is *The Structure of Scientific Revolutions* (1970). For Popper the choice is wider. Perhaps his most accessible work is *Conjectures and Refutations* (1963), especially the chapter of the same title.

Research in the context of professional practice and clinical training is discussed in Benjamin (2005), Hayes et al. (1999), McFall (2006), and Tryon (2007).

QUESTIONS FOR REFLECTION

1. Where do you stand in the debate between the realist and constructionist approaches to research?
2. Do you lean more towards basic research or applied research?
3. Why do you think practitioners often don't utilize research? Which reasons seem most justified?
4. Which professional model regarding the role of research and practice do you identify with most closely? Why?

3

Doing the Groundwork

KEY POINTS IN THIS CHAPTER

This chapter focuses on practical issues in getting a research project started:

- choosing a topic and formulating the research questions or hypotheses;
- searching and reviewing the literature;
- writing a proposal and applying for funding;
- dealing with the organizational politics of research in clinical settings.

In sharp contrast to the previous chapter, this one focuses on practical rather than theoretical issues. It covers the first stage of the research process, which we label the groundwork stage. The researcher's primary task at this stage is to formulate a set of research questions, often including specific hypotheses; the secondary task is to tackle organizational or political issues, in order to prepare the ground for data collection. Researchers often apply for ethics committee approval and for funding at this stage.

In practice, as we noted in Chapter 1, the groundwork stage overlaps with the other two planning stages: selecting the measures and specifying the design. We are separating them here for didactic reasons, but readers who are actually planning a project will need to be working concurrently on measurement and design. For example, we cover funding issues here, but if you are applying for funding, the grant-giving committee will want a full plan of your research. Similarly, when you are applying for ethics approval, you will also need to submit a detailed plan for the project.

Planning the study is usually quite anxiety-provoking, both because you are grappling with difficult intellectual and organizational problems, and also because researchers

Research Methods in Clinical Psychology: An Introduction for Students and Practitioners,
Third Edition. Chris Barker, Nancy Pistrang, and Robert Elliott.

Companion Website: www.wiley.com/go/barker

often feel that they are not being productive if they are not collecting or analyzing data. You may be tempted to get the planning over and done with as soon as possible. This is usually a mistake. We have been involved with many studies, including some of our own, which have suffered because of inadequate planning. A poorly planned study can cause hours of frustration at the analysis, interpretation, and writing-up stage; at an extreme, the results may be worthless because of design faults or poorly thought-out research questions. Furthermore, such studies can be confusing to read, as the research questions and the research methods are often not fully consistent with one another. Time put in at the early stages often repays itself with interest later on, so it is worth trying to soothe your anxiety in order to take time over the planning.

This chapter has two sections. The first considers how the research questions are formulated and specified in a research proposal. The second looks at the politics of research in clinical settings and other organizations, in particular how research in such settings can easily come to grief. Ethical issues, which often need to be addressed at this stage, are covered in Chapter 10, where we discuss the topic of the research participants.

FORMULATING THE RESEARCH QUESTIONS

Key points:

- The process of planning research is painstaking and often anxiety-provoking, but effort put in here usually pays off later.
- Research questions can be either exploratory (open-ended) or confirmatory (hypothesis- testing).
- Various databases can be used to locate relevant literature.
- Research tools help to make literature reviews more rigorous: systematic search and critical appraisal methods, quantitative meta-analysis, and qualitative meta-synthesis.
- The planning process is iterative: feedback from colleagues, and one's own second thoughts, mean that it is usual to take proposals through several drafts.
- Research should generally be question-driven rather than method-driven. The essence of planning good research is making the procedures fit the questions, rather than the other way around.

The first step in undertaking a piece of research is obviously to select a topic to investigate. By topic we mean a broad area of interest, such as "depression in children," "marital communication," or "something in the area of therapy process." This is all that is needed at the beginning of the project. As the planning progresses, the topic will become more and more focused, until you eventually arrive at some specific research questions.

It is valuable to start keeping a personal research journal from the time you begin to think about your research questions. In it you can record the development of your ideas as the project progresses. You can also note your thoughts, feelings and general reactions to the research process. For example, it is useful at the end of the project to have a record of where you've come from and why you took the decisions that you did (sometimes it's hard to remember). You might also use the research journal during data collection and analysis, especially with qualitative projects, to record your thoughts about the meaning of your data. Such "research memos" (Corbin & Strauss, 2015) can be incorporated into the analysis and write-up of the project.

Choosing the Topic

Ideally, the topic will arise naturally out of an intrinsic interest in the area, perhaps stimulated by clinical work, reading, or issues that have arisen in your personal life. All things being equal, it is better to choose a topic that excites or interests you, as you may have to live with it for a long time and the personal interest will help keep you going. In the case of PhD research, your choice of topic may influence the direction of your subsequent career.

Sometimes researchers are drawn to topics that touch them personally, as with someone who has experienced anorexia doing research on eating disorders. There are pros and cons to this. The advantage of researching an issue in which you are personally involved is that it gives you the benefit of *experiential knowledge* (Borkman, 1990) that an outside researcher cannot possess. However, there is a danger that your own experiences may lead you to overidentify with the participants. If you are going to conduct research on a topic that is close to home, it is important to have some emotional distance in order to attain the necessary critical detachment. If you're in the middle of a divorce, you probably want to avoid doing research on marital satisfaction.

If the research is being done for extrinsic reasons (the prime example being when it is undertaken to fulfill a degree requirement), the problem arises of what to do when reflecting on your natural curiosities draws a blank, or produces only unrealistic topics. Fortunately, there is another broad path to follow: locating current important research fronts or "hot topics." There are several ways of doing this. The first is to talk to a colleague or potential supervisor, ideally someone whose work you admire, to see if they can suggest a topic. The second is to browse through some current books and journals until something takes your fancy. Look at major research reviews for recommendations for further research; read Discussion sections of research reports for ideas for further study. Another possibility is to choose a topic on practical grounds. Is there some research going on in your institution that you could slot into? Is someone conducting a large project that you could take a part of? The disadvantages of working on a large project are that you may have less sense of ownership and achievement, as the study will not be directly yours. However, there are many advantages, such as the feeling of being part of a larger team, the possibility of having a mentor within the project, being able to get assistance with data collection and analysis, and generally having the opportunity to discuss ideas throughout the project. The team approach is also closer to how research is normally conducted, where a scientist may assemble a group of fellow researchers at various levels of seniority from graduate students through post-doctoral fellows to faculty in order to form a research group working on a common problem or topic area.

Developing the Questions

Having chosen your general topic area, the next step is to narrow it down into specific research questions or hypotheses. This step is important to get right, because, as we shall often argue, the methods of the research will flow naturally from the questions that are asked. Similarly, when you read a research article, the first thing to look for is precisely what questions the study is trying to answer. Some papers will clearly state their questions or hypotheses, usually at the end of the introduction section; others will leave the reader to infer them from the general drift of the paper. It makes it much easier for the reader to understand and evaluate the study if its questions are clearly spelled out.

The first step is to formulate a few initial questions that encapsulate what you wish to find out from the study. It is a good idea to ask yourself what is the most important thing that you intend the project to tell you. Keeping this in mind helps you make choices later on if the study starts to become overly complicated. The number and complexity of the research questions will depend upon the time scale and the available resources. Practitioners doing service-oriented research on their own, or students carrying out a time-limited project, will need to ask circumscribed questions. Funded research teams undertaking multi-year projects can set themselves much more ambitious targets.

Always bear in mind that the research must be able to teach you something: there is no point in conducting a study if it simply confirms what you knew before you started it. As we noted in the previous chapter, research should put one's expectations or beliefs at risk, in the sense that it could falsify cherished ideas. It is worth trying to "game out" the study (Horowitz, 1982; Patton, 2008): in other words, ask yourself what its possible outcomes are, what you would learn from each of them, and which ones would make a difference in how you think about, or what you do about, the topic you are studying. Good studies yield useful information whatever their outcome, even if they fail to confirm initial predictions.

It is not usually possible to formulate clear and useful research questions at the beginning of the planning phase of the study. Specifying the research questions is harder than it sounds. You need to pose an initial question, and then refine it by reading the literature, consulting with colleagues, deciding what is feasible in your intended research setting, considering what is practicable in terms of measurement and research design, and conducting pilot studies. This process often takes several weeks or months. In their final form, the research questions should be clear and concise, so that there is no ambiguity about what the study is aiming to achieve.

It is important to begin by formulating the research questions in advance of developing your procedures. Beginning researchers often rush into selecting the measures that they will use before considering clearly what they really want to find out. This inevitably muddles their thinking and limits the range of questions they consider. For example, we have often seen researchers use measures simply because they are easily available and well known (therapeutic alliance springs to mind as an example). The essence of planning good research is *appropriate methods:* making the research procedures fit the questions rather than the other way around. In other words, the study should generally be question-driven rather than method-driven.

Hypothesis-testing versus Exploratory Research Questions

A *hypothesis* is a statement of a proposition that will be tested by the research (although in practice it can be phrased either as a statement or as a question). It expresses a tentative prediction of the results that are expected to emerge. For example, a study of the social determinants of child and adolescent behavior problems hypothesized that "maternal depression would be positively associated with both externalizing and internalizing behavior problems [and that] greater neighborhood social capital would be associated with fewer child and adolescent behavior problems" (Delany-Brumsey, Mays, & Cochran, 2014, p. 277).

The advantages of a carefully formulated hypothesis are that it gives an immediate clarity and focus to the investigation and it enables you to know immediately whether or not the findings of the study support its predictions. It is part of the *hypothetico-deductive* view of science, which emphasizes the use of theory and previous research to generate testable hypotheses (as in Popper's view of the scientific method: see Chapter 2). Using hypotheses also has the merit of increasing precision and fitting in more closely with the theory of statistical inference (Howell, 2010).

On the other hand, stating the research questions in open-ended question form allows an *exploratory, discovery-oriented* approach (e.g., Elliott, 1984; Mahrer, 1988), in contrast to the *confirmatory* approach of the hypothetico-deductive model (see Table 3.1). There may not be sufficient theory or previous research to enable you to make meaningful hypotheses, or you may not want to constrain your investigation early on. What is important is to be clear about what you are trying to investigate. Exploratory, discovery-oriented research questions are typically descriptive. The research questions that guide exploratory research should be clearly delineated, in order to narrow the research topic to a workable size and to provide a central focus for data collection and analysis. For example, the question "How do police officers experience dealing with traumatic incidents?" would be too broad, but a more focused question such as 'What are police officers' experiences of supportive and unsupportive interactions following traumatic incidents?" would be more workable (see Evans, Pistrang, & Billings, 2013). If you take the attitude, "I want to study x, so I'll just collect some data and see what is interesting," you are likely to end up with an incoherent mishmash of findings.

Table 3.1 Hypothesis-testing and exploratory approaches to research

	Exploratory	*Hypothesis-testing*
Logic	Inductive	Hypothetico-deductive
Scientific context	Discovery-oriented	Confirmatory
Example 1: Recovery from alcohol abuse	How does the experience of sobriety evolve over the first six months?	Is the first month of sobriety more difficult (in terms of psychological symptoms) than the sixth month?
Example 2: Internet-delivered therapy	How do clients experience the therapeutic alliance in internet-delivered therapy?	Do clients rate the therapeutic alliance in internet-delivered therapy as less strong than in face-to-face therapy?

Open-ended, discovery-oriented research questions are typically most appropriate under the following circumstances:

- When a research area is relatively new or little is known about it, making it difficult or premature to ask more specific questions.
- When a research area is confusing, contradictory, or not moving forward. This may be due to narrow conceptualization or premature quantification prior to adequate open-ended descriptive work.
- When the topic is a highly complex event, process, or human experience, requiring careful definition or description.

The Role of Theory

As we noted in Chapter 2, research is always conducted within an explicit or implicit theoretical framework. Therefore, it is almost always useful to work at developing that framework and making it more explicit. For one thing, conducting your research within an explicit theoretical framework will guide the formulation of research questions. As the social psychologist Kurt Lewin famously said, "There is nothing so practical as a good theory" (Lewin, 1952, p. 169). Thus, it is an excellent idea to devote time early in the research process to locating an existing theoretical model or to formulating your own working model. You can do this by trying to map out the likely relationships between the variables you are studying. For example, if you are studying the relationship between therapist empathy and client outcome, you might think about what some of the intervening processes might be, as well as variables that might affect both empathy and outcome. Therapist empathy might facilitate client self-exploration, which might lead to better outcome; or client pretreatment self-awareness might facilitate both therapist empathy and client outcome (see Chapter 8). From a different theoretical perspective, therapist empathy might act as a reinforcer for client disclosure of feelings or statements of positive self-esteem. The theoretical model could then guide the selection of specific correlational or comparative research questions, suitable for quantitative investigation.

Even in exploratory research, it is good practice for researchers to try to be as explicit as possible about their implicit theories or "preunderstandings" (Packer & Addison, 1989), in the form of expectations, hunches, or possible biases. The difference is that, in exploratory research, these implicit theories are set aside (referred to as "bracketing": see Chapter 5) rather than explicitly tested. After an exploratory study is completed, the researcher may find it useful to compare the actual results to these original expectations, in order to determine what has been learned.

Some Types of Research Question

There are a number of different types of research question, which are often associated with different approaches to research. For example, questions about description often lend themselves to discovery-oriented qualitative research, while questions about correlation, comparison, or causality usually lead to quantitative methods. Since we have not yet discussed these specific procedures, we will not pursue this notion of appropriate methods here; we will return to it in subsequent chapters. We present some common types of questions below (see Creswell, 2009; Horowitz, 1982; and Meltzoff, 1998 for more extensive treatments).

Description
What is X like? What are its features, characteristics, or variations? How frequent or common is it?

> **Examples:**
>
> What are patients' experiences of personal safety on psychiatric inpatient units?
> What do people with bipolar affective disorder find helpful about mutual support groups?
> Which verbal response modes are most frequently used by cognitive therapists?
> How common is social anxiety disorder?

Descriptive questions usually aim to provide a full picture of an experience or condition, including its variations. They might focus on its origin and development, or seek to provide examples of typical cases. Descriptive questions might also focus on amount or frequency, such as in epidemiological survey research.

Descriptive-comparison
Does group X differ from group Y?

> **Examples:**
>
> Do men and women differ in emotional expressiveness?
> What kinds of interactions occur in families with aggressive boys, compared to those with nonaggressive boys?
> Is a history of sexual abuse in childhood more common in bulimic than in nonbulimic individuals?

This type of question extends the simple descriptive question. Note that it does not address issues of causality, although causality may be implied, as in the last example on bulimia. These questions aim to compare two or more groups of people who are defined in terms of some pre-existing differences, such as gender, socioeconomic status, or diagnostic category. However, questions addressing differences resulting from an experimental intervention (e.g., a therapeutic intervention) come under the heading of causality questions, below.

Correlation
Do X and Y covary, in other words, is there a relationship between them? Is that relationship affected by a third variable Z?

Examples:

Is degree of marital support associated with speed of recovery from depression?
Is burnout among psychiatric nurses related to their experience of physical assault?
Does ethnic background affect the relationship between school achievement and children's self-esteem?

Correlational questions focus on investigating a possible association between two or more variables. These associations are often ones that have been predicted on the basis of theory or previous research. Although correlational questions may arise from an interest in causal explanations, they cannot be used to investigate causality.

Causality
Does X cause change in Y? Does X cause more change in Y than does Z?

Examples:

Do parent training groups lead to more effective parenting?
Does family therapy prevent relapse in patients diagnosed with schizophrenia?
Does taking Ecstasy lead to impairments in memory?
Is dialectical behavior therapy more effective than treatment as usual for clients with borderline personality disorder?

This type of question goes beyond the descriptive-comparison and correlation questions in attempting to look at causal influences. Establishing causal influence usually requires some sort of experimental design (Cook & Campbell, 1979; see also Chapters 8 and 9). Some of these questions are phrased with an explicit comparison; for instance, in the final example above, dialectical behavior therapy is explicitly compared to treatment as usual. In other questions, the comparison is implicit; in the first example, parent training is implicitly compared with no parent training. More elaborate questions can also be asked, concerning interactions between variables (Meltzoff, 1998) such as "Is dialectical behavior therapy more effective than treatment as usual for male clients with borderline personality disorder, but not for female clients?"

Measurement
How well (reliably, validly, and usefully) can X be measured by means of measure M?

These questions often have some overlap with other question types. For example, the second question, on the working alliance, is similar to a descriptive-comparison question (comparing clients' views at two or more time points), and the third question, on group climate, implies a question about correlation (How well does this measure correlate with other similar measures?). However, the distinguishing feature is that these questions

> **Examples:**
>
> Can subtypes of marital conflict be measured reliably and distinguished from one another?
>
> How consistent are clients' ratings on the Working Alliance Inventory over the course of therapy?
>
> Is the Group Environment Scale a valid way of measuring the climate of multidisciplinary team meetings?

focus on the performance of a specific measurement instrument, or compare different ways of measuring a construct, rather relating different constructs to each other.

Literature Review

Once the topic area has been chosen, the process of reviewing the literature starts, proceeding in parallel and in interaction with the process of formulating the research questions and planning the study. Before the research proposal is finalized, researchers need to immerse themselves in the literature, so that they have read the key papers and have a good notion of how the field as a whole is developing. Of course, it is impossible to read everything, most fields are too vast to do that, but it is important to have a broad sense of being on top of the literature. This is done for several reasons:

- To assess how well developed the literature is, what kinds of gaps there are in it, and whether there has been sufficient preliminary descriptive research to define the phenomena of interest.
- To see how far the existing literature answers the research questions. What can the proposed study add to the existing literature? Is there a need for another study? Has the study been done before? However, study duplication is rarely a great problem, because no two people ever seem to design a study in the same way and because it is usually easy to devise variations on something that has already been done before.
- To help formulate the research questions in the light of theory or previous research, and, possibly, to give a theoretical or conceptual framework to work within.
- To help with measurement and design issues. To see what measures and approaches have been used in previous studies, and what are the strengths and weaknesses of previous designs.

Sources of Information

In established research areas, there may be an enormous amount of literature, which can seem daunting to get to grips with. Several information sources can help speed up the process of reviewing psychological literature. The following list gives the most useful ones at the time of writing (2015), but valuable new sources do tend to spring up rapidly, and others fade away equally rapidly. Because things change so fast, the URLs printed here may change: we will endeavor to keep them up to date on the companion website to this book.

- For doing a "scoping search," that is, for getting acquainted with an unfamiliar area at the outset of the study, *Google Scholar* (http://scholar.google.co.uk/), *Web of Science* (http://wokinfo.com/), and *Scopus* (http://www.scopus.com/home.url) are invaluable. Google Scholar is freely available; the other two are well established, subscription-based databases which are more focused on academic journal articles. All three have similar functionality. They enable searches on authors, titles, or topic areas. They can list retrieved publications in order of their citation count, which gives a rough index of their importance. They also enable a citation search, which is useful for identifying papers that have subsequently cited a given publication; this will give a sense of current studies in the same line of work. (It is also useful for established researchers who want to know who is citing their own publications. Aside from giving narcissistic pleasure, this can also be useful to see who is active in the same research area.)
- *Discipline-specific databases* are useful for doing formal searches, particularly as part of a systematic review (see the following section). Four main ones are frequently used. *PsycINFO* (www.apa.org/pubs/databases/psycinfo) is an American Psychological Association database that indexes journal articles, books, and technical reports. Searches can be conducted by selecting terms from the Thesaurus of Psychological Index Terms, or text from titles and abstracts, as well as authors' names. *MedLine* (http://www.ncbi.nlm.nih.gov/pubmed) is a US National Library of Medicine database which indexes journals across the whole field of biomedicine, CINAHL (http://www.cinahl.com) focuses on the nursing literature, and EMBASE (http://www.elsevier.com/online-tools/embase) is more pharmacologically oriented.
- *Handbooks.* Current editions of handbooks in the topic area can be extremely useful. Examples are: Lambert (2013a) for psychotherapy research; Ayers et al. (2007) for health psychology; Woolfe, Strawbridge, Douglas, and Dryden (2010) for counseling psychology; Weiner and Otto (2014) for forensic psychology; and Gurd, Kischka, and Marshall, (2012) for clinical neuropsychology. The American Psychological Assocation publishes a useful series of handbooks (see http://www.apa.org/pubs/books/institutions/handbooks.aspx), although they are priced for purchase by institutions rather than individuals. All of these handbooks have chapters by expert authors providing comprehensive reviews of focused topic areas.
- *Review publications.* Journals such as *Clinical Psychology Review, Clinical Psychology: Science and Practice*, and *Evidence-Based Mental Health* publish review articles on major areas of research that are relevant to clinical practice. The *Annual Review of Clinical Psychology* has authoritative reviews of contemporary developments (and there are also *Annual Reviews* in related disciplines, for example, psychology in general, public health and sociology). The *Cochrane Library* (http://www.thecochranelibrary.com/view/0/index.html) is another excellent source of authoritative, contemporary reviews of clinically relevant topics.
- *Recent books.* Library catalogues can be searched by author, title, or subject. Also, the low-tech method of just browsing along library shelves, especially new acquisitions shelves, can often yield a useful collection of books to help start your search.

- *Current journals.* It is worthwhile browsing through the tables of contents of the last couple of years' issues of the three or four most important journals covering your topic area.

Finally, remember that librarians are experts in locating information and can help you to find the best sources of reference, or to use sources that you have difficulty with. Since new resources are appear regularly, it is very useful to have specialist help in your search.

Systematic Reviews

A literature review can be a substantial academic enterprise in its own right, which may lead to a publishable paper. Many research questions can be answered by summarizing the existing literature, rather than by collecting new data. However, a weakness of traditional review methods is that they are often unclear about their methods of locating and evaluating studies, thereby leaving them open to potential sources of bias.

Systematic reviews aim to minimize potential bias by being transparent, comprehensive, and replicable (Centre for Reviews and Dissemination, 2008; Cochrane Library, 2015). In order to avoid the potential problem of reviewers cherry-picking studies, they use systematic procedures for the selection of studies for review, and in the method of appraising and synthesizing those studies.

The review should pose a specific question (e.g. "What is the effectiveness of internet-delivered psychological therapy for anxiety and depression?"). This question then dictates inclusion and exclusion criteria for the studies to be reviewed. For treatment-effectiveness studies, reviewers often use the PICOS categories: population (e.g., people seeking help for anxiety problems), intervention (e.g., internet-delivered cognitive behavioral therapy [CBT]), comparator (e.g., CBT delivered face to face), outcomes (e.g., anxiety self-report measures), and setting (e.g., community). The criteria can also include methodological issues (e.g. the study design or the measures), and publication parameters, in other words, dates, language, peer-reviewed journals or the "grey literature" (theses, conference presentations, and book chapters).

The inclusion and exclusion criteria specify the type of studies that you are looking for. These criteria are then translated into search terms, which stipulate how the studies will be located. They need to specify the databases (PsycINFO, Medline, etc.) and the search terms, including the publication type (date, peer-reviewed paper, language). You can also specify other (nonelectronic) types of searches such as a "hand search" of named key journals or a search of the reference lists of articles included in the review.

The search will produce a (usually large) initial number of hits, which can be then scanned online to see if any can be eliminated on the basis of the title and abstract (and also to remove duplicates if searching over multiple databases). Once this is done, the remaining papers need to be read in full to see which will be included in the final body of studies to be reviewed. Two or three researchers usually make the judgments about whether studies meet the criteria for review, in order to increase the reliability. In the published paper, the various stages of the study selection process are usually illustrated by a flowchart.

It can be useful to organize your references with a bibliographic database, for example, End Note, Reference Manager, Zotero, Mendeley, or (for meta-analyses) RevMan.

Once the set of studies for the review is finalized, their methodological quality (or "risk of bias") needs to be assessed. This is usually done via a "critical appraisal tool," that is, a set of checklists or guidelines for evaluating studies using different types of designs. There are many such tools available. A full list is given in the supplementary website to this book: some examples are Downs and Black (1998) for studies of interventions, the Newcastle-Ottawa Scale (http://www.ohri.ca/programs/clinical_epidemiology/oxford.asp) for epidemiological studies and the Critical Appraisal Skills Programme (CASP: http://www.casp-uk.net/) for qualitative studies.

The body of the review gives a critical appraisal and synthesis of the retrieved studies. They can be organized in any way that will make sense to the reader, for example, by intervention type or by research design. The review needs to summarize the findings of the studies and to give an overall assessment of the strength of the evidence, giving greater weight to the more methodologically sound studies.

The AMSTAR (Assessing the Methodological Quality of Systematic Reviews) checklist (see http://amstar.ca/Amstar_Checklist.php) and the PRISMA (Preferred Reporting Items for Systematic Reviews and Meta-Analyses) statement (see http://www.prisma-statement.org/index.htm) give detailed guidelines for reporting systematic reviews.

In conclusion, the advantages of systematic reviews are that they are transparent and replicable, thereby allowing other researchers to verify their findings. They are particularly good for questions concerning the effectiveness of interventions or the relationship between two variables; they are less good for reviews addressing more theoretical or conceptual issues. Another drawback is that the search strategies are not always as robust (or efficient) as they initially seem. They are often difficult to formulate and do not always identify all the relevant studies. It is not unusual for reviewers to find key studies through less formal routes such as informal conversations with colleagues or browsing through journals (Greenhalgh & Peacock, 2005).

Meta-analysis

Meta-analysis is a specialized type of systematic review and a form of research in its own right. Its name means "analysis of analyses." It is a sophisticated procedure, which was pioneered by Smith and Glass (1977) in a seminal paper analyzing psychotherapy outcome studies; subsequently, it has become the standard method for integrating quantitative research findings in all evidence-based disciplines.

The study selection procedures in meta-analysis are identical to those of systematic reviews in general (see the previous section), but the study synthesis uses quantitative methods to summarize the overall pattern of the findings. Although its general principles are not difficult to understand, conducting a meta-analysis is a technical business whose mechanics lie beyond our present scope. For a practical overview we recommend Lipsey and Wilson (2001) or Borenstein, Hedges, Higgins, and Rothstein (2009), and Cooper, Hedges, and Valentine (2009) or Schmidt and Hunter (2015) for more in-depth coverage. In brief, meta-analysis uses an index of the strength of the findings in each study (an effect-size measure – see Chapter 12), which is then averaged across all of the studies in the review, yielding a useful summary statistic which summarizes the overall result of the review. For example, the average controlled effect size for studies of the psychological treatment of social anxiety disorder is 0.77, which suggests that psychological therapy is overall a beneficial intervention for this problem (Acarturk, Cuijpers, van Straten, & de Graaf, 2009).

Meta-analysis can also be used to look at which features of a study are associated with specific results. For instance, in the psychotherapy outcome literature, you can examine whether studies that use a sample of clients from a university counseling service have a different pattern of results from studies that use a sample drawn from a clinical population, or whether the results of studies conducted by a team who have an allegiance to the therapy differ from those conducted by independent investigators.

The advantage of meta-analysis over traditional reviewing methods is that it is a more powerful way of aggregating the literature and of detecting trends across studies, although it has been criticized for giving too much weight to methodologically unsound studies or alternatively for re-introducing bias in the study selection process (see Lipsey & Wilson, 2001). In recent years it has become the standard method for systematically reviewing large bodies of literature, throughout psychology and in many other disciplines.

Meta-synthesis

Qualitative researchers have recently developed a qualitative analog to meta-analysis, often called “meta-synthesis,” or sometimes “meta-ethnography” (Pope, Mays, & Popay, 2007; Timulak, 2009). It is essentially a thematic analysis of thematic analyses. As is the case with qualitative approaches to analyzing primary data (see Chapter 5), there are a variety of approaches, which differ, for example, in how much they focus on theory development, their degree of interpretation, and the ways in which they integrate the data (Dixon-Woods et al., 2006; Noyes & Lewin, 2011).

The procedure typically involves using the themes from each individual study as the raw data to conduct an overarching thematic analysis (for a description of thematic analysis see Chapters 5 and 12). For example, Timulak (2007) analyzed seven qualitative studies of client descriptions of the impact of helpful events in therapy. The meta-synthesis produced nine overarching themes, including insight, empowerment, and feeling understood.

The Proposal

As your ideas start to become clearer, it is worth setting them down on paper. This will help you show other people what you are planning to do (if you can bear exposing your less than perfect ideas to other people) and it will also help you develop your thoughts yourself, as it is much easier to re-think something that is down on paper rather than just in your head.

At the very least, prepare a one- to three-page summary of your proposed research questions, the theoretical model, and your measures and design. You can use this to get some initial feedback, recruit research supervisors (in the United States, doctoral committee members) and get early consultations. You can then expand it into a longer proposal.

Often (e.g., for PhD research and grant applications) a formal research proposal is required. This is a useful effort, as the proposal will form the core of the introduction and method sections of your final report and will help you sharpen your thinking. It is best approached by successive approximations. The first step is to draft a rough outline, to get something down on paper, no matter how sketchy. Proposals evolve through multiple drafts – five or six is common – as you continue to read, talk, and think about the project.

The structure of the proposal is similar to the introduction and method section of a journal article. It should state what the research topic is and why it is important, briefly review what has already been done and what psychological theory can be brought to bear on the problem, and summarize the intended study and its research questions or hypotheses. The method section describes in detail the proposed design and measurement aspects of the study. A typical proposal has the following structure:

Outline of a research proposal

Introduction
- Statement of the research topic and its importance
- Focused literature review (covering previous research and psychological theory)
- Rationale for and overview of the proposed study
- Research questions or hypotheses

Method
- Participants
- Design
- Measures
- Ethical considerations
- Data analysis procedures
- Expected results and implications (optional)
- Timetable (optional; see below)

References

Costings (for grant proposals)

You may want to give an estimated timetable for the project in your proposal. Even if you do not include one, it is usually helpful at this stage to map one out for your own consumption. List each of the major tasks that comprise the project and try to estimate how long each one will take and what other tasks need to be completed before it can be done. A formal proposal, for example, for a grant application, will usually depict this using a Gantt chart. However, one rule of thumb, especially in doctoral research, is to double any time estimate: expect everything to take twice as long as you think it will (Hodgson & Rollnick, 1996). In our experience, the most common causes of problems in student projects are a slow initial start and unexpected delays later on, often out of your control (e.g., ethics committees, access to participants, and data collection problems).

Possible timetable for a two-year student project

Month 1	Start reading the background literature in your general area of interest.
Months 2–4	Decide on the topic and formulate preliminary research questions. Find a supervisor. Continue the literature review.
Months 5–6	Draft a one- or two-page proposal. Discuss the project in the setting in which you will carry it out.
Months 7–8	Apply to your local ethics committee for approval. In the process, finalize the research plan and prepare for data collection.
Month 9	Begin data collection.
Month 10	Write the first draft of the introduction and method sections.
Months 11–18	Data collection continues. Re-draft the introduction and method sections.
Month 19	Finish data collection. Begin data analysis.
Months 20–21	Complete the data analysis. Write the first draft of the results and discussion sections.
Months 22–23	Complete the write-up. Give the final draft to your supervisor for comments. Make any advance arrangements for duplication and binding.
Month 24	Make final corrections. Duplicate, bind, and submit the polished version.

Consultations

It is a good idea to get a variety of opinions on your proposal from people of different backgrounds: for example, colleagues who know the research area, potential research supervisors, psychologists outside of your area, and nonpsychologists. No research is carried out in isolation: it is always helpful to get input from lay people and from colleagues in the scientific community. Even if many of their suggestions cannot be implemented, you will often find that something of value emerges each time you present your ideas to someone else.

It is particularly useful to get feedback on your ideas and methods from current or former users of the services that you are studying. Their experiential knowledge (Borkman, 1990) can give a valuable perspective on the study. Many users of services understandably feel strongly about being marginalized by researchers and decision makers, and want to have their voices heard, on the principle of "nothing about us without us." Such service-user involvement can usefully continue beyond the proposal stage, throughout the course of the project. Trivedi and Wykes (2002) outline a spectrum of options for user involvement in research, ranging from, as the title of their paper says, "passive subjects to equal partners."

You may want to email some key researchers in the field, to ask for measures, reprints, or details of current work. Also, consider attending a conference, as this is an excellent way to meet people with similar interests in order to exchange ideas, learn about work that has not yet reached the journals, and generally make connections.

Piloting

Pilot studies are small-scale try-outs of various aspects of your intended protocol. Initial pilots may be done with colleagues or friends role-playing participants. This will help you get the administrative procedures roughly right and reveal any gross errors in measurement or design. Subsequent pilots can be with people closer to the target population that you intend to study.

The importance of piloting cannot be stressed enough. Just as a jumbo jet is not built straight from the drawing board without extensive testing, it is rarely possible to design a study in your armchair and then translate it straight into action. You always need to test out procedures, measures, and design. Some things that look good on paper just do not work in practice: they are not understandable to participants or they do not yield useful information. It is also worthwhile performing a few crucial analyses on the pilot data to try out coding and analysis procedures and to see whether the data can actually be used to answer the research questions. A few hours here can save you weeks or months of anguish later on.

Funding

It is often possible to get funding for clinical psychology research, if you plan your application in advance. The major expense in most psychological research projects is research assistant time. Other expenses will be equipment (e.g., for computing or recording) and supplies (e.g., printing, photocopying, and postage), although this part of the budget will usually be small, in contrast to biomedical research, where equipment is often a substantial component of the budget. Finally, there is payment to participants, for time and travel expenses.

The format for grant proposals varies from agency to agency; it is important to obtain applicants' guidelines from potential agencies before starting work on the proposal. However, most proposals will follow the broad outline we have discussed above, with a final section on the proposed costs and timetable of the research (Brooks, 1996). The goal of the proposal is to convince the awarding body that you have a well-thought-out plan for innovative and valuable research. The opinions of nonspecialist colleagues can help predict how the agency might react to your proposal.

Grant-giving bodies often employ a multi-stage screening process. Administrative staff will first read your proposal to check that it falls within the mission of the funding body and that its estimated costs are reasonable. Then it will be sent out to professional reviewers, who will be familiar with the area of research that you are proposing. They will give it an overall rating and supply a detailed report. These will be considered by a meeting of the grant-giving committee, who will be professionals in the field, though probably not specialists in your area. They will be looking to support proposals that demonstrate scientific excellence, have the potential for real-world impact, and have a realistic estimate of costs and timetable.

Specific sources of funds are too numerous and rapidly changing to list here. They can be classified into central and local government agencies, charities, businesses, universities, and health service bodies. Many universities, especially in the United States, have officials that can help you identify funding sources for your research. Competition is fierce, and even if your project is turned down, it does not mean that it was not worthwhile. It is worth asking to see the referees' reports, if they are available, to identify any weaknesses in your proposal before revising and resubmitting it elsewhere.

THE POLITICS OF RESEARCH IN APPLIED SETTINGS

Key points:

- A good relationship between the researcher and the setting is vital to the success of the project.
- A poor relationship can complicate, delay, or thwart the project.
- Access to research settings is often controlled by official or unofficial gatekeepers.
- It is important to get people in the setting on your side and to respond honestly to their doubts about the research.

Researchers often underestimate the organizational difficulties of conducting research in field, as opposed to laboratory, settings. Obtaining access, especially to highly bureaucratic settings such as hospitals, schools, and mental health agencies, may require months. It is vital to start doing your groundwork early on, in order to establish whether it is viable to do the study in your proposed setting. You need to develop a relationship with the gatekeepers and managers, as well as with the clients, staff, etc. Although some people will be supportive of your research, others will oppose it, not always openly.

Access

Negotiating access often requires considerable flexibility, political savvy, and interpersonal skills. Many researchers simply avoid the whole process by creating their own settings, which they can more thoroughly control (Shadish, Cook, & Campbell, 2001). However, if you want to study settings outside the laboratory or research clinic, access problems are hard to avoid.

The first step is to identify and approach the *gatekeepers* of the setting (Taylor & Bogdan, 1998), that is, those who control access and help protect it from disruptive outside interests. Gatekeepers vary in their apparent social and political power, from receptionists who screen out unwanted inquiries, to managers, senior doctors, head teachers, or school principals. An initial working knowledge of the role structure and formality of the setting greatly facilitates the access process and may prevent disasters or wasted effort. Cowen and Gesten (1980) recommend starting at the top of the setting and working your way down (otherwise, leaders are likely to be insulted or suspicious and refuse permission on general principles). They also note that newer programs tend to be less formal and more flexible. It is generally useful to have a prior association with the setting or the support of someone who is trusted (Shadish et al., 2001; Taylor & Bogdan, 1998).

If you have not already, it is important to begin your research journal at this point and, in addition, qualitative researchers may start keeping detailed field notes. The access process often yields essential information about how the setting functions and how it protects itself, which is worth describing in its own right.

The next step is to present your request to conduct research in the setting. Be clear about what you are proposing to do. It often helps to avoid misinterpretation if you put things in writing, stating the general aim of the research and how the data will be collected. This is an adaptation of the brief proposal that we suggested above, in everyday, jargon-free language; you might do it as a kind of press release, such as would be given out to a local newspaper. A draft participant information sheet and informed consent form (see Chapter 10) is often needed. It is advisable to make your own presentations to the administration or staff, rather than giving in to the temptation to let someone else do it for you, as they will often forget or do a poor job of it (Cowen & Gesten, 1980). Presentations to staff meetings should also be supplemented with personal meetings, especially with resistant individuals, preferably in their own setting rather than yours.

In addition, there is often a formal screening process, such as a human subjects review or research ethics committee. We will address ethical issues in Chapter 10, but it is worth anticipating substantial delays, which may be difficult if you are a student trying to complete your research within a tight timetable. Delays may occur for two reasons. First, ethics committees may meet infrequently or at intervals that do not fit in well with your plans. Second, they may raise objections about your research, which you will need to respond to (sometimes involving substantial changes to the protocol) before you proceed with the study.

Responding to Doubts

You often have to work to get people's goodwill and to get them on your side. They may not be convinced by your research topic – people in applied settings may have little understanding of psychological research – but at least they should trust you. A senior doctor once said to one of us, "I'm allergic to anything beginning with psych!", but he was still willing to cooperate with our project because he trusted us.

People might oppose your project for rational, practical reasons, as even the best-run projects inevitably cause disruption, and some services are constantly being asked permission to conduct studies. They might also oppose it in order to protect patients, who may be in danger of being overresearched, even with adequate informed consent procedures. For example, we are aware of projects that have been turned down in services for post-traumatic stress disorder because of patients being on the receiving end of too much research. There are also instances of researchers who exploit the people in the setting by, for example, failing to keep them informed or not acknowledging their contribution when the research is published.

In addition to these rational, practical concerns, research in service settings often arouses feelings of threat and suspicion (Hardy, 1993; Weiss, 1972). It can be seen as intrusive, critical, and a challenge to the established way of doing things. Be sensitive to such opposition: if you do not listen to and attempt to meet people's fears at the planning stage, the likelihood is that the study will be undermined later on. Often these fears may be expressed indirectly: medical and nursing staff may appear overly protective of "their" patients, forms may be mysteriously lost, and so on.

Furthermore, your research may become embroiled in the internal politics of the setting. It is important to be aware of existing organizational tensions, as your study may be used as part of a power struggle: different factions may gain from seeing it carried out or from blocking it (Hardy, 1993).

Your clinical and consulting skills are often valuable in both understanding and in responding sensitively to the doubts of other people about the research. In responding to their often complex feelings it is important to be open about what you intend to do and why. Goodman (1972) describes how he put the client-centered principles of disclosure, empathy, and acceptance into action in a large community psychology project that evaluated the effects of companionship therapy for emotionally troubled boys:

> A careless procedural mistake or two, showing cause for mistrust and generating serious community complaint, could close down the project. We therefore sought to reduce risks by establishing some global operating principles that would simultaneously protect our participants and our research goals.
>
> Eventually, the principles took the form of a general approach, or a "clinical attitude" toward the community, consistent with the client-centered theory of effective therapist-client relationships. That is, we would try to empathize with any complaints about us, accept community apprehension and protective activities, and disclose our own needs and plans – including the global intervention strategy. … Sometimes we also disclosed the motives for our disclosures (meta-disclosure). Implementing this approach took extra time initially, but it brought trust and proved efficient in the long run. (Goodman, 1972, p. 2)

The central issue is what the members of the setting get out of being involved in your research. From their point of view, research has a high nuisance factor. You need to minimize such nuisances and help them realize any possible gains: possible helpful feedback on procedures, the opportunity for patients to talk about their concerns, the increased status of being part of an academic setting, and so on. In Hardy's (1993) terms, you need to align the goals of the research with the goals of the setting. Where possible, it can be useful to include a staff member as part of the research team and to ask the staff to contribute to the design of the research (Cowen & Gesten, 1980; Patton, 2008). However, some clinicians will not want any involvement, while others will want to be kept closely informed. It is wise to provide information to all important staff members: circulate updates, show drafts of papers, etc.

Through these contacts, the researcher, gatekeepers and prospective participants engage in a process of negotiation, in order to arrive at an agreement about how the research will be conducted. This agreement, which can be formal or informal, makes clear what the researcher is asking for, as well as spelling out the researcher's obligations in regard to confidentiality, avoiding disruption, feedback of findings, etc.

Authorship

If you are intending to publish the study, it is worth considering authorship issues from the outset. In applied settings, such issues can be complicated, and occasionally emotionally fraught, because several people may be involved in different ways in the research. This is better tackled sooner rather than later. Senior staff sometimes ask to have their name on a paper simply because the research is being done in their unit. Unless they have made a significant contribution to the research, this is inappropriate and, for psychologists, unethical (American Psychological Association, 2010a, 2010b; British Psychological Society, 2011). Appreciation for permission to work in a setting should be mentioned in the Acknowledgements section of the research paper. (We discuss authorship issues further in Chapter 12.)

CHAPTER SUMMARY

The first stage of the research process involves doing the groundwork. There are two major tasks here: formulating the research questions or hypotheses and resolving any organizational or political issues, in order to prepare the ground for data collection. Researchers may also need to apply for ethics committee approval and for funding at this stage.

Planning research begins with developing a set of research questions for which you would like to find answers. After this, you can consider what type each question is and the appropriate method that goes along with it. As you progress through the groundwork phase of your research, you are likely to revise your questions substantially. Specifying useful, intellectually coherent, and realistic research questions is an iterative process that involves systematically appraising the existing literature, consulting with expert colleagues, and conducting small-scale pilot studies. The research questions can be formulated from either an exploratory, discovery-oriented or a confirmatory, hypothesis- testing approach.

An essential component of planning research is locating and getting to know the literature, so that your study can build on what has come before. Several sources of information can assist with this process. Literature reviews can range in their degree of rigor, from informal reviews to systematic reviews. Meta-analyses and meta-syntheses are systematic and rigorous ways to appraise a body of quantitative and qualitative studies respectively, and may be publishable studies in their own right.

Successfully dealing with the organizational politics of the research setting requires considerable skill. A good relationship between the researcher and the setting is vital to the success of the project, whereas a poor relationship can complicate, delay, or thwart the project. Access to research settings is often controlled by official or unofficial gatekeepers, with whom the researcher must negotiate at the start of the project. People in applied settings often have justified reservations about having research conducted in their organizations. It is important to get people on your side and to respond honestly to their doubts.

FURTHER READING

Hodgson and Rollnick's (1996) amusing chapter entitled "More fun, less stress: How to survive in research" is well worth reading, especially their tongue-in-cheek laws of research (sample: "A research project will change twice in the middle"); there is a contemporary summary on Dorothy Bishop's (2011) blog. Rudestam and Newton's (2007) book *Surviving Your Dissertation* is especially useful for students and has some good material on planning and writing.

Cooper et al. (2009) and Pope et al. (2007) are useful general references for reviewing and synthesizing studies. Lipsey and Wilson (2001) is a good practical guide to meta-analysis. For qualitative meta-synthesis, it is worth consulting the Cochrane Collaboration material (Noyes & Lewin, 2011), which is freely available online.

Several classic texts have material on the politics of working in field settings, for example, from the point of view of evaluation research (Weiss, 1972), participant observation (Taylor & Bogdan, 1998), and experimental and quasi-experimental designs (Shadish et al., 2002).

QUESTIONS FOR REFLECTION

1. What research topics are you contemplating? Why?
2. Construct one or two research questions on your topic area. Look at each one and decide whether it is exploratory or hypothesis-testing. Now, take each question and try writing it as the opposite kind (i.e., if hypothesis-testing, make an exploratory version; if exploratory, make a hypothesis-testing version). What would the implications be of shifting your research question one way or the other?
3. What types of research question have been most common in research on your topic? What types have been neglected?
4. Which theoretical frameworks could you draw upon in order to clarify your ideas?
5. Think about a setting that you might need to gain access to in order to do your research: Who are the likely gatekeepers? What stake do they have in the setting? What are their concerns likely to be?

4

Foundations of Quantitative Measurement

KEY POINTS IN THIS CHAPTER

- How to measure a psychological construct depends on the research questions, the theoretical framework, and the available resources.
- Measures can be classified into self-report and observation, and also according to whether they are qualitative or quantitative.
- The quantitative approach partly derives from the philosophy of positivism.
- Psychometric theory is the study of the properties of quantitative measures.
- Reliability concerns the reproducibility of measurement.
- Validity assesses the meaning of measurement.

We have now reached the second of the four stages of the research process, the measurement stage. It consists of deciding how to assess each of the psychological concepts that are to be studied. Before examining actual methods of measurement (which are covered in Chapters 6 and 7), we will first consider the underlying theory of psychological measurement. The present chapter will concentrate on the conceptual foundations of quantitative approaches; qualitative approaches are covered in Chapter 5. (Note that the term measurement is being used somewhat loosely here, to cover the process of finding ways of capturing the constructs of interest. Strictly speaking, measurement implies numbers, so it is stretching the concept quite far to use it to accommodate qualitative approaches as well.)

Research Methods in Clinical Psychology: An Introduction for Students and Practitioners,
Third Edition. Chris Barker, Nancy Pistrang, and Robert Elliott.

Companion Website: www.wiley.com/go/barker

Furthermore, as we noted in Chapter 3, developing psychometrically sound quantitative measures is an important research activity in itself, especially for new research areas. If the absence of adequate measures looks like an obstacle to your research, it may mean that the logical first step is measure development research, in which case the issues we present in this chapter are at the heart of your study.

The previous, groundwork, stage will have culminated in the formulation of a set of research questions or hypotheses involving various psychological constructs.

The measurement stage of the project then consists in specifying how each of these constructs is to be assessed. For example, the research questions might be about the role of social support in psychological adjustment to stressful life events. The researcher then needs to decide how to assess social support, how to capture emotional adjustment, and what constitutes a stressful life event. There are two separate but interdependent issues to be considered: how each construct is defined and how it is measured.

The boundary between the groundwork and measurement stages (and also the design stage, which we cover in Chapter 8) is not, of course, as watertight as we are implying. For instance, measurement considerations may shape the formulation of the research questions. If an investigator knows there is no valid method for measuring something, it will be difficult to study it. It would be fascinating to study the content of dreams as they are happening during sleep, but there is presently no conceivable method of doing this. Research about real-time dream images cannot, therefore, be conducted, and we can only rely on people's recall of dreams after they wake up. Furthermore, some types of measurement may be beyond the time constraints or the financial resources of the researcher. For example, in research on the process of family therapy, transcribing interviews and training raters to code them is time-consuming and expensive. It may, therefore, be inappropriate for a project with little or no funding.

However, it does not greatly distort the research process to treat groundwork, measurement, and design as three separate, sequential stages. For the rest of this chapter, we will assume that the research questions have already been decided upon (at least for the time being) and that we are now solely concerned with translating them into measurement procedures.

Separating the groundwork and measurement stages is also helpful in beginning to think about the study. Novice researchers often worry prematurely about measurement. As we argued in Chapter 3, it is better to think first about what to study and only secondarily about how to do it. Ideas about measurement will often flow from clearly formulated research questions; measurement will be less problematic if you've thought through the questions you are asking.

As in the previous, groundwork, stage, there are several important conceptual issues, some of which are controversial. We are including this material to give essential background information and in order to help you think more broadly about the framework within which research is conducted. The present chapter and Chapter 5 cover this conceptual background material, whereas Chapters 6 and 7 deal with practical issues in selecting or constructing measures. The first section of this present chapter defines some key terms and looks at the general process of measurement, the second section examines the conceptual foundations of the quantitative approaches, and the third addresses psychometric theory, in particular the concepts of reliability and validity.

THE PROCESS OF MEASUREMENT

Domains of Variables

Variables studied in clinical psychology research can be grouped into five general domains:

- *Cognitive:* thoughts, attitudes, beliefs, expectations, attributions, memory, reasoning, etc.
- *Affective:* feelings, emotions, moods, bodily sensations, etc.
- *Behavioral:* actions, performance, skill, speech, etc.
- *Biological:* physiological and anatomical, for example, heart rate, blood pressure, brain activity, immune functioning, etc.
- *Social:* acute and chronic stressors, social supports, family functioning, work, etc.

These variables form the content of psychological research: the research questions will have been framed in terms of several of them. However, each must be clearly defined and translated into one or more practical measurement methods.

Measuring Psychological Constructs

We will use the term "measurement" in a general sense to refer to the process of finding indicators for psychological concepts. Virtually all psychological concepts can have multiple possible indicators, for example:

- *Phobia:* observed avoidance; self-report of fear; physiological measures of sympathetic nervous system arousal in presence of phobic stimulus.
- *Pain:* self-report of intensity; pain behaviors (flinching, crying, avoidance of painful stimuli); clinician's judgment.
- *Perfectionism:* semi-structured clinical interview; standardized questionnaires.

The underlying psychological concept (phobia, pain, perfectionism) is known as a *construct*; the indicator, or way of observing it, is known as a *measure* of that construct. Although this language is associated with quantitative methods, it can usefully be applied to qualitative methods as well, though in this case researchers may speak of a *phenomenon* rather than of a construct.

The process of going from a construct to its associated measure is often called *operationalization*. The construct is *latent*, that is, it can never be known directly, it can only be inferred from its associated measurement operations.

$$\text{Construct} \xrightarrow{\text{Operationalization}} \text{Measure}$$

Operationalization is, however, not quite as simple as this diagram implies. As the above three examples show, there are often several different ways to operationalize a given construct. Which one(s) to use depends on the research questions, the theoretical framework, and on the resources available for the study. A second, related difficulty is that no single measure can perfectly capture a construct. Finally, the relationship between a construct and its measure(s) is not usually unidirectional: how the construct is measured can shape how we understand or define it. Thus, the whole process of measurement is more complex than suggested by the traditional view of operationalization, as there is rarely a clear-cut, direct mapping of constructs onto measurement operations.

In order to facilitate the process of operationalization, the construct may be given an *operational definition,* that is, it may be defined so that it can be easily measured. Thus, empathy may be initially conceptualized as "Entering the private perceptual world of the other and becoming thoroughly at home in it" (Rogers, 1975), but for psychotherapy process research it may be operationally defined as how accurately the therapist responds to the client's expressed feelings, which then leads to its being measured using expert ratings of audio-recorded interactions.

It is not always possible or desirable to develop an operational definition for every construct. Earlier generations of researchers were taught the doctrine of *operationism* (Stevens, 1935), which stated that a concept is identical to its measurement operations, for example, intelligence is what IQ tests measure. This doctrine was subsequently rejected by philosophers, and replaced by the critical realist strategy of *converging operations* (Grace, 2001), which advocates using multiple indicators to measure underlying constructs. In the clinical context, we often cannot adequately capture many important constructs by our current measures. Social skills may be operationalized by such indicators as good eye contact, smiling, etc., but performing only these behaviors does not produce socially skilled interactions; rather the reverse, it tends to produce people who act like robots (or breakfast TV presenters). In line with the critical realist position, we are arguing that most psychological constructs are only partially captured by their associated measures. We will take up these issues again in the following two sections when we discuss positivism and construct validity.

The operational definition of a construct clearly depends on how it is conceptualized theoretically. For example, two ways of measuring social support – by counting the number of people in a person's social network or by assessing the quality of relationships within that network – have different implications for what is meant by the social support construct itself: whether good social support means several potentially supportive relationships available or just a few good ones. This issue is known as the *theory-dependence of measurement* (see also Chapter 2). Any way of measuring a concept presupposes a view of what that concept consists of, as it is impossible to have pure observations. For qualitative researchers, new knowledge or understanding is always based on prior knowledge or understanding, in the form of presumptions or expectations; it is impossible to start from a totally blank slate (Willig, 2013).

A further complication is that the act of measurement often changes the person or situation being measured, a phenomenon known as *reactivity of measurement.* For example, people may behave differently if they know that they are being observed, and asking a client to monitor the day-to-day frequency of her anxious thoughts may in itself affect the frequency of those thoughts. Another example is that merely filling out a questionnaire about intention to donate blood has been shown to increase the likelihood of subsequent donations (Godin et al., 2010). Research participants are often influenced by their perception of what the researcher is trying to find out, and may respond to the *demand characteristics* of the study (Orne, 1962) when they are completing questionnaires.

Measurement Sources and Approaches

Sources of measurement can be categorized into self-report and observation: you can either ask people about themselves or look at what they are doing. (Strictly speaking, self-report data should be called verbal-report, since it can be gathered from several perspectives, for example, the person of interest, a significant other, or a therapist or teacher. Similarly, observational data could be gathered from an observer, a psychological test, or a physiological measure.) Data may be collected from either source using either qualitative or quantitative methods (see Table 4.1).

The distinction between quantitative and qualitative methods raises a number of fundamental epistemological issues and visions of what science is (as discussed in Chapter 2). Each method derives from contrasting academic and philosophical traditions.

Quantitative methods are identified with the so-called "hard science" disciplines, principally physics; qualitative methods, with the "soft" social sciences, such as sociology and anthropology, and the humanities. In the early decades of the 20th century, many influential psychologists felt that the road to academic prestige and legitimacy lay with being considered "hard science," and thus sought to identify psychology with physics and quantitative methods (Polkinghorne, 1983). This issue has been a continuing struggle within psychology, with its roots in older philosophical traditions (idealism and realism) and early schools of psychology (e.g., introspectionism and associationism). The structure of the present chapter and the following one reflect this debate. They have a thesis, antithesis, synthesis form: we will attempt to set out the underlying issues for each approach, and then suggest in the final section of Chapter 5 how they might be integrated in practice.

Table 4.1 Examples of measures classified by source and approach

	Self-report	*Observation*
Quantitative	Attitude questionnaires	Behavioral observation
	Symptom checklists	Psychological tests of ability
		Physiological measures
		Brain scan
Qualitative	Qualitative interviews	Participant observation
	Diaries, journals	Projective tests

FOUNDATIONS OF QUANTITATIVE METHODS

- Quantitative methods are useful both for precise description and for comparison. They fit in well with the hypothetico-deductive view of science.
- There are well-developed procedures for the analysis of quantitative data.
- The philosophical position of positivism seeks to model psychology and the social sciences on the methods used in the physical sciences.
- The positivist approach was taken up in psychology in the form of methodological behaviorism.
- Positivism has been heavily critiqued, especially by qualitative researchers.

Quantitative methods, by definition, are those that use numbers. The main advantages of quantitative measurement are as follows:

- Using numbers enables greater precision in measurement. There is a well-developed theory of reliability and validity to assess measurement errors; this enables researchers to know how much confidence to place in their measures.
- There are well-established statistical methods for analyzing the data. The data can be easily summarized, which facilitates communication of the findings.
- Quantitative measurements facilitate comparison. They allow researchers to get the reactions of many people to specific stimuli and to compare responses across individuals.
- Quantitative methods fit in well with hypothetico-deductive approaches. Hypothesized relationships between variables can be specified using a mathematical model, and the methos of statistical inference can be used to see how well the data fit the predictions.
- Sampling theory can be used to estimate how well the findings generalize beyond the sample in the study to the wider population from which the sample was drawn.

The development of science would have been impossible without quantification. The necessity of the ancient Egyptians to preserve the dimensions of their fields after the flooding of the Nile led to the development of geometry (Dilke, 1987). If the fields could be measured and mapped out, their boundaries could be restored once the waters had subsided. However, it was not until the late Renaissance that quantification and mathematics began to become an integral part of science. In the 17th century, Newton's laws of motion employed fairly simple algebra to provide a tool of great power and beauty that enabled scientists to predict the behavior of falling apples and orbiting planets.

Positivism

The success of quantitative methods in the physical sciences, especially that of Newtonian mechanics, led to an attempt to extend them into other areas of enquiry. The 19th-century philosopher, Auguste Comte articulated the doctrine of *positivism* (which is, despite its name, completely unrelated to positive psychology). This doctrine has been much elaborated by succeeding scholars, although these elaborations have not always been consistent with each other or with Comte's original formulation (see Bryant, 1985; Bryman, 1988, 2012; McGrath & Johnson, 2003; Proctor & Capaldi, 2006), making it difficult to formulate precisely what is meant by positivism. However, its three main tenets are usually taken to be:

1. That scientific attention should be restricted to observable facts. (The word "positive" is used here in its older sense of dealing with matters of fact, that is, things that people are "positively certain" about.) "Inferred constructs," such as beliefs or motives, have no place in science. This is a version of *empiricism* (the belief that all knowledge is derived from sensory experience).
2. That the methods of the physical sciences (e.g., quantification, separation into independent and dependent variables, and formulation of general laws) should also be applied to the social sciences.
3. That science is objective and value-free.

A related 20th-century development was *logical positivism*. It is associated with the Vienna Circle group of philosophers, such as Carnap and Wittgenstein. They sought to analyze which propositions have meaning and which do not, and then to restrict philosophical discourse to those things that can properly be talked about. Wittgenstein's famous dictum captures the flavor: "What can be said at all can be said clearly, and what we cannot talk about we must pass over in silence" (Wittgenstein, 1921/1961: 3). The logical positivists' central criterion, that all philosophical statements should be reducible to sensory experience, rules out discussion of metaphysical concepts. They contend that it is pointless arguing about ideas like the meaning of life, since propositions concerning it can never be verified.

Methodological Behaviorism

The positivist doctrine was incorporated into psychology in the form of *methodological behaviorism*, whose best-known proponents were Watson and Skinner. They sought to restrict psychology to a "science of behavior," eschewing consideration of any "inner variables," such as cognitions and affects. For instance, Watson urged: "Let us limit ourselves to things that can be observed and formulate laws concerning only those things" (Watson, 1931: 6). In other words, methodological behaviorists would not say that a rat was hungry, as hunger is an inferred construct; instead, they would say it had been deprived of food for eight hours, or that it was at ninety percent of its initial body weight.

Similarly, they would not talk about aggression, but rather such specific behaviors as kicking, punching, or insulting someone. As we noted in our discussion of operationism, the meaning of psychological constructs was limited to the operations or procedures used to measure them.

This attitude was a reaction to the perceived limitations of the introspectionism that had preceded it. *Introspection* consisted of the investigator observing the contents of his or her own consciousness and attempting to expound general theories therefrom. The virtue of sticking to observable behavior is that it is clear what you are talking about and your conclusions can be replicated by other investigators.

Another important manifestation of methodological behaviorism was found in clinical work, in the behavioral assessment movement (e.g., Goldfried & Kent, 1972). This called for clinical assessment to be tied closely to observable behavior, to remove the inferences that clinicians make when, for example, they label a client as having a "hysterical personality."

However, the distinction between high-inference and low-inference measures may be less useful than it seems at first, since inference must occur sooner or later in the research process to give psychological meaning to the data. There is a kind of conservation of inference law: the lower the level of inference in the measurement process, the higher the level of inference needed to connect the observed data to interesting variables or phenomena. Conversely, high-inference measures often do much of the researcher's work early on in the data collection phase, requiring less inference to make sense of the data obtained. For example, a measure of nonverbal behavior in family therapy might use a low level of inference, but the researcher may need to make further inferences in order to make sense of the data, such as interpreting certain body postures as indicating detachment from the family. On the other hand, a measure of transference in psychotherapy might require a high level of inference in rating clients' verbal statements, but no further inferences may be needed to interpret the data.

Criticisms of Positivism

The positivist, methodological behaviorist stance has been severely criticized both from within and outside of psychology (see, e.g., Bryman, 1988; Koch, 1964; McGrath & Johnson, 2003; Rogers, 1985). The central criticism is that, when carried through rigorously, it leads to draconian restrictions on what can be studied or talked about. Psychological constructs that attempt to capture important aspects of experience such as feelings, values, and meanings, are ruled out of court. It leads to a sterile and trivial discipline alienated from human experience. Although few researchers today adopt a strict methodological behaviorism, some articles in mainstream psychological journals still seem to lose sight of the people behind the statistics.

The rise of quantitative methods has also been associated with the rise of capitalism. Young (1979) argues that reducing everything to numbers is a manifestation of a balance sheet mentality. A brilliant fictional indictment of such a mentality was made by Charles Dickens's 1854 novel *Hard Times,* which starkly depicts the loss of humanity that comes from reducing all transactions into quantitative terms. This criticism is still timely, in the light of managed care in the United States and the culture of targets, performance indicators, clinical audit, and cost-effectiveness in the British National Health Service. Emphasizing easily measurable indices of workload often leaves out the less tangible – and arguably more important – aspects of quality.

Conclusions

In our view, the important message to take from the positivists is the value of being explicit about how psychological constructs are measured. It reminds researchers and theorists to be conscious of measurement issues, to tie their discourse to potential observations, and, when speculating about more abstract constructs, to have an awareness of what measurement operations lie behind them. For example, if you are attempting to study complex constructs such as defense mechanisms, you need to specify what would lead you to conclude that someone is using denial or projective identification. Cronbach and Meehl's (1955) notion of construct validity, which we discuss below, is an attempt to place the use of inferred constructs on a sound methodological basis.

It is worth noting that, although quantification and positivism are often treated as equivalent, the stress on quantification is actually only a small part of the positivist package, and possibly not even a necessary part. For example, qualitative methods can be used purely descriptively, without using inferred constructs. Also, the role of quantification in science may have been overstated by the positivists. Schwartz (1992) points to examples in the physical and biological sciences, for example, the double helix model of DNA, that use mainly descriptive qualitative methods.

Having described the rationale for quantification, we can now look at the underlying theory of measurement, including the important question of how to evaluate particular measures.

PSYCHOMETRIC THEORY

Psychometric theory refers to the theory underlying psychological measurement in general. In particular, it leads to a framework for evaluating specific measurement instruments. Although developed in the context of quantitative measurement, some of its ideas can, arguably, be translated into qualitative methods. They are essential for all would-be researchers to grasp, whatever approach they ultimately plan to adopt.

> **Key points:**
>
> Psychometric theory is the theory of psychological measurement.
> - Classical test theory leads from a simple set of assumptions to useful concepts for evaluating specific measurement instruments, notably reliability and validity.
> - Reliability refers to the reproducibility of measurement: its principal subtypes are test–retest, equivalent forms, internal consistency, and inter-rater reliability.
> - Validity assesses the meaning of measurement. It can be divided into content, face, criterion, and construct validity.
> - Utility refers to how easy the measure is to administer and to how much information it adds.
> - Two alternatives to classical test theory are generalizability theory and item response theory.

Definitions

Scales of Measurement

Measurements may have the properties of nominal, ordinal, and interval scales (Stevens, 1946). *Nominal scales* consist of a set of mutually exclusive categories, with no implicit ordering. For example, researchers in a psychology outpatient service might use a simplified diagnostic system consisting of three categories: 1 = anxiety; 2 = depression; 3 = other. In this case, the numbers are simply labels for the categories: there is no sense in which 2 is greater than 1, etc., and thus the diagnostic system forms a nominal scale.

> Scales of measurement:
> - Nominal: no ordering
> - Ordinal: ordering only
> - Interval: ordering plus distance

An *ordinal scale* is like a nominal scale but with the additional property of ordered categories, that is, it measures a variable along some continuum. For example, psychology clients might be rated on a scale of psychosocial impairment, consisting of three categories: 1 = slightly or not at all impaired; 2 = moderately impaired; 3 = highly impaired. On this scale, someone with a score of 3 is defined to be more impaired than someone with a score of 2, and thus it has ordinal properties. However, there is no assumption that the distance between successive ordinal scale points is the same, that is, the distance between 1 and 2 is not necessarily the same as that between 2 and 3.

An *interval scale* is like an ordinal scale with the additional property that the distances between successive points are assumed to be equal. For example, the Personal Health Questionnaire (PHQ-9: Kroenke, Spitzer, & Williams, 2001), a self-report measure of depression, is usually treated as an interval scale. This assumes that the increase in severity of depression from a score of 10 to a score of 15 is equivalent to the increase in severity from 20 to 25.

The importance of distinguishing between these types of measurement is that different mathematical and statistical methods are recommended to analyze data from the different scale types. A scale needs to have interval properties before adding and subtracting have any meaning. Thus it makes no sense to report the arithmetic average of nominal scale data: the mode must be used instead. Nominal and ordinal scales also require nonparametric or distribution-free statistical methods, whereas interval scales can potentially be analyzed using standard methods such as the t-test and the analysis of variance, provided that the data are normally distributed (Howell, 2010). In actual practice, however, the line between ordinal and interval data is blurred. Item-response theory analyses (Bond & Fox, 2007; see later in this chapter) have shown that many popular measures violate the equal-interval assumption, making them in fact ordinal measures, and, furthermore, item-response theory methods can be used to transform clearly ordinal measures into true interval measures.

Type of Measure

Measures can be either nomothetic or idiographic. *Nomothetic measures* compare individuals with other individuals; most psychological tests and inventories fall into this category. The scores on a nomothetic measure can be *norm-referenced* when they have no absolute meaning in themselves, but are simply indicative of how the individual stands with respect to the rest of the population. For example, the scores on the Wechsler Adult Intelligence Scale (WAIS) are norm-referenced: they are constructed in such a way as to have a population mean of 100 and a standard deviation of 15. A *criterion-referenced* measure, on the other hand, compares individuals against an absolute standard. For example, a typing speed of 40 words per minute denotes a certain degree of skill at the keyboard; scores on the Global Assessment Scale (Endicott, Spitzer, Fleiss, & Cohen, 1976) denote specific levels of psychological functioning.

The contrasting approach, *idiographic measurement*, focuses solely on a single individual, without reference to others. No attempt at comparison is made. Some examples of idiographic methods are the Personal Questionnaire (Elliott et al., 2015; Phillips, 1986; Shapiro, 1961a), Q-sorts (e.g., Jones, Ghannam, Nigg, & Dyer, 1993), and repertory grids (Winter, 2003). Such measures are often used within small-N research designs, and are discussed further in that context in Chapter 9.

Reliability

How do we go about evaluating specific measures? The two main criteria, reliability and validity, are derived from a set of assumptions known as classical test theory. We will first describe them within that framework, and then reconceptualize them in terms of a newer approach, generalizability theory.

The original idea in classical test theory was that in measuring something, one is dealing with consistency across repeated measurements. The consistent part of the score, the part that is the same across measurements, is known as the *true score*. It is conceived of either as an ideal score or as the mean of an infinitely large set of scores. The observed score is the sum of the true score and error, which is conceived of as a random fluctuation around the true score.

Expressed as an equation, this relationship is $x = t + e$ (where x is the observed score, t is the true score, and e is the error); this is called the fundamental equation of classical test theory. From this simple equation, and a few assumptions about how the error score behaves, the theory of reliability can be constructed.

Reliability refers to the degree of reproducibility of the measurement. If you repeat the measurement in various ways, do you get the same results each time? The more consistent the measurement, the greater the reliability and the less error there is to interfere with measuring what one wants to measure. It is analogous to the signal to noise ratio in electronics. To put it the other way around, unreliability is the amount of error in the measurement, mathematically speaking, the proportion of error variance in the total score. For example, if you were measuring individuals' levels of paranoid ideation using a questionnaire, you would expect their scores to stay roughly stable, at least over short time periods. If people's scores fluctuated widely over a two-week interval, the measure would be unreliable and probably not worth using.

Reasonably high reliability is important because it enables you to measure with precision, and therefore allows you to discover relationships between variables that would be obscured if too much error were present. At the other extreme, if the measurement is totally unreliable, you are simply recording random error, not whatever it is you want to measure.

If you are examining how two variables correlate, the effect of unreliability is to attenuate their observed relationship, making their underlying relationship more difficult to detect. Any relationship between measures of two variables is a joint function of the true underlying relationship of the variables and the weakening effect of the unreliability of the measures (Nunnally & Bernstein, 1994). For example, if you are studying the correlation between social support and depression, and your measures of each of those two constructs are somewhat unreliable, the correlation which you obtain may be low, even though the underlying relationship between the variables is large. The reliability of your measures can have a huge impact on your ability to find what you are looking for, especially with small samples.

However, reliability says nothing about what the measure actually means. It simply says that the measurement is repeatable. A thermometer with insufficient liquid in it will give very reliable readings, but they will be wrong. The meaning of the measure is assessed by its validity, which we will discuss later under a separate heading.

Reliability types:

- Test-retest
- Equivalent forms
- Internal consistency (including split-half)
- Inter-rater

Methods for assessing reliability depend on the scale of measurement (nominal or interval), the type of measure (self-report or observation), and the type of consistency that you are interested in. The most common methods are as follows.

Test-retest reliability examines consistency over time. The measure is administered to the same set of people on two or more separate occasions (e.g., a week or a month apart). Its test-retest reliability (sometimes called the stability coefficient) is assessed by the correlation between the scores from the different time points. There may be a problem with practice effects, unless these are uniform across individuals (in which case the overall mean would be affected, rather than the correlation). This is the most appropriate type of reliability when you are considering change over time, or when you are combining repeated measurements to produce a single index (e.g., therapeutic alliance over the first three sessions of treatment).

Equivalent forms reliability is rarely found these days, but may still be encountered in test manuals. It examines reliability across different versions of the same instrument. It is an extension of test-retest reliability, where instead of readministering the same measure on the second occasion, you use an alternate (or "equivalent" or "parallel") form. (Some instruments have a Form A and a Form B to facilitate this.) Again, the reliability coefficient is the correlation between the scores on the two administrations.

Internal consistency is the standard way of assessing the *inter-item reliability* of a scale that is composed of multiple similar items (many self-report measures fall into this category). The assumption is that the items are *equivalent* or *parallel,* that is, that they all aim to tap the same underlying construct. For instance, two parallel items on the Client Satisfaction Questionnaire (CSQ-8: Larsen, Attkisson, Hargreaves, & Nguyen, 1979), a widely used self-report scale assessing clients' satisfaction with psychological and other healthcare services, are "Did you get the kind of service you wanted?" and "Overall, in a general sense, how satisfied are you with the service you received?" Even though these items ask slightly different questions, they are assumed to be tapping the same psychological construct: satisfaction with the service. Internal consistency, figuratively speaking, is a way of assessing how well all the items of the scale hang together: are they all measuring the same thing (high consistency) or different things (low consistency)? Overall scale reliability is estimated from the covariances of all the items with each other, typically assessed using *Cronbach's alpha* (see below).

Split-half reliability is now mostly of historical interest, but, like parallel forms reliability, will be found in some test manuals. It is an old form of internal consistency, used prior to the development of high-speed computers because it was easier to calculate. Split- half reliability was assessed by dividing a measure into two equivalent halves (e.g., odd and even items), then correlating the two halves with each other. It has been replaced by Cronbach's alpha.

Inter-rater reliability is used for observational rather than self-report measures in order to check the reliability of observations. For example, researchers may be interested in measuring therapist empathy in a therapeutic interaction, or in estimating children's mental ages from their drawings. The researchers making the ratings may be referred to as coders, raters, or judges; their inter-rater reliability is the extent to which their ratings agree or covary with each other (see the next section for computational details). There are two separate issues: how good is the rating system as a whole and how good are individual raters – for example, should one be dropped? (See Chapter 7 for a further discussion of the rating process.)

Reliability Statistics

A variety of different statistics are used to measure reliability. The first step is to establish which scale of measurement is involved, since this determines the reliability statistic. For practical purposes only nominal and interval scales need be considered: as noted earlier, ordinal scales can generally be analyzed as if they were interval scales.

Nominal Scales

As psychologists frequently need to calculate the reliability of nominal scale data, we will illustrate the calculations using a simple example. Suppose that two psychologists who work in a specialized psychology outpatient service each categorize patients into three diagnostic groups – generalized anxiety, phobia, and other – and they want to know how similar their judgments are. Since there is no ordering implied in the categories, it is a nominal scale.

The first thing to do with two sets of categorical measurements (e.g., judgments across raters, occasions, instruments, or settings) is to display the data in a two-way classification table. Table 4.2 gives some possible data from the diagnostic classification study with 100 patients.

The obvious initial thing to do is to calculate the *percentage agreement* between the clinicians. This is computed from the total number of observations in the agreement cells of the table (indicated by underlining in the table), divided by the total number of observations. In the example, the agreement is (10 + 20 + 20)/100 = 0.50, or 50% agreement.

Table 4.2 Simplified example of a two-way classification table

Rater 1	*Rater 2*			
	Generalized anxiety	*Phobia*	*Other*	*Total*
Generalized anxiety	<u>10</u>	20	0	30
Phobia	10	<u>20</u>	10	40
Other	0	10	<u>20</u>	30
Total	20	50	30	100

However, since raters categorizing patients at random would still agree by chance part of the time, a way to control for chance agreement is desirable. *Cohen's kappa* (κ) is used to accomplish this (Cohen, 1960). The formula is:

$$\kappa = (p_o - p_c) / (1 - p_c)$$

where p_o is the proportion of agreement observed (i.e., the total of the numbers in the agreement cells of the table divided by the grand total), namely, 0.50 in the example above. p_c stands for the proportion of agreement expected by chance alone. To calculate p_c, first calculate the proportion of observations in each row and column by dividing each row and column total by the grand total. Then p_c is calculated by multiplying corresponding row and column proportions by each other and adding the resulting numbers together. In the example, p_c is given by $0.3*0.2 + 0.4*0.5 + 0.3*0.3 = 0.06 + 0.20 + 0.09 = 0.35$. This means that the level of agreement due to chance alone is 35%.

Using the above formula for Cohen's kappa, the corrected agreement statistic is therefore $\kappa = (0.50 - 0.35)/(1 - 0.35) = 0.23$, which is not very good (see Table 4.4).

With nominal scale data it is further possible to analyze the reliability of any particular category within the scale. That is, you can determine which categories have good agreement and which do not. This is done by carrying out a series of analyses in which you collapse the scale into two categories: the category of interest and all other categories combined. In the example above, the researchers might be interested in the reliability of the generalized anxiety category. They would then form a smaller, two-by-two table, amalgamating the two other categories, and calculate Cohen's kappa for that table (we leave this for the reader to calculate).

Ordinal and Interval Scales

With ordinal and interval scale measurements, there are several choices for assessing reliability. To begin with, if you are using a cutoff point on an interval scale (e.g., an observational rating scale for depression), you may turn it into a binary nominal scale (e.g., "depressed–nondepressed"). You could then use Cohen's kappa to compute reliability.

More commonly, however, the researcher calculates the association between the two measurements using Pearson's correlation coefficient, r. This statistic is usually robust enough to use in most applications (Nunnally & Bernstein, 1994). If more than two raters are involved, Cronbach's alpha can be used with raters treated as items; a more complicated statistic known as the *intraclass correlation* can be also used (Shrout & Fleiss, 1979).

In the common situation where a scale is formed from multiple items that are averaged or totaled together, Cronbach's alpha is the standard index of the reliability of the pooled observations (overall score across items, pooled judges' ratings, or observations pooled over time). The SPSS Reliability procedure (or any other reasonably complete statistics software package) can usually be used to perform the computations. The reliability of the whole scale will be higher than the average inter-item correlation, because adding together multiple measures averages out the errors in each of them.

Since the internal consistency of a scale increases with the number of items in the scale, it is easier to get higher reliabilities with, say, a 24-item scale than with an 8-item one. Thus you might want to see how much increasing your scale by various amounts would improve its reliability. The reliability of such combined measurements can be calculated using the Spearman-Brown Prophecy Formula (Nunnally & Bernstein, 1994):

$$r_{kk} = \mathrm{k}\left(\mathrm{r}_{11}\right) / \left(1 + (k-1)\mathrm{r}_{11}\right)$$

where r_{kk} refers to the reliability of the combined measurements; k = the factor by which you are increasing the scale (a fraction if you are making it shorter); and r_{11} is the original reliability coefficient. (This formula yields the same results as the standardized Cronbach alpha statistic.)

Two examples of common uses of this formula may clarify its application. In the first example, suppose that you have an 8-item scale with a reliability of 0.6, and you want to know how reliable a 24-item version made up of similar items would be. In this case, r_{11} is equal to 0.6 and k is 3 (because the new scale is three times as long as the original one). Then the new reliability would be $3*0.6/(1 + 2*0.6) = 0.82$.

In the second example, suppose that you wish to combine 20 parallel items with an average intercorrelation of only 0.3 into a scale. Surprisingly, the scale thus formed would have an excellent reliability of 0.89 (= $20*0.3/(1 + 19*0.3)$), proof that, statistically speaking, if you just have enough sow's ears, it is possible to make quite a nice leather purse (even if it isn't silk!).

Dimensionality

The above discussion has assumed that the measure is attempting to assess a single construct. If, instead, you suspect that it may be capturing several different dimensions, for example, on a psychological symptom checklist like the SCL-90-R (Derogatis, 1994), then factor analysis should be used to investigate the internal structure of the measure. The procedure for this is beyond the scope of the present text: readers should consult specialist references (e.g., Floyd & Widaman, 1995; Tabachnik & Fidell, 2013). However, Cronbach's alpha should also be used to assess the internal consistency of the resulting subscales.

Validity

Validity is a more difficult concept to understand and to assess than reliability. The classical definition of validity is "whether the measure measures what it is supposed to measure." For example, does a depression scale actually measure depression, or does it measure something else, such as self-esteem or willingness to admit problems?

In this chapter we are discussing validity of measurement. However, Cook and Campbell (1979) have articulated a highly influential, broader conception of validity which involves design as well as measurement. We will address this in detail in Chapter 8.

There is a two-step process in developing and evaluating measures: first you look at reliability, then validity. Reliability is a necessary but not sufficient condition for validity. To be valid, a measure must first be reliable, otherwise it would consist mainly of error. For example, if two expert raters cannot agree on whether transcripts of a therapy session show evidence of client denial, then the validity of the denial category cannot be established. On the other hand, a measure can be highly reliable but still invalid, for example, head girth as a measure of intelligence.

Validity may be assessed in several different ways, and a thoroughly researched measure will report all of them in its manual.

Content Validity

Content validity assesses whether the measure adequately covers the different aspects of the construct that are specified in its definition. For example, does a self-report depression scale have items which capture the components of lowered mood, decreased motivation, sleep disturbance, etc.? This is a qualitative judgment: there is no such thing as a content validity coefficient.

Face Validity

Face validity is similar to content validity and assesses whether the measure looks right on the face of it, that is, that it self-evidently measures what it claims to measure. For instance, the items of a depression scale should ask about low mood, but not about attitudes to authority. The Hogan Empathy Scale (Hogan, 1969) has the item "I prefer a shower to a bath"—it is not at all obvious, on the face of it, how this relates to empathy. Face validity is usually desirable, but not always so. The Minnesota Multiphasic Personality Inventory (MMPI), for instance, has a number of "subtle items," which were designed to make the test more difficult to fake (Weiner, 1948).

Face validity is partly a public relations concept, to make sure that the scale looks right to potential respondents, who will become alienated if it does not appear relevant to the purpose at hand. For example, a symptom checklist asking about such abnormal experiences as psychosis or suicide may be inappropriate for research in family practice settings because it will put people off. Like content validity, face validity is a qualitative concept: there is no face validity coefficient. Face validity, in the sense of "resonance" with the reader, is also a key criterion for evaluating qualitative research findings (see Chapter 5).

Criterion Validity

Criterion validity is a central validity consideration. It assesses how well the measure correlates with an established criterion or indicator of the construct it is measuring. It is divided into concurrent and predictive validity, depending on whether the criterion is measured at the same time or later on. For *concurrent validity,* the scale is correlated with a current criterion: a depression scale could be correlated with clinicians' ratings of depression. For *predictive validity*, the scale is correlated with a future criterion: a hopelessness scale could be used to predict future suicide attempts. The validity coefficient in both cases is the correlation between the measure and the criterion.

Note that seeing whether a measure can predict membership of two separate criterion groups (e.g., can a depression scale distinguish between depressed and nondepressed patients?) also falls under this heading. It is an example of concurrent validity, though it is often wrongly referred to as discriminant validity, which is a different concept altogether (see below).

If the measure is being used for diagnostic reasons, it is useful to specify its sensitivity and specificity. *Sensitivity* is an index of how well the measure picks out those patients who have the target condition (i.e., how few false negatives there are); *specificity* is an index of how well it avoids picking out those patients who do not have the target condition (i.e., how few false positives there are). Thus a depression scale, with a given cutoff point, would have high sensitivity if it identified

Table 4.3 Possible results of a binary diagnostic test for depression

	Actual status		
	Depressed	*Non-depressed*	*Totals*
Depressed according to test	True positives (9)	False positives (10)	Test positive (19)
Non-depressed according to test	False negatives (1)	True negatives (80)	Test negative (81)
Total	Actual positives (10)	Actual negatives (90)	Grand total (100)

almost all of the depressed patients in the sample, and high specificity if it did not identify any nondepressed people as being depressed.

This can be depicted in a two-by-two table (see Table 4.3), with columns giving the numbers who actually have and do not have the condition (i.e., depression in the current example – other examples are medical tests, e.g., for heart disease or cancer). The rows give the numbers that the test indicates have and do not have the condition.

Then the *sensitivity* = number of true positives / total number of actual positives. In the numerical example, this is 9/10 = 0.90.

The *specificity* = number of true negatives / total number of actual negatives. In the example, this comes to 80/90 = 0.89.

It is also useful to calculate the *positive predictive value* of the test, which is the proportion of those testing positive who actually have the condition, and the *negative predictive value*, which is the proportion of those testing negative who don't have the condition. In the example, the positive predictive value is 9/19 = 0.47, and the negative predictive value is 80/81 = 0.99. Thus we can be reasonably sure that a negative test result rules out the condition, but much more uncertain (less than 50% sure) that a positive test result means that the individual has the condition. This is reflects a problem with low *base rates* (the base rate is 10/90 = 0.11 in the example), initially identified by Meehl and Rosen (1955), which means that even a test with good sensitivity and specificity, like the hypothetical one in this example, may still not be very useful for individual classification if the condition is relatively rare in the population.

In practice, when one is developing a test and deciding where to place the cutoff points, there is a trade-off between sensitivity and specificity. If it is important to avoid false negatives (e.g., in assessing suicidal or homicidal risk) then the cutoff point is lower, which results in more false positives (people who are assessed as dangerous that are in fact not), but this may be regarded as an acceptable price to pay in order to save lives in the future.

For criterion validity, the measurement that is being used as the criterion must be well established and of unquestionable validity itself. Such criteria are often referred to as 'gold standard' (a clear application of the correspondence theory of truth). In cases where there is no established gold standard criterion, then considerations of construct validity are adopted instead.

Criterion validity is basically about the practical value of the measure: how well it performs in predicting the criterion. It is less concerned with the underlying construct that the measure is capturing, or the theory that links test and criterion (Strauss & Smith, 2009). The Beck Hopelessness Scale may have good predictive validity for suicide attempts, but that does not necessarily imply that it is a good measure of hopelessness. To establish whether a measure is actually assessing the psychological construct it was intended to assess, considerations of construct validity must be addressed.

Construct Validity

Construct validity is a complex consideration. As its name suggests, it examines the validity of a construct as well as that of individual methods of measuring that construct, which the previous validity types look at (Cronbach & Meehl, 1955). It asks whether the pattern of relationships between measures of that construct and measures of other constructs is consistent with theoretical expectations: how it fits with what Cronbach and Meehl (1955) termed the "nomological net." Construct validity is established by accumulating studies which test predictions about how the construct in question should relate to other constructs and measures (Strauss & Smith, 2009).

In one classical type of construct validity study, the relevant associations are displayed in a *multitrait-multimethod matrix* (Campbell & Fiske, 1959). This is a table that sets out the correlations between several ways of measuring several different constructs. For example, if a researcher were interested in the construct validity of public-speaking anxiety, she might measure it by using, say, two different self-report scales, in addition to an observational measure and measures of heart rate and galvanic skin response taken while the person is speaking. In addition she would collect comparable self-report and observational measures from the same people on different constructs, such as IQ, trait anxiety, extraversion, and self-esteem. The multitrait-multimethod matrix displays the correlations among all of these variables.

The matrix reveals the extent to which measures of the construct of interest are positively correlated with measures of related constructs (*convergent validity*) and uncorrelated or weakly correlated with measures of unrelated constructs (*discriminant validity*). In the above example, all of the different measures of public-speaking anxiety would be expected to correlate at least moderately with each other. They would also be expected not to correlate significantly with age or IQ, and to correlate only moderately with trait anxiety and self-esteem, but more highly with extraversion.

The multitrait-multimethod matrix also reveals the extent of *method variance*, the tendency of measures of a similar type to correlate together. For example, scores from self-report measures are often moderately intercorrelated, even though they were designed to assess quite different constructs. This is why it is desirable, where possible, to use different measurement methods within a study or research program, and not to rely on any one viewpoint or type of measure.

Generalizability Theory

An alternative to classical test theory is generalizability theory, which was developed by Cronbach, Gleser, Nanda, & Rajaratnam (1972). It uses a multifactorial model rather like analysis of variance (see Shavelson., Webb, & Rowley, 1989; Wasserman, Levy, & Loken, 2009). It asks, "To which conditions of observation can a particular observation be generalized?" or "Of which other situations can a measurement be considered to be representative?" It de-emphasizes the concept of the true score in favor of the central activity of analyzing sources of variations in

Table 4.4 How reliability and validity involve generalizing across measurement facets

Facet to generalize across	*Traditional psychometric concept*
Observers: across raters, judges	Inter-rater reliability
Occasions: across time	Test-retest reliability Predictive validity
Instruments: across various ways of measuring the same thing (including individual items)	Equivalent forms reliability Internal consistency Concurrent validity Convergent validity
Settings: across situations (usually going from more to less controlled situations)	Criterion validity Convergent validity

the scores. The theory deliberately blurs the distinction between reliability and validity, a distinction that it turns out is not clear-cut even within classical test theory (Campbell & Fiske, 1959).

Generalizability theory assumes that measurement comprises three elements: persons, variables, and facets (or conditions) of measurement. Four facets can be distinguished: observers, occasions, instruments, and settings. Generalization across these facets corresponds to several of the traditional psychometric concepts (see Table 4.4).

In other words, generalizability theory examines the confidence with which you can generalize measurements to other observers, occasions, instruments, or settings. If you are developing a test or scale, it is a good idea to define these conditions, and determine generalizability across the desired range. Such an examination is referred to as a generalizability study, and is typically set up as a multifactorial research design (see Chapter 8) that incorporates each relevant facet as a factor. However, even if you do not actually carry out such a study, the conceptual framework of measurement facets is still useful for understanding the factors important to your instruments.

The more qualitative or conceptual forms of validity, content, face, and more complex forms of construct validity, do not fit neatly into the generalizability theory framework. They can be treated separately, or could be considered as aspects of a fifth facet, level of abstraction: generalization from the specific working definition of the variable to other representations—theoretical, empirical, and phenomenological—inferred on the basis of what it is theorized *not* to be as well as what it *is* related to.

Item Response Theory

A second alternative to classical test theory is item-response theory and, like generalizability theory, it is fairly complicated mathematically. It grew out of dissatisfaction with the limits of traditional test theory and was originally developed for tests of knowledge or ability, although it can be applied more generally. In particular, item-response theory models attempt to repair problems with the unequal intervals and lack of discriminability that occur with most rating scales. For example, the five-point Likert scales used on common measures of psychological distress typically have serious scaling problems, including violations of the equal interval assumption and a failure to discriminate between adjacent scale points, such as "moderately" and "quite a bit" (Elliott et al., 2006).

The most straightforward form of item-response theory is Rasch analysis (Bond & Fox, 2007), which models a single parameter of item performance, known as *difficulty*, defined as how much of the variable being measured has to be present in order for an informant to endorse a given item at a given level (e.g., at step 3 on a 5-point scale). Rasch analysis is geared toward constructing unidimensional measurement instruments in which items range from easy to difficult to endorse, arranged in uniform steps of equal intervals. It allows researchers to answer a range of questions about a measure, such as:

1. What is the optimal number of rating scale categories for the instrument?
2. Can we improve the internal reliability of the instruments by dropping misfitting items and unnecessary scale points?
3. How many distinct clinical groups (strata) can be distinguished using the instrument?
4. What measurement gaps exist along the continuum measured by the instrument, indicating the need for adding or deleting certain types of items?
5. For a given sample, what sampling gaps exist along the measured continuum?
6. What can the ordering of items along the continuum measured tell us about what it is actually measuring (construct validity)?
7. Does the construct have different meanings for different client populations?

The basic idea is to plot the probability of endorsing to a particular item (e.g., "fear of fainting in public") at a given level (e.g., 3 = "quite a bit") against levels of the underlying latent trait that the test is trying to assess, for example, social anxiety. This graph, which has an elongated S-shape, is known as the *item-characteristic curve*. It demonstrates the difficulty level of the item for various levels of the latent construct being measured, and is a rapid way to summarize its performance. There are various mathematical models of the relationship between the latent construct and the probability of endorsing the item at a given level: the one parameter (or Rasch) model, and the two and three parameter models. For further details, see Bond and Fox (2007) or Reise and Waller (2009).

One clinical example of item response theory is Roberson-Nay, Strong, Nay, Beidel, and Turner's (2007) examination of the properties of a short form of the Social Phobia and Anxiety Inventory. Detailed analysis of the performance of each item was carried out, as a result of which the original 7-point Likert scale was collapsed to a more efficient 5-point scale. Item response theory is also particularly useful for computerized psychological testing, as it enables the computer to present items that are determined by the participant's previous performance, thus greatly increasing the efficiency of the testing procedure.

Utility

In addition to reliability and validity, measures also vary in their utility or practical value. Measures which are easy to complete, or take little time to administer or score, are more convenient than measures which require more skill and time. Another aspect of utility is the incremental value of the information provided. Does the instrument yield information which has not been obtained from other measures and which therefore adds something or can be put to good use?

For example, the utility criterion weighs against using the Thematic Apperception Test as a measure of pre-post change in therapy, because it is time-consuming and difficult to administer and score – except in circumstances where it provides critical information that can be gathered in no other way. On the other hand, piling on additional easy-to-administer self-report outcome measures may also violate utility considerations (in addition to imposing an unacceptable burden on the participants), because such measures are typically highly intercorrelated and thus do not add useful information.

Standards for Reliability and Validity

Reliability and validity calculations are useful both for off-the-shelf measures and for measures that you are constructing yourself. The usual practice is to report the reliability of new or uncommon measures in the Method section of a research paper. Table 4.5 gives some suggested standards for evaluating the reliability and validity of measures. These have no logical basis; they are simply rules of thumb that represent current standards in the research community (although there are variations between different researchers and journals). We have drawn from the recommendations of Kraemer (1981) and Nunnally and Bernstein (1994), in addition to our own experience in scale development and editorial reviewing (although see Lance, Butts & Michels, 2006, for a cautionary note).

Statistical significance tests of reliability coefficients are usually irrelevant, since they are too lenient, as the null hypothesis of no agreement at all should be easily rejected in most cases. What matters is the magnitude of the coefficient, not whether it attains statistical significance.

Generally speaking, the higher the reliability the better. However, it is possible to have too much of a good thing. Reliabilities greater than 0.90 may indicate either overkill (i.e., too many items or raters) or triviality (selection of superficial but readily ratable variables).

Values in validity research (i.e., research which attempts to test predicted relationships among constructs) are typically substantially lower than in reliability research (i.e., research which attempts to generalize across raters, occasions, or within measures). In this case, values of 0.70 or higher generally mean that one is really tapping reliability instead of validity (i.e., that the two measures are really measuring the same thing instead of two different things that are supposed to be related). Validity values of 0.50 can be considered good, and 0.30 acceptable, but these recommendations are much more tentative, as they depend considerably on the particular application area.

Table 4.5 Suggested reliability and validity standards

	Reliability	*Validity*
Good	0.80	0.50
Acceptable	0.70	0.30
Marginal	0.60	0.20
Poor	0.50	0.10

In particular, validity coefficients in epidemiological research tend to run much smaller than in clinical or personality research.

CHAPTER SUMMARY AND CONCLUSIONS

This chapter has examined the theory and philosophical background of psychological measurement, looking at how to conceptualize the measurement process and how to evaluate the quality of particular measures. The process of going from an underlying construct to the measurement of that construct is known as operationalization. There are typically several ways to measure any given construct; how it is done depends on the research questions, the theoretical framework, and the available resources.

The quantitative approach to measurement partly derives from the philosophical position of positivism, which seeks to model psychology and the social sciences on the methods used in the physical sciences. The positivist approach has been heavily critiqued, especially by qualitative researchers.

The central framework for conceptualizing the properties of quantitative measures is known as psychometric theory. Making some simple assumptions within psychometric theory allows us to develop a set of ideas, known as classical test theory, about how to evaluate measures. The central concepts are reliability and validity. Reliability concerns the reproducibility of measurement: its principal subtypes are test-test reliability, internal consistency, and inter-rater reliability. Validity assesses the meaning of measurement. It can be divided into content, face, criterion, and construct validity. Reliability is a necessary, but not a sufficient, condition for validity. Finally, there is the concept of utility, which asks: How easy is the measure to administer and what information does it add? Two promising alternatives to classical test theory are generalizability theory and item-response theory; however they are both more complicated mathematically and in spite of their promise are not yet in widespread use.

The measurement criteria of reliability, validity, and utility relate to the four epistemological truth criteria discussed in Chapter 2. Criterion validity is an instance of the correspondence criterion of truth, while construct validity and internal consistency are examples of the coherence criterion. Furthermore, inter-rater reliability is an example of the consensus criterion, and utility fits the pragmatist criterion. Thus, the different principles of quantitative measurement are all part of a “system of inquiry” (Polkinghorne, 1983) into the truth of psychological phenomena.

Considerations of reliability and validity are central to evaluating quantitative measures, but whether they can be extended to qualitative methods is still being debated. We will address these issues at the end of the next chapter, after examining the rationale behind qualitative approaches in general.

FURTHER READING

For classical psychometric theory, Nunnally and Bernstein’s (1994) text gives a thorough treatment, and Rust and Golombok (2008) cover recent approaches. Haynes, Smith, and Hunsley (2011) give a thorough treatment of the issues from a clinical assessment perspective. It is worth becoming acquainted with two classic papers in psychometric theory: Cronbach and Meehl (1955) on construct validity and Campbell and Fiske (1959) on convergent and discriminant validity. They are both difficult to read in their entirety, but dipping into the first few pages of each will provide a flavor of the reasoning. Strauss and Smith (2009) covers construct validity, examining both historical and contemporary issues.

QUESTIONS FOR REFLECTION

1. "Everything that can be expressed in words can be measured quantitatively." Do you agree or not? Why?
2. What is "measurement error"? If there's so much of it, why don't we study what it consists of?
3. How would you go about validating a new measure of one of the central constructs in your research area or in your clinical work?
4. Rasch analysis (the most basic type of item-response theory) is controversial within psychology. Why do you think this is?

5

Foundations of Qualitative Methods

KEY POINTS IN THIS CHAPTER

- Qualitative research uses language as its raw material.
- It aims to study people's thoughts, experiences, feelings, or use of language in depth and detail.
- The main advantage of qualitative methods is that they allow a rich description.
- They draw upon two main philosophical traditions, phenomenology and constructionism, although there is considerable diversity within, and overlap between, these traditions.
- Phenomenologists attempt to understand the person's perceptions and experiences.
- Constructionists focus on how language is used in social interactions, and how discourse is affected by culture, history, and social structure.
- Qualitative approaches can be grouped into four main families: thematic analysis, narrative approaches, text-based approaches, and ethnographic approaches.
- It is possible to specify criteria for evaluating qualitative research studies.

The raw material for qualitative research is ordinary language, rather than the numbers that form the raw material for quantitative research. The language may be obtained in many ways. It may be the participant's own descriptions of himself or herself, recorded during a qualitative interview. Or it could be words transcribed from a conversation, such as that between a client and a therapist during a therapy session. Or it could be something printed, such as a newspaper article or the operational policy statement of a hospital's management committee. It could also take the form of the researcher's field notes of the participants' behavior, as written down after a qualitative

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observation session. Whatever source it may come from, linguistic data can give the researcher rich, deep, and complex information, sometimes referred to as "thick description" (Geertz, 1973). These data can be used to understand people's feelings, thoughts, ways of understanding the world, or ways of communicating with others.

A simplified illustration of the difference between the quantitative and the qualitative approach is shown in the differing responses to the question "How are you feeling today?" A quantitatively oriented researcher might ask the participant to respond on a seven-point scale, ranging from 1 = "Very unhappy" to 7 = "Very happy," and receive an answer of 5, signifying "Somewhat happy." A qualitative researcher might ask the same person the same question, "How are you feeling today?", but request an open-ended answer, which could run something like "Not too bad, although my knee is hurting me a little, and I've just had an argument with my partner, which is upsetting me. On the other hand, I think I might be up for promotion at work, so I'm excited about that." In other words, the quantitative approach yields data which are relatively simple to process, but are limited in depth and hide ambiguities; the qualitative approach yields a potentially large quantity of rich, complex data which may be difficult and time-consuming to analyze.

It is worth noting in passing that there is one potential source of confusion over the meaning of the word "qualitative," as it also has a second distinct meaning in research terminology. In quantitative research, the term "qualitative data" is used to refer to nominal scale data, to distinguish it from ordinal or interval scale data (see Chapter 4 on psychometric theory). Thus, census categories measuring ethnic background (white European, black African, Asian, etc.) may be referred to as a "qualitative" variable (because it has no ordering property), even though the data are analyzed by quantitative methods. However, in this book we will reserve the term "qualitative" for data that are collected by open-ended questions or by observations that yield verbal descriptions. Simple yes-no responses or nominal categories will be considered as a form of quantitative data, since they are narrowly delimited. The qualitative–quantitative distinction, as we are using it, boils down to whether the data are collected and analyzed as words or numbers (including counts, proportions, multiple choice, and yes–no responses).

However, the difference between quantitative and qualitative approaches to research is about much more than the difference between numbers and words; it is also about epistemology, the theory of what knowledge consists of (see Chapter 2). As we noted in Chapter 4, quantitative research is largely based on the philosophy of positivism. Qualitative researchers usually reject positivism, often quite vehemently, instead preferring nonrealist naturalistic or interpretative paradigms based on developing understanding rather than on testing hypotheses (see Bryman, 1988, 2012; Lincoln, Lynham, & Guba, 2011; McGrath & Johnson, 2003; Proctor & Capaldi, 2006). Rather than claiming to study a universal objective reality, as the positivists do, qualitative researchers are more interested in examining the personal meanings and subjective interpretations of each individual's reality.

Furthermore, in qualitative research, not only are the subjective understandings of the research participants foregrounded, but so also is the subjectivity of the researcher. Qualitative research is essentially a human encounter, and the researchers themselves act as the measuring instrument. Therefore, the researchers' beliefs, understandings, and feelings about the research topic will inevitably influence the collection and interpretation of the data. Rather than attempting to eliminate this source of "bias," as the positivists would, many qualitative researchers argue that researcher subjectivity can enrich the process of the research and so should be embraced (Gough & Madill, 2012).

We will describe these epistemological positions more fully below, in the section on philosophical background.

Advantages

The main advantages of using qualitative methods are:

- They avoid the simplifications imposed by quantification, since some things cannot be easily expressed numerically. That is, they enable more complex aspects of experience to be studied and impose fewer restrictions on the data or the underlying theoretical models than quantitative approaches.
- They allow the researcher to address research questions that do not easily lend themselves to quantification, such as the nature of individual experiences of a psychological condition (e.g., eating disorders) or event (e.g., being a victim of crime).
- They enable the individual to be studied in depth and detail.
- The raw data are usually vivid and easy to grasp: good qualitative research reports make the participants come alive for the reader. In general, the reports of qualitative studies are often more readable than those of quantitative studies (except that some qualitative researchers, especially those with postmodernist or existential-phenomenological leanings, tend to write in an impenetrable jargon all of their own).
- Qualitative methods are good for hypothesis generation, and for exploratory, discovery-oriented research. They permit a more flexible approach, allowing the researcher to modify his or her protocol in mid-stream. The data collection is not constrained by pre-existing hypotheses.
- Qualitative self-report methods usually give more freedom to the participant than structured quantitative methods. For example, open-ended questions give interviewees a chance to respond in their own words and in their own way.
- They can be used to "give voice" to participants, especially those who are disadvantaged or socially excluded and whose experiences are rarely represented in psychological research.
- Since the data collection procedures are less constrained, the researchers may end up in the interesting position of finding things that they were not originally looking for or expecting.

Historical Background

Qualitative methods can be traced back to the ancient Greek historians. For example, Herodotus, who is often called the father of history, traveled widely in the ancient world and recounted in his *Histories* the stories he had heard from the people he met. His successors down the ages recorded their observations of the people that they encountered in their travels. These kinds of observations eventually became formalized in the discipline of anthropology.

In their modern form, qualitative methods were first used in ethnographic fieldwork in the early decades of the 20th century. The founders of cultural anthropology, such as Malinowski and Boas, conducted ethnographic observations on cultural groups that were remote from their own: Malinowski in the Trobriand Islands in Papua New Guinea and Boas with the Kwakiutl tribe in the Pacific Northwest Coast of North America. They spent many months living with and observing the cultures they were studying. In the 1920s and 1930s, sociologists adapted these methods to study subcultures within their own society. For example, the "Chicago school" of sociology tended to focus on people at the fringes of society, such as criminals and youth gangs. A classic example of this genre is Whyte's (1943) *Street Corner Society,* which was based on fieldwork with an Italian-American youth gang in Boston, Massachusetts. Ethnographic methods started out being used to study the "weird and wonderful" (from a Eurocentric viewpoint), for example, Pacific Island tribal cultures, and have been brought progressively more closely to bear on the investigators' own culture, culminating in such contemporary specialties as medical anthropology, which use anthropological methods to study health and illness in our own culture (Helman, 2008).

Some ethnographic work is located on the rather fuzzy boundary between social science and journalism. A good example is Blythe's (1979) *The View in Winter,* a moving account of people describing how they experience being old. The distinction is that journalism seeks to report accurately and produce an engaging story, whereas social science brings a body of theory to bear on the subject matter, or seeks to develop theory from the data, and it articulates its assumptions and procedures in order to enable replication.

In clinical research, qualitative methods were first used in case histories (see Chapter 9), for instance, Breuer and Freud's (1895/1955) first cases, which began the psychoanalytic tradition, and Watson and Rayner's (1920) study of "Little Albert," which helped establish the behavioral tradition. There is also a tradition of participant observation methods in mental health research, though they are more often conducted by sociologists than by psychologists: classic examples are Goffman's (1961) *Asylums* and Rosenhan's (1973) "Being sane in insane places" study.

The two main qualitative data collection methods currently used in clinical psychology research are in-depth interviewing (see Chapter 6) and qualitative observation (see Chapter 7). There are various different approaches to conceptualizing the procedures and the underlying philosophy of qualitative research, which we will look at in the remainder of this chapter. In Chapter 12 we will cover approaches to qualitative data analysis.

PHILOSOPHICAL BACKGROUND

Qualitative research is unfortunately not immune to the usual kinds of factions that bedevil most academic enterprises. Although qualitative researchers are united by their wish to move beyond the perceived limitations of the quantitative approach, they dispute the underlying epistemology and philosophy of science that characterizes their endeavors. Here, we will consider two of the main sets of ideas that underpin qualitative research: phenomenology and social constructionism.

Phenomenologists attempt to understand the person's thoughts, feelings, perceptions, and interpretations of the world. Social constructionists, and the postmodernists with whom they are often allied, look at language as a social product in itself, questioning many of the familiar concepts, such as reality, truth, or the person, that are taken for granted in other branches of the discipline. Although these two positions draw on distinct philosophical sources, they are not mutually exclusive: some approaches to qualitative research take ideas from both phenomenology and social constructionism.

Phenomenology

Central tenets of phenomenology:

- The primary objects of study are people's experiences and perceived meanings;
- Understanding is the true end of science;
- Multiple valid perspectives are possible;
- Individuals' perceptions of their life-worlds are based on implicit assumptions or presuppositions.

The word "phenomenology" is itself a bit of a mouthful, and much of the underlying theory is couched in off-putting jargon. However, phenomenology is simply the study of phenomena (singular: phenomenon), and "phenomenon" is simply a fancy word for perception (that is, what appears to us). In any case, the essence of phenomenology is relatively simple: it is the systematic study of people's experiences and ways of viewing the world.

Sometimes the approach is known as "phenomenological-hermeneutic," to stress its interpretive aspect. ("Hermeneutic" is a fancy word for interpretive, and can be used interchangeably with it.) However, there is a potential source of confusion here, as there is a particular brand of phenomenological research known as "Interpretative Phenomenological Analysis" (Smith, Flowers, & Larkin, 2009), which we will discuss below. (Interpretative is given here in its British spelling, because the approach originated in the UK.) Here, we will use the term "phenomenological" in its general sense, to also encompass phenomenological-hermeneutic methods.

Phenomenological methods in psychology derive from the phenomenological movement in philosophy, which developed in the late 19th and early 20th centuries. It, in turn, is descended from the rationalist, idealist philosophical tradition of Plato and Kant. Husserl was its founder; Brentano, Heidegger, Merleau-Ponty, and Sartre were key figures in its development (Jennings, 1986; Spinelli, 2005). Their ideas were introduced into psychology by Giorgi, Laing, May, and others (e.g., Giorgi, 1975; Laing, 1959; May, Angel, & Ellenberger, 1958), and were a major influence on the client-centered, humanistic, and existential approaches to psychological therapy.

Assumptions

We can distinguish four central assumptions of phenomenology. First, perception is regarded as the primary psychological activity, since our perceptions give rise to what we do, think, and feel. Because of this, *perceived meaning* is more important than objective reality, facts, or events.

Second, *understanding* is regarded as being the true end of science (in contrast, for example, to the aim of causal explanation, prediction, and control that more traditional hypothetico-deductive approaches espouse). The goal is to produce understandings of the person's experiences and actions in terms of intentions, purposes, and meanings.

A third key assumption is that of *multiple perspectives,* also known as "epistemological pluralism." Each person's perspective has its own validity (i.e., it is how they see things); therefore, multiple, differing perspectives are equally valid and of interest for study. These multiple perspectives constitute different life-worlds (in German, *Umwelten*). For example, the same aging oak tree is radically different when perceived by the forester, the lost child, the squirrel, or the wood beetle. These life-worlds are the object of study for the phenomenologist.

Fourth, individuals' perceptions of their life-worlds are based on implicit assumptions or *presuppositions*, which phenomenologists also try to understand. That is, what we perceive is built on multiple assumptions about ourselves, others, and the world. These assumptions are the taken-for-granted, unquestioned context for our actions and perceptions. For example, if an acquaintance greets you with "How are you?", you are not usually expected to give an accurate or detailed answer; in fact, to anyone but a close friend, it would seem quite odd to do so – the underlying, taken-for-granted assumption is that we respond with a brief, positive answer. Although we accept these underlying assumptions, we are not generally aware of them and do not question them; they usually only become apparent when someone breaks the unwritten rules of social interaction. In other words, they are believed to be part of what everybody knows that everybody knows, a "world common to all and taken for granted" (Garfinkel, 1967, p. 37).

A key set of underlying assumptions is known as the "natural attitude" or "mundane reason" (Pollner, 1987). This is made up of the unquestioned belief that things are what they appear to be, and that all sane persons share the same world. In fact, in everyday life it is considered strange or deviant to talk about many of these presuppositions, so that their very obviousness at the same time hides them or prevents them from being noticed.

Phenomenological researchers use two key processes – bracketing and describing – in order to identify, and reduce the influence of, their preconceptions.

Bracketing is an attempt to set aside one's assumptions and expectations, as far as is humanly possible (Fischer, 2009). However, because one's underlying assumptions are often hidden, it requires a special act of reflection to identify them. This act has been described in several different ways. The most common is "bracketing the natural attitude" (or "bracketing" for short). It involves a process of stepping back from the phenomenon in order to see it as if from the outside, as if we were the proverbial observer from Mars. Bracketing involves a special kind of turning away from the natural attitude, in which the researcher does not accept a description as a statement about the world, but simply as a statement about an experience of the world.

In the clinical context, bracketing is one aspect of the process of empathy in such exploratory, humanistic psychotherapies as person-centered and experiential therapy. When a client says that she is "trapped" in a situation, the client-centered therapist is not interested in determining whether this is factually the case; what is important is that the client feels trapped (Rogers, 1975). In contrast, beginning therapists generally prefer to stay "within the natural attitude" by trying to talk the client out of such presumed irrational beliefs, often questioning the facts of the situation. One important component of empathy is letting go of one's own presuppositions in order to understand what the client is trying to say. A similar idea is found in the ideal therapist state of "evenly hovering, free-floating attention" referred to in the psychoanalytic literature (e.g., Greenson, 1967, p. 100).

A naive approach to bracketing might be to mentally steel oneself and promise to give up one's biases. However, a more fruitful alternative is to begin by carefully reflecting on one's assumptions. At the beginning of a study, the researcher can conduct a thought experiment of carrying out the study in imagination in order to identify expectations of what it might find. This thought experiment might also be repeated at the end of the study in order to identify additional expectations that only became clear in the course of the study. These expectations take the place of hypotheses in traditional research, but they are not the same. In phenomenological research, expectations are not given a place of honor at the end of the introduction, instead, they are figuratively locked in a drawer until the study is over. Phenomenological research is perhaps most exciting when it uncovers understandings that are unexpected or even startling.

The second step in the empirical phenomenological method is *describing*. Several principles are involved (see, for example, Spinelli, 2005). First, good descriptions focus on concrete or specific impressions, as opposed to the abstract or general. Second, they avoid evaluative terms such as "good" or "bad" and their many synonyms and euphemisms (e.g., "ineffective," "helpful"), except where these are part of the experience itself. Third, they tend to avoid explanations, particularly early in the research. The task is to discover meaning, not invent it. This means that, in interview studies, interviewers avoid "why" questions or anything that encourages the informant to speculate on causes or reasons: such questions encourage intellectualization and interfere with the slow, careful process of attending to concrete experience.

Conclusion

Phenomenological methods are often congenial to clinicians, since the research aim of understanding participants' experiences has much overlap with the clinical activity of exploring a client's cognitions or feelings. The desire to understand people's inner experience is often what draws psychologists into the profession in the first place. The strength of phenomenological approaches is their generation of an in-depth understanding of individuals' experiences. However, as we discuss in the following section, some psychologists take a different stance to qualitative work, focusing on the person's discourse rather than on their inner world.

Social Constructionism

Social constructionists (constructionists, for short) are interested in how language is used to order and manage the world. In contrast to phenomenologists, constructionists do not see language as necessarily reflecting the individual's underlying thoughts and feelings; rather they are interested in how people use language to structure things, or to get things done. For example, constructionist researchers have examined psychiatric diagnostic systems from the point of view of how diagnosis may be used by mental health professionals to impose a particular view of the world on people's experience (e.g., Georgaca, 2000; Harper, 1994).

Central features of social constructionism:

- Part of the postmodernist and poststructuralist movements;
- Nonrealist;
- "Radical pluralism";
- Often focuses on language in text or speech;
- Interested in language as social action;
- Does not assume that language reflects cognition;
- Emphasizes the reflexivity (circular nature) of psychological theory.

The basis of the constructionist position is an opposition to the realist approach to social science, in particular as articulated by adherents of positivism (see Chapters 2 and 4). Social constructionists reject, or at least dispense with, the assumption of an underlying, independent reality (Gergen, 1985; Madill, Jordan, & Shirley, 2000; Willig, 2013). They may speak in terms of multiple realities – that each individual constructs their own personal reality. This rejection of realism is to some extent shared by the phenomenologists, although the constructionist position tends to be more forcefully expressed and may be more thoroughgoing: phenomenologists do not explicitly reject realism, they just accept that different people may have different concepts of what reality is.

However, as we have mentioned above, there is a diversity of views within many qualitative traditions, and social constructionism is no exception. There is a radical version of constructionism that completely rejects any notion of reality. Thus Guba and Lincoln (1989) write that their constructivist paradigm "denies the existence of an objective reality, asserting instead that realities are social constructions of the mind, and that there exist as many such constructions as there are individuals (although clearly many constructions will be shared)" (p. 43). Such radical constructionists also do not wish to "privilege" one worldview over any other. Thus they see traditional scientific methods as one possible way of understanding the world, but would not necessarily regard them as being any more valid than other systems of belief, such as shamanism or astrology. However, they would accept that scientists' own criteria for validity are meaningful within the scientists' own domain of discourse.

Social constructionists pay close attention to language, spoken and written. However, they analyze language in a different way to researchers working within realist traditions, who are usually concerned with whether statements expressed in language are true or not. Thus, if a psychiatrist says, "This patient is paranoid," a realist approach would be to see whether the statement was accurate – to ask, in other words, whether the patient is paranoid or not. In constructionism the focus shifts toward looking at how people construct their arguments and what work their constructions do: for example, what rhetorical devices does the psychiatrist use to convince us of the validity of her position, that the patient is indeed paranoid (see Harper, 1994)?

The type of focus on language also distinguishes constructionism from phenomenology. Phenomenologists and social constructionists may share the assumption that objective reality is not of primary concern. Furthermore, phenomenologists also use spoken language, in the form of qualitative interviews, as their primary medium of research. However, the phenomenologist is using language to understand the thoughts and feelings of the participants – to try to understand their inner world. For radical constructionists, this act of understanding, too, is a social construction, leaving us with only the process of construction to study, especially as this plays out in language use. As Reicher (2000) puts it "language is a form of social action which we use in order to create our social world. The focus is on how apparent descriptions serve to manage our social relations. Psychological categories such as beliefs, desires, and even experience, are only of interest in so far as participants themselves put them to use in their discourse" (pp. 3–4).

Social constructionism versus constructivism—what's the difference?

The terms "constructionism" and "constructivism" are often used interchangeably, but they are not identical. Constructionism usually refers to the view that the concepts we use – for example, madness or masculinity – are socially determined, that is, they don't refer to an independent reality but may vary across cultures or over time. Constructivism is a more psychological concept; it refers to the process by which individuals arrive at the constructs they use. One important example of constructivist thinking is Kelly's (1955) personal construct theory, which looks at the central constructs each individual uses in order to make sense of their world.

Another example of constructivism can be found in contemporary cognitive therapy. Historically, cognitive therapists have viewed the external world as less important than how clients make sense of the world. The ancient Stoic philosopher Epictetus is often quoted: "Men are not disturbed by things, but by the view which they take of them." However, as Neimeyer (1993) points out, most cognitive therapists follow in the realist tradition, in that thoughts are viewed as rational (and therefore healthy) if they correspond to reality. This realist stance is similar to that adopted by traditional psychiatry, in which the prime criterion for psychosis is loss of contact with reality. However, some contemporary cognitive-behavioral therapists have begun to move toward a constructivist position, which is, as we have noted, more internally consistent with its philosophical roots.

The other feature of many constructionist theories is that they stress the *reflexivity* (from "reflection," a function of mirrors) of psychological theorizing. By this is meant that psychologists are doing the theorizing, but the psychologists themselves, as human beings, are also the object of the theory. Furthermore, researchers' values and choices inevitably shape the course of the study. Psychological research is thus a circular process, which is what the reflexivity metaphor is attempting to capture. Some constructionists maintain that the existence of reflexivity undermines any claims of psychology to be an objective science (see Gough & Madill, 2012).

Postmodernism

Social constructionism is closely aligned with the body of thought known as *postmodernism*, or sometimes poststructuralism (the two terms overlap, but are not synonymous). This can be difficult territory. One major difficulty is that it is often hard to pin down exactly what many of the authors writing in this tradition are actually saying, as their prose style is often opaque, and the ratio of useful ideas to verbiage can seem frustratingly low. However, postmodern thought is currently fashionable and much discussed within several fields of study, so it is important to come to grips with it.

A second difficulty, however, is that the term "postmodernism" itself is hard to define. Literally, it is a contradiction in terms, since its base meaning is "what comes after modernism," where modern means current, up to date, or in fashion. However, it makes more sense when one realizes that modernism was an artistic and intellectual movement of the early 20th century, which included modern music, art, architecture, and literature, as well as positivist philosophy. When this movement began to become dated, the new thinking was labeled post-modernism. Today, postmodernism refers to a rather loose collection of ideas that have found expression in a number of different fields such as literary theory, sociology, and architecture. The key figures are all French: the literary theorist Derrida, the historian Foucault, the psychoanalyst Lacan. Some of its central themes are:

- *A rejection of grand theories* that provide overarching explanations, such as psychoanalysis or Marxism; instead micro or composite theories are favored. This is coupled with a questioning of the personal and social interests that lie behind scientific theories, particularly where those theories seem to serve the interests of those in power (Prilleltensky, 1997).
- *An intellectual playfulness*, that borrows from many different traditions within the same piece of work. For example, postmodernist architecture often quotes from earlier traditions, inserting a Gothic turret here, a Georgian window there. This is exemplified in the image of the qualitative researcher as "*bricoleur*," a sort of handy-person who uses whatever is at hand to construct things that are useful but not elegant (Levi-Strauss, 1958/1963; McLeod, 2011).
- *A focus on language*. Lyotard (1979) borrows Wittgenstein's phrase, "language games," to capture both the aspect of playfulness and the idea that language is governed by rules.
- *The indeterminacy (ambiguity) of language*. Post-modernists stress that all communication carries multiple meanings, so that understanding is always an act of interpretation, and what one reader makes of a text may differ from another reader's understanding.

Constructionism is not identical to postmodernism, and it was first articulated before postmodernism became popular (e.g., Berger & Luckmann, 1966). However, the postmodernist viewpoint has been adopted by many social constructionists (e.g., Gergen, 1994, 2001), and it provides a useful framework in which to understand their ideas.

Critiques of Postmodernism

The postmodernist position (and also the extreme versions of constructionism) have been fiercely criticized, in a debate which has generated much heat, and perhaps a little light. The main lines of argument are:

- The lack of interest in people's mental states is open to the same criticisms that were earlier leveled at the methodological behaviorists, who adopted an identical stance for completely different reasons (i.e., to become, as they saw it, more scientific). A psychology that sidesteps the role of inner experience is severely limited, and ultimately presents an alienating view of the person.
- Likewise, the nonrealist emphasis can make research into an ivory tower exercise. Reality is important, especially unpleasant reality. Studying rape, child abuse, racism, or genocide purely from a discursive viewpoint can easily seem to diminish their importance or even appear to deny their existence or the need to prevent them.
- The underlying model of the person is that of a fragmented, unintegrated self; it is not the model that many psychological therapists, who are trying to help their clients feel more whole, would endorse. Likewise, the person can be viewed as a manipulator of language, whose goal is to manage the impression they make or to get others to act in a certain way. This is not an image of human beings that can support the enterprise of helping people lead more fulfilling, meaningful, and honest lives.
- The language that postmodernists employ is often riddled with impenetrable jargon, which seems designed to convey an impression of erudition and profundity. Sokal and Bricmont (1999) exposed the ridiculousness of much postmodernist writing, especially its use of scientific metaphors, in their critique, *Intellectual Impostures.* They describe how they managed to publish, in a prestigious journal, a spoof article entitled "Transgressing the boundaries: Towards a transformative hermeneutics of quantum gravity," consisting mostly of postmodern gobbledygook.

Conclusion

In summary, the strong points of the constructionist position are that they remind us to look closely and critically at how language is used to construct reality and to accomplish practical purposes. What "position" is the speaker or writer trying to adopt, how is their language being used to bolster this position, and what is finally achieved by this? It also stresses the theory-dependent nature of scientific observation: a view that the constructionists share with Popper (see Chapter 2), who reached this position from a completely different philosophical standpoint. Finally, it stresses the social nature of psychological concepts. Instead of treating a concept such as "racism" or "mental illness" as an individual trait, constructionists urge us to look at the term in its wider social and political context.

FAMILIES OF QUALITATIVE APPROACHES

There are many different approaches to qualitative research, and it is often hard for researchers new to the area to make sense of their similarities and differences. Ultimately, the choice of which approach to adopt depends on your specific research question(s) and which philosophical tradition you identify with. For didactic purposes, we will divide qualitative approaches into four main families, according to the kinds of questions they are attempting to address. We will use Pistrang and Barker's (2012) pragmatically based categories: thematic analysis approaches, narrative approaches, language-based approaches, and ethnographic approaches.

Some researchers, however, may regard this classification as simplistic or otherwise contentious. For example, Willig (2013) depicts qualitative approaches as being arranged on a continuum, ranging from realist to relativist. As in qualitative analysis generally, there is no overall best way to organize the material, just various possible alternative constructions.

It is also worth noting that, regardless of the overall classification, there is considerable diversity within, and overlap between, the various qualitative approaches. Also, some approaches have different variants under the same label: for example, there is more than one version of grounded theory and of discourse analysis.

Thematic Analysis Approaches

The first family of approaches is the thematic analysis family. Although we have not done a formal audit, it almost certainly covers the most commonly used approaches within published clinical psychology research. The characteristic feature of these approaches is that they attempt to extract the main themes or concepts that run through the body of a data set (which usually consists of qualitative interview transcripts). This process can be thought of as a qualitative analog of the quantitative approaches of factor analysis or cluster analysis.

Content Analysis

Content analysis (Joffe & Yardley, 2004; Krippendorf, 2013) sits on the boundary between quantitative and qualitative methods. It is included here to give an indication of the range of possible thematic analysis approaches, rather than to specifically illustrate the qualitative approach. Content analysis aims to give the frequencies of the important content categories in the data set. It is qualitative in that the raw data for the study is qualitative, but it is quantitative in that the output is a frequency count of the various themes or codes. For example, the transcript of a therapy session could be content-analyzed for different types of client emotional expression, such as anger, sadness, anxiety, and so forth. The output would be something like 15 instances of anger, 7 of sadness, and 3 of anxiety. Content categories can either be defined before the research starts (as would probably be the case in this example) or they can be derived post hoc from the data set. Content analysis can also be conducted in an automated way. For example, there is computer software to analyze the emotional content of therapy sessions; this implements the Gottschalk and Gleser (1969)content analysis of therapeutic interaction, yielding categories such as Anxiety, Hostility

Outward, and Hostility Inward (see http://www.gb-software.com/develop.htm). Another automated method is Pennebaker's LIWC (Linguistic Inquiry and Word Count) computer program (www.liwc.net), which analyzes emotion words and also some grammatical properties of the text, such as pronoun usage.

Framework Approach

Framework (Ritchie & Spencer, 1994; Ritchie, Lewis, Nicholls, & Ormston, 2014) is a structured approach to thematic analysis. It has some similarities to content analysis, but it belongs more firmly in the qualitative camp. Its central feature is that the researcher develops a detailed coding framework. This is usually done a priori, based on theory, previous research, or the questions asked in the interview protocol, but it can be developed inductively, based on the data collected. The analytic process is illustrated by charts, which show the instances of each code for each participant, thereby making the whole process more transparent (although the final step – synthesizing the codes – may require interpretation). Framework's origins are in social policy analysis, but it is particularly popular in medical contexts, since it was featured in Pope, Ziebland, and Mays's (2000) influential *British Medical Journal* paper on 'analyzing qualitative data'.

Grounded Theory

Grounded theory is one of the oldest and more widely used qualitative research methods. It was developed by two North American medical sociologists, Glaser and Strauss, in their 1967 book, *The discovery of grounded theory: Strategies for qualitative research*. As the title suggests, they were attempting to articulate how qualitative data could be used not just to provide rich descriptions, but also to generate theory. Originally used for participant observation research (see Chapter 7), this approach has come to be used with a range of qualitative material, such as semi-structured interviews, focus groups, and diaries.

The term "grounded theory" is potentially confusing, as it refers both to a method – a set of systematic procedures for analyzing data – and also to the outcome or product of the analysis, which is theory "grounded" in the data. The basic process involves identifying categories at a low level of abstraction and then building up to more abstract theoretical concepts. The end point is often one or more core categories, which capture the essence of the phenomenon (see Chapter 12). This process of analysis occurs concurrently with the process of data collection, and the developing theory guides the sampling strategy ("theoretical sampling": see Chapter 10).

The original Glaser and Strauss (1967) volume was more theoretical and polemical than practical; it was aimed at challenging the prevailing quantitative paradigm in American sociology. The practical implications for researchers, in other words, the steps to be taken in actually carrying out a grounded theory study, are developed in Glaser (1998) and Corbin and Strauss (2015). (It is worth noting that Glaser and Strauss subsequently disagreed about how grounded theory should be done, and their texts have different emphases – see Willig, 2013.)

The grounded theory approach was taken up by psychologists in the 1980s and 1990s. Articles by Rennie, Phillips, and Quartaro (1988) and by Henwood and Pidgeon (1992) were aimed at introducing it to an audience of psychologists.

Rennie and Brewer's (1987) study entitled "A grounded theory of thesis blocking" (i.e., writer's block among research students) may well be of personal interest to some readers of this text. As more psychologists have taken up the invitation of Rennie et al., and of Henwood and Pidgeon, grounded theory has become a popular approach to qualitative research.

One example of the method in clinical psychology is Bolger's (1999) study of the phenomenon of emotional pain. The participants were women in a therapy group for adult children of alcoholics; they were interviewed on several occasions following group therapy sessions in which they had explored painful life experiences. The interviews focused on how emotional pain was experienced and what was significant in that experience for them. The core category that emerged from the analysis was labeled the "broken self," characterized by the four subcategories of woundedness, disconnection, loss of self, and awareness of self.

Another, well-known, example of a grounded theory study, in a more popularized book format, is Charmaz's (1991) analysis of the experience of living with chronic illness (see box).

Grounded theory example: Charmaz (1991)

The sociologist Kathy Charmaz conducted in-depth qualitative interviews with people who had a chronic illness. The results, written up in her book *Good Days, Bad Days* (1991), give compelling accounts of the impact of chronic illness on people's lives. In accordance with the grounded theory approach, she also used the data to construct a theory of how the person's experience of time changes, and how this impacts on their sense of self.

Consensual Qualitative Research

Consensual Qualitative Research (Hill, 2011; Hill, Thompson, & Williams, 1997) is a systematic approach drawing on grounded theory and phenomenology and is currently widely used in North America. It features the use of multiple qualitative analysts and auditors, as well a system for interpreting the nature of qualitative themes based on their frequency in the data; for example, themes reported by all or almost all informants are defined as "general themes" that may define the experience being studied, while "typical themes" (reported by at least half of the informants) are useful for constructing a narrative of a typical experience.

Empirical Phenomenology

Empirical phenomenology is another early form of thematic analysis and the oldest systematic qualitative research method to emerge in psychology. It is an application of the phenomenological method described earlier in this chapter and was developed at Duquesne University (Pittsburgh, USA) in the 1970s (Giorgi & Giorgi, 2003). Much of the work has been published in the *Journal of Phenomenological Psychology* and the *Duquesne Studies of Phenomenological Psychology*. Giorgi, Wertz, and Fischer are three of the better known proponents. The approach stresses in-depth analysis, often at first of single cases, aiming to describe the main defining features of an experience (e.g., that of being criminally victimized), and the different variations that the experience may have in the population (analogous to the statistical ideas of mean and standard deviation).

Hermeneutic Approaches

A somewhat more flexible approach to qualitative research is represented by approaches that bill themselves as hermeneutic or phenomenological-hermeneutic (e.g., Packer & Addison, 1989). Researchers who describe themselves in this way find other phenomenological approaches too restrictive and use a wider range of methods. They argue that it is important to go beyond the surface meaning of research protocols, in order to identify the implicit or even unconscious meanings embedded in texts.

An example is a study by Walsh, Perrucci, and Severns (1999), which used a hermeneutic approach to explore "good moments" within a videotaped psychotherapy session. They identified the differing values of professionals and students at various stages of training about what constituted good psychotherapy.

Interpretative Phenomenological Analysis

Interpretative Phenomenological Analysis (IPA: Smith et al., 2009) is, as its name suggests, an explicitly phenomenological approach to thematic analysis. Many of the other thematic analysis approaches can also be used from a phenomenological standpoint, but it is hard-wired into the IPA method. IPA often appeals to newcomers to qualitative research, because it is an accessible, systematic, and practical approach to collecting and analyzing phenomenological data. It articulates the steps involved in conducting an investigation, for example, how to generate meaningful lower order and higher order categories from the data. Smith et al. (2009) set out the basis of the method, illustrating its steps using examples of data drawn from clinical and health psychology.

Generic Thematic Analysis

Finally, there are some generic approaches to thematic analysis that do not have a "brand name" attached to them, for example those described by Boyatzis (1998) and by Braun and Clarke (2006, 2013). Braun and Clarke's (2006) paper is a particularly clear exposition of the thematic analysis method, which the authors intend as an "accessible and theoretically flexible approach" (p. 77). It is capable of use within a number of epistemological stances, including the phenomenological and social constructionist ones.

Narrative Approaches

Many writers have proposed that narrative is fundamental to human communication and experience (e.g., Bruner, 1991, McLeod, 1997; Murray, 2003; Polkinghorne, 1988; Sarbin, 1986). Indeed, the first sentence of Chapter 1 of this book starts us off on a narrative journey. Stories surround us, and it is therefore important that research methods be developed to help make sense of them.

Narrative Analysis

There are a number of different versions of narrative analysis, both qualitative and quantitative (Avdi & Georgaca, 2007; Gonçalves & Stiles, 2011; Riessman, 2008). Their common feature is that they take stories as the raw data for the research, and set out to analyze the properties of stories in the context in which they occur. Various kinds of culturally defined narratives may be of interest, such as narratives of illness and recovery, victimization, identity, and faith journey.

Several different formulations of narrative structure have been proposed (for a review, see McLeod, 1997), but the most basic consists of three elements. First, there is a beginning, in which the setting is described (e.g., "When I was 18, still living at home …"), the main character is introduced ("I had a friend Angel who I used to visit …"), and a situation or problem is introduced ("… and Angel got cancer"). Second, a series of actions, obstacles, conflicts, reactions, and attempted solutions is described, often leading to a climax or turning point. Third, there is an ending or resolution to the story, often with some attempt to state the point or the person's current perspective ("Anyway, I still think about her; 29 is too young to die!").

A good example of a qualitative approach to narrative analysis is Humphreys's (2000) analysis of the stories told in Alcoholics Anonymous (AA) group meetings. AA meetings are peer-led mutual support group meetings in which members' narratives play a central role. Humphreys used qualitative observation of the meetings (see Chapter 7) in order to identify the prototypical stories. He arrived at a set of five types – which he labeled drunk-a-logs, serial stories, apologues, legends, and humorous stories – identifying the characteristic properties of each and their function in the context of the AA meetings.

Life History Research

One important type of story, possibly the most important type of story, is the story we tell about our own, or other people's, lives: the biography, autobiography, or life history. Curiously, psychology has barely taken on board the study of whole lives. Almost all psychological research looks at small slices of behavior, and rarely places them into the context of the whole person and how they have lived their life over time. (One major exception to this is the extended case history, especially those written by the masters of the genre, starting with Freud's early work at the beginning of the last century.)

Formal life history research aims to build an understanding of individual lives over time. One moving example is Bogdan and Taylor's (1976) study of an articulate "mentally retarded" man ("person with intellectual disabilities" in the UK), "Ed Murphy", who describes his life in state institutions. This account both demonstrates the trials of living as a marginalized, institutionalized person, and also documents the existence of perceptive awareness in people whose voices are rarely listened to.

Language-Based Approaches

Language-based approaches focus on text (a general term used to encompass all kinds of verbal communication, such as interviews, letters, diaries, official documents). They tend to draw on social constructionist ideas – they are more interested in the character of the language usage and what it accomplishes, and tend to be less interested in the inner world of the person who produced the text. The most popular approach with psychologists is discourse analysis; other approaches include conversation analysis, critical and feminist approaches, and deconstructionism.

Discourse Analysis

There are many kinds of discourse analysis (Potter, 2012; Wetherell, Taylor, & Yates, 2001); it is an interdisciplinary field spanning psychology, sociology, communication science, linguistics, and literature. Within psychology, the most popular approach is that articulated by Potter and Wetherell (1987). In general, discourse analysis involves rigorously examining texts in order to analyze the repertoires of discourse that a speaker is drawing upon, and the kinds of "subject positions" that the speaker is adopting (see box for an example).

> Example of discourse analysis:
>
> Madill and Barkham (1997) examined the transcripts of a single case of time-limited psychodynamic therapy. They showed how, during the course of the therapy, the client took on three different subject positions—which they labeled as the dutiful daughter, the bad mother, and the damaged child—and the discourses that she drew upon which exemplify each of these. For instance, they argued that the dutiful daughter position "draws on 18th and 19th century discourses of female subjectivity. During this period, subject positions were provided for women based primarily upon their domesticity…" (p. 242). Thus they were able to analyze the client's talk within the context of its historical and social antecedents.

So-called critical approaches (Hepburn, 2003) often use forms of discourse analysis to examine how language perpetuates power differentials, for example in terms of social class, race and ethnicity, gender, or sexual orientation. These approaches include some branches of feminist research, neo-Marxist approaches, and emancipatory approaches, such as Freirian and Foucauldian research (Hepburn, 2003; Lather, 1991; Sprague, 2005). For example, critical feminist approaches to alcohol use have looked at the gendered nature of discourses about female versus male alcohol consumption (e.g., Lyons & Willott, 2008).

Conversation Analysis

Although sometimes grouped with discourse analysis, conversation analysis has its own tradition and its own particular methods. An outgrowth of the work of sociologists Garfinkel and Goffman, it was developed by Harvey Sacks (1995) as a rigorous method for identifying the common conversational sequences and strategies used by people to carry out everyday tasks. Conversation analysis attempts to study how speakers perceive each other's utterances, based on how they respond to each other. Although it attempts to develop general models of the strategies people use to accomplish practical work in conversation, it emphasizes the ad hoc, contextually embedded nature of "talk-in-interaction" (Schegloff, 1999). Over the past 40 years, conversation analysis has built up a large repertoire of provisional understandings of everyday and professional speech, including many investigations of psychotherapy (Peräkylä, Antaki, Vehviläinen, & Leudar, 2008; ten Have, 2007).

One interesting clinical application of conversation analysis is McCabe, Heath, Burns, and Priebe's (2002) study of the interaction between psychiatrists and patients with psychosis. They found that although the patients made active attempts to engage their psychiatrists in discussion about the content of their beliefs, the psychiatrists seemed uncomfortable and tended to disengage or otherwise avoid the topic.

Deconstructionism

Finally, deconstructionist researchers engage in self-critique, embracing a postmodern view of the research process. They see the major task of researchers as being "deconstruction" of the cultural, social, or epistemological assumptions of their work and that of others. They embrace radical pluralism, and attempt to speak or give air to multiple voices while eschewing any attempt to bring these voices together into a single message. In essence, they attempt to mirror fragmented, postmodern, multicultural society in their research. For example, a deconstructionist researcher such as Lather (1991) might present her findings as a kind of research collage.

Perhaps most importantly, deconstruction is an essential component of the process of evaluating research, in which one attempts to identify the implicit assumptions that drive a research study. Slife and Williams (1995) provide an excellent introduction to this approach. In our view, deconstructionism is less useful as a primary research method than as a method for reflecting on and critiquing research. This issue will be taken up in the last section of this chapter.

Ethnographic Approaches

As discussed at the beginning of this chapter, ethnography was the earliest form of systematic qualitative research. Initially developed within anthropology, and later within sociology, its principal applications continue to be within those fields. However, some psychologists have conducted ethnographic work, and it is an approach that has potential for use within the field (Suzuki, Ahulwalia, Mattis, & Quizon, 2005), although it is not an easy one to learn or carry out – ethnography is an art that requires many dedicated hours of the researcher's time.

The essence of ethnography is to spend time in the field setting, conducting participant observation (see Chapter 7) and talking with the people there. As soon as possible after each session, the researcher writes extensive field notes. From the mass of field notes accumulated in the study, the underlying social rules and norms that govern interaction in that culture are distilled (Emerson, 2001).

A recent variant is focused ethnography, also known as applied ethnography (Knoblauch 2005; Savage, 2006; Simonds, Camic, & Causey, 2012). This is a smaller scale enterprise than general ethnography, which makes it more amenable for clinical psychologists to use. It sets out to answer a specific research question, say about the functioning of a particular service, and observations and interviews are focused around that question. For example, Alcock, Camic, Barker, Haridi, & Raven (2011) used this method to examine the impact of a community-based intergenerational project, that is, a project that involved bringing together older people and younger people on a deprived London housing project. The ethnography documented the outcomes that occurred in both groups of participants, such as a reduction in age group stereotyping and an enhanced sense of community.

WAYS OF EVALUATING QUALITATIVE STUDIES

As must be obvious from the above discussion, the traditional psychometric criteria of reliability and validity do not easily carry over to qualitative approaches. The concepts of face and content validity can be used without much stretching, and a case can be argued for adapting some of the other concepts. However, it appears that a more fruitful approach is to articulate specific criteria for evaluating qualitative studies. Several scholars have attempted to do this (e.g., Elliott, Fischer, & Rennie, 1999; Mays & Pope, 2000; Morrow, 2005; Stiles, 1993, 1999; Yardley, 2000), although some doubts have also been expressed about the usefulness of specifying criteria for qualitative research (Barbour, 2001; Reicher, 2000).

We are partial to the Elliott et al. (1999) version, not only because Elliott is a co-author of this text, but also because their guidelines, although having a broad applicability, were mainly developed and published within a clinical psychology context. Elliott et al. were attempting to help journal reviewers and editors evaluate qualitative studies that have been submitted for publication, but their framework is relevant to any readers of qualitative studies, as well as to researchers themselves. They describe some common guidelines shared by both quantitative and qualitative approaches, for example respect for participants and use of appropriate methods, and then guidelines specific to qualitative approaches (see Table 5.1). They describe each one and then give examples of good and bad practice under each. In summary, their guidelines for qualitative studies are:

Owning one's perspective. The authors describe their theoretical orientations and biases, in order to help readers evaluate the researchers' interpretation of the data. For example, they would state if they were coming to the research from a psychoanalytic, or from a feminist, perspective.

Situating the sample. The authors describe the research participants so that readers can judge how widely the findings might apply.

Grounding in examples. The authors provide enough examples of their raw data to illustrate the analytic procedures used and to allow the reader to evaluate their findings. They also stay close to the data; any speculations that exceed the data are clearly labeled as such.

Providing credibility checks. The researchers use methods for checking the credibility of the results, for example, *analytic auditing* (e.g., using multiple researchers or an additional person who checks the results against the data), *triangulation* (examining the phenomenon from multiple, varied perspectives) and *testimonial validity* (checking the results with the original informants or similar others).

Table 5.1 Summary of Elliott et al.'s (1999) evolving guidelines

A.	*Publishability guidelines shared by both qualitative and quantitative approaches*
1.	Explicit scientific context and purpose
2.	Appropriate methods
3.	Respect for participants
4.	Specification of methods
5.	Appropriate discussion
6.	Clarity of presentation
7.	Contribution to knowledge
B.	*Publishability guidelines especially pertinent to qualitative research*
1.	Owning one's perspective
2.	Situating the sample
3.	Grounding in examples
4.	Providing credibility checks
5.	Coherence
6.	Accomplishing general versus specific research tasks
7.	Resonating with readers

Note. This table has been reproduced with permission from the *British Journal of Clinical Psychology* © The British Psychological Society.

Coherence. The interpretation of the data is coherent and integrated, but at the same time it does not oversimplify the data.

Accomplishing general versus specific research tasks. If the research aims to achieve a general understanding, then the appropriate range of people or situations is sampled. If it aims to achieve a specific understanding of a particular case, that case is described thoroughly enough for the reader to gain a full understanding.

Resonating with the reader. From the point of view of the reader, the results are not only believable but seem to capture or make sense of the phenomenon, enabling the reader to understand the phenomenon more fully.

CONCLUSION: CHOOSING AND COMBINING METHODS

Qualitative methods have now become much more fully accepted within psychology, and the heat seems to be going out of the old polarized quantitative versus qualitative debate. Researchers, and research methodologists, are now focusing their attention on when best to use either a quantitative or a qualitative approach, what is the appropriate qualitative method for any given research question, and how best to appraise qualitative studies. However, given that we have now described the fundamentals of both quantitative and qualitative approaches, it is worth briefly considering how researchers might decide between them, and how they might be combined in a study or research program.

We espouse the notion of *methodological pluralism*: that different research methods are appropriate for different types of research question (see Chapter 3). For example, qualitative methods are good for descriptive questions within a discovery-oriented framework, for instance, when you are trying to learn about a phenomenon that has not been previously researched. Quantitative methods are good for delimited questions of covariation and comparison, for instance, looking for relationships between variables and for investigating causality.

On the other hand, all methods have weaknesses or limitations, so, if possible, it is better to use multiple methods of measuring important variables, an approach known as *triangulation* (Creswell, 2009). In other words, it is unwise to rely solely on one perspective, source or approach (Campbell & Fiske, 1959; Cronbach & Meehl, 1955; Patton, 2002), because all of these have their limitations. For example, in psychotherapy outcome research it is useful to assess client change from the perspective of the client, the therapist, and a clinical interviewer. Moreover, a qualitative study focusing on how change occurred would complement the quantitative data. Several recent papers have called for randomized controlled trials (RCTs: see Chapter 8) to be augmented by qualitative studies using a subsample of the same RCT participants (e.g., Hill, Chui, & Baumann, 2013; Lewin, Glenton, & Oxman, 2009; Midgley, Ansaldo, & Target, 2014).

Clinical psychology may be gradually entering a more pluralist phase for pragmatic reasons. A variety of publications have urged psychologists to adopt a qualitative approach to research (e.g., Camic, Rhodes, & Yardley, 2003; Smith, 2008; Willig, 2013). However, the acid test – whether qualitative studies get published in prestigious journals – still reveals a strong quantitative bias in the field (Rennie, 2012). There appears to be a residual attitude that qualitative methods are second class: the saying of Rutherford, the eminent physicist, that "qualitative is bad quantitative" (quoted in Stewart, 1989: 219) expresses this viewpoint succinctly. However, one sign that a pluralist attitude may be taking root is the interest among the newer generation of researchers. Qualitative methods seem to appeal particularly to graduate students in clinical and counseling psychology, because they allow much closer contact with clinical phenomena. In the institutions we are familiar with, an increasing number of dissertations and theses now employ qualitative methods, perhaps so much so that there is a danger in some places that traditional quantitative skills are no longer being acquired. We believe that clinical psychologists should be competent in both quantitative and qualitative methods.

There has been a recent upsurge of interest in pluralistic and mixed-method research, particularly research that combines both quantitative and qualitative methods (see, e.g., Barker & Pistrang, 2005; Creswell, 2009; Tashakkori & Teddlie, 2009). Quantitative and qualitative approaches can often complement each other, in a single study or within a larger program of research. The different components can occur in various ways, such as:

1. Beginning research in a new area with qualitative studies, either pilot research or more elaborate qualitative investigations.
2. Building quantitative studies on earlier qualitative research.
3. Using qualitative methods such as interviews and focus groups to develop quantitative measures.
4. Using qualitative data to elucidate or explore quantitative findings, either as an adjunct to a primarily quantitative study or as a follow-up investigation.

5. Using quantitative data to elucidate qualitative findings, that is, the reverse of point 4, often found in sociology articles.
6. Developing mixed-methods approaches that combine both kinds of data in a complementary fashion in the same study (e.g., case studies by Elliott et al., 2009; Parry, Shapiro, & Firth, 1986).
7. Carrying out separate qualitative and quantitative studies of the same participants, either to address different questions, or to address the same question from different angles (e.g., Klein & Elliott, 2006; Madill & Barkham, 1997; Patton, 2002).

As we hope to have made clear, choosing the approach, or combining approaches, depends largely on the question you are trying to answer. The next two chapters examine practical issues in selecting and constructing measures, covering the two major approaches to psychological measurement: self-report and observation, looking at each from both qualitative and quantitative points of view.

We hope that this chapter has given readers a taste of the range of available qualitative methods, and an understanding of their underlying philosophies, particularly the distinction between phenomenological and constructionist methods. This chapter has been mostly theoretically oriented; Chapter 12 will look in more detail at practical issues in analyzing qualitative data.

CHAPTER SUMMARY

Qualitative approaches use language as their raw material, in order to examine the participants' thoughts, feelings, behavior, or linguistic strategies. Their main advantage is that they allow a rich description of the phenomena in depth and detail, sometimes called "thick description" (Geertz, 1973). There are two broad philosophical traditions underlying qualitative research: phenomenology and constructionism. Phenomenologists attempt to understand the person's perceptions and experiences, their inner world, whereas constructionists focus on how language is used in social interactions. The main approaches to qualitative research can be grouped into four families: thematic analysis, narrative analysis, language-based approaches, and ethnography. The criteria of reliability and validity do not translate easily to qualitative research, but it is nevertheless possible to specify criteria for how qualitative research studies can be evaluated. At the same time, there appears to be increasing scope for combining qualitative and quantitative methods, both across and within studies.

FURTHER READING

Many treatments of qualitative methods have recently been published. Camic et al. (2003), Smith (2008), and Willig (2013) give accessible accounts of the theory and practice of the commonly used approaches. McLeod (2011) has a clear and thorough account of the application of qualitative methods to psychological therapy and counseling research. Pistrang and Barker (2012) explain how to choose a particular qualitative approach, using a running example to illustrate when each is suitable.

Braun and Clarke (2005) have a good description of general thematic analysis, possibly the mostly commonly used approach. Bruner (1991) on narrative is erudite and thought-provoking.

Some of the old stalwarts, such as Lincoln and Guba (1985) and Patton (2002), still hold up, and Taylor and Bogdan's (1998) sociologically oriented text includes some illustrative studies, although their current edition omits the single-case account of "Ed Murphy" (originally published in Bogdan & Taylor, 1976), discussed above. For more extensive but accessible treatments of the quantitative versus qualitative debate, see Bryman (1988), Polkinghorne (1983), and Proctor and Capaldi (2006). Since many qualitative approaches have their roots in literary theory, it is also worth reading about them in that context. Eagleton (2008) gives an excellent exposition and critique of, among other things, phenomenology, hermeneutics, and poststructuralism as applied to the analysis of literary texts.

QUESTIONS FOR REFLECTION

1. Which philosophical position do you prefer: phenomenology or social constructionism? Neither? Why?
2. Do you think it is ever possible to set aside one's assumptions? Is this what "bracketing" refers to?
3. There is a huge variety of labels for different types of qualitative research. How similar or different do you think the various approaches are?
4. It could be argued that guidelines for qualitative research are features of good research practice in general. What do you think?

6

Self-Report Methods

KEY POINTS IN THIS CHAPTER

- Self-report methods, such as interviews and questionnaires, ask the person for information directly.
- Their advantage is that they give you the person's own perspective; their disadvantage is that there are potential validity problems (e.g., people may deceive themselves or others).
- The main qualitative self-report approach is the semi-structured interview.
- Qualitative interviewing is a distinct skill, related to but different from clinical interviewing.
- The main quantitative self-report approach is the written questionnaire, but structured interviews and internet surveys are also used.
- There are several principles to follow in constructing quantitative self-report instruments.
- Response sets, such as acquiescence and social desirability, refer to tendencies to respond to items independently of their content. They need to be taken into account when designing and interpreting self-report measures.

When you want to know something about a person, the most natural thing is to ask. Research methods that take the approach of asking the person directly are known as *self-report methods*, and mainly take the form of interviews, questionnaires, and rating scales. They are the most commonly used type of measure in the social sciences in general and in clinical psychology in particular.

Research Methods in Clinical Psychology: An Introduction for Students and Practitioners,
Third Edition. Chris Barker, Nancy Pistrang, and Robert Elliott.

Companion Website: www.wiley.com/go/barker

For example, suppose that you have set up a new counseling service for adolescents and want to evaluate its effectiveness. You ask the service users to rate the severity of their problems before and after counseling and how satisfied they are, using standardized instruments. You also use a semi-structured interview to assess the adolescents' overall experience of the service, including what they feel they has changed, what they found helpful, and any specific criticisms they had of it. *Client satisfaction studies* and *clinical effectiveness studies* like this have become important, with the increased emphasis on accountability to service users.

Instead of asking the person directly, you may instead, or in addition, ask someone who knows the person, such as a friend, family member, or therapist (Connelly & Ones, 2010). This is often called using an *informant* (a term which has unfortunate connotations of sneakiness). It allows you to get the views of someone who knows the person well and who has greater opportunity than you to observe him or her in a natural setting. It is also useful when the respondent cannot give you reliable information. For example, in research with children, it is often useful to have the parents' and the teacher's views of the relevant behavior. This is why, as we discussed in the previous chapter, a more accurate term would be "verbal-report" rather than "self-report." However, the term "self-report" is commonly used to cover reports from both the person of interest and from other respondents, and we will retain that usage here.

Advantages and Disadvantages

The great advantage of self-report is that it gives you the respondents' own views directly. It gives access to phenomenological data, that is, respondents' perceptions of themselves and their world, which are unobtainable in any other way. As Kvale and Brinkman (2009) put it, "If you want to know how people understand their world and their lives, why not talk with them?" (p. xvii). Furthermore, self-report methods can be used to obtain information in situations where observational data are not normally available, for instance, for studying life histories, or behavior during a major disaster.

The main disadvantage of self-report is that there are a number of potential validity problems associated with it. The data are personal and idiosyncratic and thus may bear little relationship to "reality," as seen by you or others. Moreover, people are not always truthful. They may deceive themselves, such as when an alcoholic cannot admit his dependency to himself, or they may deceive the researcher, such as when a young offender does not want to reveal his socially undesirable thoughts or behavior. Furthermore, research participants may not be able to provide the level of detail, or use the concepts, that the researcher is interested in. This is especially the case when people have limited expressive language, for instance, young children or people with neurological conditions such as dementia or aphasia.

Arguments arising in social psychology, psychoanalysis, and cognitive psychology cast doubt upon the validity of self-reports. From the social psychological perspective of attribution theory, Nisbett and his colleagues (e.g., Nisbett & Ross, 1980; Nisbett & Wilson, 1977) have argued that people often do not know what influences their behavior, and that there are pervasive biases in the way that we account for our own and others' behavior. One common source of bias, known as the *actor-observer effect*, is the tendency for people to say that their own behavior is caused by situational factors and that other people's behavior is caused by dispositional factors (Fiske & Taylor, 2013; Jones & Nisbett, 1971). For example, a student might say that she failed an exam because she slept badly the night before, whereas she might say that her room-mate failed the exam because she was too lazy to study for it. Another related type of bias, known as self-serving bias, is the tendency to take credit for success and deny responsibility for failure (Fiske & Taylor, 2013).

Psychoanalysts similarly emphasize the limits to the person's conscious self-knowledge. They argue that many important feelings and experiences are unconscious, and prevented by defenses such as repression or denial from becoming conscious. Thus, a person's accounts cannot be taken at face value. Some psychoanalytically oriented researchers prefer projective measures, principally the Thematic Apperception Test (TAT), the Rorschach inkblot test, and sentence completion methods, which are designed to assess the person's unconscious thoughts and feelings, although the validity of these measures can also be hard to establish (Meyer, 2004; Westen, Feit, & Zittel,, 1999).

Cognitive psychologists focus on the complex mental processes involved in responding to self-report questions, particularly quantitative ones. There is general agreement that responding involves four separate cognitive components: understanding the question, recalling information from memory, integrating the information, and formulating a response (Tourangeau & Bradburn, 2010). Each of these steps is prone to error or bias. These problems particularly apply to *retrospective self-report* questions, where the respondent is required to recall past behaviors, thoughts, or feelings (e.g., "How many alcoholic drinks did you consume last week?"); such recall is prone to various limitations of memory retrieval (Piasecki, Hufford, Solhan, & Trull, 2007).

These strictures about the limits of self-report methods are important to bear in mind. However, this does not mean that all self-report data are invalid, only that they cannot be trusted in all cases (Ericsson & Simon, 1993). All measurement methods have their drawbacks, and the potential limitations of the data must be considered at the analysis and interpretation stage. Thus, we should not abandon this method of data collection, although it is often advisable to supplement self-report data with observational data (or at least self-report data from other perspectives). In addition, it is a good idea to be sensitive to the possibilities for self-deception in verbal protocols (see Churchill, 2000, for an example of "seeing through" self-deceptive self-reports).

Constructing an interview or questionnaire may appear to be straightforward, but the apparent simplicity is deceptive. Most people have been on the receiving end of an irritating, poorly designed questionnaire or interview, often in the context of market research. Designing good self-report measures is an art and a craft. For this reason, it is preferable, where possible, to use well-designed established measures rather than attempting to design your own from scratch. There is a huge literature on research interviews and questionnaires, including many books (e.g., Bradburn, Sudman, & Wansink, 2004; Dillman, Smyth, & Christian, 2009; Josselson, 2013; Kvale & Brinkman, 2009).

Terminology

An *interview* is a special type of conversation aimed at gathering information, although the interviewer usually has a written guide, known as an *interview protocol* or *interview schedule*. (Note that the interview protocol is not the same thing as the research protocol, which refers to the plan for the study as a whole, including, for example, the research design and the sampling procedure.) Interviews are usually conducted face to face, although they may be done over the telephone or over the internet.

A *questionnaire,* on the other hand, refers to a structured series of written questions, which usually generate written responses. *Checklists* and *inventories* (the terms are used almost interchangeably) are a type of questionnaire that presents a list of items in a similar format and asks respondents to rate all that apply to them. Two widely used examples of inventories are the Generalized Anxiety Disorder scale (GAD-7: Kroenke., Spitzer, Williams, Monahan, & Löwe, 2007)—a 7-item scale assessing anxiety—and the CORE Outcome Measure (CORE-OM: Evans et al., 2002), a 34-item inventory measuring the number and frequency of psychological symptoms. Questionnaires may be composed of several *subscales,* each of which measures an internally consistent construct (such as the Well-being, Problems, Functioning, and Risk subscales of the CORE-OM), although the subscales may often overlap with each other.

The term *survey* is widely used but imprecisely defined. It usually denotes a systematic study of a medium to large sample done either by interview or by postal ("mail-out") or internet questionnaire. A *census* means a survey of the whole population (as opposed to a sample from that population: see Chapter 10); the best known example is the government population census.

Mode of Administration

Since self-report data may be gathered either by written questionnaires or by interview, researchers need to consider which mode of administration would better suit their purposes. The advantages of written questionnaires are that:

- they are standardized (i.e., the wording is exactly the same each time);
- they allow respondents to fill them out privately, in their own time;
- they can be used to ensure confidentiality, via a code numbering system, and so they can potentially cover embarrassing, socially undesirable, or illegal topics (e.g., sexual behavior or drug use); and
- they are cheaper to administer.

The advantages of interviews are that they can use the rapport and flexibility of the relationship between the interviewer and the respondent to enable the interviewer to:

- ask follow-up questions, in order to clarify the respondent's meaning, probe for material that the respondent does not mention spontaneously, and get beyond superficial responses;
- ensure that the respondent answers all the questions;
- give more complicated instructions and check that they are understood;
- vary the order of the questions;
- allow the respondents to ask their own questions of the interviewer; and
- allow researchers to gather enough information to make judgments about the validity of the respondent's self-report.

Interviews are additionally appealing to clinical psychologists because their clinical skills can be used. However, clinicians also have some unlearning to do, as conducting a research interview is quite different from conducting a therapeutic or assessment interview (we will elaborate on this point later in this chapter).

Open-ended and Closed-ended Questions

Self-report methods can yield either qualitative or quantitative data, depending largely on whether open-ended or closed-ended questions are used.

Open-ended questions are those that do not restrict the answer, which is usually recorded verbatim. For example, the question "How are you feeling right now?" might yield the responses "Fine, thanks," "Like death warmed up" or "Better than yesterday, at least." However, content analysis may be used at a later stage to classify the responses (e.g., into positive, negative, or neutral). Also, some open-ended questions may yield quantitative data (e.g., "How old are you?").

The advantages of open-ended questions are that they enable the researcher to study complex experiences: respondents are able to qualify or explain their answers and also have the opportunity to express ambivalent or contradictory feelings. Furthermore, their initial responses are potentially less influenced by the researcher's framework. Respondents are free to answer as they wish, using their own spontaneous language.

The main disadvantage of open-ended questions, from the researcher's point of view, is that it is more difficult to evaluate the reliability and validity of verbal data. It is hard to ascertain the extent of such potential problems as interviewer bias and variability, and respondent deception, exaggeration, fabrication, and forgetting. It is not that the reliability and validity of qualitative self-report measures are inherently worse, they are just harder to evaluate, so that both the researchers and the readers are more likely to feel on shaky ground. (On the other hand, careful examination of the respondent's manner and word choice can provide important hints about the credibility of verbal data.)

A second issue is that open-ended questions typically generate large amounts of data (the "data overload" problem; Miles,Huberman, & Saldaña, 2013), which are usually time-consuming to analyze. For a start, most qualitative interviews need to be transcribed, which often takes considerable effort (this is where having sufficient funding to pay for transcription can save the researcher time and frustration). Furthermore, the analysis itself requires effort and skill. This will be considered further in Chapter 12, where we cover the analysis and interpretation of qualitative data.

A final issue is that open-ended questions tend to produce a great variability in the amount of data across respondents. Verbally fluent respondents may provide very full answers, while less fluent respondents may find open-ended questions demanding to answer, and give very terse responses. In particular, open-ended questions in written questionnaires are often left blank, because they require more effort to complete.

Closed-ended questions constrain the answer in some way. Answers are usually recorded in an abbreviated form using a numerical code. For instance, the possible responses to the closed question "Are you feeling happy, sad, or neither, at the moment?" might be coded as 1 = "Happy," 2 = "Sad," and 3 = "Neither/Don't know." Responses can be made in the form of a dichotomous choice (i.e., when there are two possible responses, such as Yes/No), a multiple choice (i.e., where the respondent has to choose one response from several possibilities), a rank ordering (i.e., where a number of alternatives have to be put in order of preference or strength of opinion), or ticking one or more applicable items on a checklist.

The advantages of closed-ended questions are that the responses are easier to analyze, quantify, and compare across respondents. They also help to prompt respondents about the possible range of responses.

The major disadvantages of closed-ended questions are succinctly summarized by Sheatsley (1983, p. 197): “People understand the questions differently; respondents are forced into what may seem to them an unnatural reply; they have no opportunity to qualify their answers or to explain their opinions more precisely.” For example, in research on stressful life events, information from a checklist measure simply tells you whether an event has occurred, but you have no information about the meaning of the event for the individual. “The death of a pet” might mean that the goldfish passed away, or that an elderly person’s sole companion has died. A semi-structured life events interview (e.g., that of Brown & Harris, 1978) allows the interviewer to probe further in order to establish the meaning and significance of each reported event. Furthermore, interview or questionnaire studies that consist entirely of closed questions can be an annoying experience for respondents, as they may feel that they are not getting a chance to put their views across, and may resent being controlled by the format.

The following sections examine qualitative and quantitative methods in turn. This structure is mainly for didactic purposes: we do not wish to artificially polarize the two types of method. In practice, there is a continuum, ranging from unstructured, open-ended methods, through semi-structured interviews or questionnaires, to structured quantitative methods. As we will state repeatedly, it is possible, and often desirable, to combine both qualitative and quantitative procedures within the same study.

QUALITATIVE SELF-REPORT METHODS

For illustrative purposes, we will discuss qualitative self-report methods mostly in the context of the qualitative interview, since the interview is the most frequently used method within the qualitative tradition. However, there are various other qualitative self-report methods, such as: (1) open-ended questionnaires, for example, the Helpful Aspects of Therapy form (Llewelyn, 1988); (2) personal documents approaches, which use pre-existing written records, such as personal journals (Taylor & Bogdan, 1998); and (3) structured qualitative questionnaires, for example, the repertory grid (Kelly, 1955), although repertory grids are often analyzed quantitatively (see Winter, 2003).

- The semi-structured qualitative interview is the most common qualitative self-report method.
- It is usually based on an interview schedule, which lists the major questions to be asked and some possible probes to follow up with.
- The interview style is mostly based on open-ended questions, but can use other active listening responses, such as reflections.
- The interviewer should have an interested stance with a kind of free-floating attention, and guard against putting words into the respondent’s mouth.

Types of Qualitative Interview

In addition to using open-ended questions, qualitative interviews are usually loosely structured, and aim to get an in-depth account of the topic (Josselson, 2013; Kvale & Brinkman, 2009; Rubin & Rubin, 2005). They have similarities to psychological assessment and to journalistic interviews (but also important differences, which we will discuss below).

There are several different forms of qualitative interview (Patton, 2002). The most common is the semi-structured interview. Such interviews can vary in length from a few minutes to many hours and can take place on one occasion or across many occasions. Typically, qualitative interviews last around an hour (both parties tend to get tired by longer interviews). At the upper end, intensive life-story interviewing, described by Taylor and Bogdan (1998), may involve many interviews totaling up to 50 or 120 hours of conversation.

Alternatives to the semi-structured interview include: (1) the informal or unstructured conversational interview, which is most common as an element of participant observation; (2) the standardized open-ended interview, which consists of a uniform set of questions that are always administered in the same order, often with fixed follow-up questions; and (3) the questionnaire-with-follow-up-interview method favored by phenomenological researchers of the Duquesne school (e.g., Giorgi & Giorgi, 2003; Wertz, 1983). In the last, open-ended questionnaires are used to identify promising or representative respondents who are then interviewed in detail.

One other option is to conduct *focus group* interviews (e.g., Kitzinger, 1995; Stewart, Shamdasani, & Rook, 2007). This method, which originated in market research and public opinion polling, involves assembling a small group of respondents. The interviewer interacts with the whole group, following the same kind of semi-structured protocol as in an individual interview. The group format has the advantage of enabling respondents to react to each other's contributions, and thus possibly to explore the topic more deeply. The disadvantages are that the interview is subject to the usual group dynamics, such as conformity pressures, and giving more weight to the opinions of more vocal or prestigious members, which may affect its validity.

A note on terminology: we will tend to use the terms "respondent" or "interviewee" to refer to the person on the receiving end of the interview. Other possibilities are "informant" or "participant." We avoid the term "subject" because of its connotations of powerlessness (see Chapter 10).

Likewise, there are a number of models of the relationship between the interviewer and interviewee. At one end of the range are the traditional "subject" models, in which the interviewee is seen as a passive information provider responding to the researcher's questions. At the other end are the various "co-researcher" models, which try to minimize the distinction between researcher and "subject" in order to create research in which participant and researcher interact as equals. Examples are feminist (Oakley, 1981; Wilkinson, 1986), new-paradigm (Reason & Rowan, 1981), and participatory action (Jason, Keys, Suarez-Balcazar, Taylor, & Davis, 2004) research.

Some feminist researchers (e.g., Belenky, Clinchy, Goldberger, & Tarule, 1986; Carlson, 1972; Riger, 1992; Wilkinson, 1986) see traditional paradigms, where the researcher is in charge of the relationship, as replicating patriarchal power relationships.

Their critique is mostly aimed at quantification and experimental manipulation, but it also extends to the more traditional forms of qualitative interviewing (Oakley, 1981). They argue that to empower women one must listen directly to what they are saying and respond personally without hiding behind the facade of the objective researcher. However, other researchers argue that it is mistaken to discard certain methods as being insufficiently feminist (Hughes & Cohen, 2010; Peplau & Conrad, 1989).

Interview Schedule

The first step is to prepare an interview schedule that lists the important areas to be addressed, with some important questions to be asked. The schedule is informed by the research questions, the previous literature, and personal experience. It is usually a good idea to structure it around some sort of logical framework, which could be, for example, conceptual or chronological, but it is important to use the schedule flexibly, not rigidly. It is vital to pilot test the interview schedule on a few respondents and revise it accordingly.

Young and Willmott (1957), in their classic study, *Family and Kinship in East London*, describe the use of their interview schedule:

> We used a schedule of questions, but the interviews were much more informal and less standardized than those in the general survey. Answers had to be obtained to all the set questions listed (though not necessarily in the same order), but this did not exhaust the interview. Each couple being in some way different from every other, we endeavored to find out as much as we could about the peculiarities of each couple's experiences and family relationships, using the set questions as leads and following up anything of interest which emerged in the answers to them as the basis for yet further questions. (Young & Willmott, 1957, p. 207)

The interview typically starts with general questions, as a warm-up, and then more detailed, or more sensitive, questions come later in the interview. The standard questions need not be covered in a fixed order, but the interview schedule serves as an aide-mémoire, to remind you what needs to be asked. The schedule lists the main questions, but it is important to use follow-up questions in order to obtain more detail as necessary. An example of an interview schedule is given in the box below.

Sample Interview Schedule

For illustration, here is part of an interview schedule from a study of peer support for women with gynecological cancer (Pistrang, Jay, Gessler, & Barker, 2013): the Women Helping Women project. The peer supporters were former patients who had been successfully treated for gynecological cancer; they supported more recently diagnosed patients by telephone. The interview aimed to understand the impact on the peer supporters themselves—what they experienced as beneficial or detrimental—and to understand the challenges of delivering peer support. (A separate interview focused on the perspective of the woman receiving support.) The excerpt in the box includes three central sections of the interview schedule. The first section starts with broad questions, in order to get a sense of the respondent's overall experience before moving on to more focused questions which address the main topic of the study. An extract from one of the interviews follows later in the chapter.

Excerpt of interview schedule for peer supporters of women with gynecological cancer (Women Helping Women project: Pistrang et al., 2013)

Overall experience of being a peer supporter

The aim of this introductory section is to get an overall picture of the woman's experience of being a peer supporter, before asking more specifically about processes and impact.

What was it like being a peer supporter?
What were some of the best things about it for you?
What were the things you didn't like, or things that could have been better?
Was it what you expected?

Processes of support

The aim here is to elicit a detailed picture of how peer support operated in practice, for example, the nature of the telephone conversations and how the peer supporter attempted to be helpful.

What sort of things did you talk about with the woman you were paired with?
What were your conversations like?
In what ways did you try to help?
What was it about your conversations that you think helped/ didn't help her?
What difficulties, or dilemmas, did you face in trying to be supportive?

Impact of peer support on recipient and provider

The aim here is to elicit the participant's views about how peer support may have helped (or not helped) the woman she was supporting, as well as the impact (positive or negative) on the peer supporter herself.

Do you think the woman you were supporting benefited in any way?
Were there any ways in which you think it was unhelpful or caused problems for her?
Were there any ways in which *you* benefited?
Were there any ways in which it was unhelpful or caused problems for *you*?

Notes for interviewer

Examples of follow-up questions:

What was that like for you?
How did that work?
Can you give an example of that?
What were you thinking at that point?

Interviewing Style

The interviewer's general stance should be one of empathic and nonjudgmental attention, giving the respondent plenty of space to think and talk, and avoiding leading questions. If you are unclear about anything, probe further, although legal-style interrogation is obviously to be avoided.

In order to be an effective qualitative interviewer, you must start with an attitude of genuine interest in learning from others, in hearing their story, and you must be able to listen to them with tolerance and acceptance. The schizophrenia researcher, John Strauss, realized after 30 years of quantitative research that he had learned very little about the nature of schizophrenia; he felt that he had only really begun to learn when he started to listen to what the patients had to say when he asked them about their experiences (Strauss, Harding, Hafez, & Lieberman, 1987).

Qualitative interviewing has increasingly been viewed as a key method for helping respondents "tell their stories." It could be argued that the urge to tell stories is so strong that qualitative researchers proceed at their peril if they try to ignore the power of narrative. In narrative approaches (see Chapter 5), the interviewer's main job is to help the respondent to tell their story, perhaps beginning with something like, "I wonder if you could tell me the story of [e.g., when the depression began] in as much detail as you feel comfortable giving me." Then, the interviewer's job is to encourage the respondent to keep going, or to back up and provide missing information if they skip over something important. (Narrative also has therapeutic functions, especially in the treatment of traumatic or other difficult life situations; for example, McLeod, 1997.) However, the interviewer does need to balance the respondent's need to tell their story, with their own need to have a clear focus on obtaining material relevant to the research questions.

Your therapeutic skills, such as empathy and clinical intuition, come very much to the fore here. However, there must be a clear distinction between research and therapy (or clinical assessment) interviews, as all therapeutic orientations involve interventions which are inappropriate for qualitative interviewing. For instance, it would be wrong to conduct a qualitative interview in cognitive-behavioral style, as this approach, like most therapies, is ultimately aimed at changing the client's thoughts and experiences rather than finding out about them. Even client-centered therapists may engage in too much paraphrasing, which can easily end up putting words in the respondent's mouth or a loss of focus for the interview. It is also important not to assume too much common understanding due to shared culture or experiences. The interviewer needs to take a partial step back from what the respondent is saying, taking a stance like the proverbial Martian, who knows nothing about how Earthlings conduct their affairs. In general, clinical assessment interviews are also quite different from research interviews, as the former tend to be aimed at assembling the information into a coherent clinical formulation.

It is important, for two reasons, to audio-record the interview. First, retrospective notes or memory are prone to inaccuracies and incompleteness. Second, extensive note taking runs the risk of distracting the respondent and interrupting the flow of the interview. Notes may suffice for interviews which are brief and highly structured; in such situations, note taking may also be acceptable to the respondent. However, if you have to interview without a recorder, your notes need to clearly identify which parts are the respondent's verbatim statements and which are your own summary. Written notes can also be used as a reminder during the interview, for example, jotting down a particular phrase used by the respondent that you want to return to later on. In this case, note taking is brief and should be limited to those essential reminders needed to help you conduct the interview. Finally, as we suggested in Chapter 3, it is worth keeping a research journal to record your general impressions of each interview.

Specific Qualitative Interviewing Skills

If one is genuinely motivated to understand and learn about people by interviewing, then a number of technical skills in information gathering and listening become useful. One useful way to describe these skills is in terms of what are called "response modes" (Goodman & Dooley, 1976), that is, basic types of interviewer speech acts or responses. These can be divided into three groups: responses which are essential for qualitative interviewing; supplementary responses which are sometimes useful; and responses which should generally be avoided.

Essential response modes. These lean heavily on the "active listening" responses such as those made famous by client-centered therapy. Thus, two key responses are open questions—to gather information and to encourage the respondent to elaborate—and reflections—to communicate understanding and to encourage further exploration of content. Questions to guide the discussion ("Could you tell me about …") are also essential for beginning and structuring the interview, while brief acknowledgments (e.g., "I see" or "Uh-huh") build rapport and help the respondent keep talking. If a more active, paraphrasing style is used, you are more likely to need to account for the interviewer's possible influence on the data when you do your analysis.

As noted earlier, the interview schedule will typically start with some general questions in order to get a broad overview of the respondent's experiences, followed by more focused questions addressing the topics of the study. However, it is often the unscripted follow-up questions that are important. These are hard to get right, even for experienced interviewers. The interviewer needs to decide in the moment what to follow up and what not to, given that there won't be time to follow up on every point.

Follow-up questions (sometimes called probes) are used to get a detailed, fine-grained description of material relevant to the research question, or in cases when the respondent's answer to the initial question is unclear or ambiguous. The interviewer needs to listen carefully to what the respondent is saying; it's useful to pay particular attention to the emotional valence of what is being said (not just the content), as this often signifies important material. (The use of metaphors is also worth listening out for: they often capture essential meanings.) On the other hand, material tangential to the research questions, even if it is personally meaningful to the respondent, usually does not need to be followed up. Some typical follow-up questions are listed at the end to the interview schedule above. They are normally brief and encourage elaboration: for example, "Could you say more about that?" or "What was that like for you?"

Supplementary response modes. In addition, several other types of response can be useful, although they should not be overused. For example, closed questions can be used to test out ideas near the end of the interview. If you have a hunch about something, for example, an idea that has arisen from earlier interviews or background reading, you may wish to ask the respondent about this, but it is important to save this for late in the interview in order not to "lead the witness." Other supplementary response modes include self-disclosures, which allow the interviewer to explain his or her goals for the interview and to build rapport by answering questions about him- or herself; and reassurances or sympathizing responses ("It's hard"), to encourage openness in the respondent.

Responses to be avoided. These include problem-solving advisements, which give respondents suggestions about how to solve their problems; interpretations, which try to tell the respondent why they did something or what they actually felt; disagreements or confrontations, which cut off communication by criticizing or putting the respondent down (e.g., do not try to "catch out" respondents in contradictions but instead try to express curiosity at the complexity of the person's experiences); and giving respondents information (other than information about the structure and purpose of the interview itself).

Tracking the Respondent's Answers

As Kvale and Brinkman (2009) caution, it is important for the researcher to track the relevance of the respondent's answers during the interview, in order to make sure that the research questions are being answered and the meaning of the respondent's statements is clear. Once the interview is transcribed and you sit down to analyze your data, it is generally too late to go back to your respondents in order to ask them to clarify what they meant. It is also important to scrutinize the data from your first interview before embarking on further interviews. This will make you aware of problems with any of the interview questions or with ambiguous or vague answers from respondents, so that you can modify your interview schedule and technique.

Qualitative interviewers are sometimes confronted with apparently contradictory information from respondents. This should not necessarily be regarded as evidence of unreliability or invalidity. People will often have multiple, sometimes contradictory, feelings and views. It is a good idea to listen for such contradictions, because they may reflect ambivalent feelings or avoidance of painful experiences. During the interview, you may become aware of possible inconsistencies, which could be: (1) internal, between different parts of the story; (2) external, with another source such as a document or another respondent; or (3) between manifest and latent content, for example, between the words and the tone of voice. Rather than pouncing on them, it is a good idea to gently and tactfully inquire about them ("That's interesting, it sounds like you have several different kinds of feelings about your clients. Can you tell me more about that?").

Sample Interview

The following excerpt comes from a semi-structured interview in the Women Helping Women project (Pistrang et al., 2013), which followed the interview schedule described above. The participant (R) is a former patient who became a peer supporter; the interviewer (I) was Nancy Pistrang, this book's second author. The interview has been edited for readability, and also shortened for didactic purposes; ellipses (…) indicate where cuts have been made.

As Young and Willmott (1957) point out, there is not an exact correspondence between the questions in the schedule and those asked by the interviewer: the schedule is a vehicle for enabling the respondent to talk about the important issues. The excerpt of the transcript shown in the box focuses on the participant's response to a single question in the interview schedule: "Were there any ways in which *you* benefited [from giving peer support]?" and illustrates the interviewer's follow-up questions and reflections.

Part of an interview transcript of a peer supporter of a woman with gynecological cancer (Women Helping Women project: Pistrang et al., 2013)

I: We've talked quite a bit about how it [peer support] benefited [the patient]. And I'm just wondering whether you felt you benefited in any way from supporting her—how it was for you?

R: I think it did a number of things for me, really. One, I think it allowed me to take a step back from my own situation. So, instead of being the patient, it allowed me to look at my own experience in a more objective way. It brought me out of my journey, if you see what I mean, to be able to look at it in a more objective way. It also nicely reminded me that really, it is a pretty tough thing to go through … it just does remind you, actually, of how difficult it was and how far you've come … So you kind of reflect on some of the stages you've been through … [it] allowed me to put things in perspective.

I: Right. It's an interesting thing, that, really—that somehow through talking to someone else who's earlier on in that journey, it somehow allows you to step out of it in a way, you're saying, and look at it more objectively. What is it, do you think? What's that process? What's going on?

R: It's a weird kind of—almost like a parental thing, if that makes any sense.

I: Can you say more about that?

R: It's almost like you get a chance to turn this really ugly, rotten, horrible thing that's happened to you into something—a way of helping somebody else. And I think it then allows you to sort of flip your own feelings on the head and realize, "I've got something to share. I've got something to give from this that's positive. I've got something that could perhaps impact somebody else's life in a positive way through this—you know, through my own rotten experiences." It kind of gives you a bit of a feeling, I guess, of pride … that's what I mean. In a certain parental way, you get to feel quite—almost protective over the person and —and encouraging—you become very encouraging.

I: Yeah, and sort of nurturing another person.

R: Yes. And in a way, I think it sort of drifts back to you. And you end up kind of of realizing that you've probably not been doing quite enough nurturing of yourself. And again, that's an important sort of thing.

This excerpt illustrates how the interviewer carries out her aim—to understand the participant's experience—by using questions to clarify and explore, and reflections to confirm understanding and encourage elaboration. It also shows the richness of the data that can come from a qualitative interview, and also gives a foretaste of the "qualitative overload" problem that is involved in analyzing the material (see Chapter 12). In the following sections, we will look in more detail at the procedures used in conducting qualitative interviews.

QUANTITATIVE SELF-REPORT METHODS

The literature on quantitative self-report methods is enormous, and we can only hope to scratch the surface here. More extensive treatments can be found in a number of specialist texts, for example, Bradburn et al. (2004), Butcher (1999), Dawis (1987), DeVellis (2012), Dillman et al. (2009), Marsden & Wright (2010), Saris and Gallhofer (2007), and Streiner and Norman (2008). For convenience, we will focus on written questionnaires with rating scales; however, everything that we have to say applies equally well to interviews and internet questionnaires designed to yield quantitative data.

- The central quantitative self-report method is the written questionnaire or rating scale.
- Questionnaire design may seem simple, but it is not—there is no shortage of badly designed questionnaires in circulation.
- The central maxim is "take care of the respondent."
- Most questionnaires use a Likert scale.
- Good items are clear, simple, and brief.
- There are a number of issues in designing the response scale, such as the number of scale points, the type of anchors, unipolar or bipolar scales.
- Response sets, such as acquiescence and social desirability, refer to tendencies to respond to items independently of their content. They need to be taken into account when designing and interpreting self-report measures.

As in other places in this book, we will describe the process from the viewpoint of constructing a measure, in order to give readers a better feel for the difficulties that are involved. The central point is that it is not just reliability and validity considerations that need to be taken into account when appraising a measure; it is worth looking closely at the fine detail of how the measure is put together.

Steps in Measure Development

If you are doing research involving a variable for which there is no adequate existing self-report instrument, you may need to construct your own measure. This is not a step to be undertaken lightly, as it is time consuming and requires skill to do well. However, because many areas are either undermeasured or are poorly measured, this is a common type of research. One often approaches a new research area only to find that no good measures exist, and then ends up by reformulating the research toward developing such a measure. (A common experience of researchers is to discover that such studies are often more widely cited and influential than their other research.)

If you need to construct a measure, the steps are roughly as follows:

- Having done a literature search to make sure that no existing instrument is suitable, develop a first draft of the scale based on theory, pilot qualitative interviews, or analysis of existing questionnaires.
- Progressively pilot the scale on respondents nearer and nearer to the intended target population (known as *pretesting*), modifying it accordingly. Expect to take it through several drafts, for instance, first to colleagues, second to friends or support staff (ask them to point out jargon or awkward phrasings), third and fourth to potential respondents. It is often worthwhile running small informal reliability and possibly factor analyses on a pilot sample of 20 or 30 respondents to see whether any items should be dropped or added before doing the larger, formal study.
- Once a satisfactory version of the scale has been developed, do a formal reliability study by giving the measure to a large sample (e.g., over 120 respondents) drawn from a population which approximates the population you are interested in. You can then examine its item characteristics (e.g., means and standard deviations), internal consistency, and factor structure. It is also typical to administer the measure twice to some of the participants, in order to assess its test-retest reliability.
- If the reliability and factor structure are satisfactory, you can conduct appropriate validity studies (see Chapter 4), which examine the measure's correlations with other criteria or constructs. (These studies may also be combined with the previous step.) The new measure is administered, along with a set of similar and different measures, such as a social desirability measure and measures that should not correlate with the new measure (to establish discriminant validity). It is also a good idea to use measures of more than one type or perspective, in order to reduce the problem of method variance (e.g., to use self-report measures plus observer ratings). The goal is to see whether the measure fits in with the pattern of correlations that would be predicted by the theoretical framework from which it was derived.

Questionnaire Design

Designing a questionnaire involves deciding on the topics to be covered and their sequence, writing the questions or items, and selecting an appropriate response scale. We will deal with each of these in turn.

In all aspects of questionnaire design, the golden rule is "take care of the respondents." Put yourself in their shoes and ask what the experience of being on the receiving end of the questionnaire is like. Make it as easy, rewarding, and free of frustration as possible. As part of the pilot testing, it is a good idea to fill out your questionnaire yourself (often a salutary experience) and give it to a few friends who will be able to give you constructive criticism.

The goal is to not get in the way of respondents' being able to communicate their thoughts and experiences. Trying not to alienate your respondents makes sense not only from a general human relations point of view, but it also makes good scientific sense. Irritated people will not give you good data (or even any data at all—they may just discard your questionnaire).

Topic Coverage and Sequence

The questionnaire is often broken into subsections representing different topics or variables. The primary consideration is that, as a whole, it should adequately capture all of the concepts needed to answer the research questions. In other words, the data set should yield an answer to each of the research questions, or enable each of the hypotheses to be tested. Once this coverage has been achieved, the issue is then how to order the topic areas within the questionnaire.

It is best to start off with easy, interesting, and nonthreatening questions that all respondents can answer (Dillman et al., 2009). This engages the respondents and helps to establish rapport with them: even a written questionnaire is a form of interpersonal relationship. Demographic questions (i.e., about the respondent's age, sex, etc.) should usually be placed at the end of the questionnaire, as it is better to start with questions relevant to the topic of the interview.

Structured interviews often adopt a *funnel approach*, that is, they start out broadly and then progressively narrow down. This reduces the risk of suggesting ideas to the respondents or influencing their answers. The interview typically begins with open-ended questions, then moves in the direction of increasing specificity. The pollster George Gallup (see Sheatsley, 1983) recommended the following ordering for public opinion research (e.g., to study opinions about sexual harassment): (1) test the respondents' awareness of, or knowledge about, the issue; then (2) ask about their level of interest or concern; then (3) about their attitudes; then (4) about the reasons for these attitudes; and finally (5) about the strength of their opinions.

Item Wording

Having established the coverage of topics, the next step is to write the individual questions or items. The wording of an item is of crucial importance, as the way that a question is phrased can determine the kind of response that is given (Bradburn et al., 2004; Saris & Gallhofer, 2007; N. Schwartz, 1999). It is worth heeding some key principles of item construction:

Neutrality. The language of the item should not influence the respondent, that is, it should not suggest an answer. Possible errors take the form of *leading questions* (questions which are not neutral, which suggest an answer), questions with *implicit premises* (built-in assumptions that indicate the questioner's viewpoint) and *loaded words or phrases* (ones that are emotionally colored and suggest an automatic feeling of approval or disapproval). Some examples of such problematic questions follow, with commentary after each:

> "Do you think that …?" and "Don't you think that …?"

These are leading questions that pull for a "yes" response.

> "When did you stop beating your wife?"

The latter question has become the clichéd example of an implicit assumption; it assumes the respondent has been beating his wife (Payne, 1951). Such questions are usually to be avoided. However, there are times when implicit premises are useful for normalizing behavior, by giving the respondent permission to talk honestly. For example, studies of sexual behavior may sometimes use questions such as: "How old were you the first time you …?", rather than saying "Did you ever …?"

"How often do you refer to a counselor?"

This question is a subtler variant of the implicit premise; it assumes that the respondent does refer to a counselor. It would be better to include "if at all," or, even better, to have two separate questions, for instance, "Do you refer …?" and "If yes, how often … ?"

"Why don't you refer to a counselor more often?"

This question assumes that referring more often is desirable. A better question would be: "What factors influence your referral decisions?"

"How often did you break down and cry?"

"Break down" is a loaded phrase which gives crying a negative connotation. In this case, it could simply be omitted.

Clarity and simplicity. It is better to use simple, clear, everyday language, adopting a conversational tone. Make sure that the item does not demand a reading level or vocabulary that is too advanced for your respondents. In particular, try to avoid psychological jargon (it is helpful to ask a nonpsychologist to read your questionnaire to detect it). Psychologists often become so used to their own technical language that they forget that members of the public do not understand it or find it strange. This is another reason why it is vital to pilot the questionnaire on ordinary people.

Specificity. Lack of specificity gives rise to *ambiguities,* for example:

"Do you ever suffer from emotional problems?"

The phrase "emotional problems" means different things to different people. Therefore, it is better to define it or use alternatives. On the other hand, you could leave the phrase as it is, if you want to leave it open to people's own interpretations.

"Do you suffer from back pain?"

It is better to give a time frame, for example, "in the last four weeks," and also to specify the anatomical area, perhaps with the aid of a diagram (since, for example, respondents may not know if shoulder or neck pain should be included).

"Do you like Kipling?" ("Yes, I kipple all the time.")

People will often respond to a question that they do not understand, rather than saying explicitly that they do not understand it.

Single questions. It is better to ask one thing at a time. Problems arise with *double-barreled questions,* that is, questions that have two independent parts:

> "Were you satisfied with the supervision and range of experience at your last clinical placement?"

The respondent could be satisfied with the supervision, but not the range of experience.

> "Were you satisfied with your placement or were there some problems with it?"

The two parts are not mutually exclusive: the respondent could be satisfied with a placement even though there were problems with it.

> "In order to ensure patients take their medication, should psychiatrists be given more powers of compulsory treatment?"

The respondent could disagree with the implications of the initial premise, but agree with the main statement.

Brevity. Short items are preferable. Sentences with multiple clauses can be difficult to process. As a final example of what to avoid, here is a classic of its kind, from no less a figure than the behaviorist John Watson. This monstrous item violates this and most other principles of item writing:

> Has early home, school, or religious training implanted fixed modes of reacting which are not in line with his present environment—that is, is he easily shocked, for example, at seeing a woman smoke, drink a cocktail or flirt with a man; at card playing; at the fact that many of his associates do not go to church? (Watson, 1919, quoted by Gynther & Green, 1982, p. 356)

Constructing the Response Scale

With a *rating scale*, the respondent gives a numerical value to some type of judgment. There is a wide variety of scale types: Likert scales, Guttman scales, Thurstone scales, rankings, etc. (Nunnally & Bernstein, 1994). Here we will focus on by far the most commonly used one, the Likert scale. This consists of two parts: the items (a set of statements, such as "I feel tense") and the response scale, a set of alternatives of increasing intensity, with an integer numerical scale and verbal anchors (see Figure 6.1 for some examples). Guttman scales are also sometimes used. The format is to ask the respondent to choose between a number of statements of increasing intensity. The Beck Depression Inventory uses a Guttman scale approach.

Just as the form of the question can influence the response, so can the form of the response scale (Saris & Gallhofer, 2007; N. Schwartz, 1999). The major considerations in constructing response scales are:

How many scale points? The number of scale points can range from two upwards. (Scales with two choices are known as *binary* or *dichotomous*, with three or more, Agreement

How much do you agree or disagree with each of the following statements?

1	2	3	4	5	6	7
Disagree strongly	Disagree moderately	Disagree mildly	Neither agree nor disagree	Agree mildly	Agree moderately	Agree strongly

Frequency

How often do you ...?

0	1	2	3	4
Never	Seldom	Sometimes	Often	Very often

Quantity/proportion	Degree/strength
How many ... ?	How (much) ... ?
0 None	0 Not at all
1 Very few	1 Slightly
2 Some	2 Moderately
3 Very many	3 Very (much)
4 All	

Figure 6.1 Examples of anchor words for Likert scales

multiple choice.) There may be logical reasons for using a certain number of responses: for example, some questions clearly demand a yes/no answer. However, it is more frequently the case that the response scale must be decided by the researcher. The main issues are:

- There is a lot of researcher folklore about optimal numbers of scale points; however, item response theory (Bond & Fox, 2007) now provides empirical methods for evaluating rating scale use in order to evaluate how informants are actually using rating scale categories and in particular whether they are able to reliably discriminate neighboring points.
- Rating scale category use appears to interact with item content: a five-point rating scale that works for one item or type of content might not work for another item or type of content. For example, it appears that informants can reliably discriminate more rating scale points for individualized outcome problems that they have created themselves than for general psychological distress measures (Elliott et al., 2004).
- Although it is sometimes claimed that reliability increases with more scale points (Nunnally & Bernstein, 1994), a common finding is that informants cannot reliably discriminate two or sometimes even three of the middle scale points on the standard five-point rating scales used on measures of general psychological distress. In any case, there seem to be diminishing returns beyond five points (Lissitz & Green, 1975).

- More can be less when it comes to rating scale categories: adding more categories does not increase reliability, and often decreases it, because when asked to discriminate beyond their ability, informants tend to answer randomly (Bond & Fox, 2007).
- The extreme ends of rating scales tend to be underutilized by informants either because the events are infrequent (as might be expected under normal curve assumptions) or because they actively avoid using the extreme ends of the rating scale, a phenomenon known as the *central tendency*. This means that the upper and lower ends of rating scales often need careful attention to wording. (Relatedly, there is more measurement error at the extreme ends of rating scales; the usual reliability statistics only apply to the middle of the rating scale.)
- Instead of using discrete scale points, another approach is to ask respondents to put a mark on a 10 centimeter line (a *visual-analog scale),* and then use a ruler to make the measurement (McCormack, Horne, & Sheather, 1988). This is used, for example, in pain research, to assess the intensity of the respondent's pain experience. However, Thomeé, Grimby, Wright, and Linacre (1995) reported that, while such scales create an illusion of fine discrimination, they are in fact equivalent to seven- or even four-point scales.

Unipolar or bipolar. Response scales can either be unipolar or bipolar. A *unipolar* scale has only one labeled construct, which varies in degree. For example, a scale measuring intensity of pain might range from "No pain at all" to "Unbearable pain." A *bipolar* scale has opposite descriptors at each end of the scale (e.g., "Happy" at one end and "Sad" at the other). In Figure 6.1, the Agreement scale is bipolar; the others are unipolar.

Mid-point. Bipolar scales may or may not have a mid-point, representing such options as "Don't know," "Unsure," "Neutral," or "Neither one way or the other." In other words, the scales may have either an odd or an even number of steps.

The argument against having a mid-point is that people usually hold an opinion, one way or the other, which they will express if you push a little. This procedure is known as *forced choice*: for example, "Do you agree or disagree with the following statements?" Forced choice makes data analysis easier, because respondents can be divided into those expressing a positive and those expressing a negative opinion. Furthermore, according to item-response analyses, mid-points sometimes misscale and are best left out (e.g., Bourke-Taylor, Pallant, & Law, 2014). However, if a question is worded well you should not get a lot of middle responses in the first place.

The argument for having a mid-point is that neutrality represents a genuine alternative judgment, and so it is coercive not to allow respondents to express their opinions in the way that they want to.

Anchoring. Anchoring refers to labeling the points of the scale in words as well as numbers. You usually want to define the steps explicitly, so that people are rating to the same criteria. However, this does make two measurement assumptions: (1) that the scale has interval properties (see Chapter 4), that is, that its steps are all equal (for example, that the distance between "not at all" and "slightly" is the same as between "very" and "extremely"); and (2) that people understand the same thing by all the adjectives. Try to avoid modifiers with imprecise meanings, for example, "quite" can sometimes intensify (equivalent to "very") and sometimes diminish (equivalent to "somewhat").

Sometimes researchers just anchor the end-points of the scales, as in visual-analog scales and *semantic differentials* (which use pairs of bipolar adjectives, such as good–bad, hard–soft, heavy–light). It is also possible to anchor alternate scale points as a compromise between anchoring every point and only anchoring the extremes. However, not anchoring scale points leaves their meaning unclear.

Response Sets

Response sets refer to the tendency of individuals to respond to items in ways not specifically related to their content (Bradburn, 1983; Nunnally & Bernstein, 1994). They may be conceptualized as personality variables in their own right. The most commonly encountered response sets are acquiescence and social desirability.

Acquiescence ("yea-saying") refers to a tendency to agree rather than disagree. The classic example of acquiescence problems is with the California F-scale (Adorno, Frenkel-Brunswick, Levinson, & Sanford, 1950), which was developed to measure authoritarian tendencies (the F stands for fascist). Early *item-reversal studies*, in which some of the items were replaced by their inverse, seemed to show that this scale was mostly measuring acquiescence rather than authoritarianism (although there is some dispute about this conclusion, see Rorer, 1965).

The way to get around acquiescence problems is to have an equal number of positively and negatively scored items in the scale. For example, in an assertiveness scale, the item "If someone jumps to the head of the queue, I would speak up" would be scored in the positive direction, while "I tend to go along with other people's views" would be scored in the negative direction. Thus, when the items are reversed and averaged, any tendencies to acquiesce would cancel themselves out.

Acquiescence has been noted as a particular problem when working with people with mental retardation ("intellectual disabilities" in the UK terminology). The title of Sigelman, Budd, Spanhel, & Schoenrock's (1981) paper, "When in doubt, say yes," is often quoted in this context. Sigelman et al. recommend some guidelines for good practice, for example, that researchers avoid "yes/no" questions and instead use open-ended questions with this population. However, Rapley and Antaki (1996) argue, from a conversation analysis point of view, that the assumption of an acquiescence bias in people with mental retardation is not fully substantiated by the evidence.

Social desirability refers to a tendency to answer in a socially acceptable way ("faking good"), either consciously or unconsciously (Crowne & Marlowe, 1960, 1964). This is especially a problem in occupational testing, as the following humorous advice for aspiring businessmen illustrates (it also embodies the outdated assumption that business executives are men):

> When an individual is commanded by an organization to reveal his innermost feelings, he has a duty to himself to give answers that serve his self interest rather than that of The Organization. In a word, he should cheat … Most people cheat anyway on such tests. Why then, do it ineptly? … When in doubt about the most beneficial answer, repeat to yourself: I loved my father and my mother, but my father a little bit more. I like things pretty much the way they are. I never worry about anything. (Whyte, 1959, p. 450, quoted in Crowne & Marlowe, 1964)

In clinical research, it is also important to consider the opposite tendency, that is, where respondents may attempt to "fake bad." This may occur in forensic contexts, when offenders may be trying to obtain a lighter sentence or a softer prison regime; in the case of insurance claims for psychological trauma, where people may be attempting to get a larger settlement; or in the context of being on a waiting list for psychological therapy, where clients may be trying to get help sooner.

Possible ways to get around social desirability problems are:

- Embed a social desirability scale within the main instrument, such as the Marlowe–Crowne (Crowne & Marlowe, 1960) Social Desirability Scale, the L (Lie) scale on the Eysenck Personality Questionnaire (EPQ: Eysenck & Eysenck, 1975) and the Minnesota Multiphasic Personality Inventory (MMPI: Hathaway & McKinley, 1951), and the K (Defensiveness) scale on the MMPI. These provide a direct measure of the extent of socially desirable responding. Factor-analytic studies have found these scales to have two separate components, self-deception and impression management (Paulhus, 1984).
- Use a forced choice format, where the respondent chooses between alternatives of equal social desirability. For example, the Edwards Personal Preference Scale (Edwards, 1953), which measures personality dimensions such as achievement and affiliation, has paired items balanced for social desirability. However, some respondents may object to the constraining nature of such instruments.
- Use "subtle items," on which the acceptability of the response is not apparent, for example, on the MMPI (Weiner, 1948). However, this practice raises questions about the face validity of the scale, and is not without controversy (Hollrah, Schlottmann, Scott, & Brunetti, 1995).

Assembling the Questionnaire and Looking Ahead

Having designed the questions and response scales, the final task is to assemble them into a coherent questionnaire. Once again, the maxim "take care of the respondent" should be primary. Try to make the experience of completing the questionnaire as engaging as possible, and minimize anything which might exhaust or irritate respondents.

Make the questionnaire look attractive by giving it a pleasing layout with readable typefaces, and use language which is easily understandable and welcoming. It also helps respondents work through the questionnaire if the topics are ordered in a logical sequence, and the transitions between different topic areas are made as smooth as possible. Simple things, such as introducing each section with phrases like "This section asks about . . ." can make the respondent's task easier.

Think about data analysis before the final draft, as you may want to print data entry instructions on to the questionnaire. If possible, try some quick analyses to examine your main research questions on the pilot sample.

We will deal with sampling in general in Chapter 10, but there are some issues specific to mail surveys. Dillman et al. (2009) suggests aiming for a response rate of over 60%, and sending out reminders to increase the initial response. Bear in mind that people who return questionnaires are not usually representative of the whole target population: they tend to be higher on literacy, general education, and motivation.

In order to conceptualize what would lead to sample bias, ask yourself why someone would not fill out the questionnaire. It is sometimes possible to estimate bias by comparing respondents with nonrespondents on key variables. For instance, in a client satisfaction survey, it may be possible to see if the clients who filled out the survey questionnaire differed from those who did not, in terms of severity of problems or length of time in therapy.

The internet is an important alternative medium for conducting surveys. A recent development in questionnaire research is the internet-based questionnaire (Dillman et al., 2008). Several applications exist for helping researchers construct and conduct web-based surveys, such as Survey Monkey, Opinio, and Qualtrics. The internet also has the advantage of providing access to a wider potential sample of respondents, particularly important with difficult-to-access populations (see also Chapter 10). For example, Barry, Elliott, and Evans (2000) found that Arab immigrants to the United States were more willing to respond when approached in this way than face to face, and that respondents recruited via the internet did not appear to differ from those obtained in the usual way.

Integrating Qualitative and Quantitative Self-report Methods

It is worth re-emphasizing that our separation of interview and questionnaire, and qualitative and quantitative methods was for didactic, not practical, purposes. Our view is that all combinations of self-report/observational, qualitative/quantitative data collection methods have their uses. It is possible to use written questionnaires within observational protocols and to combine open-ended and closed-ended questions in the same questionnaire or interview. For example, it is often a good idea to begin and end structured quantitative interviews with general open-ended questions. Questions at the beginning give the respondents a chance to talk before they have been influenced by the researcher's framework, and questions at the end give them a chance to add anything that may not have been addressed within that framework.

CHAPTER SUMMARY

This chapter has covered the procedures for constructing self-report methods, such as interviews and questionnaires. The advantages of self-report are that it gives the person's own perspective, and that there is no other way to access the person's own experience. Its disadvantage is that there are potential validity problems: people's reports may contain errors due to deception, inaccurate recall, or the unavailability of the information to conscious processing.

There are both qualitative and quantitative approaches to self-report. The main qualitative self-report approach is the semi-structured interview. This allows a flexible interview style, with probes where necessary, and helps respondents describe their own experience in their own words. Qualitative interviewing is a distinct skill, different from clinical interviewing, including interviewing for psychological assessment.

The main quantitative self-report approach is the written questionnaire using a Likert scale. There are a number of principles to follow in constructing quantitative self-report instruments, both concerning the wording of the items and the form of the response scale. Response sets, such as acquiescence and social desirability, refer to tendencies to respond to items independently of their content. They need to be taken into account when designing and interpreting self-report measures.

Although we have discussed qualitative and quantitative approaches separately, they can easily and fruitfully be combined within a single interview or questionnaire, or within a study as a whole.

FURTHER READING

Josselson (2013), Kvale and Brinkman (2009), and Rubin and Rubin (2005) give valuable guidance on constructing and conducting qualitative interviews. There are an enormous number of references on assembling quantitative self-report instruments. Ones that we have consulted in preparing this chapter include Bradburn et al. (2004), Butcher (1999), Dawis (1987), DeVellis (2012), Dillman et al. (2009), Marsden & Wright (2010), Saris and Gallhofer (2007), and Streiner and Norman (2008).

QUESTIONS FOR REFLECTION

1. The limitations of self-report methods are well known, and yet the vast majority of psychological research uses self-report methods. Why do you think this is? Is this a problem, as some have said?
2. Qualitative researchers disagree about what constitutes "leading," and how careful to be in avoiding leading responses, for example, whether it is OK to ask closed questions or to paraphrase back to the informant what he or she says. What do you think?
3. How detailed should an interview schedule be? Qualitative researchers sometimes find themselves developing increasingly elaborate interview schedules (say with 20 or more questions), while others argue that a single question is often enough ("Tell me the story of…") What do you think? What are the consequences of simple versus complex interview schedules?
4. Develop a brief interview schedule consisting of three or four central questions on your research topic. Do a 5–10 min interview with a friend or family member who has had an experience that approximates your topic. Describe what you learned about (a) your research topic and (b) your interview schedule.
5. Select a psychological construct you're interested in and construct a very short (e.g., 6–8 item) self-report instrument to assess it. Reflect on alternative ways to word the items and alternative formats for the response scale.

7
Observation

KEY POINTS IN THIS CHAPTER

- Observation provides a direct measure of behavior.
- It is useful when precise records are needed, or for behaviors that are not amenable to self-report.
- The main qualitative approaches are participant observation and text-based methods.
- Quantitative approaches use structured methods to give precise counts of behavior.
- There are several different methods for conducting quantitative observation.
- Careful selection, training, and monitoring of raters are important to achieve good reliability.

In the biological and physical sciences, various forms of observation are the only possible data collection method, since the objects of study are animals, plants, and inanimate objects. In psychology, however, researchers have a choice: whether to use observation, self-report, or a combination of the two. However, in psychology, observational methods have generally been underused, to the potential detriment of the discipline as a whole (Baumeister, Vohs, & Funder, 2007).

Observation can take many forms. You may observe the person in their own natural setting, such as at home or at school. The ecological psychology studies of Roger Barker and his colleagues (Barker, Wright, Schoggen, & Barker, 1978), which provide extensive accounts of behavior in community settings, are classic examples of this type. Or you may observe under standardized conditions in the clinic or laboratory. The Strange Situations Test (Ainsworth, Blehar, Waters, & Wall, 1978), which assesses

Research Methods in Clinical Psychology: An Introduction for Students and Practitioners,
Third Edition. Chris Barker, Nancy Pistrang, and Robert Elliott.

Companion Website: www.wiley.com/go/barker

a young child's reactions to being separated briefly from his or her parent under standardized conditions, is a widely used example in attachment research.

Observation is a special activity, a form of disciplined description that does not come naturally to most people. It is different from evaluation (of whether something is good or bad), explanation (of why something happened), or summary report. Observational methods thus require special training.

We are referring here to observation as a measurement method, not as a research design. Correlational research designs are sometimes called passive observation studies (see Chapter 8); this is something of a misnomer, as they may not use observation at all. Observational data may be used in descriptive, correlational, or experimental designs: there is no logical link between the measurement method and the design.

Advantages and Disadvantages

The advantage of observation is that it is a direct measure of behavior, and thus can provide concrete evidence of the phenomenon under investigation. For example, if you are studying couples' communication, the members of a couple may say in an interview that they "can't communicate and always end up getting nowhere." However, if you actually observe them interacting, you usually get a clearer indication of the nature of their problems: for example, one member of the couple may be critical and the other may withdraw. Another example is in studying children referred for behavior problems, where a father might say "my daughter is disobedient." Observing the interaction between the father and his daughter allows you to see for yourself what actually goes on between them.

Furthermore, observation enables you to assess the behavior within its context and examine sequences of behavior over time. Observing children within their family setting and also in the classroom allows the researcher to identify and measure situational variables (e.g., critical remarks from siblings or peers) that might contribute to problem behaviors, and to see which events precede or follow other events.

Observation is also good for studying behavior that people may not be aware of (e.g., nonverbal behavior) or behavior that is inaccessible using self-report methods (e.g., because of denial, distortion, or simply forgetting). It is also good for research involving individuals with limited language ability, such as young children or people with cognitive impairments.

The disadvantage of observation is that it can only be used to answer certain research questions, principally where you are interested in overt behaviors. Often research questions are more complex than this, and the overt behavior is only one aspect. Observational methods can, however, be used to study internal processes, in two ways. One is that the research participant can be the observer (i.e., engage in self-monitoring, see below). The other is that observations of behavior can be used to make inferences about cognitive processes or emotional states, for example using the emotional Stroop test (Williams, Mathews, & MacLeod, 1996), or using projective tests such as the Rorschach or the Thematic Apperception Test (TAT) (Westen et al., 1999).

Another disadvantage is that observational studies often have problems with reactivity of measurement: people may behave quite differently if they know they are being observed. A potential solution to the reactivity problem is to observe covertly, but this clearly raises ethical issues about deception (which we address below).

Qualitative and Quantitative Observation
Like self-report methods, observational data collection methods have been developed independently within the qualitative and the quantitative traditions.

Although the distinction between the two approaches is not always clear, since qualitative material can be analyzed in a quantitative way using content analysis, this chapter will look at each of these traditions in turn.

QUALITATIVE OBSERVATION

In qualitative observation, the observer attempts to record a narrative account, which, like a literary description, brings the scene to life. However, unlike a literary description, the account attempts to be explicit and systematic rather than metaphorical and intuitive.

As we discussed in Chapter 5, the historical roots of qualitative methods lie in ancient Greek and medieval histories and travelogues. Systematic qualitative observation as a research method was developed as part of the ethnographic approach in anthropology, as, for example, in the early work of Malinowski (1929) in the South Pacific (see Emerson, 2001). It also was found in medical case studies, Freud (1905/1977) being the outstanding example within psychological medicine. The data in such case studies are often not purely observational, as the clinician will also draw upon self-report data (to a major extent in the case of psychoanalytic case studies).

A number of different approaches fall under the qualitative observation umbrella. This section examines two major ones: participant observation and text-based methods. (The analysis of data obtained from such methods is covered in Chapter 12.)

Participant Observation

Features of participant observation:

- Its roots lie in the ethnographic approach to anthropology.
- It involves the researcher becoming "immersed" in the setting and taking extensive field notes.
- Methodological problems include reactivity and observer bias.
- Covert observation raises ethical issues.

Participant observation refers to a procedure in which the observer enters an organization or social group (such as a psychiatric hospital or a youth gang) in order to gain first-hand experience of its workings. It is characterized by a period of intense social interaction between the researcher and the people being observed, in their own setting, during which the data are collected unobtrusively and systematically (Angrosino, 2007; Emerson, 2001; Miller, Hengst, & Wang, 2003; Taylor & Bogdan, 1998). Thus, participant observation involves: (1) the observer's immersion in the situation; (2) systematic, but unstructured observation; and (3) detailed recording of observations, generally from memory. (Note that the term "participant" is somewhat ambiguous in this context, as it can refer both to the researcher—the participant observer—and also to the people being observed—the research participants.)

The observer's role in the setting can be anywhere on the continuum from a complete participant, such as when Goffman worked as a mental hospital aide to make the observations in *Asylums* (1961), to a complete observer, such as when traditional ethnographers lived in cultures which they were not a part of. Taylor and Bogdan (1998) warn about the dangers of observing a setting with which one is overly familiar (due either to friendship or to expertise), as the study may be compromised by the researcher's inability to take multiple perspectives and by the temptation to censor reports or data which may offend colleagues or friends.

As the object of study is more often an organization or social group, rather than an individual, participant observation is more compatible with the framework of sociology and anthropology than psychology. It is particularly associated with the Chicago School of sociology and with the sociology of deviance (Emerson, 2001). Whyte's (1943) *Street Corner Society,* a study of Italian-American youth gangs, is a classic example of the genre. Whyte, who was a researcher at Harvard University, spent several years living in the Boston community that he was studying and talking to key informants (mostly members of the gang) in order to understand the structure of their organization. His observations involved joining in with their everyday activities, for example, gambling, bowling, etc. After each period of observation, he wrote extensive field notes, which he later analyzed with the help of one of the informants in his study.

The narrative case study (see Chapter 9) also can be considered a form of participant observation, at least where the focus is on describing the therapeutic process, rather than on giving an account of the client's history. Studies of individual therapy represent the psychologically interesting situation where one member of a dyad is observing the development of that dyad. There are many such accounts from the perspective of both the therapist and the client, and Yalom and Elkin (1974) interestingly combine their parallel accounts of the same therapy in one volume.

Research Questions

In line with the phenomenological approach, most participant observation researchers try to start with no preconceptions about the phenomena under study. They will often go through a process of bracketing, that is, an attempt to identify their preconceptions and set them to one side (see Chapter 5). Anthropologists, in particular, will attempt to set aside their ethnocentric biases when observing other cultures. However, as we have discussed in Chapter 2, disinterested, theory-free observation is an unattainable ideal, since researchers are always observing from within a theory or world view that says what is important and what is trivial. The issue is being aware of and minimizing, rather than eliminating, the extent of one's biases. In any case, the participant observer usually attempts to start the observation unconstrained by prior hypotheses or specific variables of interest.

In practice, participant observation research usually has a clear focus (e.g., to study the social structure of a psychiatric in-patient ward). The research questions are usually discovery-oriented. Participant observation is often conducted within the grounded-theory approach (see Chapter 5), in which the theory evolves as the study progresses.

Pragmatics

As we described in Chapter 3, you often gain access to the research setting via a gatekeeper, for example an administrator or senior doctor who decides whether to allow you into the organization. It is worth starting field notes at this point, since the process of negotiating entry says much about the workings of the organization that you are entering. Organizations that are conflict-ridden, suspicious, or highly bureaucratic will each have their characteristic ways of admitting (or excluding) outsiders.

Once in the setting, you may develop a set of *key informants* who provide in-depth accounts. However, be wary of over-reliance on any one informant, or of implicitly selecting informants whose views agree with your own; try to obtain stories from a variety of perspectives.

In the participant observation tradition, the period of observation is fairly extensive, usually lasting several months. The researcher is initially passive and works at establishing rapport, and attempts to avoid being forced into roles (e.g., "volunteer"). Some guidelines (e.g., Taylor & Bogdan, 1998) suggest that the researcher limit observation sessions and observe at different days and times (e.g., nights, weekends).

It is useful to pay special attention to any unusual use of language in the setting, as this can often be a clue to important aspects of its structure (Taylor & Bogdan, 1998). The vocabulary used by staff to refer to clients may give important clues to their underlying feelings towards them. For example, do staff in a drug dependency unit refer to their clients as "patients," "addicts," or "junkies," or is there some local terminology by which they distinguish between different types of clients?

Field Notes

The observations are recorded in the form of field notes, which describe the setting and the people in it (possibly including a diagram), as well as their verbal and nonverbal behavior (Emerson, 2001; Emerson, Fretz, & Shaw, 2011; Taylor & Bogdan, 1998). Good field notes bring the scene to life. In addition, things that do not make sense should be recorded for later clarification, and your own actions should also be noted, in order to help to judge your effect. Finally, it is important to differentiate the behavior you are observing from your own reactions and interpretations; the latter should be noted and labeled as, for example, "Observer's Comments." As in all research, try to separate description from evaluation and be aware of how your preconceptions may be influencing your observations.

The researcher does not usually take notes or make recordings during the observation period, as this often distracts those being observed and is more likely to influence their behavior. Part of the skill of being a participant observer lies in developing your memory. In order to prevent memory overload, limit your time in the setting to an hour or two and write the notes immediately after leaving the field. Remembering key words and drawing a diagram of the setting are useful strategies. The guiding principle (which applies to other areas of life too) is: "If it's not written down, it never happened."

To illustrate, here is an excerpt from field notes taken during Taylor and Bogdan's (1998) study of institutions for the mentally retarded (UK: people with intellectual disabilities). The field notes include detailed description as well as the observer's comments (labeled as O.C.):

> As I get to the dayroom door, I see that all the residents are in the room. I can only see two attendants: Vince and another younger man. (O.C. It's interesting how I automatically assume that this other man is an attendant as opposed to a resident. Several hints: long hair, moustache, and glasses; cotton shirt and jeans, brown leather boots. He's also smoking a cigarette, and a resident, Bobby Bart, is buffing his shoes with a rag. Thus this attendant's dress and appearance differ from that of the residents.) Vince, who is 21, is wearing jeans, brown leather boots, and a jersey that has "LOVE" printed on it. He has long hair, sideburns, and a moustache.
>
> I wave to Vince. He half-heartedly waves back. (O.C. I don't think that Vince has quite gotten used to me coming.) The other attendant doesn't pay any attention to me.
>
> Several residents wave or call to me. I wave back.
>
> Kelly is smiling at me. (O.C. He's obviously happy to see me.) I say to Kelly, "Hi, Bill, how are you?" He says, "Hi, Steve. How's school?" "OK." He says, "School's a pain in the ass. I missed you." (O.C. According to the attendants, Kelly attended school at the institution several years ago.) I say, "I missed you too."
>
> I walk over to Vince and the other attendant. I sit down on a hard plastic rocker between Vince and the other atten., but slightly behind them. The other atten. still doesn't pay attention to me. Vince doesn't introduce me to him.
>
> The smell of feces and urine is quite noticeable to me, but not as pungent as usual.
>
> I, along with the attendants and perhaps five or six residents, am sitting in front of the TV, which is attached to the wall about eight feet off the floor and out of the residents' reach.
>
> Many of the 70 or so residents are sitting on the wooden benches which are in a U- shape in the middle of the dayroom floor. A few are rocking. A couple of others are holding onto each other. In particular, Deier is holding onto the resident the attendants call "Bunny Rabbit." (O.C. Deier is assigned to "Bunny Rabbit"—to keep a hold of him to stop him from smearing feces over himself.)
>
> A lot of residents are sitting on the floor of the room, some of these are leaning against the wall. A few others, maybe 10, just seem to be wandering around the room. (Taylor & Bogdan, 1998: 266)

This extract illustrates the richness and vividness of observational data, but also its wide-ranging and unfocused nature. It is difficult at first reading to know how to make sense of it all.

Ethical Issues

Two major ethical issues arise with participant observation: whether the observation should be overt or covert, and what to do when you observe illegal or immoral acts. These issues also may occur in quantitative observation, but have been more salient in the participant observation literature. (We discuss ethical issues in general in Chapter 10.)

There are many examples in the literature where the observers concealed the fact that they were conducting research. This was usually done in settings where reactivity of measurement would have been a major problem. Two well-known historical examples are Humphries's (1970) study of homosexual activity in men's public lavatories, which generated an enormous debate over its ethics, and Rosenhan's (1973) pseudopatient study. Researchers conducting covert observations argue that the nature of their research precludes their asking the consent of those being observed and that their findings bring benefits that justify the deception. However, such deception is contrary to the ethical principle of informed consent and lays the researcher open to charges of being a spy or a voyeur. Proposed research involving covert observation should be subjected to thorough consultation on its ethical status.

A related issue is what to do in cases where you observe illegal or immoral acts. For example, in the above study of state institutions for the mentally retarded, Taylor and Bogdan (1998) observed attendants beating and abusing residents. They argue that stepping out of the observer role could have had some short-term benefits, in that they may have been able to stop the specific instance of abuse. On the other hand, it would have effectively terminated their project, which had the potential to end the abuse by documenting it, and possibly leading to permanent changes in the institutional structure to prevent future abuse. In cases like this, researchers face complex dilemmas to which there are no clear-cut answers. In such situations, it is always important to seek out research colleagues or supervisors to help you explore the ethical issues.

Quality of the Data

Finally, participant observation raises some specific issues of reliability and validity:

- *Reliability.* It is hard to check observer accuracy in participant observation. Although it is theoretically possible to have two or more simultaneous observers in the setting, this is rarely done in practice. It is, however, quite possible to replicate an observation in several settings at once, as was done in Rosenhan's (1973) pseudopatient study. Reliability in participant observation can also be examined by considering the consistency of behavior across time.
- *Observer bias.* As we have discussed above, all observation is to some extent biased, in the sense of being governed by previous understandings and expectations, whether consciously by theory, or unconsciously by ethnocentricity or general worldview. These biases affect how observers see things; reports from informants may also be biased because of their own particular perspectives and special interests (Kurz, 1983).
- *Reactivity.* The presence of an observer may alter the behavior of those being studied. This reactivity problem is not unique to qualitative approaches; it occurs with all types of observation. Participant observers may be able to mitigate it by allowing time for the people in the setting to become accustomed to them. Some researchers try to get around this problem by conducting covert observations, but this of course raises ethical problems (see above).

Text-based Research

The second area of qualitative observation is text-based research. Texts of written or spoken forms of communication provide the basis for a loosely organized set of research approaches that we have referred to as *language-based approaches* (see Chapter 5). These texts include transcripts of conversations, official documents, television broadcasts, and newspaper articles.

Such methods are not new. There is a long tradition of discourse analysis within sociology, heavily influenced by linguistics (Labov & Fanshel, 1977; Potter &

Wetherell, 1987; Sudnow, 1972). In the middle of the 20th century, some psychologists (e.g., Allport, 1942) advocated "personal documents" research, although this did not become a mainstream avenue for research at the time.

Text-based approaches involve a close study of the text under examination: the focus is on its structure, or its underlying assumptions and meanings, rather than on what it is supposed to be describing. This differs from self-report in that the intention is to analyze the text as a sample of communication, rather than to understand what the speaker or author is thinking or feeling.

Sources of Texts

Studies using these approaches can draw on a wide range of possible sources:

- *Personal documents.* The classic personal documents approach collected letters, diaries (e.g., of language acquisition), or other personal accounts (e.g., William James's, 1902, *Varieties of Religious Experience).*
- *Administrative records or archives* can be used, for example, court records, and also, within the limits of confidentiality and consent, clinical case records including intake reports and contact notes (e.g., Todd, Kurcias, & Gloster, 1994).
- *Cultural texts* include widely disseminated published records. These could be self-help books, political speeches, entertainment media (e.g., TV, movies, computer games), or educational texts.
- *Visual representations* may include photographs, advertisements, and home videos.
- *Naturally occurring interactions.* Researchers may be interested in ordinary, everyday conversations (e.g., children's talk in a school playground) or specialized ones (e.g., psychotherapy interactions or telephone helplines).
- *Collected or "found" examples of language usage* include slang and metaphor, for example, to examine how people talk about mental illness. Researchers can also collect examples of a phenomenon of theoretical or practical interest. This is a heritage from natural history and linguistics in which research was done by collecting examples (e.g., Freud's collection of "slips of the tongue").
- *Invited or constructed texts* are set up or solicited by the researcher. For example, the researcher might ask participants to write personal accounts of difficult experiences, or might set up a family interaction task (e.g., to generate parent–teenager conversations). Qualitative interview transcripts can also be approached from a discourse rather than a self-report point of view (i.e., with a focus on how things are talked about, rather than on the content per se).

Examples

Historical research is the prototypical example of text-based research. From the point of view of psychology, historical research can be valuable for bringing the past alive, in order to help us understand the historical context of important ideas in psychology (e.g., the origin of psychoanalysis, J. Schwartz, 1999), for exemplifying important psychological processes in the lives of individuals (e.g., Erikson's (1969) psychobiography of Gandhi), and for showing the psychological influence of the past on people's current experiences (Zeldin, 1994). Historical research generally makes use of multiple sources (e.g., the range of types of texts mentioned above), and in some cases, qualitative interviews (as in oral history). The power of careful historical analysis is illustrated by Runyan (1982), in his analysis of the large number of explanations for why the painter Vincent van Gogh cut off his ear. By careful analysis of historical records, Runyan was able to rule out all but a few explanations as being inconsistent with what is known about van Gogh's life.

Three other examples illustrate the wide variety of discourse analytic studies, particularly in relation to clinical psychology. The first is Madill and Barkham's (1997) study of a successful case of psychodynamic psychotherapy, which we outlined in Chapter 5. The second is Labov and Fanshel's (1977) classic study: a book-length report which analyzed a single 15-minute segment of a psychotherapeutic interview. They used microanalytic methods to examine both the content of the speech and also its paralinguistic features, such as voice spectrogram patterns. They revealed how much rich meaning is carried in subtle, barely noticeable variations in speech, and demonstrated the complex nature of the mutual responsiveness between client and therapist.

The third example is Harper's (1994) study of how five mental health professionals used the term "paranoia." His analysis identified a number of discourses, or systematic ways of talking, about paranoia. For example, these included the "empiricist" account, where lists of characteristics and symptoms were the focus, and the "contingent" account, where personal and social values were acknowledged in interviewees' discussions. These discourses, and the ways in which professionals moved between them, seemed to serve particular functions, such as the assertion of professional legitimacy.

QUANTITATIVE OBSERVATION

- Quantitative observation involves the systematic counting or timing of specified behaviors.
- A clear operational definition of the behaviors is essential (but not always easy).
- Lower levels of inference usually lead to better reliability.
- There are several different methods of conducting observations, for example, interval recording and time sampling.
- The observers (also known as raters or judges) need to receive careful training and monitoring throughout the study.

The essence of quantitative observation (aside, of course, from its using numbers) is that the variables being observed and the methods for observing them are explicitly defined. It is characterized by the use of predefined behavior codes by trained observers (also called *raters*, *coders*, or *judges*) of demonstrated reliability. Quantitative observations are usually targeted at a small number of prespecified behaviors, although they sometimes can be more wide-ranging.

For instance, researchers observing aggression on a children's playground must specify precisely what constitutes and what does not constitute an aggressive act: when, for example, does a touch become a push or a punch? They must also specify which aspects of such acts will be recorded, for instance, type, frequency, or intensity.

Thus, compared to qualitative observation, quantitative methods represent a gain in precision at the expense of a narrowing of scope and context.

Quantitative observations can be used to address several different types of research question. For example, they can be used for questions of description (e.g., which types of verbal response modes are used in child psychotherapy?), for sequential analysis (e.g., which types of client response are most likely to follow a therapist interpretation?), and for questions of covariation (e.g., does observer-rated empathy correlate with treatment outcome?). A further use is the assessment of therapist adherence or competence, to answer evaluation questions (e.g., is this therapist delivering this type of treatment at an adequate level?).

Background

Historically, quantitative observation methods developed independently in four different applied areas: behavioral observation, psychotherapy process research, developmental psychology, and content analysis in communication. However, despite differences in language and underlying philosophy, many of the same methodological issues apply in all four areas. We will mostly draw on examples from behavioral observation, as that is where the method is most systematically articulated.

Behavioral observation has its conceptual roots in methodological behaviorism, which argues that psychology should restrict itself to observable behavior (see Chapter 4). Also, Mischel's (1968) argument, that the validity of traditional, trait-based assessment procedures was unacceptably low, gave an impetus to the development of practical methods for behavioral assessment in the clinical context. These methods attempt to eliminate inferences to internal constructs (Goldfried & Kent, 1972). There is now a substantial practical literature on behavioral observation in clinical work (e.g., Bellack & Hersen, 1988; Hayes et al., 1999; Haynes & Heiby, 2004; Haynes & O'Brien, 2000). Since, for behaviorists, research and practice are closely related, many of the procedures can equally well be applied in research.

Psychotherapy process research began with the work of Carl Rogers and the client-centered group in the 1940s and 1950s. These were the first researchers to study recordings of actual therapeutic interactions, and the first to quantify aspects of the therapeutic relationship, such as therapist empathy (Kirschenbaum, 1979). Subsequent investigators have examined an enormous number of different process variables, ranging from global constructs, such as the quality of the therapeutic alliance, to specific types of responses used by the therapist and client (Greenberg & Pinsof, 1986).

Developmental psychology researchers often employ observational methods, partly for the obvious reason that infants and very young children have insufficient language abilities to use self report methods, but also as a way of assessing aspects of parent–child interactions that cannot be obtained via self-report (e.g., parental sensitivity). The most famous example is the Strange Situation Test (Ainsworth et al., 1978), which involves briefly separating an infant from his or her parent and then observing their dyadic behavior when they are reunited. From these observations the child can be placed in one of four attachment categories (e.g., secure, anxious-ambivalent) with respect to the parent in question.

Content analysis arose out of mass media communication research, which uses such material as newspapers or transcriptions of broadcasts as its subject matter (Joffe & Yardley, 2004; Krippendorff, 2013; Neuendorf, 2002). For example, newspaper stories about mental illness might be content-analyzed according to the underlying etiological model they espoused. However, the raw material need not be restricted to the mass media. Content analysis can be used with self-report data, transcriptions of meetings, etc. For example, Fewtrell and Toms (1985) used content analysis to classify the discussion in psychiatric ward rounds into categories such as medical treatment, mental state, and social adjustment. Content analysis provides a useful means of bridging quantitative and qualitative approaches, in that it applies quantitative analysis to verbal (qualitative) descriptions (see Chapter 5).

Procedures for Conducting Observations

As we discussed in Chapter 6 in the context of self-report measures, it is usually better to use an existing measure than to attempt to develop your own. Measure development is time-consuming and difficult, and it is hard to publish work with unfamiliar measures. This is equally true in the context of quantitative observation; if at all possible, it is better to use an existing coding manual and rating scheme with established inter-rater reliability. We discuss observational measures here from the viewpoint of the researcher developing a measure; this viewpoint is taken partly in order to clarify the process involved in measure development, and partly to provide guidelines for measure development research.

Operational Definitions

The first step in quantitative observation is to operationally define the behavior to be observed. The goal is to specify the behavior sufficiently well, so that it can be observed with high inter-rater reliability. Often this means that the behavior should be defined so that it can be rated without the raters having to make large inferences, but for some variables this may not be possible. Giving clear definitions is harder than it seems, as even apparently simple behaviors such as head nods or eye contact, or giving advice in therapy, pose difficulties in delineation. More inferential constructs, such as the level of empathy offered by a therapist, are even more difficult to define.

Developing a good definition is an incremental process. It is often useful to start with informal qualitative observation, supplemented by a review of the literature on the variable of interest and similar observational measures. The researcher then develops an initial version of the codes and tries them out on some data. This leads to revision of the codes and an iterative cycle of testing and revision. When the researcher has a coding system that he or she can use, the next step is to attempt to teach the codes to raters, who then test them out on data. This leads to a further cycle of testing and revision, which improves the likelihood that others besides the researcher will be able to use the measure (a form of inter-observer generalizability referred to as *portability*). Finally, the researcher utilizes the measure in a study, the results of which may suggest yet more revisions, and so on.

Since many different dimensions of behavior can be examined, it is useful to have a framework to help guide one's choices. Table 7.1 gives one such framework, adapted

Table 7.1 Five dimensions of observed behavioral process

1. *Perspective of Observation*: What is the point of view of the person doing the observation? (varies with involvement and role in setting, expertise)
   a. Researcher (trained observer)
   b. Expert participant (e.g., therapist, teacher)
   c. Index participant (e.g., client, student)
   d. Secondary participants (in supportive roles, e.g., family member)

2. *Person/Focus*: Which element of the behavioral process is studied?
   a. Index participant (client or client system; i.e., individual, family)
   b. Expert participant/system (clinician/clinical agency; e.g., therapist, teacher, clinic)
   c. Interaction of index and expert participant (e.g., quality of relationship, "fit")

3. *Aspect of behavior*: What kind of behavior or process variable is studied?
   a. Content: *What* is said, meant or expressed (type of content: ideas, themes)?
   b. Action/intention: What is *done* by what is said (actions or behaviors carried out by participants, including intentions, tasks, response modes)?
   c. Style: *How* is it done, said, or expressed (e.g., duration, frequency, intensity, paralinguistic and nonverbal behavior, vocal quality, apparent mood, interpersonal manner)?
   d. Quality: How *well* it is done, said, or expressed (e.g., accuracy, appropriateness, acceptability, skillfulness)?

4. *Unit Level*: At what level or "resolution" is the process studied?
   a. Sentence (idea unit): A single expressed or implied idea.
   b. Action/speaking turn (interaction unit): A response by one person, preceded and followed by actions by another person or different actions by the same person.
   c. Episode (topic/task unit): A series of action/speaking turns organized by a common task or topic, within an occasion.
   d. Occasion ("scene" unit): A time-limited situation in which two or more people meet to do something (e.g., session).
   e. Relationship (interpersonal unit): The entire course of a relation between two people.
   f. Organization (institution unit): A system of relationships organized toward a specific set of goals and located in a setting (e.g., a clinic).
   g. Person (self unit): Includes a person's system of relatively stable beliefs and characteristics and history of self-, other-, and organizational involvement.

5. *Sequential Phase*: What is the temporal or functional orientation taken toward a unit of process (i.e., toward what happened before, during and after the unit)?
   a. Context ("antecedents"): What has led up to a unit of process (e.g., previous speaking turn, earlier relationships)?
   b. Process ("behaviors"): The process that is targeted for study at a given level (unit).
   c. Effects ("consequences"): The sequelae of a unit of process (e.g., reinforcement, treatment outcome).

(Adapted from Elliott, 1991, by permission of the Guildford Press.)

from research on psychotherapy process (Elliott, 1991). Similar schemes could easily be constructed for other content areas of observation, e.g., children's behavior in the classroom or family interaction. Which aspects of which dimensions are important depends partly on the variables being observed and partly on the research questions.

Methods of Observation

Having specified the dimensions of the behavior to be observed, the next step is to choose an observational method. There are several choices (Bakeman & Quera, 2011; Cone, 1999; Haynes & O'Brien, 2000; Ostrov & Hart, 2013).

- *Narrative recording,* that is, writing an account of what happens, is equivalent to qualitative observation. It is used in the behavioral observation and ecological psychology traditions (e.g., Bakeman & Quera, 2011; Barker, et al., 1978). It is useful for hypothesis generation, measure development, and for arriving at ideas about causal relationships (in behavioral terms, the antecedents, behaviors, and consequences). It is also good for low-frequency behaviors. However, it is difficult to assess the reliability of such observations. Narrative recording is often a preliminary step to developing more structured methods of observation.
- *Event recording* yields the simplest form of frequency data. The observer counts every occurrence of the behavior within the entire observation period. For example, if the observation is focusing on therapist response modes used during a 50-minute therapy session, the final frequency count might be 35 questions, 32 reflections, five advisements, four interpretations, and one self-disclosure. The advantages of event recording are that it is simple and that it can be done alongside other activities; the disadvantages are that you cannot analyze sequences or other complexities and that it is hard to keep observer attention or assess observer reliability at the event level.
- *Interval recording* divides the observation period into equal intervals (e.g., a 50-minute therapy session might be divided into 10 five-minute intervals) and the number of behaviors is recorded during each one. In *whole interval sampling,* the behavior is only recorded if it is present for the whole of the interval, as opposed to *partial interval sampling*, when it can be present for any part of the interval. The advantages of interval recording are that it allows you to analyze sequences and that it gives a rudimentary estimate of both the frequency and the duration of a behavior. It may be adapted to record several behaviors concurrently. Having timed intervals also helps keep the observers alert. The disadvantages are that it requires more observer effort, as you have to attend to timing as well as to the behavior.
- *Time sampling*. Observations are made only at specific moments of time, for example, every five minutes or every half-hour. When observing large groups, *scan sampling* can be used, where each member of the group is observed sequentially. For example, Hinshaw, Henker, Whalen, Erhart, and Dunnington (1989) used scan sampling to observe the social interaction of hyperactive boys. The advantages of time sampling are that it yields a direct measure of the prevalence of a behavior in a group and that it is good for high-rate, continuous behaviors; it also means that raters do not have to maintain continuous attention. The disadvantage is that low-frequency behaviors may be missed, as they might only occur between the observation times.

- *Sequential act coding* records events in the order in which they occur. In contrast to event recording, it usually requires a comprehensive coding system to cover all possible events. (Event recording may just focus on one or two events, for example, specific aggressive acts in a school classroom.) To take a simplified example, researchers may classify events in a therapeutic interaction into client speech (C), therapist speech (T), and silence (S). Then a sequential act coding record might look like this: C,T,S,C,S,C. … This strategy is ideal for sequential analysis, because it relies on natural units (such as talking turns), not artificial units (such as time segments). However, disagreements on where the units begin and end can complicate reliability, and the method is inefficient if you are not interested in sequences.
- *Duration recording* is similar to sequential act coding, except that the focus is on timing the occurrence of a single behavior rather than categorizing events into codes. You can measure both *duration,* the interval between the start and the end of each behavior, and *latency*, the interval between behaviors. For example, Brock and Barker (1990) used this method to study the amount of "air time" taken up by each staff member during team meetings in a psychiatric day hospital.
- *Global rating scales,* in which the observer makes an overall judgment, often of the quality of the behavior, are usually based on a long period of observation. Clinical examples include the Brief Psychiatric Rating Scale (BPRS: Overall & Gorham, 1962), which rates several dimensions of psychiatric symptomatology, and the Global Assessment Scale (GAS: Endicott et al., 1976) which rates overall psychiatric impairment. Global ratings, for example, of therapist treatment adherence or competence, have often been used in therapy process research, for example the Revised Cognitive Therapy Scale (CTS-R: Blackburn et al., 2001). Although they use behaviorally anchored rating scales, these are still less precise than the behavioral observation methods, in that the observer is being asked to quantify an impression or judgment. On the other hand, global ratings are useful for complex or inferred constructs and can provide useful summaries of events. Many global rating scales have acceptable reliability.
- *Environmental measures.* Finally, an interesting category of observation focuses on the psychological environment as a whole, rather than specific individuals within it. Procedures include *behavioral mapping*, where the observers record the pattern of activity in a given environment. For example, Kennedy, Fisher, and Pearson (1988) used behavioral mapping to study the patterns of patient and staff activity in a spinal cord injury unit over the course of a single day.

  Environmental observation may also involve the use of *unobtrusive measures* (Webb, Campbell, Schwartz, & Sechrest, 1966), in which features of the physical environment are used to yield data on patterns of activity. Classic examples of unobtrusive measures are using the wear and tear on a carpet as an index of the popularity of museum exhibits, and using the accretion of graffiti as an index of youth gang activity. Italian drugs researchers (Zuccato et al., 2005) have cleverly applied this idea in order to estimate the level of general population cocaine usage, by measuring the concentration of the drug's urinary metabolites in the waters of the River Po.

Mechanics

The mechanics of recording the observations need to be as simple as possible, so that the recording does not interfere with making the observations themselves. Possible aids include coding sheets, stopwatches, counters, and electromechanical devices, including computer software (e.g., ObsWin, ODLog, or The Observer XT: see Kahng & Iwata, 1998). The observations may be conducted in real time, or the interactions may be audio or video recorded for subsequent observation and analysis.

It is not always necessary or possible for the researchers to conduct the observations. An alternative is for participants to carry out the observations themselves, using *self-monitoring* methods (Korotitsch & Nelson-Gray, 1999; Piasecki, Hufford, Solhan, & Trull, 2007). For example, an evaluation of couples' therapy might include the participants keeping written records of the number and type of arguments that they have over the course of several weeks. Self-monitoring can also be done by proxy: parents, for instance, could keep records of their child's sleep problems. The advantage of self-monitoring is that it allows the researcher to obtain observational data over long time periods and also from private settings. It can also be adapted to monitor cognitions or feelings, using *ecological momentary assessment* procedures (Shiffman, Stone, & Hufford, 2008). For example, in a study of obsessive-compulsive disorder, participants might be asked to record their moment-by-moment intrusive thoughts in real time in their natural environment.

If you have sequential data from your observations, for example, if you use interval or time-sampling methods, you can do more complex analyses of how the behaviors develop over time. This is a technical topic involving special statistics; Gottman and Roy (1990) describe some of the options.

Reliability and Validity Issues

An advantage of quantitative observation methods is that they facilitate the calculation of reliability (see Chapter 4 for a discussion of the statistical aspects of assessing inter-rater reliability). One practical problem is *observer drift*, where observers start out with high reliability, but then tend to develop idiosyncratic rules or become careless as the observation proceeds. To prevent this occurring, it is important to continually monitor the observers' reliability.

The main validity issue, aside from problems with the operational definition of the variables, is the *reactivity of observation*. As we discussed above, in the context of qualitative observation, the act of observing may alter the behavior being observed. The only solution is to make the observations as unobtrusive as possible, and to allow time for the people being observed to become habituated to the observers' presence. This may be easier with qualitative observation, which is usually done to a more leisurely timetable.

Practical Suggestions for Working with Raters

Researchers have various strategies available for maximizing the reliability and validity of observer ratings. These include:

- design or selection of measures with clear, well-defined variables and good examples of categories;
- careful selection of raters; and
- thorough training and management of raters.

Some suggestions on how best to work with raters are given below (see also Cone, 1999; Hartmann, Barrios, & Wood, 2004; Moras & Hill, 1991). Many of these considerations also apply to the use of multiple analysts in qualitative research (Hill, 2012).

Rater selection. It is usually better to work with motivated volunteers, such as top students in advanced undergraduate seminars, who are interested in a career in clinical psychology. Occasionally you may have to drop a rater's data later due to consistent unreliability, so it is best to start out with at least three and preferably four raters.

Training. It is a good idea to begin with a didactic presentation and modeling of the rating process, followed by group rating. This is followed by extensive practice, including weekly feedback on progress and problems. The SPSS Reliability procedure offers useful analyses for this, providing solid evidence both of progress and of problems, which can be shared with the group. For example, reliability checks will tell:

- which categories or dimensions show reliability problems;
- whether a reliability problem is general (spread across all raters) or specific (restricted to one or two raters);
- if a rater has misunderstood a category;
- if two raters have formed a clique which sets them apart from everyone else; and
- if particular raters differ greatly in their base rates for a category.

Training should continue until ratings on all variables reach an acceptable standard of reliability. The statistic used to assess reliability depends on the scale of measurement. For nominal scale data, Cohen's kappa (which corrects for chance agreement) is used; for interval data, Pearson's correlation can be used for evaluating the reliability of single raters compared to other raters (Bakeman & Quera, 2011; see also Chapter 4). When ratings are to be combined across raters, Cronbach's alpha or an intra-class correlation coefficient can be used to estimate the reliability of the combined ratings (Bakeman & Quera, 2011; Shrout & Fleiss, 1979).

Management. The management and nurturing of raters is at least as important as their selection and training. To foster the "research alliance," communicate to the raters that their views will be taken seriously and encourage them to contribute to the refinement of the rating system. Regular meetings and feedback during the rating process help prevent alienation and produce more reliable and valid data. As far as practicable, raters should feel part of the whole research process, including the conceptual framework and the research questions (unless it is necessary to keep them blind to the hypotheses or questions), and analyses and interpretation; this may occasionally also include co-authorship, if important contributions are made to the study, as is the case with multiple qualitative analysts.

CHAPTER SUMMARY

This chapter has examined when and how observational methods of measurement might be used. The advantage of observation is that it provides a direct measure of behavior, thereby overcoming some of the validity problems of self-report that were discussed in the previous chapter. It is useful when precise records of behavior are needed, or for studying behaviors that are not amenable to self-report (e.g., non-verbal behavior, or physiological responses).

As in self-report, there are both qualitative and quantitative approaches to observation. The main qualitative approach is participant observation, which derives from ethnographic approaches in anthropology. Text-based methods can also be considered as observational in nature.

Quantitative approaches use structured methods to give precise counts of behavior. They have been developed in four disparate areas—behavioral observation, therapy process research, developmental psychology, and content analysis—but the methodological issues are similar in all applications. There are a number of different methods for conducting quantitative observation, for example, interval recording and time sampling. The choice between them depends on the nature of the research questions and the resources needed to make the observations. For all types of observation, careful selection, training, and monitoring of raters is important to achieve good reliability.

FURTHER READING

There is a good treatment of participant observation in several texts (e.g., Angrosino, 2007; Emerson, 2001; Patton, 2002; Taylor & Bogdan, 1998). We recommend perusing some of the classic studies using this method, such as Goffman (1961) and Whyte (1943), as they are mostly stimulating and readable. The classic text on personal documents methods is Allport (1942); for further discussion of text-based methods, see Taylor and Bogdan (1998) and Potter and Wetherell (1987).

Quantitative observational methods are reviewed by Bakeman and Quera (2011), Cone (1999), Haynes and O'Brien (2000), and Ostrov and Hart (2013). Greenberg and Pinsof (1986) review measures for use in psychotherapy process research, while Llewelyn and Hardy (2001) give a brief introduction to therapy process research in general.

QUESTIONS FOR REFLECTION

1. Why is there so little research in psychology using observation as opposed to self-report? What might be gained by more balance between self-report and observation research? What could be done to encourage more observation research?
2. What would a participant observation study on your topic look like? What kind of setting would you need to enter? Who would you observe? What would you look for? What kinds of research questions would this allow you to answer?
3. Think about doing text-based research on your topic: What kinds of texts might bear on your research topic? How could you obtain them? What could you find out from them?
4. How would you go about turning your research topic into a behavioral observation study? Would measure development be a necessary first step? If so, what constructs would you operationalize? What situations might be suitable for carrying out your observations?

8
Foundations of Design

KEY POINTS IN THIS CHAPTER

- Design, in the sense we are using it here, refers to the logical structure of the study.
- Nonexperimental designs are ones in which the researcher gathers data without making any active intervention. They can be classified into descriptive and correlational designs, according to the type of analysis conducted.
- The golden rule is "correlation does not equal causation."
- There are several possible models of the causal relationships of two or more variables, including ones with mediator and moderator variables.
- Campbell (e.g., Cook & Campbell, 1979) made two major contributions to the study of design: (1) the classification of validity types; and (2) the analysis of quasi-experimental designs.
- Cook and Campbell's four validity types apply to generalized causal inference: they are statistical conclusion validity, construct validity, internal validity, and external validity.
- Experimental designs are ones in which the researcher makes an active intervention or manipulation. They are classified into randomized and nonrandomized (quasi-experimental) designs, according to whether or not there is random assignment to experimental conditions.
- There are several commonly used nonrandomized designs. Each has its associated validity threats.
- Randomized designs facilitate inferences about causality. They are central to the discussion of evidence-based practice and empirically supported therapies. However, they also have limitations and validity threats, and cannot be regarded as a scientific panacea.

Research Methods in Clinical Psychology: An Introduction for Students and Practitioners,
Third Edition. Chris Barker, Nancy Pistrang, and Robert Elliott.

Companion Website: www.wiley.com/go/barker

Previous chapters have covered the groundwork and measurement phases of the research process; the present one begins our examination of the design phase. This order, first measurement then design, roughly corresponds to how you go about planning an actual research project: you usually begin by thinking about which variables interest you and how to measure them; next you think about design. From now on, we will put measurement behind us, working, for didactic purposes, on the assumption that it is unproblematic, even though we recognize that this is often not the case.

To clarify what we mean by the term "design," think of the questions "what, when, where, and who?" about a research project. Measurement is the "what" aspect: what is being studied, what measurements are made. *Design*, in the sense we are using it here, denotes "when, where, and on whom" the measurements are taken: the logical structure that guides the data collection of the study. It covers such topics as the relative merits of large-sample versus single-case studies, what type of control group, if any, is required, and who the participants will be.

The terms "research design" and "experimental design" are also sometimes used in a broader sense to denote everything to do with planning and executing a research project, synonymously with our use of the term "research methods." The more restricted sense of the term "design" that we are using here is consistent with its use in the statistical literature. It is usually clear from the context whether the broader or narrower meaning is intended.

Research designs can be classified into two fundamental types: experimental and nonexperimental. *Experimental designs* involve an active intervention by the researcher, such as giving one type of therapy to some clients and a second type to others, whereas *nonexperimental designs* simply involve measurement, without changing the phenomenon or situation to be measured. These two approaches to design reflect "the two disciplines of scientific psychology" (Cronbach, 1957, 1975). Experimentalists are often more concerned with examining the causal influence of external factors, which are amenable to experimental manipulation; nonexperimentalists are often more concerned with variation between people.

This chapter will examine nonexperimental and experimental designs, and their various subtypes, and will also look at some general principles for assessing validity in research designs.

NONEXPERIMENTAL DESIGNS

Nonexperimental designs can be classified, according to their aims, into descriptive and correlational designs. As is obvious from their names, descriptive designs usually aim simply to describe, whereas correlational designs aim to examine associations in order to make predictions or explore causal linkages.

Descriptive Designs

Examples of descriptive studies frequently appear in the mass media: public opinion surveys, in which respondents are asked which political party they intend to vote for; the national census, which reports, for instance, the percentage of people living in various types of accommodation; and national unemployment statistics. However, the importance of systematic descriptive research is generally overlooked by clinical psychologists, even though such research is often valuable as a preliminary step in understanding a phenomenon of interest. Some examples of descriptive studies are:

- Descriptive epidemiological research, which aims to document the incidence and prevalence of specified psychological problems.
- Consumer satisfaction research, which assesses clients' satisfaction with a psychological service.
- Phenomenological research, which aims to understand the nature and defining features of a given type of experience.

Quantitative descriptive studies report their results using *descriptive statistics*, naturally enough. This is a technical term covering such statistics as percentage, mean, median, incidence, and prevalence. However, it is rare to have a purely descriptive study, as researchers often want to examine the associations between two or more variables of interest. For example, in a consumer satisfaction study you may want to see whether there is an association between client satisfaction and various client demographic characteristics, such as gender or ethnicity. This leads on to the next type of study, the correlational design.

Correlational Designs

Correlational studies aim to examine the relationship between two or more variables: in technical language, to see whether they covary, correlate, or are associated with each other. Such studies are also called *passive observation* or *naturalistic* studies, in contrast to studies employing active methods of experimental manipulation. (Passive observation, as a research design, should not be confused with participant observation, which is a data-gathering method.) In correlational studies, researchers measure a number of variables for each participant, with the aim of studying the associations among these variables.

A well-known example of a correlational design is Brown and Harris's (1978) study of the social origins of depression, which looked at the association between women's depression, their experience of stressful life events, and vulnerability factors (such as low intimacy with the husband and loss of the mother before the age of 11). Correlational designs are often also used to examine individual differences, for example, in predicting which clients respond best to a psychological intervention. Examining such correlations is a common step in attempts to construct causal explanations. That is, one typically tries to predict what happens to whom (e.g., in therapy) in order to understand why it happens (e.g., what are the effective ingredients) or in order to improve an application (e.g., to learn how to enhance its outcome).

Measure development research, which aims to develop, evaluate or improve measures, uses both descriptive and correlational designs. As we discussed in Chapter 6, developing a new measure involves extensive testing of reliability and validity, using a correlational framework; it also involves providing normative data for the measure, using descriptive methods.

Correlational designs may be *cross-sectional*, in which all observations are made at the same point in time, or they may be *longitudinal*, in which measurements are made at two or more different time points. Correlational studies may use simple statistical measures of association, for instance, chi-square and correlation coefficients, or multivariate methods, such as multiple regression, factor analysis, and log-linear procedures. They may also use more advanced methods, which aim to map the underlying structure of complex data sets. These go under various names – path analysis, latent structure analysis, causal modeling, or structural equation modeling (e.g., Hoyle & Smith, 1994; Tabachnik & Fidell, 2013; Tomarken & Waller, 2005) – but the underlying logic is the same. They are used for evaluating how well conceptual models generated from previous research or theory fit the data.

Path analysis is both a method of conceptual analysis and a procedure for testing causal models. Its framework is a useful tool for planning out research, even if you never actually carry out a formal path analysis, in that it forces you to spell out your theoretical model. It is also useful for trying to conceptualize the results of correlational studies. The essence of path analysis is to tell a story in diagrammatic or flowchart form, showing which variables influence which others: the examples of different kinds of causal linkages that are given in the next section are depicted in the form of elementary path diagrams.

Correlation and Causation

The major drawback of correlational studies is that they cannot be used to make unequivocal causal inferences. The golden rule of research design is: *correlation does not equal causation*. Correlations are necessary but not sufficient for establishing causality. They may strongly suggest causal influences, but cannot firmly establish them, although the absence of a correlation generally does rule out a causal relationship. You will see this rule frequently ignored in popular journalism and sometimes in the professional literature too.

The existence and nature of causal relationships involves some difficult philosophical problems (Cook & Campbell, 1979; White, 1990). First, it is not clear exactly what psychologists mean when they use the words “cause” or “causality.” In the natural and biological sciences, causes are understood as mechanical or biochemical physical processes. In the 20th century, starting with Freud, psychologists extended the idea of causation to include mental mechanisms (Slife & Williams, 1995). Over the years, psychologists have used a wide variety of explanations to understand people, including intentions, developmental precedents, situational cues and opportunities, trait or diagnostic categories, unconscious meanings, biochemical processes, and genetic factors. Furthermore, there are various euphemisms for “cause” in psychology: the words “structural” (as in structural equation modeling) and “functional” (as in functional analysis) often stand in for “cause.”

Second, in psychology and epidemiology we are often dealing with probabilistic rather than deterministic causes. Thus, when we say that smoking causes lung cancer or that poverty causes ill-health, we are not talking about certain causation (there are always exceptions) but about increased risk. Similarly, in clinical psychology, when we say that an intervention causes change, we mean that the intervention sets the conditions for likely client change, but that change is not certain.

Third, going back to Bacon, Hume, and J.S. Mill (see Shadish, Cook, & Campbell, 2002; Haynes & O'Brien, 2000, Hill, 1965), philosophers and scientists have struggled with defining a set of conditions for inferring a causal relationship. Four generally agreed conditions are:

- *Covariation*: the two variables must consistently occur together. The stronger the correlation, the more convincing is the causal inference.
- *Precedence:* the hypothesized causal variable must reliably precede the effect variable.
- *Exclusion of alternative explanations:* other explanations for the observed covariation must be reasonably excluded.
- *Logical mechanism*: there must be a plausible account for the hypothesized causal relation.

Correlational studies can establish the first condition, that two variables, A and B, covary. Information relevant to the second condition, how they are ordered in time, may also be known. The third condition, that of eliminating alternative explanations, can be addressed to some extent with a correlational framework, although experimental designs are much more suited to this, while the fourth condition, providing a plausible account, can be derived from previous theory and research. (It is worth noting that this fourth condition has frequently been overlooked, and that many experimental designs are "causally empty" in that they shed no light on specific causal processes; see Elliott, 2002; Haynes & O'Brien, 2000.)

Let us take a simplified example, derived from early formulations of client-centered theory (Rogers, 1957). Suppose that variable A represents therapist empathy and variable B represents the client's outcome at the end of therapy, and that research has established a significant positive correlation between therapist empathy and client outcome. Then a number of inferences about their causal relationships are possible, some of which are depicted in the following simple path diagrams (in which an arrow indicates the direction of a causal relationship):

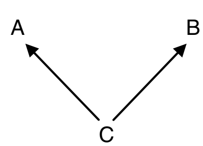
1. $A \longrightarrow B$

First it may be that A causes B: higher therapist empathy brings about better client outcomes.

2. $B \longrightarrow A$

On the other hand, it is also possible that B causes A: clients who are improving in therapy may tend to draw more empathic responses from their therapists.

3.



A very common situation is that A and B may both be caused by a third variable, C, such as client psychological mindedness. It is plausible that clients who are more psychologically minded could have better outcomes and also generate more empathy in their therapists. Thus the apparent causal relationship between A and B might be spurious: that is, entirely explained by the influence of the third variable, C. The presence of such third variables which provide competing causal explanations prevents the researcher from drawing accurate causal inferences and thus reduces the study's validity.

4. $A \longrightarrow D \longrightarrow B$

Yet another possibility is that A does not influence B directly, but only indirectly via D. Variables such as D are known as *mediator variables* (Baron & Kenny, 1986; Frazier, Tix, & Barron, 2004; Kazdin, 2007): they mediate (come in the middle of) the relationship between two other variables. For example, higher therapist empathy could lead specifically to increased client self-exploration, which could then lead to better client outcome. We would then say that the causal relationship between empathy and outcome was mediated by client self-exploration. The research enterprise is often characterized by the search for mediating variables: researchers do not simply let it rest when they observe a correlation, rather they attempt to understand the links in the chain between the two variables.

5.
$$\begin{tikzcd} A \arrow[rr] & {} & B \\ & E \arrow[u] & \end{tikzcd}$$

A fifth possibility is where the relationship between A and B differs according to the values of E. Variables such as E are known as *moderator variables* (Baron & Kenny, 1986; Frazier et al., 2004): they moderate the relationship between two other variables, acting like a gate or valve or volume control. E could represent the type of client presenting problem, for example, anxiety or depression, or a demographic variable such as age or gender. If, for example, therapist empathy led to better client outcomes in men but not in women, we would say that the causal relationship between empathy and outcome was moderated by client gender.

6. The five possible relationships we have reviewed all assume that the obtained correlation reflects some kind of causal relationship. There is, however, a sixth possibility, which is often overlooked, that the correlation may reflect a *conceptual confound* between A and B. In other words, it may be due to overlapping meaning between the two variables: that they are two aspects of the same construct. Thus, therapist empathy (particularly when rated by the client) may actually be a kind of outcome, which means that the correlation may simply reflect a correlation of one kind of outcome with another kind of outcome.

It follows from this that the art of research design is to collect data in such a way as to examine the influence of third variables and of mediator and moderator variables, and to evaluate for conceptual confounding, in order to be able to draw clear inferences about the relationships between the variables under study. Experimental designs can help to do this by systematically manipulating one or more variables at a time.

EXPERIMENTAL DESIGNS

The word "experiment" has the same root as the words "experience" and "peril": all derive from the Latin for try or test, with the connotation of risk or danger. To carry out an experiment is to put your hypotheses in danger of failing or being falsified (cf. Popper, 1963: see Chapter 2). Psychologists today usually think of an experiment as a study involving random assignment to two or more groups or conditions. However, this view is a recent development (within the past 60 years or so) and represents a methodological narrowing under the influence of positivism and biomedical research. Before that time, and today in its ordinary usage, an experiment referred to "the action of trying anything, or putting it to proof; a test, trial" (Oxford English Dictionary).

An experimenter interferes with the natural course of events, in order to construct a situation in which competing theories can be tested. These theories often concern causal influences between variables. In physics, formal experimentation began with Galileo, who attempted to test his theories of dynamics by rolling balls down inclined planes, initiating what we now call the hypothetico-deductive method. Before Galileo, science had relied on drawing generalizations from passive observations and from informal trial and error experimentation.

Experimental designs are of particular interest to clinical psychologists because therapeutic work itself can be thought of as an experimental intervention. In collaboration with the client, the therapist considers a problematic situation in the client's life, forms a hypothesis about what is causing it and what might be done to improve it, helps the client try to change something about it, and then observes the results. Here the tentative connotation of experiment is apt: if the intervention does not work, client and therapist then repeat the cycle by reformulating the problem, trying something else and once more observing the results. This experimental approach to therapeutic work lies at the core of the applied scientist model (see Chapter 2).

Most psychotherapy outcome studies use an experimental design. This chapter will use a comparative outcome trial by Dimidjian et al. (2006) as a running example (see box).

Terminology

Example of a clinical trial: Dimidjian et al. (2006)

Participants (N = 241) met DSM criteria for major depressive disorder. They were randomly allocated into one of four treatment conditions: (1) behavioral activation (2) cognitive therapy (3) anti-depressant medication, and (4) medication placebo. The main outcome measures, the Beck Depression Inventory (Beck, Steer, & Brown, 1996) and the Hamilton Rating Scale for Depression (Hamilton, 1960), were completed at baseline and mid- and post-treatment (approximately weeks 0, 8, and 16). (The results showed that, for severely depressed clients, both behavioral activation and anti-depressant medication were superior to cognitive therapy, but for the less severely depressed patients, there were no differences between treatments.)

There is a considerable amount of terminology in the experimental design area. The treatment or intervention that is varied by the experimenter is known as the *independent variable* or the *experimental condition* or the *experimental manipulation*; the measure of the independent variable's effect is known as the *dependent* or *outcome variable*. In the Dimidjian et al. (2006) study the independent variable, or experimental condition, was the type of therapy: behavioral activation, cognitive therapy, anti-depressant medication, or placebo. (Note that the term "group" is often used to denote one of the experimental conditions. It is potentially confusing in clinical applications, as it wrongly suggests that group rather than individual therapy was used.) The dependent variables were the two depression measures.

Frequently, in addition to one or more of the groups in the design being subjected to an experimental intervention, another group provides a *control group*, so called because it is used to rule out or *control* for the influence of one or more third variables. For example, the medication placebo group in the Dimidjian et al. (2006) study was used to control for the instillation of hope of successful outcome and also for the potential therapeutic benefits of the assessment interviews. It is important to keep in mind that some experiments ("quasi-experiments," see below) may use only one treatment condition. In the treatment literature, such one-group experiments are referred to as *open clinical trials* or *uncontrolled trials*, often used to provide an initial test of a promising treatment, prior to pitting it against a control group or other treatment (a *randomized clinical trial*).

Many types of experimental design are covered in the statistical literature (e.g., Field, 2013; Howell, 2010; Shadish et al., 2002; Winer, Brown, & Michels, 1991), such as factorial designs, repeated-measures designs and Latin squares. Here we will look at some of the simpler ones. The statistical method used in experimental studies is usually the analysis of variance (ANOVA), or related methods such as the t-test, multivariate analysis of variance (MANOVA), or the analysis of covariance (ANCOVA). However, before examining some specific types of designs, it is helpful to consider some general principles of validity, in order to provide a framework for thinking about the strengths and weaknesses of any given design.

Cook and Campbell's Validity Analysis

The work of Campbell and his collaborators (Campbell & Stanley, 1966; Cook & Campbell, 1979; Shadish et al., 2002) has been enormously influential: Cook and Campbell's (1979) book or its successor, Shadish et al. (2002), is required reading for all serious applied psychology researchers. Campbell's ideas were developed specifically in the context of designs that attempt to infer causality, which is why we are addressing them here under the heading of experimental designs. However, the concepts can be applied to all designs, descriptive and correlational as well as experimental. They are invaluable both for planning one's own research and for evaluating other people's.

The central thrust is an analysis of different types of design validity for generalized causal inference. We are now using the concept of validity in a broader sense than in Chapter 4, when we discussed the reliability and validity of measures; here we will be talking about the validity of the conclusions that you can draw from the study as a whole. Campbell and Stanley (1966) introduced the fundamental distinction between internal and external validity. *Internal validity* refers to the degree to which causality can be inferred from a study: that is, in the language of experimentation, is the independent variable producing the changes in the dependent variable? (It is not to be confused with *internal consistency*, which is a type of reliability.) *External validity* refers to the degree to which the results of the study may be generalized over time, settings or persons to other situations, for instance, whether the patients studied in a clinical trial are representative of the patients seen in other settings. A related concept is *ecological validity*, which assesses how artificial the measures and procedures of the study are, compared to the world outside of the research setting.

Cook and Campbell's (1979) expanded treatment considered *statistical conclusion validity* in addition to internal validity (both of which concern interpreting covariation) and *construct validity* in addition to external validity (both of which concern the generalizability of the study – construct validity can be seen as assessing generalizability to the underlying construct which the different measures attempt to tap). In this chapter, we shall principally look at internal validity. Construct validity, which derives from the work of Cronbach and Meehl (1955), was introduced in Chapter 4. External validity is covered in Chapters 10 and 12, and statistical conclusion validity, which concerns the appropriateness of the statistical methods, is also covered in Chapter 12.

Each of the four validity types can be defined by a key question (see Table 8.1); these questions are asked sequentially about any study that claims to examine causal relationships. The first one asks "Is there an effect there at all?" (statistical conclusion validity). If the answer to this is affirmative, the next question is "Is the effect causal?" (internal validity). Then you ask "What does it mean (both in terms of the outcome variables and the experimental manipulation)?" (construct validity). Finally, "Does it generalize?" (external validity).

The central dilemma in designing a study is that there is often a trade-off between internal and external validity. It is possible to achieve high internal validity in a laboratory, where the researcher can exert considerable control. A common criticism of social psychology experiments of the 1960s and 1970s was that, although they had achieved high internal validity by being conducted in a controlled laboratory setting with a homogeneous population (often young white male US undergraduates), they had in so doing sacrificed their external validity, that is, their generalizability or real-world relevance (Sears, 1986). In a nutshell, the designs were clever but artificial (Armistead, 1974; McGuire, 1973). The same criticism also applies to early analog studies of behavior therapy, which were conducted in artificial laboratory conditions with volunteer clients on specific phobias such as spider or public speaking fears (Shapiro & Shapiro, 1983). Conversely, field research, which is conducted in natural settings with clinical populations, usually has high external validity. Unfortunately,

Table 8.1 Four validity types (Cook & Campbell, 1979)

Validity type	*Defining question*
Statistical conclusion	Is there an effect?
Internal	Is it causal?
Construct	What does it mean?
External	Does it generalize?

this is often at the expense of lower internal validity, since experimental control is much more difficult to obtain in field settings, for a variety of reasons which we discuss below.

The main thrust of Cook and Campbell's work is that all designs are imperfect, but that it is possible to analyze systematically the potential nature and consequences of their imperfections, which are known as *threats to validity*. The researcher's task is to try to achieve an optimal design given the aims and constraints of the research. To quote one prominent psychotherapy researcher: "it is therefore impossible to design the perfect study. The art of outcome research design thus becomes one of creative compromise based upon explicit understanding of the implications of the choices made" (Shapiro, 1996: 202). Cook and Campbell's framework is an indispensable tool for thinking about the consequences of such compromises.

Cook and Campbell's Classification of Research Designs

In addition to analyzing validity issues, Campbell and his collaborators (Campbell & Stanley, 1966; Cook & Campbell, 1979; Shadish et al., 2002) also proposed a taxonomy of experimental designs. They introduced the fundamental distinction between *quasi- experimental* and *experimental* designs. Quasi-experiments are defined as "experiments that have treatments, outcome measures, and experimental units, but do not use random assignment to create the comparisons from which treatment-caused change is inferred" (Cook & Campbell, 1979: 6). However, in the light of our earlier discussion about the term "experiment" being too narrowly defined within psychology, it seems preferable to use the more precise terms nonrandomized and randomized designs instead of quasi-experiment and experiment. Cook and Campbell give an extensive listing of possible nonrandomized and randomized experimental designs. Here we will consider the most commonly used ones as illustrative examples.

Nonrandomized Designs

One-group Posttest-only Design

This rudimentary design can be depicted in the following diagram:

X O

In Cook and Campbell's notation, X stands for an experimental treatment, that is, something done to the participants, such as a clinical intervention. (The notation can also be extended to cases where X is not an experimental treatment, but rather some other event that occurs to the participants, such as a disease or a disaster.) O stands for an observation or measurement, of one or of several variables.

The one-group posttest-only design, originally labeled the one-shot case study by Campbell and Stanley (1966), is the simplest possible design. It is characterized as a quasi-experimental design because of the experimental intervention, X, although it can also be conceptualized as a type of descriptive study. One common application is in consumer satisfaction studies, in which clients are surveyed during or after a psychological intervention to find out how they felt about it. This design is useful for generating hypotheses about causation, but it is almost always insufficient for making causal inferences, because doing so invites the logical fallacy referred to as *post hoc ergo propter hoc* (because B happens after A, B must result from A).

However, as Cook and Campbell (1979) note, this design should not be dismissed out of hand in research aimed at testing causal explanations. It can be rescued if enough contextual information is available, and especially if one takes a detective work approach, looking for signs or clues about causality – what Cook and Campbell (1979) call "signed causes." In the clinical context, such signs might include post-treatment ratings of perceived change or retrospectively completed estimates of pre-treatment levels of functioning. (The detective metaphor is appropriate here, since Sherlock Holmes and his successors based their causal inferences – concerning who-dunnit – upon post-hoc data.) Shadish et al. (2002) note that the interpretability of this design is enhanced when the effect is clear and measured in multiple ways; this allows for a pattern-matching approach that compares effects to potential causes, as an epidemiologist works backwards from a disease outbreak to possible causes.

One-group Pretest–Posttest Design

$O_1 \quad X \quad O_2$

This design extends the previous one by adding a pre-measure, which then allows a direct estimate of change over time. It is often known as an *open clinical trial* or an *uncontrolled trial*. For example, this design may be used in evaluating the outcome of a clinical service. The psychologist might administer a measure of problem severity, such as the Generalized Anxiety Disorder scale (GAD-7: Kroenke et al., 2007), to all clients before and after therapy.

However, it is not immediately possible to attribute change in the outcome variables to the experimental treatment, X. This is because, as in the previous design, it is risky to infer *post hoc ergo propter hoc*. For example, a newspaper headline in a feature on mental health stated: "Despite being the target of suspicion, the evidence that antidepressants work is indisputable: more than two-thirds of people taking them recover" (London *Observer,* 12 January 1992). The inference appears to be that, since taking antidepressants is associated with a good chance of recovery, therefore they must cause that recovery. (To see the logical fallacy more clearly, try substituting taking antidepressants with some less obviously psychotherapeutic activity, such as watching television.) In addition, the implication that the antidepressants cause recovery is further called into question since the recovery rate of depressed people who did not take antidepressants is not supplied—perhaps two-thirds of them recover also. The availability of such data would result in a nonequivalent groups pretest–posttest design, described below.

Cook and Campbell (1979) provide a checklist of possible *threats to internal validity* in this design. For researching the effects of psychological interventions, the most important ones are:

- *Endogenous change,* which refers to any kind of change within the person. The most important instance is *spontaneous recovery*, also called *spontaneous remission*, which means recovery occurring with no apparent external reason.

- *Maturational trends* refer to the growth or maturation of the person. This is a special case of endogenous change. It is, of course, especially relevant to research with children, who may often "grow out of" their psychological problems.
- *Reactivity of measurement*, where the act of making a measurement changes the thing being measured. For example, there may be practice effects on a psychological test, where participants perform better on a second administration of a test because they have learned how to respond. As another example, there is evidence that participants who are interviewed as part of their initial assessment may experience a clinical benefit (Svartberg, Seltzer, Choi, & Stiles, 2001).
- *Secular drift*, that is, long-term social trends, taking place over a time scale of years. These are relevant if you are doing large-scale longitudinal research on public health interventions, such as on smoking or on safe-sex behaviors, where it is important to take such trends in the target outcome variable into account when looking at the impact of the intervention.
- *Interfering events*, that is, significant events other than the experimental intervention that occur between the pretest and the posttest. For example, fiscal changes such as increases in tobacco or alcohol taxes may reduce consumption; or an international crisis may increase general anxiety.
- *Regression to the mean*. Participants in clinical studies are often selected on the basis of their extreme scores on a measure of psychological distress. Clients for a therapy outcome study, for example, might be selected (or select themselves) on the basis of high scores on an anxiety scale. The regression to the mean phenomenon is due to unreliability of measurement, which means that scores on the posttest will tend to show that clients have improved, even if the therapy was ineffective. This is because the extreme scores at the pretest will have partly reflected random measurement errors, which will tend not to be as extreme at the posttest.

One further problem in interpreting the findings from this and other experimental designs has to do with the *construct validity of the experimental intervention* (Cook & Campbell, 1979), in other words, what the experimental intervention actually represents. The distinction between internal validity and the construct validity of the experimental intervention is sometimes hard to grasp. The question of internal validity asks whether change can be attributed to the intervention, X, or to something else; whereas the question of the construct validity of the experimental intervention accepts that X is producing the change and asks what about it (what construct) actually accounts for the change?

Some possible construct validity problems are:

- *Confounding variables.* "Confounding" means occurring at the same time as, and thus inextricably bound up with. In the Dimidjian et al. (2006) study described earlier, the type of therapy was confounded with the person of the therapist, since each of the two psychological interventions (behavior activation and cognitive therapy) was delivered by a different small set of therapists. It is possible that the differences between the therapies could simply have reflected differences in the personality or skill of the therapists who delivered them.

- *Expectancy effects.* Clients may benefit from a service simply because they expect to, rather than as a direct result of what the service actually delivers. This expectancy effect is related to the *placebo effect* in drug studies, where patients may benefit from pharmacologically inert treatments. Related phenomena are that of the *demand characteristics* of the study (Orne, 1962), where the participants attempt to understand what is expected of them in the social situation of the experiment, and the more general issue of *experimenter effects* (Rosnow & Rosenthal, 1997), by which is meant any influence that the researcher may exert, often unconsciously, on participants' behavior.
- *Hawthorne effect,* in which the research itself produces beneficial change. This effect takes its name from a famous study in occupational psychology, in which increasing the level of illumination in a factory was found to increase industrial output, but so also was decreasing the level of illumination (see Adair, 1984; Rosnow & Rosenthal, 1997).

The difference between O_1 and O_2 (i.e., the total pre–post change) is sometimes called the *gross effect* of the intervention (Rossi, Lipsey, & Freeman, 2004). The *net effect* is defined as the effect that can reasonably be attributed to the intervention itself, that is, the gross effect minus the effect due to confounding variables and error. In clinical research it is often a good first step to use a simple design such as the one-group pretest–posttest to demonstrate that a gross effect exists at all. Subsequent studies can then use more sophisticated designs with control or comparison groups to estimate the net effects, rule out the effects of possible confounding variables and examine which components of the intervention are actually responsible for client improvement.

Schmidt and Hunter (2015) argue that Cook and Campbell's (1979) list of validity threats has been widely misinterpreted as indicating that one-group pretest–posttest designs are automatically fatally flawed. Rather, researchers should be aware of, and attempt to mitigate the effect of, potential validity threats. If this is done, such designs will frequently allow causal inference of treatment efficacy.

Nonequivalent Groups Posttest-only Design

NR	X	O
NR		O

In the notation, NR stands for a nonrandomized assignment to experimental groups.

This is like the one-group posttest-only design, except that the group receiving the experimental treatment is compared to another similar group that did not receive the treatment. For example, an intervention could be instituted on one hospital ward and another ward be used as a comparison group. Another variation is to use the design to compare several different active treatments, for example, three different treatment regimes on three hospital wards.

Unlike the previous and the following design, this one provides no direct estimate of pre–post change. It can be used for retrospective studies, where there is no pre-measure, and post-measures are all that one can manage in the circumstances. (This design can also be regarded as correlational, since what is being studied is the association between the group membership variable and the outcome variable.) In clinical applications, X usually represents an intervention. However, the conceptual framework of the design can also be used for comparing groups who differ in having experienced some life stressor or other risk factor (e.g., child maltreatment, passive smoking, or trauma), which it would be clearly unethical to experimentally manipulate. The consequences of this stressor or experience can then be evaluated and causal hypotheses generated. Similarly, some epidemiological case-control studies can also be considered under this category, where X would represent having some illness or predisposition to illness, for example, being HIV positive.

When used in this way, particularly to study the impact of negative events, this design is often known as a *natural experiment* (Rutter, 2007; Shadish et al., 2002). One example is the Romanian orphans study (Kreppner et al., 2007), which followed up children who had suffered severe emotional and physical deprivation in orphanages in 1980s Romania and then had subsequently been adopted in the United Kingdom. By analyzing at what age they had been adopted, it was possible to test hypotheses about critical developmental periods for developing psychological problems as a result of maltreatment. There appeared to be a threshold of about 6 months old: if the deprivation had ended before that, its consequences were less severe, but if it had continued beyond that, the consequences were more pronounced.

The major threat to internal validity in this design (and equally in the following design) is *uncontrolled selection*. That is, since the assignment to groups is not random, one cannot assume that the two groups were the same before the treatment, X. Participants in the different groups may differ systematically on, for example, motivation, problem severity, or demographic characteristics. Even if the researcher is able to compare the groups on these variables, there still may be other important systematic differences that are not tested for. Beyond this, Shadish et al. (2002) provide an extensive list of threats to the validity of such case-control designs.

Example of a nonequivalent groups posttest-only design: the *consumer reports* study

Seligman (1995) reported on the results of a large survey, conducted by the U.S. consumer magazine, *Consumer Reports*, of people who had received psychological therapy. Respondents were asked about the kind of help they had received and their evaluation of it. The large sample made it possible to compare different groups within the design: for example, those that had been treated in different modalities of therapy or by different types of professionals. It can, therefore, be regarded in part as a nonequivalent groups posttest-only design. Generally patients reported substantial improvement, and there were no differences between modalities or types of therapist, but there was a positive effect for length of therapy.

Nonequivalent Groups Pretest–posttest Design

NR	O	X	O
NR	O	(Y)	O

This commonly used design combines the features of the previous two, and helps to rule out some of the associated internal validity threats. The group in the lower part of the diagram that does not receive the experimental intervention is called a *control group*. Several different types of control groups are possible, ranging from an alternative comparison treatment (Y in the diagram) to no treatment at all (in which case no letter is used in the diagram). The type of control group depends on the research question: whether you are trying to show that the experimental treatment is as good as or better than an established treatment, or simply better than nothing at all. We will discuss these issues more fully below under the heading of the randomized groups pretest-posttest design.

This design can easily be extended to encompass two or more experimental or control groups. An example is the Stanford Three Community study (Farquhar et al., 1977), which studied the effects of the mass media and community interventions on the prevention of heart disease. It was conducted in three small towns in California. One town received only the pre–post measurement, another a sustained mass-media campaign, and a third mass-media plus community intervention in the form of face-to-face instruction. The results showed encouraging effects for the mass-media condition, which were augmented in the community support condition.

Another large-scale example is Stiles, Barkham, Mellor-Clark, and Connell's (2008) study of the outcome of three different therapeutic orientations delivered in a naturalistic setting in the U.K. National Health Service. The sample consisted of 5613 patients who received either CBT, person-centered therapy or psychodynamic therapy in National Health Service primary care settings. Clients in all types of therapy had virtually identical pre-test values, showed large amounts of pre–post change, and outcomes across the three different therapies were approximately equivalent. Like all naturalistic studies, this had its shortcomings (see Clark, Fairburn, & Wessely, 2008, for a methodological critique, and Stiles, 2008, for the authors' rebuttal).

Prospective case-control studies, where a cohort of individuals is studied longitudinally to evaluate the impact of a disease or of stressful life events, can also be considered under this category. Because participants are being studied prospectively, then measures are obtained before the event of interest. For example, a cohort of elderly people might be studied at one-year intervals to examine the psychological impact of bereavement by comparing the individuals who become bereaved with those who do not.

As in the previous design, the major threat to internal validity is uncontrolled selection: that the groups may differ systematically in ways other than the presence or absence of the experimental treatment or event of interest. Sometimes experimenters try to compensate for differences in the groups by statistical methods, for example, analysis of covariance or multiple regression (Shadish et al., 2002). For example, if the experimental group turns out to be younger than the control group, age might be used as a covariate in the analysis (often described as *partialling out* the effects of the covariate, in this case age). This can be misleading when the nonequivalent groups are drawn from two different populations (Miller & Chapman, 2001): it is like trying to equate an elephant and a mouse by adjusting for their relative weights. However, when the groups are drawn from the same population, such analyses may be performed in nonrandomized designs. Another approach is to construct a control group by *matching* participants in each group on key variables, such as age, gender, and problem severity (Rossi et al., 2004). However, this is difficult to accomplish and again can be misleading if the groups represent two different populations.

The interpretability of this and other nonrandomized experimental designs can be enhanced by adding pretests and later assessments to examine the process of change more closely and also by adding specific control groups to deal with specific internal validity threats.

Interrupted Time-Series Design

$O_1 \quad O_2 \quad \ldots \quad O_{20} \quad X \quad O_{21} \quad \ldots \quad O_{40}$

This design extends the one-group pretest–posttest design to cover multiple measures over time. It has a different rationale to the previous designs, in that it attempts to pinpoint causal influences by looking at discontinuities between a series of baseline measures and a series of follow-up measures (we have arbitrarily depicted 20 baseline and 20 follow-up points; in practice there may be considerably more). It is good for studying naturally occurring chronological data and can be used to analyze existing data from large samples, for instance, to look at changes in national alcohol or tobacco consumption following taxation changes, or reductions in injuries following legislation on car seat belts legislation (Guerin & MacKinnon, 1985). It can also be used in single-case designs, in which participants serve as their own controls (see Chapter 9). The major threat to internal validity is the presence of interfering events—other things occurring at the same time as the treatment, X, that affect the outcome variable—but this can be dealt with by careful monitoring for such events.

Randomized Designs

The essential feature of randomized experimental designs (as opposed to nonrandomized or quasi-experimental designs) is the *random assignment* of participants to experimental groups or conditions. The rationale is that the groups will therefore then be equivalent except for the experimental manipulation: in other words, randomization eliminates the likelihood of selection bias as a threat to internal validity. It also allows the use of the statistical theory of error. Note that *randomization* (random assignment to experimental conditions) is not to be confused with *random sampling* (a method of unbiased selection of participants for the study: see Chapter 10).

Randomized experimental designs enable the experimenter to manipulate a single variable at a time, and thus any relationships established between the independent and dependent variables are more likely to fulfill the first three conditions for inferring causality discussed earlier in this chapter (covariation, precedence, and exclusion of alternative explanations). The judicious choice of control and comparison groups (see below) allows the effects of key variables to be isolated.

Randomized designs are regarded by influential bodies (e.g., the Cochrane Collaboration, NICE, York Centre for Reviews) as the "gold standard" research design for evaluating treatments. They are the standard design used in medicine to evaluate new drugs or other medical or surgical treatments. They are known as *randomized controlled* (or sometimes clinical) *trials*, abbreviated to RCTs in either case. In clinical psychology, RCTs are often known as *efficacy studies,* as opposed to *effectiveness studies*, which are uncontrolled studies conducted in field settings. There is considerable debate about the relative place of each type of research (see, e.g., Seligman, 1995), which essentially boils down to how much weight one gives to internal validity at the expense of other validity types, especially external validity. (We will address these issues further below, and also in Chapter 11.)

The theory of experimental design was developed by the statistician Fisher in the 1920s. His early work was mostly done in agriculture, looking at how crop yields were affected by different fertilizers or different varieties of grain. This agricultural origin accounts for some of the terminology which is still used to describe different experiments (e.g., split plots or randomized blocks refer to parts of fields); it also provides another area of application in which to picture specific designs. Here we will tend to focus on designs in therapy outcome research; it is one in which a lot of work has been done, and it is central to discussion of evidence-based practice and empirically supported therapies. The statistical textbooks (e.g., Field, 2013; Howell, 2010; Shadish et al., 2002; Winer et al., 1991) cover many different experimental designs. We will illustrate the issues in the context of the paradigmatic design in clinical research, the *randomized groups pretest–posttest design*:

R	O	X	O
R	O	(Y)	O

This design, or its close relatives, is the standard design for randomized controlled trials in medicine, in which a new therapy or drug is tested against a pill placebo or a no-treatment control.

In the notation, R denotes a randomized assignment of participants to experimental conditions. Such assignment needs to be done without bias, in order to ensure that each participant has an equal chance of being in each condition. This may be done in several ways, for instance, by using random number tables, a web-based program (e.g. http://www.randomizer.org), or an independent statistician. On the other hand, nonrandom or pseudo-random methods of allocation, for example, by assigning the first 10 participants to the experimental group and the next 10 participants to the control group, may introduce systematic error (Cook & Campbell, 1979).

The independent variable, that is, whether or not the participants receive the experimental intervention, is known as a *between-groups factor* (since it divides the participants into groups). The design depicted above is said to have a between-groups factor with two levels (i.e., the experimental group and the control group). This basic design may be extended in several ways, such as:

More than two levels. There can be more than two levels of the between-groups factor, that is, there may be more than one experimental group or more than one control group. In our running example, the type of intervention factor in the Dimidjian et al. (2006) study had four levels: behavioral activation, cognitive therapy, antidepressant medication, and placebo.

Multi-factorial designs, in which there is more than one between-groups factor. In our running example, the Dimidjian et al. (2006) study had only one between-groups factor, the type of intervention, but it could have possibly built in a second one, such as a two-level length of intervention factor (e.g., brief treatment versus medium-term treatment).

The pretest–posttest design is an example of a *repeated-measures design*, that is, one in which the same individuals are assessed at two or more points in time. Additional levels of the repeated-measures factor may be introduced, for example, there may be a follow-up assessment six months or a year after the intervention has ended.

Blocking factors are ones that represent participant individual difference variables (e.g., type of presenting problem) within the overall research design (this is also referred to as *stratification)*. Such factors are included in order to examine their effect as potential moderator variables or in order to ensure that the experimental groups are balanced on crucial variables. The procedure is that participants are grouped into the relevant categories before the randomization to experimental treatments takes place. For example, the Dimidjian et al. (2006) study had a blocking factor of the client's initial level of depression (moderate or severe). The researchers first allocated potential clients to the appropriate severity level and then randomly assigned people from the same cell to the therapists within each of the four experimental treatment conditions.

In the educational context, analyses addressing the question of which interventions work best for which students are referred to as *aptitude-treatment interaction* studies (Snow, 1991), and this terminology has been adopted in studies of psychological therapies (Shoham-Salomon & Hannah, 1991). Designs that incorporate many treatment and client factors can attempt to analyze what treatment works best in what circumstances or, as Paul's (1967, p. 111) famous question states: "*What* treatment, by *whom,* is most effective for *this* individual with *that* specific problem, and under *which* set of circumstances.". However, such large-scale designs, within what has been called the matrix paradigm (Stiles, Shapiro, & Elliott, 1986), have serious practical limitations, not least because of the large number of participants required.

Analysis of covariance designs are similar to blocked designs, but are used when it is known that the individual difference variable being investigated, for instance, psychological mindedness or severity of symptoms, has a linear relationship with the outcome variable. Analysis of covariance is a more powerful procedure than blocking, but the statistical assumptions that must be met before it can be employed are more restrictive. Keppel and Wickens (2004) give a useful discussion of the relative merits of the blocking versus the analysis of covariance approach.

Control and Comparison Groups

The terms *control* and *comparison* group are rather loosely defined. The implication of the term "control group" is that some active ingredient in the experimental group is missing (as in agricultural experiments, where the experimental groups might be given various fertilizers and the control group none), whereas the term "comparison group" implies that a viable alternative treatment is given. We will use the term "control group" as a shorthand for control or comparison groups. As we discussed above in the section on the nonequivalent groups pretest–posttest design, several types of controls are possible, depending on the research questions, although the selection of suitable control groups for psychotherapy research (and other applications) raises ethical, scientific, and practical problems.

No-treatment controls, in which the control group receives no treatment at all, are used to provide a maximum contrast with the therapy under investigation. However, there are serious ethical issues in withholding treatment from clinically distressed clients. Researchers must balance the possible harm resulting from an untested treatment with the denial of benefit resulting from a potentially effective one. This problem may not arise in a nonrandomized design, since a group of clients who might be unable (for geographical or other reasons) to receive the experimental treatment could be used as a control group. Furthermore, no-treatment controls are only useful in the early stages of research on a clinical problem area or diagnosis; once the effectiveness of psychological therapies for a clinical problem has been demonstrated, no-treatment controls become scientifically uninteresting and ethically questionable.

Wait-list controls often provide a workable compromise, particularly with short-term treatments or with mildly distressed clients. Clients who are randomly assigned to the wait-list group are given the same initial assessment, and then placed on a waiting list to receive the intervention once the experimental group has completed it. Thus they control for the reactivity of the initial assessment, the instillation of hope, and spontaneous recovery.

Expectancy and relationship control groups control for expectancies of benefit, or instillation of hope, and also for the effects of contact with the therapist. In drug trials, where patients are given a sugar pill or other pharmacologically inert substance, they are known as *placebo control groups*. However, this terminology is too imprecise for the psychological context: it is better to be specific about what the control group is intended to control for. In pharmacology, clinical trials are ideally done in a *double-blind* study, where neither patients nor doctors know which experimental condition each patient is in, or *triple-blind studies*, where, in addition, the experimenters do not know. However, even in drug trials, this is not always practicable because patients and their physicians may be able to distinguish between active drugs and placebos, for example, by their side effects. Even having the clients blind to the status of the intervention is nearly impossible in psychological applications, where any placebo control treatment should appear as credible to clients as the experimental one (Boot, Simons, Stothart, & Stutts, 2013). Expectancy or relationship controls generally work well for clinical trials in drug research, but are questionable for psychotherapy research, where relationship factors or aspects of the therapeutic alliance (so-called "nonspecific factors" or "common factors") are generally the best predictors of outcome (Duncan, Miller, Wampold, & Hubble, 2010; Norcross, 2011).

Comparative treatment groups use a credible, established comparison treatment, rather than a placebo. One common approach is to compare a new approach to "*treatment as usual*" (often abbreviated to TAU), although the lack of specificity about what this means can also be a problem. Comparative treatment groups provide an ethical way of doing research. Given the broad equivalence of most major forms of therapy, however, they are unlikely to produce statistically significant effects unless the sample is quite large (over 60 clients per group; Kazdin & Bass, 1989). In these situations, the researchers' concern is often to show comparability rather than differences, but this requires performing statistical equivalence testing (see Chapter 12), something that is rarely done. A recent approach is to conduct a *non-inferiority trial*, which aims to demonstrate that a new treatment is no worse than a standard treatment. For example, in the Dimidjian et al. (2006) trial that we have been using as a running example, behavioral activation and antidepressant medication were found to be within the range of noninferiority (i.e., their outcomes were statistically equivalent to each other). Occasionally, comparative treatment groups appear to have been selected in order to make the researchers' favored treatment look good (sometimes pejoratively referred to as "intent to fail" control groups; Westen & Bradley, 2005).

Dismantling studies aim to see what the effective components of a treatment are. The full treatment is compared to a comparison group, which receives that treatment minus one component. For example, a cognitive-behavioral treatment for fear of heights that includes relaxation training might be compared to a treatment that is exactly the same except for the relaxation component. The reverse strategy is also possible (and logically equivalent): *constructive* (or *additive*) designs examine the effects of adding additional components intended to enhance the effectiveness of a therapy.

Limitations of Randomized Designs

Although randomized designs are scientifically valuable, they may be not be conceptually appropriate in certain situations.

- Randomization cannot be used ethically to study the impact of negative experiences, for example, cigarette smoking, illicit drug use, disasters, or psychological trauma. In these cases, nonrandomized experimental designs or correlational designs must be used (which is why there is more scope for interested parties, such as tobacco companies, to dispute the results).
- Sometimes RCTs may be used to study something blindingly obvious, for which a randomized trial is superfluous, or worse. Smith and Pell's (2003) tongue-in-cheek paper entitled "Parachute use to prevent death and major trauma related to gravitational challenge: Systematic review of randomized controlled trials" says it all – perhaps we shouldn't recommend that aircrew use parachutes, since their efficacy has never been evaluated in an RCT?
- The design requirements of RCTs may mean that they are unrepresentative of normal clinical practice, and therefore that their results do not generalize outside of the research setting. The clients may be selected according to narrow inclusion criteria (e.g., no co-morbid problems), the therapists may be highly trained "super-therapists" with low case loads and a strong allegiance to the treatment under investigation. In addition, the therapies may be delivered inflexibly according to a prespecified protocol or treatment manual.
- Randomized trials do not take account of patient choice (Bower, King, Nazareth, Lampé, & Sibbald, 2005; Brewin & Bradley, 1989). Outside of research studies, some clients will select a treatment based on their individual preferences: randomized trials may thus give clients a treatment that they do not want and which they may consequently fare less well with. King et al. (2000) carried out an interesting example of a *preference trial* in which clients with depression could

either consent to be randomized to one of three different therapies, or they could express a preference for one of them.
- Randomization does not control for one of the most pervasive factors affecting the results of comparative treatment studies: *researcher allegiance*. This is the tendency of researchers to find positive results for the treatment that they favor; this phenomenon has been demonstrated in a variety of studies (e.g., Elliott, Watson, Greenberg, Timulak, & Freire, 2013; Luborsky et al., 1999).

In addition to these conceptual problems, there are also a number of practical issues that arise when implementing RCTs in clinical practice (Haaga & Stiles, 2000; Rossi et al., 2004; Shadish et al., 2002):

- Random assignment to experimental groups does not ensure that the groups will be equivalent or that they will stay equivalent. Randomization is, by definition, a chance process, and will thus occasionally produce some unusual distributions. Problems of nonequivalence become more acute if the sample sizes are small or if there are a large number of "nuisance variables" on which the researcher is trying to equate the groups (Hsu, 1989).
- Many experiments suffer from *attrition*, that is, some participants may drop out of the study before the treatment is completed and the post-measures are collected. Attrition reduces the equivalence of the experimental and control groups (Flick, 1988; Howard et al., 1986). One potential way to deal with this is to conduct *intent-to-treat analyses*, in which all participants who enter the study are included in the analysis, and the latest available data is used from those who drop out before the end, a procedure known as *last observation carried forward* (Comer & Kendall, 2013). However, such analyses are likely to be too conservative: in order to preserve the benefits of randomization, they focus on the effects of assigning clients to treatments. They thus sacrifice an accurate assessment of the effects of completing treatment, which is the goal of *completer analyses*.
- There may be *leakage* between the conditions. For example, if half of the patients on a hospital ward are taught a useful skill, for example, mindfulness meditation, they may then teach it to other patients in the control condition. In drug trials, there is anecdotal evidence that patients in the experimental group may sometimes share their medication with their fellow patients in the control group who have been deprived of it.
- Other staff may not understand the need for randomization, seeing it as antithetical to the principle of giving individualized care to each client, and thus it may be hard to obtain the necessary cooperation for the study.
- Randomized experiments are costly and time-consuming, and should therefore only be used where there is prior evidence that the experimental treatment is beneficial, or at least not inferior to existing treatments.

A realization of these conceptual and practical problems with randomized experimental designs, which are still regarded by many as a scientific panacea, has shaped the growing interest in nonrandomized experimental designs and in correlational designs ("effectiveness studies"), especially in naturalistic clinical settings (West, 2009).

Evaluating RCTs

It is easy to feel bewildered by all the complexities of randomized designs. Few readers are likely to be involved in conducting large treatment trials, but all practicing psychologists need to be aware of the results from such trials in order to keep up with and evaluate the state of the evidence. What are some of the features of RCTs that lend credibility to their findings? A number of *quality appraisal tools* exist that set out checklists of criteria for evaluating RCTs, for example, the "risk of bias" chapter in the Cochrane handbook (Cochrane Collaboration, 2011) and Downs and Black's (1998) checklist.

In addition to quality criteria for the design of trials, there are also guidelines for reporting them. The CONSORT ("Consolidated Standards of Reporting Trials") statement gives authoritative recommendations on writing up journal articles describing RCTs (see http://www.consort-statement.org). There are also equivalent statements for non-randomized designs (the TREND statement: http://www.cdc.gov/trendstatement/) and for epidemiological observational studies (the STROBE statement: http://www.strobe-statement.org/).

The following list summarizes the main criteria for high quality RCTs:

- The study uses *randomized assignments* to conditions, in order to rule out selection effects, together with an analysis to demonstrate that the groups actually were *similar after randomization.*
- *Specific interventions.* The intervention is specified, so that it is clear what therapy is being delivered, it is constant across therapists, and it can be repeated by other investigators if necessary. This requirement has led to the *manualization* of therapy and to the inclusion of *fidelity checks* or *adherence checks* to ensure that these treatment protocols are adhered to (Comer & Kendall, 2013).
- *Appropriate control groups* are used, so that it is clear what the therapy is being compared with (e.g., wait-list controls, treatment as usual, or other comparative treatment groups).
- The *groups were treated equivalently* apart from the experimental variable in question (e.g., they had the same length of treatment, equivalent therapists, the same assessments, etc.).
- The raters and assessment interviewers were *blind* to the experimental condition the patient was in. However, it is usually not possible to conduct double-blind, or triple-blind studies in psychology, as it is in medicine, since the therapists know what they are giving, and the clients usually know what they are getting (Seligman, 1995).
- The clients form a *specific, homogeneous group*. This usually means that they meet criteria for a single DSM diagnosis: patients with comorbidities are often excluded.
- There was a *low attrition rate* in the study.
- The clients are *followed-up* after the termination of therapy (e.g., at six months or a year).
- Demonstrations of the efficacy of a treatment are *replicated* by independent teams of researchers, thus demonstrating the portability of treatments across research settings.

Some authors (e.g., Chambless & Hollon, 1998) have attempted to use criteria such as these in order to conclude that certain therapies can be considered as *empirically supported treatments* and others not. However, these attempts have been controversial (Elliott, 1998; Westen, Novotny, & Thompson-Brenner, 2004). Key issues include the use of criteria that favor some treatments over others and the dismissal of all non-RCT research. While we regard RCTs as potentially powerful research designs, we believe that nonrandomized designs also have a place, particularly when they use naturalistic client populations or clinical settings. It seems unproductive to force all treatment research into the same mold, as different research designs have complementary strengths and weaknesses—no single design provides a "royal road" for evaluating therapy outcomes.

Conclusion: Choosing a Research Design

The central issue for researchers is to choose a design appropriate to the research questions and to the stage of the research program and research area. In the early stages of investigating the treatment of a problem, or where there has been little previous research, you are probably unsure of the nature of the phenomena you are looking at. Furthermore, a newly established clinical service may not be stable: its operational policies and modus operandi may be constantly changing (Patton, 2008; Rossi et al., 2004). In these cases, a simple descriptive or correlational design is usually better. Later on, when you are clearer about what the important variables are and how they interrelate, and are more able to specify the nature of the treatment, you can proceed to the next stage by using a one-group pretest–posttest design or systematic case study designs (see Chapter 9). Then, if the treatment appears effective and resources and circumstances allow, you can move on to more sophisticated randomized experimental designs to test efficacy, perhaps adding correlational components in order to pin down the effects of crucial variables and to test competing theoretical models (Campbell et al., 2007). Also, several studies taken together can often help eliminate specific competing theoretical explanations.

We have drawn heavily on Cook and Campbell's (1979) analysis of threats to validity (see also Shadish et al., 2002). Their central theme is that no research design is perfect: the important thing is to be aware of the strengths and weaknesses of whatever design you decide to adopt. As we have discussed above, it is important not to read Cook and Campbell as saying that research designs must have no validity problems at all, or that certain designs are automatically flawed (Schmidt & Hunter, 2015). Their message is to do the best that you can in the circumstances, but to be aware of potential problems that may arise later on in interpreting the findings. Thus, research designs require careful planning and analysis in order to anticipate the potential results and competing explanations.

CHAPTER SUMMARY

Design, in the restricted sense in which we are using it here, refers to the logical structure of the study. It encompasses such issues as whether there is a control group and whether the data are collected at one time point or several. The central classification is into experimental and nonexperimental designs, which depends on whether or not the researcher is making an active intervention (also known as an experimental manipulation).

Nonexperimental designs can be classified into descriptive and correlational designs, according to the type of analysis conducted. The golden rule when interpreting the results from nonexperimental designs is that "correlation does not equal causation": if two variables are correlated there is not necessarily a causal relationship between them. There are several possible models of the causal relationships of two or more variables, including mediator and moderator variable models, as well as conceptual confounding between cause and effect variables.

The work of Campbell and colleagues (e.g., Cook & Campbell, 1979, Shadish et al., 2002) has made two major contributions to the study of research design: (1) the classification of validity types; and (2) the analysis of quasi-experimental designs. Cook and Campbell (1979) analyze several nonrandomized (quasi-experimental) designs, examining the validity threats associated with each one.

Cook and Campbell's four validity types are statistical conclusion validity (which assesses the appropriateness of the statistical methods), internal validity (which assesses the evidence for the existence of causal relationships), construct validity (which assesses the meaning of the measurement operations, including the experimental manipulation), and external validity (which assesses the study's generalizability).

Experimental designs are classified into randomized and nonrandomized (quasi-experimental) designs, according to whether or not there is random assignment to experimental conditions. There are several commonly used nonrandomized designs. Each has its associated validity threats.

Randomized experimental designs have the potential to address many of the validity problems associated with nonrandomized designs and thus to facilitate inferences about causality. They are central to the discussion of evidence-based practice and empirically supported therapies. However, they do have practical, scientific, and ethical limitations, and should not be regarded as a scientific panacea.

FURTHER READING

Cook and Campbell's (1979) ideas on causation, validity, and on the different experimental and correlational designs are essential reading for all serious researchers; their classic book has been updated by Shadish et al. (2002), but the original is still worth reading. Christensen, Johnson, and Turner (2013) give an overview of design from the standpoint of experimental psychology. A statistical treatment of experimental designs is given in the standard texts, such as Field (2014), Howell (2002), and Winer et al. (1991). Comer and Kendall (2013) illustrate the issues as applied to therapy outcome research. Seligman's (1995) paper on the *Consumer Reports* effectiveness study, together with subsequent commentary in the *American Psychologist* (October 1996, volume 51, issue 10), air the central issues in the efficacy versus effectiveness debate. A 1998 special issue of *Psychotherapy Research* (volume 8, issue 2) and also of the *Journal of Consulting and Clinical Psychology* (volume 66, number 1) both debate the research issues raised by the empirically supported therapies movement, and Westen, Novotny, & Thompson-Brenner (2004) provide a thoughtful commentary.

Persons and Silberschatz (1998) set out their contrasting views, using a debating format, about the value of RCTs for clinicians.

As in other areas of research, it is well worth reading some sample studies. The ones that we have looked at in this chapter provide a good starting point: Brown and Harris (1978) for a descriptive, correlational design, Stiles et al. (2008) for a quasi-experimental design, and Dimidjian et al. (2006) for a randomized controlled trial.

QUESTIONS FOR REFLECTION

1. Use the Cook and Campbell (1979) design validity framework to identify problems that could arise in a study you would like to do.
2. Which designs do you see as most useful for studying the outcome of psychotherapies? Why?
3. An interesting variant of the nonequivalent comparative treatment design, mentioned in this chapter, is the "preference trial" in which clients are allowed to choose between two available treatments. Think about the pros and cons of this design.
4. Try to identify an aspect of your research topic that lends itself to experimental manipulation. Alternatively, if you're already planning an experimental study, think about particular experimental designs that could be used.
5. What is your view of the Empirically Supported Treatments debate? Take a position pro or con and defend it.

9
Small-N Designs

KEY POINTS IN THIS CHAPTER

- Small-N designs follow the idiographic approach of looking in depth at the individual.
- They are often appealing to clinicians, as a way of combining research and practice.
- They derive from several different traditions: narrative case studies in neuropsychology and medicine, operant behaviorism, Shapiro's single-case approach, and idiographic research in personality theory.
- There are two main types of design: single-case experiments and naturalistic case studies.
- Single-case experiments are characterized by frequently administered measurements and the experimental manipulation of an intervention, in which participants serve as their own controls. They are most often used within operant behavioral approaches to therapy.
- Naturalistic case studies range along a continuum of approaches with varying degrees of rigor, from narrative approaches, such as Freud's, to more structured studies using systematic measurement of process and outcome and time-series studies using sophisticated statistics.

Small-N designs, such as systematic case studies and single-case experiments, are a potentially appealing way of blending science and practice, since they enable clinicians to integrate formal research methods into their everyday work (Hayes et al., 1999; Kazdin, 2011). From the practitioner's point of view, the advantages of small-N

Research Methods in Clinical Psychology: An Introduction for Students and Practitioners,
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research are that it is usually inexpensive, not very time–consuming, and, more importantly, that its underlying philosophy is often congenial to practitioners, since it addresses individual uniqueness and complexity.

Recall the nomothetic versus idiographic distinction that we introduced in Chapter 4. Nomothetic methods compare across individuals, looking for general patterns or laws; idiographic methods look intensively within a single individual, to gain greater understanding of that person's unique personality or psychological responses. Nomothetic approaches, particularly the large-group experimental and quasi-experimental designs that we examined in Chapter 8, have long been criticized on the grounds that individual variation and uniqueness get submerged by the process of averaging across a larger group (Dukes, 1965).

For example, in a seminal paper on psychotherapy research, Kiesler (1966) drew attention to the existence of "uniformity myths": the implicit assumption by researchers that clients are all similar, that different therapists each deliver an identical intervention, and so on. For instance, in an outcome study, the overall difference between the mean pre-therapy score and the mean post-therapy score on a depression measure may indicate that the therapy is beneficial. However, as Bergin (1966) originally pointed out, such positive mean changes may conceal the fact that, although most clients have improved, a significant minority have deteriorated (Barlow, 2009). Such differential responses would not be discovered without challenging the uniformity myth and paying attention to each individual client's unique pattern of change.

As a second example, in neuropsychological case-control research, client heterogeneity may obscure important effects (Shallice, 1979). Clients may vary in their responses to neurological lesions according to such factors as age, premorbid functioning, or the size of the lesion, and this variation will only become apparent if one looks in detail at each single case.

Small-N designs can therefore make up for some of the drawbacks of nomothetic, group-comparison designs. The idiographic approaches described in this chapter provide ways of rigorously examining individuals' responses, particularly in the context of evaluating psychological interventions.

Historical Background

Small-N studies were the dominant paradigm in medicine and in psychology until the beginning of the 20th century. Before that time, statistical theory barely existed. Then, in the early decades of the century, Pearson and Fisher developed methods such as correlation and the analysis of variance. These methods were originally developed within the agricultural context, in order to systematically assess the yields of different fertilizers or strains of wheat, but they were rapidly adopted in medicine and psychology. In agricultural research, large samples and group-comparison designs work very well; examining the response of individual plants is less relevant. However, the agricultural metaphor does not translate easily to clinical psychology, where individual differences are often of major importance. In recognition of this, there has been a resurgence of small-N methods in the past 30 years, deriving its impetus from several different traditions.

Historical traditions of small-N research:

- Single-case studies in medicine and neuropsychology
- Operant behaviorism
- Shapiro's single-case approach
- Idiographic personality research

First, there is the narrative case study, which is a continuation of the long tradition of descriptive case studies in medicine. The first published studies of psychological therapy were case studies, those of Sigmund Freud being the outstanding example. Often case studies are reported to illustrate the development of new theoretical approaches. For example, both Rogers's (1951) *Client-Centered Therapy* and Beck's (1976) *Cognitive Therapy and the Emotional Disorders* exemplify their theoretical ideas by presenting case illustrations, including excerpts of verbatim transcripts from therapy sessions.

Arising out of the same medical tradition is the single-case study in neuropsychological research. Luria (1973) dates the birth of scientific neuropsychology to 1861, when Broca described a case of speech impairment that was associated with a localized lesion of the brain. The famous individual cases of Phineas Gage and of the amnesia patient known as "HM" contributed to the development of the discipline. Case studies continue to play an important role in the development of the area, and currently seem to be enjoying a resurgence of popularity (Crawford, 2014; Evans, Gast, Perdices, & Manolov, 2014; Shallice, 1979). Methodologically, examples range from qualitative narrative case studies (e.g., Sacks, 1985) to case series studies using intensive quantitative neuropsychological test data (e.g., Rapp, 2011).

Second, there is the tradition of applied behavior analysis (i.e., operant behaviorism). Skinner (quoted in Barlow & Nock, 2009) said that "instead of studying a thousand rats for one hour each or a hundred rats for ten hours each the investigator is more likely to study one rat for a thousand hours." In his view, the goal of behavioral science is "to predict and control the behavior of the individual organism" (Skinner, 1953, p.35). Single-case experimental designs, aimed at demonstrating such prediction and control, were first developed in the 1950s and 1960s (Davidson & Costello, 1969), and studies using these designs proliferated in the 1970s. *The Journal of Applied Behavior Analysis* continues to be devoted to publishing examples of this kind of work.

Third, innovative measurement methods for single-case designs were pioneered in the United Kingdom by Monte Shapiro, who developed a measurement technique known as the Shapiro Personal Questionnaire, which enables each patient's problems to be quantified and monitored on a day-by-day or week-by-week basis (Phillips, 1986; Shapiro, 1961a, 1961b). In contrast to the operant work, Shapiro's approach takes a more phenomenological stance, being tailored to the individual client's view of his or her problems, and it is less concerned with the experimental manipulation of treatments. A simplified version of the Personal Questionnaire is a key element in contemporary single-case research (Elliott et al., 2015).

Fourth, there is the idiographic tradition in personality research. Allport (1962) passionately criticized psychology's almost exclusive reliance on nomothetic methods: "Instead of growing impatient with the single case and hastening on to generalization, why should we not grow impatient with our generalizations and hasten to the internal pattern?" (Allport, 1962, p. 407). Murray (1938) developed an approach to studying personality based on intensive investigation. Proposition one of his theory, which captures the key idea of this chapter, is "The objects of study are individual organisms, not aggregates of organisms" (Murray, 1938, p. 38).

The terminology of the area partly reflects these different traditions. *Single-case designs* (also referred to as *N = 1 designs)* are characterized by repeated measures on a single case. They usually involve an experimental manipulation of a treatment, although there are quasi-experimental versions, such as time series designs. Studies that do not use intensive repeated measures and do not have an experimental manipulation, for example, the classic case-history approach, are referred to as *case studies* (Bromley, 1986; Dukes, 1965), although the boundary between the two approaches is not always clear. This chapter will focus first on single-case experimental designs and then on case studies. Although the focus is on design, we also include some suggestions about measurement, because small-N designs call for some specific measurement approaches.

SINGLE-CASE EXPERIMENTAL DESIGNS

Single-case experimental designs are used to test an experimental intervention on a single individual. The essence of the design is that each individual participant serves as their own control. Single-case experiments also involve repeated measurements, which allow the process of change to be closely monitored. A key assumption of these designs is that they are based on an adequate functional (i.e., causal) analysis of the problem, providing an understanding of the situational variables (cues and reinforcers) which appear to be controlling the problem behavior (Haynes & O'Brien, 2000; Morgan & Morgan, 2001). Such a behavioral case formulation is established during a preliminary behavioral assessment period. The experimental design then serves as a test of the functional analysis.

Procedure

As with group-comparison designs, the first step is to select the measure, or measures, to be used. In single-case experimental designs, the measures need to be capable of frequently repeated administration (Smith, 2012): that is, they must be brief and minimally reactive. The two most common types are observer ratings (e.g., staff ratings of a patient's self-injurious behavior on an in-patient ward) and the client's own ratings from self-monitoring (e.g., of their obsessional thoughts). Having chosen the measure, the next step is to select an appropriate frequency of measurement: usually it is daily, but it may also be, say, hourly or weekly, depending on the nature of the behavior being monitored.

All single-case experimental designs start with a series of *baseline* measures. These are measures taken to establish the level of the target variable before the clinical intervention is introduced. They continue until the measurements are stable, preferably for 10 to 20 observations. After that, the first experimental treatment phase begins. These designs have their own special notation, based on the first few letters of the alphabet: A stands for the baseline, or no-treatment phase; B, C, D, etc. stand for the various treatments or interventions. There are many possible single-case designs, each of which raises practical and sometimes ethical issues. We will look at four commonly used ones here; more elaborate designs are given in specialist textbooks (e.g., Barlow, Nock, & Hersen, 2008; Hayes et al., 1999; Kazdin, 2011).

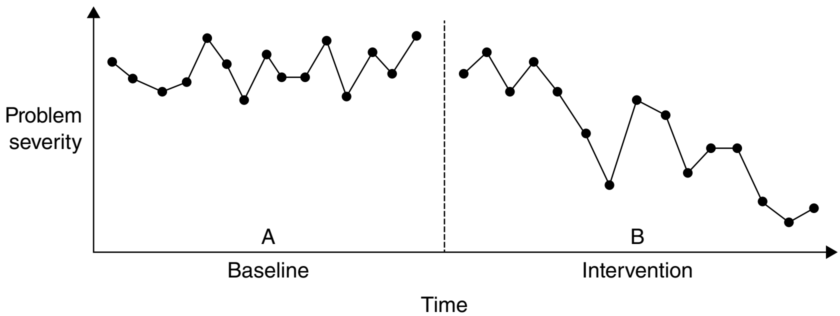


Figure 9.1 The AB design

> ## Common single-case designs:
>
> - AB design
> - Reversal (ABAB) design
> - Multiple-baseline design
> - Changing-criterion design

AB Design

The AB design (see Figure 9.1), in which the baseline is followed by an intervention, is the simplest form of single-case experiment. For example, the effectiveness of a positive parenting approach to manage a six-year-old girl's tantrums might be investigated. The parents would be asked to observe the number and severity of their daughter's tantrums (suitably operationalized) every day for two weeks (this constitutes the baseline, or A, phase of the design). Then, in the intervention, or B, phase of the design, they would be taught a new way of responding to their daughter, such as time-outs for the tantrums and praise for good behavior. If the intervention is effective, there will be a reduction in the target problem behavior's severity and frequency.

The drawback of the AB design is that, in the absence of other information, it only gives weak evidence for the causal influence of the experimental treatment. It suffers from many of the same threats to internal validity as the one-group pretest–posttest design (Cook & Campbell, 1979; see also Chapter 8), for example, that an interfering event may occur at the same time as the treatment is introduced (e.g., the girl could make a new friend at school, and so be happier, which reduces her need to tantrum).

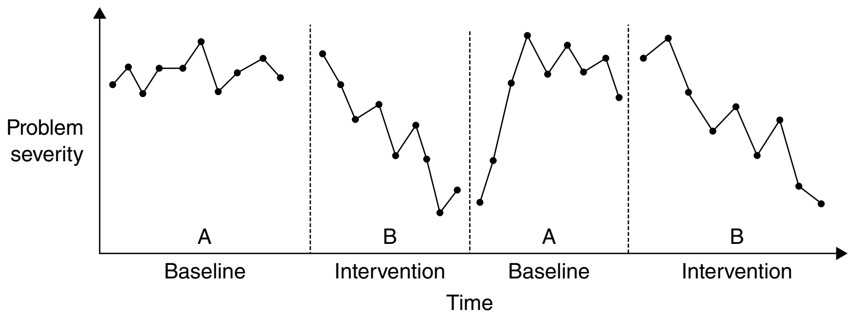


Figure 9.2 The ABAB design

More elaborate designs have therefore been developed to try to overcome this problem, and, in particular, to build in opportunities to replicate the treatment effect, thus increasing its credibility.

Reversal (or ABAB) Design

The reversal (or ABAB) design is an AB design immediately followed by its own replication (see Figure 9.2). It is the classic operant behavior modification design. For instance, in the child's tantrum example above, once the intervention had been shown to be effective, the baseline, no-treatment, phase would be reinstituted, followed finally by the intervention again. The rationale is that changes in frequency of the target behavior after these reversals demonstrate the experimental control of the intervention.

There are also more complicated variants of this design, for example, the ABACAB design in which a second intervention, C, is introduced after the second baseline phase. For example, a token economy on an in-patient ward might have its contingencies modified in the second phase.

The ABAB design suffers from three major problems. First, the effects of many interventions are not reversible. In other words, clients do not automatically relapse when treatment ends: permanent learning or personality change may occur, or the problem may not recur once it has been dealt with. Thus, this design could not be used to study the impact of psychodynamic or cognitive therapy, for example, because these therapies, if successful, effect irreversible changes in the way that clients think and feel about themselves. The design's expectation of reversibility is based on the assumption that external processes control behavior. Second, even if the intervention is reversible, there are serious ethical problems with the withdrawal of treatment in the second and subsequent baseline phases. This problem is similar to the ethical dilemma faced in having no-treatment control groups in group-comparison designs, but it is more acute because in this case treatment is withdrawn rather than withheld. For example, when the intervention is withheld, the design is "successful" if the child reverts to having tantrums, or if psychiatric in-patients recommence self-injurious behaviors. Thus, the design creates a conflict of interest between clinical and scientific goals. Third, switching the intervention on and off may have undesirable psychological consequences. For example, it may lead to the client's losing trust in the therapist, or may even lead to the problem behavior being harder to extinguish because it is maintained on a partial reinforcement schedule.

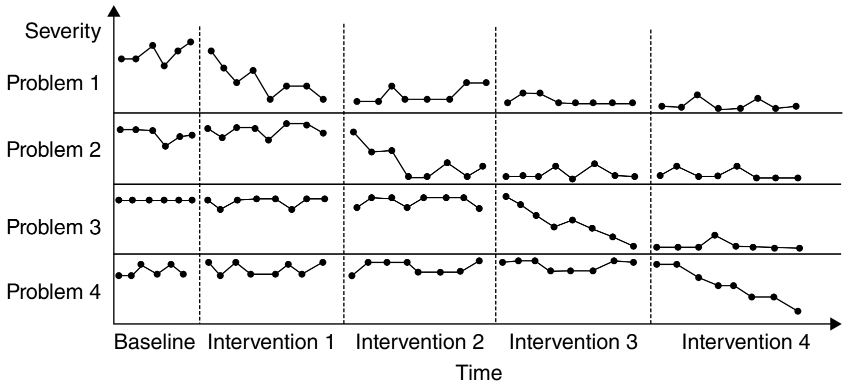


Figure 9.3 The multiple-baseline design

Multiple-baseline Design

With several (presumed independent) target behaviors (e.g., a child who suffers from tantrums, nocturnal enuresis, and dog phobia) or one target behavior in several independent settings (e.g., aggression at home, in the classroom, and on the playground), you can use a multiple-baseline design. Similar interventions targeted at each behavior, or in each setting, are introduced sequentially and their impact on all the target behaviors is measured (see Figure 9.3). The idea is to replicate the effect of the intervention on each particular problem or setting. For example, Chadwick and Trower (1996) used this design to investigate the effects of cognitive therapy for paranoid delusions. The intervention was targeted sequentially at the client's negative self-evaluation and two separate delusional ideas, and each of these problems was reduced in severity in the predicted order.

Barlow et al. (2008) interestingly fit the famous early psychoanalytic case of Anna O. (Breuer & Freud, 1895/1955) into this schema, since Breuer targeted various separate interventions, such as hypnosis and interpretation, at each of Anna O.'s symptoms in turn. However, this design assumes that changes will not generalize from one problem or setting to other problems or settings—that is, like the previous design, it is based on the behaviorist assumption that behavior is situationally specific. Thus, although the within-participant version of this design is theoretically amenable to nonbehavioral therapies, it is not really applicable to therapies that aim for general change, in spite of claims to the contrary (Morgan & Morgan, 2001).

An extension of this design involves replication across multiple cases, which is a special case of a clinical replication series (Hayes et al., 1999: see section below on generalization). For example, Bennun and Lucas (1990) used a version of this design to investigate the impact of a two-component intervention with couples in which one partner had a long-standing diagnosis of schizophrenia. Using a "multiple single-case design" with a sample of six couples, they showed that the first component of the intervention—education—had an impact on the well spouse's perception of their ability to cope, but had no effect on presenting symptoms. The second component of the intervention—problem solving and communication training—had an impact on positive symptoms of schizophrenia.

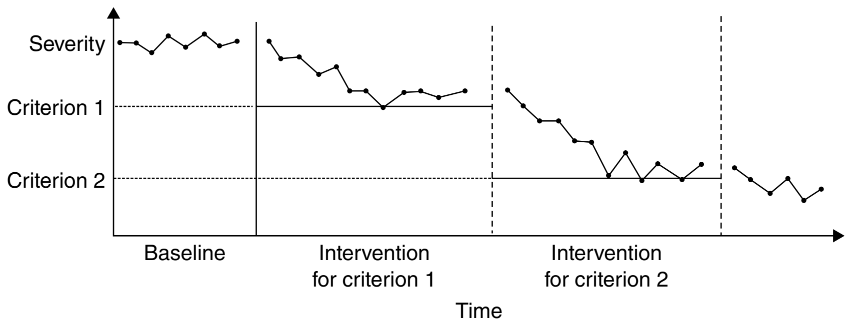


Figure 9.4 The changing-criterion design

Changing-Criterion Design

This design is used to demonstrate experimental control over a single problem behavior that may be progressively reduced in severity (see Figure 9.4). It is particularly useful in working with clients who are dependent on drugs or alcohol. For example, it may be used in a smoking-cessation program, in which the client progressively cuts down to more stringent targets (e.g., Criterion 1 would be 20 cigarettes a day, Criterion 2 would be 15 a day, and so on down). Or it could be used with a positive behavior that is being shaped, for instance, appropriate social interaction in a child with severe autistic behaviors.

Data Analysis

Data from single-case designs are normally displayed on a graph, similar to those in Figures 9.1 to 9.4. Part of the appeal of these designs is that the success or failure of the intervention is usually immediately obvious from the graph (Morley & Adams, 1991). It can often be helpful to show such graphs to the clients, to enable them to monitor their progress and to demonstrate clearly that the intervention is working. An emerging use, discussed in Chapter 11, is to provide feedback to therapists (Shimokawa, Lambert, & Smart, 2010), especially to alert them when the treatment is not going well, but also to provide encouragement when positive progress is occurring.

However, in some cases, the changes may be less clear cut, or a measure of their magnitude may be required. This has led some researchers to call for the use of statistical methods in single-case designs. The topic of which, if any, statistical tests to use for extended time series of related observations is too technical to cover here: Morley and Adams (1989) and Smith (2012) outline some possibilities.

Generalization

Although single-case studies are essentially idiographic, the investigator often wishes to generalize beyond the specific individuals studied in order to make broader claims about the effectiveness of the treatment tested. This can be done by conducting a *clinical replication series* (Hayes et al., 1999), that is, by replicating the same study on several individuals. In this way the external validity of the findings are strengthened. (See Chapter 10 for further discussion of generalization issues.) The notion of a clinical replication series is derived from Cronbach's (1975) concept of *locally intensive observation*. As a finding is tested in other settings, varying conditions will test the limits of its external validity and lead to richer theory:

> As [the researcher] goes from situation to situation, his first task is to describe and interpret the effect anew in each locale, perhaps taking into account factors unique to that locale. … As results accumulate, a person who seeks understanding will do his best to trace how the uncontrolled factors could have caused local departures from the modal effect. That is, generalization comes late and the exception is taken as seriously as the rule. (Cronbach, 1975, p. 125)

Such an approach can be applied equally well in experimental and naturalistic, non-experimental, approaches.

NATURALISTIC CASE-STUDY DESIGNS

As we have noted, behaviorally oriented researchers (e.g., Morgan & Morgan, 2001) often claim that experimental single-case designs can readily be adapted to nonbehavioral treatments. However, the emphasis on observable events and experimental manipulation makes most of these designs problematic for studying psychodynamic, experiential, and cognitive therapies, which focus on cognitions and emotions, and often lead to irreversible changes in the client. Naturalistic case-study designs—the narrative case study, the systematic case study, and time-series designs—are usually more appropriate to these types of therapy.

Narrative Case Studies

The narrative case study is the traditional description of a client or treatment, based on the clinician's case notes and memory. Freud's case histories, such as "Little Hans" (Freud, 1909/1955) or "Dora" (Freud, 1905/1953), are classic examples of this genre. Case studies have played an important role in the development of the psychological therapies. They can serve a number of purposes (Barlow et al., 2008; Dukes, 1965; Lazarus & Davison, 1971). These include (1) documenting the existence of a clinical phenomenon, often a rare one (e.g., early case studies of multiple personality disorder), (2) disproving a universal proposition by demonstrating a counter-example (e.g., the proposition that only women suffer from hysteria could be disproved by documenting the case of a man with hysteria), (3) demonstrating a new intervention, and (4) generating hypotheses about causes. Valuable information can be gathered from case studies, as long as their nature and limitations are understood. In general, case studies can tell us what is possible, but not what is typical. Similarly, they can suggest a possible connection or cause, but cannot provide strong confirmatory evidence for it.

Uses of narrative case studies:

- Documenting the existence of a phenomenon
- Disproving a universal proposition
- Demonstrating a new intervention
- Generating causal hypotheses

However, Spence (1986) and others have argued that narrative case studies such as Freud's contain too much "narrative smoothing": that is, they are too selective and have often been altered (either deliberately or unconsciously) to tell a better story. Narrative distortions can be investigated by a self-experiment on one's own clinical work (see box below).

Like the one-group posttest-only design that we discussed in Chapter 8, narrative case studies can be used to infer possible causal explanations if sufficient additional information is available. In psychohistorical case studies (see Chapter 6), for example, Runyan (1982) points out that careful consideration of the known facts often allows the researcher to rule out most of the possible competing explanations for an event.

Self-experiment on narrative distortion

Audio-record a therapy session. A day later (or even an hour later), write down from memory a brief chronological account of what happened during the session. Then, listen to the recording while taking detailed notes and noting any inaccuracies. In addition to large amounts of missing material, you will also find that you have collapsed things that happened at different times, got some things out of order and may have attributed statements to the wrong speaker or even completely fabricated things.

Systematic Case Studies

Given the problems with narrative case studies (reliance on memory, anecdotal data collection, narrative smoothing), it is worth considering how to improve the quality of information from case studies, in order to strengthen the conclusions which may be drawn from them. Methodologists such as Kazdin (1981, 2011) and Hayes et al. (1999) have considered more systematic approaches to single-case research on clinical interventions, and have proposed the following general features for improving their credibility:

- systematic, quantitative (versus anecdotal) data;
- multiple assessments of change over time;
- multiple cases;
- change in previously chronic or stable problems; and
- immediate or marked effects following the intervention.

The combination of these features substantially improves the researcher's ability to infer that a treatment caused an effect (i.e., it increases the internal validity of the study). Note that the first three features are design strategies over which the researcher has some control, while the last two (previous stability and discontinuous change) are outcomes specific to the particular case.

As part of a recent renewal of interest in systematic case studies (e.g., McLeod, 2010; Miller, 2004), several writers have recently proposed adding further elements of good practice in case study research to this list. In his development of Hermeneutic Single-Case Efficacy Designs, Elliott (2002) has advocated expanded single-case designs that take an interpretive approach to examining client change and its causes. These designs aim to: (1) demonstrate that change occurred; (2) examine the evidence for concluding that therapy was responsible for the change; (3) examine alternative explanations for the change; and (4) examine which processes in therapy might have been responsible for change. They emphasize the use of a *rich case record* of comprehensive information on therapy outcome and process (e.g., using multiple perspectives, sources, and types of data), and *critical reflection* by the researcher, who systematically evaluates the evidence.

A number of procedures, involving varying degrees of time and effort, may be used. We will address each of the above four areas, giving suggestions for how systematic case studies could be carried out by practicing clinicians. A good example of a systematic case study which illustrates many of the design features described here is Parry et al.'s (1986) study of "the anxious executive" (see box).

Example of a systematic case study: "The anxious executive" (Parry et al., 1986)

Parry et al. (1986) present a systematic case study of a senior manager who sought help for anxiety and depression related to stress at work and in his marriage. The case was drawn from a large research project examining psychotherapy outcome. Using multiple quantitative measures, including the Shapiro Personal Questionnaire, as well as therapist and client session-by-session accounts, the study examined in detail the changes that occurred over the course of therapy. It was able to identify the characteristics of sessions that had particular short-term outcomes, both positive and negative. It offers a good example of the potential strength of systematic case studies for providing a rich description of process and outcome.

Demonstrating that Change Occurred

The task here is to improve upon anecdotal impressions of client improvement or deterioration. There are several options, which we have roughly ordered from least to most time-consuming, so that clinicians may begin with a minimum requirement and work up to more elaborate procedures.

- Administer a *simple standardized measure*, tailored to the particular client's problem, before and after therapy. For example, give the Generalized Anxiety Disorder scale (GAD-7: Kroenke et al., 2007) to an anxious client.

- Add an *individualized measure,* before and after therapy (Mintz & Kiesler, 1982; Sales & Alves, 2012). These ask clients to identify the major problem areas that they want to change, and to rate the severity of these problems. For example: goal attainment scaling (Kiresuk & Sherman, 1968), the target complaints procedure (Battle et al., 1966) or the Personal Questionnaire in both its original (Phillips, 1986; Shapiro, 1961a) and simplified versions (Elliott et al., 2015).
- Use *additional standardized measures*, covering a broader range of variables. These may include measures of clinical or interpersonal distress, for example, CORE Outcome Measure (Barkham et al., 2001), the Inventory of Interpersonal Problems (Horowitz, Rosenberg, Baer, Ureno, & Villasenor, 1988), or a global symptom inventory such as the SCL-90-R (Derogatis, 1994).
- Add *more assessment points*, for example at mid-treatment (or every 5 to 10 sessions) or at follow-up (e.g., six months after treatment). A session-by-session measurement procedure, known as *case tracking* (Leach & Lutz, 2010), can also be extremely useful. It ensures that some measure of outcome is collected if the client drops out of treatment, and, more importantly, gives feedback to therapist and client about the progress of therapy (Shimokawa et al., 2010). Any brief, easy-to-complete client or therapist-rated measure can be used for this purpose, including many of the measures mentioned above.
- Use a *qualitative approach.* As McLeod (2011) has argued, outcome has both qualitative as well as quantitative elements, and qualitative interviews may be more sensitive to negative or unexpected effects, as well as allowing researchers to evaluate the plausibility of clients' claims to have changed. The Change Interview (Elliott, 1999; Elliott, Slatick, & Urman, 2001) is one example of a semi-structured qualitative outcome interview that can be given every 5 to 10 sessions, and at the end of therapy.
- Add further cases, creating a *clinical replication series* (Hayes et al., 1999).

Linking Change to the Therapy

Here the task is to provide evidence to support a causal link between therapy and client outcome. As in single-case experimental designs, your conclusions are more credible if data suggesting causal links can be replicated within the case. Such evidence may also help to identify the effective ingredients of the intervention. Forms of potential evidence may include:

- *Client self-report* about therapeutic effectiveness. This may include general client satisfaction measures (e.g., the Client Satisfaction Questionnaire; Larsen et al., 1979) or measures that identify specific helpful aspects of therapy (e.g., the Helpful Aspects of Therapy form; Llewelyn, 1988).
- Significant *within-case correlations* between theoretically relevant within-session processes (e.g., therapeutic alliance) and session outcomes.
- Qualitative evidence of important *within-therapy events* immediately preceding client improvements (e.g., particular themes addressed within a therapy session are followed by changes related to those themes).
- Evidence of *reliable change* in stable or chronic problems.

Evaluating Alternative Explanations

In addition to evaluating evidence that links client change to therapy, it is also important to search systematically for evidence that nontherapy processes may account for change. Cook and Campbell's (1979) list of internal validity threats (see Chapter 8) can be used; in addition, Elliott (2002) highlights validity threats that are specific to single-case studies. For example, Elliott proposes eight nontherapy explanations for apparent client change. The first four involve the possibility that the client has not really improved:

- *Nonimprovement.* Apparent changes are trivial or even negative.
- *Statistical artifacts.* Apparent changes reflect statistical artifacts, such as measurement error or regression to the mean.
- *Relational artifacts.* Apparent changes reflect attempts to please the therapist or researcher.
- *Client expectations.* Apparent changes reflect client expectations or wishful thinking.

The next four explanations assume that client improvements are real, but that nontherapy factors account for them:

- *Self-correction.* Changes are due to client self-help efforts independent of therapy, or the self-limiting nature of short-term or temporary problems.
- *Extra-therapy factors.* Changes result from life events outside of therapy, such as changes in relationships or work, or from help obtained from friends or family.
- *Psychobiological factors.* Changes are caused by medication or other remedies, or by recovery from physical illness.
- *Reactive effects of research.* Changes can be attributed to taking part in research, including interactions with research staff, altruism, and increased self-monitoring.

The researcher's task is to systematically evaluate both positive evidence (in favor of therapy as the cause of change and against nontherapy factors) and negative evidence (against therapy as the cause of change and in favor of nontherapy explanations). This weighing of both sides is analogous to political debate or legal proceedings, and it can be carried out by the researchers themselves or by independent judges. Case study researchers including Elliott et al. (2009) and Miller (2011) have developed adjudicational or legalistic procedures for weighing the complex, often contradictory information collected using systematic case study methods.

Examining Therapy Process

There are a variety of systematic ways to assess therapeutic process, that is, what happens in a session and the client's reactions to that session (see Greenberg & Pinsof, 1986). Such information helps to elucidate the nature of therapeutic relationships and has the potential for generating theory about the mechanisms of change (Kazdin, 2007; Laurenceau, Hayes, & Felman, 2007). Stiles (2007) proposes a theory-building model of research in which a general theoretical understanding (e.g., of the process by which clients come to assimilate problematic experiences in therapy) is constructed and elaborated through a series of single-case studies. Process data can therefore also be used to support inferences about linking client change to the therapy (see section above). Some ways of examining therapy processes include:

- *Records of therapy sessions.* Audio or video recordings, or detailed process notes, are an excellent source of information about what actually happens in sessions. (They can also be used to corroborate or clarify self-report data.)
- *Therapeutic relationship measures* can be administered every session, or less frequently (e.g., every three to five sessions). The most widely used such measure today is the revised short form of the Working Alliance Inventory (Hatcher & Gillaspie, 2006).
- *Self-report session measures* can be completed by the client or the therapist. The Helpful Aspects of Therapy Form (Llewelyn, 1988) is a qualitative measure of client perceptions of significant therapy events. The Session Evaluation Questionnaire (Stiles, 1980) and the Session Impacts Scale (Elliott & Wexler, 1994) are quantitative measures of clients' immediate reactions to sessions.
- *Orientation-specific measures,* completed by the therapist or the supervisor after each session, can be used to assess the therapist's adherence to the treatment model (e.g., the Revised Cognitive Therapy Scale: Blackburn et al., 2001).

Time-Series Designs

The final example of naturalistic case study designs is the *time-series design* (Borckardt et al., 2008). The aim of this design is to evaluate causal processes using correlational methods. Two or more variables are monitored over time and their interrelationship is examined statistically; a large number of observations is needed in order to meet the statistical assumptions behind the analysis. These designs originated in econometrics, where, for example, the effect of one year's interest rates on the following year's economic activity may be analyzed using monthly data over 25 years, which yields 300 data points.

Gottman and his co-workers have promoted these methods within clinical psychology in general and in the study of psychological therapies in particular (e.g., Gottman, 1981; Gottman & Roy, 1990). Complex statistical methods are needed to assess the evolving relationships within and between variables (Borckardt et al., 2008; Gottman, 1981). An interesting application was Moran and Fonagy's (1987) use of time-series methods to study the process and impact of child psychoanalysis on an adolescent girl with diabetes. They demonstrated an association between certain psychoanalytic content themes, for instance, the girl's anger with her father, and the study's principal outcome variable, variations in her blood glucose level.

CONCLUSION

Small-N designs thus represent both a way to look at individual uniqueness and complexity, and also a viable research method for practicing clinicians. Like all research methods, they have their strengths and weaknesses. They are good for looking at phenomena in depth, demonstrating that certain phenomena exist, or disconfirming theories by providing counterexamples. They are poor at establishing typicalities or general laws.

In line with our methodological pluralism stance, we would argue that a thorough investigation of any topic area needs to combine both large-N and small-N approaches. It is possible, even desirable, to examine single cases within the context of a larger group-comparison study. Rogers's (1967) classic case of "A silent young man" is taken from a larger experimental study, as is Parry et al.'s (1986) case of "The anxious executive." These two examples both give a human dimension that is lacking in the predominantly statistical reports from the larger projects. Beyond this, Stewart and Chambless (2010) showed that practicing therapists are more likely to be influenced by case study data than statistically significant group results.

CHAPTER SUMMARY

There is a central distinction between nomothetic and idiographic approaches to research. Nomothetic designs look at groups of individuals (see Chapter 8); idiographic designs look at separate individuals in depth. Idiographic designs, often called small-N, single-case, or N = 1 designs, can be appealing to clinicians as a way of combining research and practice. They derive from several different traditions: narrative case studies in neuropsychology and medicine, operant behaviorism, Shapiro's single-case approach, and idiographic research in personality theory. Although varieties of case study design exist along a continuum of measurement and experimental control, they can be roughly grouped into two main types of design: single-case experiments and naturalistic case studies.

Single-case experiments are most often used within operant behavioral approaches to therapy, to demonstrate the intervention's control over a problem behavior. They are characterized by frequently administered measurements and the experimental manipulation of an intervention. They have a baseline phase before the intervention is introduced; the participant thereby serves as their own control.

Naturalistic case studies range from narrative approaches, such as Freud's, to more structured studies using systematic measurement of process and outcome. Several authors have articulated criteria for increasing the credibility of case studies. A number of procedures can be used to demonstrate that client change occurred and that it was linked to the therapy.

FURTHER READING

Most of the references on single-case experimental designs cover similar ground. Barlow et al. (2008) and Kazdin (2011) are the two standard textbooks; there are good chapter-length treatments by Gaynor, Baird, and Nelson-Grey (1999) and McMillan & Morley (2010). Hayes et al. (1999) and Morgan and Morgan (2001) set these designs against a background of scientist-practitioner professional issues, while Smith (2012) reviews published research and gives some suggested standards.

McLeod (2010) and Yin (2009) discuss case studies as a general research method. It is a good idea to read some of the classic narrative case studies, both from a research and a clinical point of view. Any of Freud's are worthwhile: "Little Hans" (Freud, 1909/1955) or "Dora" (Freud, 1905/1953) provide a good starting point. On the behavioral side, there is Watson and Rayner's (1920) famous (and ethically dubious) case of Little Albert. Carl Rogers pioneered the use of audio recording to study client-therapist interaction in single cases: his case of "A silent young man" (Rogers, 1967) is an excellent example of a process-oriented narrative case study. Parry et al. (1986) and Moran and Fonagy (1987) are interesting examples of case studies using more intensive quantitative methods. McLeod (2010) provides a useful summary of a new generation of systematic case study methods; Elliott et al. (2009) provide a detailed example of one of these, using an adjudicated interpretive approach.

The Barkham et al. (2010) edited volume on practice-based evidence, in which, to declare an interest, the present authors all have chapters, is a valuable collection of ways that practitioners can generate useful data from their day-to-day clinical work.

More information on the measures mentioned in this chapter can be obtained from the references cited; several can be obtained from the website of the Network for Research on Experiential Therapies (http://experiential-researchers.org).

QUESTIONS FOR REFLECTION

1. What, if any, conclusions do you think can be drawn from Freud's case studies? What could he have done to make them more convincing?
2. Under what circumstances, if any, do you think it is permissible to generalize the results of a case study with an individual client beyond the specific individual studied?
3. If you haven't already done so, carry out the exercise described earlier in this chapter in which you compare process notes composed from memory with the recording of the session. What kinds of discrepancies are there?
4. Think about a recent client you have worked with. How could you have documented what kinds of changes occurred and why?

10

The Participants: Sampling and Ethics

KEY POINTS IN THIS CHAPTER

- Sampling refers to the process of obtaining the participants for the study.
- It involves specifying the target population, choosing the sampling procedure, and determining the sample size.
- The external validity of a study is the degree to which its results can be generalized.
- In most types of research, it is important to have an unbiased sample that is representative of the target population from which it is drawn.
- In quantitative research, sample size is determined by statistical power analysis.
- There are several alternative approaches to sampling, used in small-N and qualitative research.
- Ethical principles are concerned with protecting the rights, dignity, and welfare of research participants.
- The central ethical issues are informed consent, minimization of harm, and confidentiality.

The final aspect of design concerns the participants in the research. It addresses the "who?" question that we posed in Chapter 8: who will you be studying, and to whom do you intend to apply the findings of the study? We will also consider ethical issues here, since they concern the researcher's relationship with the participants.

We usually prefer the term "participants" to the old-fashioned, but still current, term "subjects." The latter term, with its monarchic connotations, has undesirable

Research Methods in Clinical Psychology: An Introduction for Students and Practitioners,
Third Edition. Chris Barker, Nancy Pistrang, and Robert Elliott.

Companion Website: www.wiley.com/go/barker

implications of powerlessness and passivity. The stock phrase "running the subjects" is especially to be avoided: one of our students once wrote something like "the subjects were run in their own homes," which conjures up an image of indoor jogging. For interviews and questionnaires, you can speak of "respondents" or "interviewees." (For observational research, the term "observees" has not yet taken off.) In ethnography, the term "informants" is typically used, although this has unfortunate connotations of surreptitiousness. New paradigm researchers (e.g., Reason & Rowan, 1981) and participatory action researchers (e.g. Jason et al., 2004) may use the term "co-researchers," to emphasize the idea of the participant as an equal partner in a collaborative research enterprise.

This chapter has two separate sections: sampling and ethics. We have placed this material here, after the chapters on measurement and the first two chapters on design, because some of the issues to be considered depend on knowledge of those topics. Furthermore, beginning researchers often focus on the population and how it will be sampled before they have formulated what they will be studying. However, some of the issues covered in this chapter will inevitably need to be thought about during the groundwork phase of the project, after choosing the area to be investigated and developing the research questions (see Chapter 3). Problems of access to populations are bound up in some of the organizational and political issues that we discussed in that chapter. At an extreme, if there is no sample available, there is no study.

SAMPLING

Sampling refers to the process of specifying and obtaining the participants for the study. There are three steps: (1) specifying the target population; (2) choosing the sampling procedure; and (3) determining the sample size. Usually the steps are sequential, though they can be iterative. Sometimes, for example, the sample size can influence the sampling procedure. We will deal with each in turn, and then consider some alternative approaches. Although we will mainly be using language associated with the quantitative research tradition, we intend our discussion to have a general application. Qualitative researchers may sometimes be less concerned about representativeness, but we contend that all researchers must decide, implicitly or explicitly, how to respond to these sampling issues.

It helps to think in terms of three nested sets (see Figure 10.1):

- The *universe* is the broad population to which eventual generalization of the findings is desired.
- The *target population* is the defined group from which the participants in the study are to be selected.
- The *sample* is the subset of the target population consisting of those participants who actually take part in the study. There may be a gap between the ideal and the actual sample: the terms *intended* versus *achieved sample* can be used to denote this.

For example, you may be interested in the prevalence of depression in British women who consult their general practitioners (family doctors). In this case, the universe may

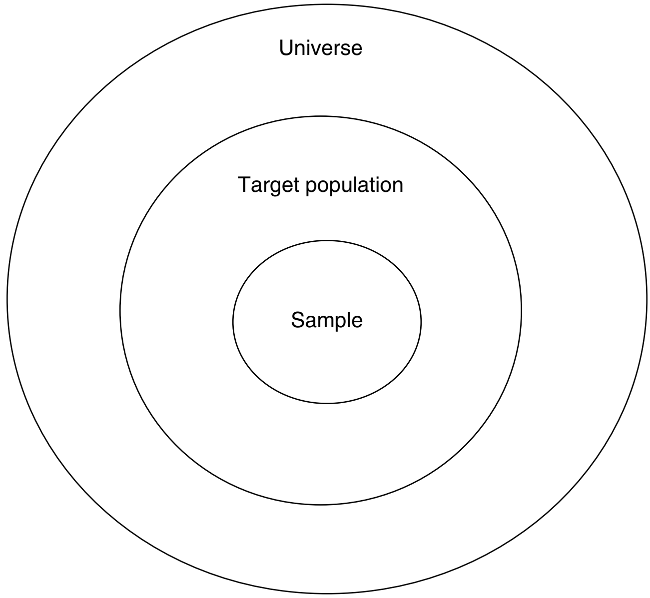


Figure 10.1 The universe, the target population, and the sample

be all British women who visit their general practitioners, the target population all women who consult 10 specific doctors in September 2015, and the intended sample one in 20 of those women. The achieved sample will be the subset of women actually interviewed. In the case of a census, the universe, the target population, and the intended sample are one and the same (e.g., in a national census they consist of all members of that country's population), although the achieved sample will fall short of this, as some people are inevitably missed out.

A quantitative measurement made in a sample is called a *statistic;* it is usually done to estimate a population *parameter*. For instance, the prevalence of depression in a sample of 163 women visiting their general practitioners is a statistic; this may be used to estimate the overall prevalence of depression in the target population of women users of those practices, which is a parameter.

Generalizability

Usually researchers are not just interested in the specific sample itself; rather they want to extend the findings to other groups. The extent to which this is possible is referred to as the *external validity* of the study (Cook & Campbell, 1979). External validity is captured by the question, "Does it generalize?" or, more fully "To what extent do the results of my study apply beyond the specific people, situations or incidents in the sample, to others like them?" Of course, generalizability is not just a matter of sampling, since it also involves consideration of the setting, the time, the measures, and so on. We consider these aspects of external validity later on, when we discuss analysis and interpretation in Chapter 12.

From a purely sampling point of view, there are two types of generalization, corresponding to the transitions from one subset to the next in Figure 10.1. The first type is generalizing from the sample to the target population. In quantitative research this is known as statistical inference, and there is a well-established set of procedures to accomplish it. However, these procedures make certain assumptions, such as unbiased sampling from the target population, which we will examine below. The second type is generalizing from the target population to another population or to a larger universe. This is done on the grounds of general plausibility, rather than any statistical argument. For example, can the results of a study of socially anxious patients seen at hospital X in Los Angeles be generalized to hospital Y in New York? To people with social anxiety who do not seek help? To other countries or cultures? If these groups of people are plausibly similar enough, then the results can be generalized across them; if not, then the findings must be considered as specific to the original target population until replications in other populations are conducted.

In the case of qualitative and small-N research, the argument for generalizability always depends upon plausibility, in a similar way to the second step of generalization in quantitative methods. Some qualitative researchers reject the whole notion of generalizability across populations (see Chapter 12).

The importance of external validity depends on the type of research. Basic research on general human processes assumes external validity, since it seeks universal generalizability. Applied research may also seek to generalize, though often less widely, for example, to a particular client group. For evaluation and action research, external validity is often less important, since the research seeks an understanding of, and solutions to, a particular problem in a particular setting, and seeks generalizability only to the immediate future.

The Target Population

The first step in sampling is to specify the target population. It can be defined in terms of, for instance, gender, social class, problem type, or problem severity. The definitions are usually phrased in terms of specific *inclusion* or *exclusion criteria*. The sample may be defined narrowly (e.g., married women aged 35–45 living within the Liverpool city boundaries with no significant medical or psychiatric history) or broadly (e.g., all British women aged 18 and over). Narrowly defined populations are called *homogeneous,* broadly defined ones *heterogeneous.*

Researchers must make a trade-off when deciding upon the breadth of the target population. A homogeneous sample has the advantage of reducing the degree of extraneous variability (i.e., statistical noise) in the sample, which gives more power to detect effects that you are interested in and more precision in estimating the magnitude of those effects. In analysis of variance terms, homogeneity reduces the proportion of error variance to total variance. For example, if you are researching the influence of stressful life events on depression, any relationship will be harder to detect in a more heterogeneous sample, since depression is a function of many variables other than life events.

On the other hand, the increase in precision from a narrow definition of the target population is bought at the expense of the following:

1. There will be reduced generalizability to a larger universe (e.g., if you are studying women in their thirties, the findings will not necessarily apply to women of all age groups).

2. Practical difficulties will result, including the problem that the more stringent are the inclusion criteria, the harder it is to find participants, since more people have to be screened or you have to get referrals from more specialized services.
3. Having a homogeneous sample precludes examining individual differences, for example, if there is little variability in age within your sample, you cannot look at age as an individual difference variable.

Bias and Representativeness

In order to make inferences from the sample to the target population from which it is drawn, the sample should ideally be an *unbiased* sample of that population. This means that every member of the target population should have an equal chance of being selected for the sample. A number of different sampling techniques may be used to generate a representative sample (Minke & Haynes, 2011; Sudman, 1976). For example, in probability sampling every member of the target population has a given chance, say one in 10, of being included in the study; whereas in stratified sampling, the target population is first subdivided into groups, for instance, according to social class or diagnostic variables, before making the allocation to the study.

Psychologists are typically careless about sampling methods: they tend to rely on *convenience sampling* (i.e., whoever they can get) and hope that their results will generalize, if the sample is large enough. However, it is wrong to assume that a sample large enough to have sufficient statistical power is large enough to ensure generalizability. No matter how large the sample is, you can only generalize safely if the sample is representative of the target population. A sample of 5000 male college students still does not allow you to generalize your findings to a population of female factory workers.

However, eliminating bias is not always feasible. Even with a well-designed sampling plan, there is usually a gap between the intended and the achieved sample. For example, research using postal questionnaires often has at least a 30% nonresponse rate (Dillman et al., 2009). Nonresponders usually differ considerably from responders, in terms of interest, motivation, educational level and so on. Similarly, studies which recruit volunteers via advertisements or the internet may get an unrepresentative sample.

Sometimes, it is possible to estimate the nature of the sampling bias and partially control for it statistically when you analyze the data. For example, if respondents are older on average than nonrespondents, you can look at the association of age with whatever variable you are studying, and possibly use partial correlations to remove its influence. However, as we discussed under the nonequivalent groups pretest–posttest design in Chapter 8, post-hoc statistical adjustments can only partially compensate for a biased sample, because of unreliability of measurement and because you can never fully compensate for all possible variables on which bias may occur. Such post-hoc analyses are often worth doing, but must be treated with caution.

Another serious drawback of the convenience sampling approach is that minority populations may be underrepresented. For example, Graham (1992) analyzed the characteristics of participants in studies published in the major American Psychological Association journals. She concluded that all too often papers reported that "most of the subjects were white and middle class" and that psychological research has ignored black and ethnic minority participants. In the same vein, Arnett (2008) has also pointed out that most papers in these journals focus narrowly on Americans, to the exclusion of the rest of the world's population, and Henrich, Heine, and Norenzayana (2010) describe the typical research participant as WEIRD, that is from Western, educated, industrialized, rich, democratic societies.

Sample Size

From the point of view of inferential statistics, the obvious rule of thumb is that the larger the sample is, the better, since you are then more able to separate out the variance associated with the effects you are interested in from the variance due to errors of sampling and measurement. In other words, with a large sample you are more able to separate the signal from the noise. However, as Cohen (1990) has pointed out, a sample can be too large, in the sense that it exceeds the requirements for statistical power (see below), thus involving a waste of research effort, and it is also likely to identify trivially small effects. If you are fortunate enough to be well funded, a better strategy may be to carry out several smaller studies on different populations, rather than one large one.

The attainable sample size will also depend on practical issues, such as recruitment difficulties, time constraints, finances, and the rarity of the condition studied.

Statistical Power Analysis

The main way of estimating the appropriate sample size is known as statistical power analysis (Cohen, 1988, 1990, 1992). In a nutshell, the statistical power of a study is the likelihood that it will detect an effect that is actually present, for example, a difference in effectiveness between two treatments. It is analogous to the power of a microscope in laboratory research. Just as a study using a low-magnification microscope will miss out fine details, so a low-power study in psychology will have a low chance of detecting subtle effects; conversely a high-power study will have a good chance. Many studies in clinical psychology have simply not been powerful enough and thus may have overlooked the presence of important effects (Cohen, 1990; Kazdin & Bass, 1989).

In any study, there are four related parameters. For any given statistical test, if you know any three of them, you can calculate the fourth.

- The *sample size* (N) is usually what you want to determine, but, if you know it in advance, you can calculate the size of effect that the study is powered to detect.
- *Alpha* (α) is the probability of detecting an effect when in fact none exists (this is called a *Type I error* or *false positive*). In most psychological research, alpha is set by arbitrary convention at 0.05, but a more lenient value of 0.10 is sometimes used for exploratory research or defining nonsignificant trends. On the other hand, more stringent values (e.g., 0.01 or 0.001) may be used to increase the confidence in one's findings or to control for the effects of conducting multiple tests of statistical significance.
- *Beta (β)* is the probability of missing an effect which is in fact present (this is called a *Type II error* or *false negative*). *Statistical power* is defined as 1 minus beta ($1 - \beta$): it is the probability of detecting an effect that is really there. As Cohen (1988, 1992) recommends, the standard, widely used level for power

is 0.80. It is inadvisable to design a study whose power is less than 0.50; you are wasting your time and that of your participants if you design a study that has less than a 50–50 chance of finding an effect that is present.

- *Effect size* is the key concept in power analysis. It is a measure of the strength of the underlying relationship that you are interested in. Effect sizes are usually talked about in terms of small, medium, and large effects. A large effect can be thought of as one that is large enough to see with the naked eye—that is, without statistical analysis. The way the effect size is calculated depends on the type of statistical methods used in the study (e.g., chi-square, t-test, correlation, or analysis of variance). This is reviewed by Cohen (1988, 1992), who presents rough standards for what amounts to a small, a medium, and a large effect with each type of statistical test (see also Vacha-Haase & Thompson, 2004). For example, in correlational studies, a Pearson correlation coefficient of 0.10 is considered to be a small effect, 0.30 a medium effect, and 0.50 a large effect. Clinical psychology researchers usually deal with medium effect sizes, though small effects may be of interest in epidemiological research. Note that effect size is not the same as clinical significance (see Chapter 12); an effect may be large but trivial, if the variable which shows the effect is trivial.

In order to estimate the required sample size for your study, you need to carry out a statistical power calculation. For this, you first need to select your alpha and beta levels and establish your effect size. As discussed above, the most commonly adopted values are an alpha of 0.05 and a power of 0.80. A rough estimate of the effect size can be obtained from previous research or theoretical knowledge of the topic area. It is usually worth trying out the calculation for a range of effect size estimates.

Power analysis tables are provided in various sources. Cohen (1988) and Kraemer and Thiemann (1987) give detailed treatments, and Cohen's (1992) "power primer" presents a clear summary of the central concepts and a useful table to calculate sample sizes for common designs. There is also software available online to do the calculations such as G*Power (Faul, Erdfelder, Lang, & Buchner, 2007), which is freely downloadable, and there is a list of web-based calculators at http://statpages.org/#Power. Tables 10.1 and 10.2 summarize the sample size estimates for two common statistics, t-tests and correlations. For example, in a design which compares two groups using a t-test, with medium effect sizes and an alpha of 0.05, a sample of 64 per group is needed to attain a power of 0.80; with a larger effect, the required sample size decreases. Studies with many variables (e.g., factor analytic studies of long inventories) or many subgroups (e.g., norming a psychological test on different subpopulations) require larger samples. In fact, the sample size requirements for certain types of research, for

Table 10.1 Estimated sample sizes for t-tests

Effect size (Cohen's d)	*n per group*	*total n*
medium (d = 0.5)	64	128
large (d = 0.8)	26	52

Note: alpha = 0.05; power = 0.80

Table 10.2 Estimated sample sizes for correlations

Effect size (Pearson's r)	*total n*
medium (r = 0.3)	84
large (r = 0.5)	28

Note: alpha = 0.05; power = 0.80

example, comparative therapy outcome research, are so large that we recommend that you only conduct such studies if you have the adequate funding and staffing to do so.

Alternative Approaches to Sampling and Generalizability

Qualitative research typically uses smaller samples than traditional quantitative research, as does, obviously, small-N research. Unsurprisingly, the most common criticism of such research is that you cannot generalize the results. In this section, we will describe some alternatives to the traditional approach to sampling and generalizability.

Generalizability through Replication

A rational (as opposed to a statistical) approach to generalizability and sampling can be found in the behavioral N = 1 tradition (see Chapter 9), in which research is carried out one case at a time, varying the conditions and relevant client characteristics and measuring the effects until you achieve an understanding of the causal relationships involved. The relevant characteristics of the case, including any background and situational variables that appear to be important, are carefully described.

In this approach, you then attempt to replicate the first case study by finding a case as similar as possible to the first case (this is referred to as *direct replication:* see Sidman, 1960). If you obtain different results (i.e., there is a failure to replicate), you try to understand what made this case different from the first, and then try to find a case that matches the first (or second) on this variable. If the same results are obtained, you next begin to vary apparently relevant features of the case in order to establish the limits of generalizability in a rational manner (this is referred to as *systematic replication)*. Replications establish the breadth or range of generalizability, while failures to replicate establish the limits of generality, just as a control group would in traditional research; the two complement each other. Thus, as Cook and Campbell (1979) note, external validity is better served by a number of small studies with specified samples than by a single large study. Cronbach (1975) refers to this approach as *locally intensive observation*; Hayes et al. (1999) call it a *clinical replication series* (which can also be thought of as a form of multiple baseline design).

Bayesian Approach

Statistical power analysis relies on the traditional null hypothesis testing approach to statistics. In contrast, researchers working from a Bayesian approach consider that any new data adds to the sum of knowledge. Therefore small-sample, "underpowered" studies are not necessarily to be avoided, but it is still the case that the larger the sample size, the more the study will add to prior knowledge (Dienes, 2011, 2014; Edwards, Lilford, Braunholtz, & Jackson, 1997).

Falsificationist Approach

From a falsificationist framework (cf. Popper, 1963; see Chapter 2), researchers are not concerned with representativeness or generalizability, but with looking for counterexamples to existing theory. These could consist of a single case (Dukes, 1965; Meehl, 1978). For instance, in clinical neuropsychology, a single example of a patient with a certain pattern of abilities may invalidate a proposed model of mental structure (Shallice, 1988). In these circumstances, qualitative or quantitative descriptive research that establishes the existence of the counter-example can be of crucial importance.

Networking or Snowballing

When the size and composition of the target group is unknown at the outset, it is possible to use a sampling procedure known as *networking* or *snowballing* (Patton, 2002; Biernacki & Waldorf, 1981), which operates by asking each respondent to name one or two other people who fit the research criteria. Sampling continues up to the point where no new respondents are identified or the intended sample size is reached. For example, Pistrang (1990) used this method to study the mental health needs of London's Chinese community. She wanted to interview community and health workers who were involved with the Chinese population in London's West End. Before the project started, it was not known precisely how many such workers there were or where they were to be found, but increasing numbers of interviewees were located via networking as the project progressed, up to a final total of 20.

A potential problem with the snowballing procedure is that the initial respondents might direct you to other like-minded people who share their viewpoint, and thus the researcher needs to be aware of possible biases in the achieved sample.

Purposive Sampling

In qualitative and case study research, the term *purposive sampling* is often used to denote a systematic strategy of selecting the participants according to criteria that are important to the research questions. It is similar to specifying the target population in quantitative research, in that the researcher attempts to select participants fitting specific criteria, but it is a less rigid process, being guided by the researcher's judgment (Robson, 2014). One example is *heterogeneity sampling*, where the researcher attempts to recruit participants with a broad range of demographic characteristics, for example, for members of a focus group in qualitative research.

Theoretical Sampling

In grounded theory, the sampling approach is referred to as *theoretical sampling* (Corbin & Strauss, 2015). It is a type of purposive sampling, in which the researcher's emerging theory determines the sampling strategy as the study develops. In this approach, you need to start analyzing your data early on in the data collection process. Theoretical sampling resembles the replication sampling approach of the behavioral single-case researchers. The difference is that the behaviorists are trying to establish control over behaviors, while the grounded theorists are trying to develop a rich description and test emerging theory.

The procedure is that the researcher analyzes the data as they are collected, and develops tentative theoretical concepts from early on in the study. As ideas form about what the important dimensions or conditions are, the sampling strategy is modified to take these into account. For example, in a study of postpartum depression, the researcher may theorize, after interviewing several women, that an important aspect is the degree of control that the women experienced over their childbirth. She may then sample women who had different types of childbirth procedures, in order to examine variations in perceived control and its consequences.

In grounded theory, sampling stops when little new information emerges and a rich set of categories has been developed. This is referred to as *saturation* (Corbin & Strauss, 2015), a useful concept that applies to sampling within a wide range of qualitative methods.

Internet Sampling

The internet provides a convenient way for researchers to recruit participants and also to carry out research procedures, such as surveys, experiments, and interviews. Recruitment can be via websites, targeted emails, or social media. It is particularly useful for recruiting people experiencing rare conditions (Birnbaum, 2004).

Some concerns have been raised about the quality of samples gathered via the internet: potential problems are unrepresentative samples, fraudulent data, and multiple responding (Kraut et al., 2004; Wright, 2005). It is also important to remember that a digital divide still exists, with economically disadvantaged members of the population being less likely to have internet access (Dillman et al., 2009). However, the emerging evidence appears to indicate that the characteristics of internet samples are largely comparable to those in studies reported in mainstream journals, using samples recruited offline (Gosling, Vazire, Srivastava, & John, 2004).

Summary and Conclusion

The essential point is that researchers need to think carefully about whom the conclusions of their study can apply to and how they are going to support the strength of those conclusions. All too often, clinical psychology researchers seem to neglect sampling and generalizability issues. Unfortunately, there is a long tradition of clinicians making overconfident generalizations based on observations of the biased sample of clients who have appeared in their consulting rooms. Freud's case histories were partly responsible for this, as modesty in drawing inferences was not one of Freud's characteristics. Late-19th-century neurotic Viennese women seeking psychoanalysis are not a good foundation on which to base general theories about the human condition; or, more precisely, it is possible to form one's theories with such a population, but they must be replicated in other ways if they are to have credibility. Clinicians often seem unaware that people who seek professional help for their psychological problems are in a minority (e.g., Wang et al. 2005). Thus clinical researchers need to develop more humility about the limits of application of their findings.

True random sampling, in the sense of drawing participants randomly from a large population of potential participants, is rarely performed in clinical psychology research. Usually, convenience sampling is used – that is, whoever can be obtained at the time of the study (e.g., all the participants who can be recruited in a given time period). Researchers need to take this into account when analyzing the data and making generalizations.

Having dealt with sampling issues, we will now examine the other major topic area that is raised by working with the participants, that is, ethics.

ETHICAL ISSUES

The major ethical principles in clinical psychology research are:

- *Informed consent.* The researcher gives full information about the study and participants freely choose whether to enter it.
- *Avoidance of harm.* Harm may be direct (such as stress or humiliation) or consist of deprivation of benefit (such as in control groups in clinical trials). There may be a difficult trade-off between the potential harm to individual participants and the potential benefits of knowledge to humanity.
- *Privacy* is the right not to provide information; *confidentiality* is the right to have any personal information kept securely.
- All clinical psychology research should be reviewed by experienced researchers external to the project, including ethics committees or Institutional Review Boards.

Ethical principles are concerned with protecting the rights, dignity, and welfare of research participants. Interest in the ethics of psychological research grew out of outrage at earlier abuses, including medical research in Nazi concentration camps during World War II. The 1947 Nuremberg Code and the 1964 Declaration of Helsinki set out the ethical principles by which medical (and psychological) research is now governed.

Early stress-induction research by psychologists also caused ethical concerns (Bersoff & Bersoff, 1999). These concerns were further fuelled by the widespread use of deception in the social-psychological research of the 1950s and early 1960s, which shaped the public attitude toward psychologists as scientific deceivers. The civil rights movement and populism of the 1960s and 1970s resulted in a greater sensitivity to ethics on the part of psychologists (Imber et al., 1986; Korchin & Cowan, 1982). Finally, concerns about litigation and the general trend toward increased bureaucratization and governmental control of research led in the 1970s and 1980s to government-mandated practices for the review of research involving human participants. In the United States, there is a "Federal policy for the protection of human subjects" (the "Common Rule": Department of Health and Human Services, 2014); in the United Kingdom, the government has set out a comprehensive "Research Governance Framework" (Department of Health, 2005) covering all aspects of the conduct of research on health and social care in public settings.

Previous chapters have touched on some ethical issues associated with particular research methods or designs, such as covert observation or no-treatment control groups. Here we will examine some central principles common to all psychological research. Following Korchin and Cowan (1982), we group them under the headings of: (1) informed consent; (2) minimization of potential harm/ deprivation of benefit; and (3) confidentiality and protection of privacy.

However, before examining these principles, some general points need to be made. First, the researcher is under an obligation to explore and seek others' advice and judgments about the specific ethical issues involved in his or her study. Second, as Korchin and Cowan (1982) noted, validity and ethics should not be seen as separate issues. Instead, unethical practice reduces the external validity of the research, because it results in research procedures that cannot be translated into practice. Conversely, poorly designed research reduces the ethical standing of the research, because, in such situations, there are usually only minimal scientific or social benefits possible to counterbalance the possible risks or costs of participation in the research. Finally, it is worth noting that we are operating in the domain of value judgments, in which one needs to balance negative effects (usually accruing to the participant) with positive effects (usually accruing to society in general). Sometimes there are conflicting ethical considerations, and difficult choices need to be made for which there are no clear-cut answers.

Informed Consent

Informed consent involves disclosure by the researcher, before the study, of what will happen during the study and of any other information that might affect the person's decision to participate. This enables prospective participants to make a free and informed decision about whether or not to enter the study. Thus informed consent consists of both full information and freedom of choice.

Full Information

Full information refers to the principle of telling prospective participants everything that they need to know in order to make a rational decision about whether to take part in the study. An important corollary is that the participant is able to understand the information provided (i.e., that it is not written in overly technical or bureaucratic prose or in a language in which the participant is not fluent).

Problems arise when the person's understanding of the issues is limited. Informed consent becomes difficult with children or with adults who are not fully competent to make their own decisions (Bersoff, 2008; Bersoff & Bersoff, 1999), or even with well-informed and educated adults in clinical trials in medicine (Thornton, 1992). For example, if the child is under 7, parental permission plus the child's verbal agreement is usually required. If the child is between 7 and 17, then his or her written assent is usually required in addition to parental permission. Similarly, with adults with severe dysfunctions (e.g., people with severe mental retardation – intellectual disabilities in UK usage – or people who are psychotic), then sensitivity and clinical skills are required, and the level of readability and comprehensibility of the description is important.

A second issue is the role of deception in psychological research. Although it is much less common in clinical than in social psychology, there are some well-known examples of deception, such as Rosenhan's (1973) "pseudopatient" study, in which participant observers faked a psychotic symptom in order to gain admission to a mental hospital as a patient. There is also the less dramatic issue of deception by omission: good scientific practice dictates that participants should not be aware of the hypotheses under investigation, since this knowledge may cause them to alter their behavior. Thus deception is a matter of degree, ranging from relatively trivial instances of withholding information about specific hypotheses or naturalistic observation of public behavior, to more serious cases of lying to the participants. Deception is an especially serious problem when the study uses fictional environments or contrived situations (e.g., in Good Samaritan studies when a serious crime or accident is feigned), or when double deception (i.e., false debriefing) is used.

At a minimum, a full debriefing is needed at the end of any study in which deception is used, in order to provide complete information, including the rationale for the deception, and to answer all questions about the study. However, debriefing cannot always be relied upon to undo the effects of the deception, because this may cause greater pain when the participants learn that they have been deceived. For this reason, Korchin and Cowan (1982) recommend that alternative methods be used wherever possible, including obtaining the person's consent to be uninformed, seeking feedback from surrogate participants who are similar to proposed participants, role playing and simulation research, and naturalistic, descriptive research.

Freedom of Choice

Freedom of choice requires that the participant's consent be voluntary, without direct or indirect pressure to take part. There should be no coercion, explicit or implicit. Thus, the researcher must foster the possible participant's autonomy and self-determination and should evaluate implicit situational or personal factors that may limit freedom.

There is often a considerable power imbalance between the researcher and the potential participant. In this case, the problem of making sure that there is no implicit coercion becomes acute. This is often an issue in clinical settings, where a therapist or doctor wishes to conduct research with his or her patients, who may fear that refusal will prejudice their treatment. It is also an issue with "captive" populations such as psychiatric inpatients, prisoners, or students or where there is a shortage of mental health services. For example, one of us was required by an ethics committee to destroy 1000 promotional leaflets for our research clinic because the title said "Free Therapy"; it was all right for the service to be free of charge, but the prominent presence of the word "free" was seen as potentially coercive for clients facing limited services and long waiting lists. Clearly, power imbalances inevitably limit freedom.

Informed Consent Form

In practice, the study is described and the participant's consent is recorded by means of an information sheet and informed consent form. Although specific requirements vary (depending on the particular study, the setting in which it is conducted, and the type of ethics committee), at a minimum these should contain:

- a description of the study's procedures;
- an explanation of its risks and potential benefits;
- an offer by the researchers to answer questions at any time;
- the statement that participants may withdraw their consent at any time during the study without prejudice, especially without prejudice to their present or future treatment; and
- a space at the end of the form for the potential participant to sign in acknowledgement that they understand what the study involves.

The informed consent form is given to participants to read and sign after the study is fully described to them and after they have had a chance to ask any questions about it, but before the study proper begins. It is good practice to give participants a duplicate copy of the information sheet to retain for their records. With treatment studies or more involved research it is also a good idea to allow participants time to reflect on whether they wish to take part or not.

Harms and Benefits

In general, research should not harm the participants. In ethical theory this is referred to as the principle of *nonmalificence*. However, some people may freely consent to expose themselves to potential harm for the greater good of humanity, for example, in testing new medical procedures. There is a trade-off between any harm caused to the participants versus the potential gain to humanity from the knowledge acquired, and the sense of altruism that goes along with that.

Harm can be either *direct*, from harmful psychological or physical events, or *indirect*, from withholding of benefits (such as deprivation of treatment in a control group). In psychological research, direct harm is most likely to come from such things as stirring up painful feelings or memories, threats to one's self-image, and humiliation. Two extreme examples are Milgram's (1964) obedience studies, in which participants believed themselves to be giving dangerous electric shocks to other participants, and Zimbardo's (1973) prison simulation, in which college students, role-playing prison guards, brutalized other participants who were role-playing prisoners. In addition to psychological risk to the individual, there is also the possibility of social risk, for instance, to members of ethnic or cultural groups who may be harmed by the findings of studies examining group differences (Scarr, 1988). In all cases, researchers need to set up procedures to ensure that the possibility of harm is minimized and to respond to any harm that may occur during the study.

As part of debriefing the participants after the data collection, you should ask whether they experienced any distress or had any concerns during the study. Furthermore, if the respondent becomes upset during the study itself, you may need to terminate, or at least suspend, data collection. Your clinical skills become useful here, both in detecting the presence of distress and also in being able to respond to it appropriately. However, in some cases participants may need to be referred to sources of help outside of the study, for example, if an interview about psychological trauma stirs up painful memories, or if a study of marital interaction brings out conflict in the couple that they weren't fully aware of. Occasionally, people may volunteer for psychological studies in order to find a way of getting help for their difficulties, and may feel let down when they don't receive benefits they had hoped for.

Withholding of Benefit in Clinical Trials

Randomized controlled trials (RCTs) highlight a number of ethical issues (Imber et al., 1986). Although participants are unlikely to be harmed, there are several dilemmas about *withholding of benefit* (which in ethical theory violates the principle of *beneficence*). In other words, there are tensions between the clinical perspective, which emphasizes doing the best for each individual patient, and the scientific perspective, which emphasizes having a well-designed study. The researcher must therefore balance the need for useful knowledge about a treatment's efficacy against the likely consequences for individual participants, who may receive less than optimal treatment in the study. These tensions appear in the following areas:

- *Control and comparison groups.* No-treatment or placebo controls mean that some patients are deprived of a potentially valuable treatment, being instead given an inferior treatment or none at all (see Chapter 8). Wait-list controls pose a less serious problem, but they still mean that, for some patients, treatment is delayed. Ideally, RCTs are only conducted when there is *equipoise* (rough perceived equivalence) between conditions, or a lack of prior knowledge about their equivalence: if the researcher thinks that a particular treatment condition is clearly better for a patient, their clinical duty is to give the patient that treatment and not enter them into the RCT. Many contemporary RCTs test the treatment of interest against *treatment as usual* (TAU), or the best available treatment, but in practice this can be a disguise for a no-treatment condition
- *Specified treatments versus clinical judgment.* Patients in RCTs receive specified, pre-determined treatments, often manualized ones, thereby diminishing the capacity of the clinician to make informed judgments about how the patient is responding, and to vary the therapy accordingly. (On the other hand, a certain degree of flexibility is built into many contemporary treatment manuals.)
- *Randomization.* As we discussed in Chapter 8, Brewin and Bradley (1989) argued that many patients have preferences about which treatments they want, and that the act of random assignment to experimental conditions deprives them of choice. Being in a less preferred treatment (even though they have consented to the lack of choice) may result in a less than optimal outcome.
- *Narrow inclusion criteria.* Since clinical trials usually have specific inclusion criteria, often based on a single DSM diagnosis, people with significant clinical concerns may not be admitted into the treatment program, on the grounds that their problems are too complicated.
- *Referrals at termination.* In normal clinical practice, a therapist can refer a patient for further help at the end of therapy. However, in an RCT which has a follow-up assessment, this may be discouraged, as the researchers need to see how the patients fare without any additional therapy. Again, patients are deprived of optimal treatment as a result, and it may not be ethical practice.

Privacy and Confidentiality

Invasion of privacy and loss of confidentiality are special cases of harm. *Privacy* refers to the person's right not to provide information to the researcher, while *confidentiality* refers to the person's right (and the researcher's corresponding obligation) to withhold information from third parties.

In a trivial sense, all psychological research invades privacy, since otherwise it would not be finding out anything new. However, the ethical issue of privacy is concerned with the intrusiveness of research. Different people have different personal boundaries: some do not mind disclosing intimate information about themselves, while others want to maintain a tight control on what is known about them. The researcher needs to be aware of each participant's limits on disclosing information and respect their right to withhold certain information.

Types of confidentiality protection include anonymity, in which no identification is possible, and the more usual situation of protecting the participant's identity through secure research codes that are separated from the data itself. It is also important that data be securely stored, using such procedures as password protection or encryption, and also avoiding insecure means of transferring data, such as emails and unencrypted memory sticks. Participants are likely to be more open and to provide better data if they feel assured of confidentiality safeguards. Finally, it is important to keep in mind that no confidentiality guarantee is absolute, in that research records are always vulnerable to hacking, theft, or legal subpoena.

Ideally, the information sheet or informed consent form should specify who will have access to the data and the findings. (As an aside, the adjective "strict," which often precedes "confidentiality" is superfluous, since something is either confidential or it is not.) When audio or video recordings are made, it should be clear who will hold them, for what purposes, and for how long; it is good practice to have a separate informed consent form to cover consent to make, retain, and possibly publish extracts from recordings, or use them for teaching or publicity. When case material is written up, the participants' personal details should be altered so that they are unrecognizable (this sometimes requires creativity). However, no guarantee of confidentiality can ever be absolute. Just as in clinical practice, the researcher has a duty of care, and if they suspect that there will be potential harm to the participant or to others, or if they become aware of serious clinical malpractice, then confidentiality may need to be broken.

The issue of confidentiality becomes increasingly critical as the information becomes more sensitive or potentially damaging, should it become known to others. The kinds of danger from potential breaches of confidentiality include embarrassment, loss of employment, legal action, labeling, and social stigma. In these situations, the researcher should give details on the information sheet or informed consent form about the kinds of information that the participants will be asked to provide.

Ethics Self-study Exercise

We recommend that the researcher review his or her study early on, in order to appraise its risks and benefits (Davison & Stuart, 1975). This self-appraisal begins by asking: "What risks are possible? How serious are they? How likely are they?"

The risk estimates typically increase when new procedures (i.e., new measurement or intervention methods) are employed, as opposed to established, tested procedures. Another important situational factor is the degree of coercion. The researcher should ask "What obvious or implicit pressures are operating on prospective participants, which may prevent them from refusing to take part?" These may include the need for psychological or medical treatment, in order to impress legal authorities or to be released from prisoner or patient status.

Having evaluated the study's risks, the researcher should then ask "What benefits are likely? For whom? How realistic are they?" Some benefits may accrue directly to the participant, including help with problems, self-knowledge or growth, general education, and increased self-esteem or altruism; other benefits are more general, such as the knowledge gained and the increased potential for helping others.

In general, greater potential risks, lesser benefits, unknown procedures, and coercive situations call for stronger safeguards for informed consent and participant safety. These safeguards include greater disclosure of risks; careful screening and exclusion of at-risk participants; supervision and monitoring of the participant's condition during the course of the study; and the use of contingency plans for removing participants from the study and finding appropriate treatment for research-induced problems. Finally, Davison and Stuart (1975) argue that there are some situations in which it is impossible to conduct ethical research. Prisons, for instance, can be said to be inherently coercive to such an extent that no valid consent can be obtained; however, on the other hand, it may be unethical for such institutions to go unresearched.

In evaluating the risk–benefit ratio, be aware of the dangers of self-deception: there is a tendency for researchers to rationalize and underestimate research risks while overestimating benefits, under implicit assumptions such as "The ends justify the means" and "What is good for me must be good for psychology." You will ultimately have an easier conscience if you follow the precept that "People are more important than data."

Ethics Committees

You cannot do psychological research without coming into contact with the committee delegated by your university, hospital, or other agency to review the ethical treatment of human participants in research (Bersoff, 2008; Bersoff & Bersoff, 1999; Ceci, Peters, & Plotkin, 1985). These committees are known as Institutional Review Boards (IRBs) in the United States and ethics committees in the United Kingdom. The purposes of this review process are to protect the participants in the research, and also to protect the institution from legal reprisals for ethical lapses and harm done to research participants. Another purpose is to comply with the regulations of grant-giving institutions.

Ethics committees are typically made up of academics, drawn from a range of disciplines, medical doctors, and lay members. Many committee members may be unfamiliar with psychological research. In the United States, their make-up is dictated federally, including a balance of gender and scientific discipline and the inclusion of physicians and lay persons from the community. This range of backgrounds usually provides a breadth of perspectives to evaluate the ethical appropriateness of the research. However, occasionally, ethics committees appear to exceed their brief, and to make decisions on political rather than ethical grounds (Ceci et al., 1985). For example, we know of one project, which aimed to examine how much psychiatric patients knew about the side effects of their psychotropic medication, that was refused ethical permission. It seemed that this was not because the project was unethical, but rather because certain committee members felt threatened by what the results might say about the state of professional practice.

Committees can sometimes take months to process an application, so it is wise to apply early, especially if your research is being done to a tight deadline (this particularly applies to student projects). However, there is a dilemma here, since if you apply for ethical approval early in the planning stage, before your protocol is finalized, your application may look less polished and your study may also change somewhat after it has been approved. The application has to be carefully thought through before submission. If your protocol subsequently changes, for example, as a result of pilot studies, you can file an amendment with the ethics committee or IRB.

There are often three levels of review: exempt, expedited, and full review.

Exempt status. A study may pose such minimal risks as to be exempt from regular review. Such research includes: (1) Surveys using interviews or questionnaires, where the participants are not identifiable or are not asked to reveal sensitive information of a personal or potentially damaging nature; (2) Research on established educational practices, where the participants are not at risk and are not identifiable; (3) Research using existing archival or public data, where the participants cannot be identified; and (4) Overt observation of public behavior, under the same conditions of confidentiality and unintrusiveness.

The catch with exempted review status is that you are not allowed to make this decision yourself (because of possible vested interests). Thus, there is usually some form of screening required to determine whether a study should be exempted or not. A typical procedure for doing this is to consult with the ethics committee chair or one's departmental review committee.

Expedited review. The next level of review is expedited, a fast-track review process for low-risk studies. Examples include the use of archival data where a particular use of the data has not previously been consented to; and non-stress-inducing behavioral research without manipulation of participants' behavior or emotions. In expedited review, the researcher still submits an application to the committee, which may subject the study to limited review by a subcommittee (e.g., the chair plus one other committee member).

Full review. The third level is full review, which applies to everything that does not fit the exempt or expedited criteria, to all government grants, and to all research with people who are not competent to give informed consent, such as adults with cognitive impairments, and children and adolescents. Often, the researcher may be requested to meet with the committee to answer questions about the study.

Some research practices, such as deception and covert observation, raise red flags and are usually scrutinized carefully by ethics committees. These practices have a number of potential costs (Bersoff & Bersoff, 1999), including the fact that they tend to undermine trust in psychology; they may change people's behavior (e.g., decreasing bystander intervention in emergencies because people now think it might be an experiment); and their artificiality may yield distorted findings of low external validity. Finally, proposals for work in socially sensitive areas (Sieber & Stanley, 1988), such as child sexual abuse, are more thoroughly scrutinized.

CHAPTER SUMMARY

This chapter has covered issues concerning the researcher's relationship with the participants: how participants are obtained for the study and how they are treated once they are in it. The process of obtaining the participants is known as sampling. It involves specifying the target population from which the sample will be drawn, choosing the sampling procedure, and determining the sample size. It also involves thinking about the universe to which the results of the study are intended to be applied. The degree to which a study's results can be generalized is known as external validity: it is one of Cook & Campbell's (1979) four validity types.

In most types of research, it is desirable to have an unbiased sample, representative of the target population from which it is drawn. There are various sampling techniques available to achieve this. However, psychological research often tends to rely on convenience samples, which may introduce biases. In traditional quantitative research, the sample size is determined by statistical power analysis. There are alternative approaches to sampling, such as systematic replication in N = 1 research and purposive or theoretical sampling in qualitative studies.

Ethical principles are concerned with protecting the rights, dignity, and welfare of research participants. The central ethical issues are informed consent, minimization of harm, and confidentiality. Informed consent has two components: that the researcher gives full information about the study and that participants are able to freely choose whether to enter it or not. It is important to be aware of the subtle pressures on people to participate, particularly when the researcher is in a position of power or authority. In clinical psychology research, harm may be direct (such as stress or embarrassment) or it may consist of deprivation of benefit (such as when participants in control groups in clinical trials get an inferior treatment, or none at all). There may be a difficult trade-off between potential harm to individual participants and potential benefits of knowledge to humanity. Privacy and confidentiality are special cases of potential harm: privacy is the individual's right not to provide information; confidentiality is the right to have any information kept securely.

All clinical psychology research should be reviewed by peers, including ethics committees or Institutional Review Boards.

FURTHER READING

Sudman's (1976) book, *Applied Sampling*, though dated, is still a useful resource, and Minke and Haynes (2011) have a more recent, chapter-length treatment. There is an accessible summary available on the internet from the UK National Audit Office (http://www.nao.org.uk/wp-content/uploads/2001/06/SamplingGuide.pdf). Cohen (1990) gives a good overview of the issues in statistical power analysis and Cohen (1992) provides a "power primer" covering the most commonly used cases. Alternative views of sampling and generalization are covered in Patton (2002), Sidman (1960), and Corbin and Strauss (2015).

Researchers should familiarize themselves with their relevant set of ethical principles (e.g., American Psychological Association, 2002, 2010a; British Psychological Society, 2010). Bersoff's (2008) comprehensive edited volume gives a general overview of ethics in psychology, with useful case vignettes. The chapters by Korchin and Cowan (1982) and Bersoff and Bersoff (1999), in successive editions of Kendall, Butcher, et al.'s *Handbook of Research Methods in Clinical Psychology*, both give interesting discussions of ethics in clinical research; we have drawn on them heavily here.

QUESTIONS FOR REFLECTION

1. What sampling strategies make the most sense for your topic area?
2. Discuss the social or ethical consequences of carrying out underpowered research.
3. Carry out the risk/benefit assessment exercise for your project (see Ethics Self-Study Exercise) and reflect on what you learned from the process. For example, what changes to your study would you make as a result of the exercise?
4. An emerging area of research ethics is social costs. Comment on: (a) ethical issues involved in research that may be damaging to particular groups of people (e.g., based on ethnicity, gender, etc.); (b) the risks of excessive regulation of research stifling scientific creativity and spontaneity.

11
Evaluation Research

KEY POINTS IN THE CHAPTER

- Evaluation is applied research that aims to assess the worth of a service, often judging it against specified goals.
- It includes service audit, quality assurance, needs assessment, and outcomes evaluation.
- Organizational and political issues are crucial in evaluation research.
- The evaluator begins by asking two basic questions:
  - "What is the service trying to do?"
  - "How will you know if it has done it?"
- Evaluations can address the process or outcome of a service.
- Process evaluation examines who is coming to the service and what service they are being given.
- Outcome evaluation examines the impact of the service—whether users benefit or not.
- In addition to examining clinical outcome, evaluators may look at client satisfaction, and at economic indicators of costs and benefits.

In everyday parlance, evaluation means judging the worth of something. Good applied psychologists do this informally: they build up a personal knowledge base of which interventions work best with whom. Clinical psychology training, in particular, emphasizes a reflective, self-critical attitude towards one's work, and encourages evaluation of one's own practice.

Here we will use the term "evaluation" in a more formal sense, to denote applied research into the implementation and effectiveness of clinical services.

Research Methods in Clinical Psychology: An Introduction for Students and Practitioners,
Third Edition. Chris Barker, Nancy Pistrang, and Robert Elliott.

Companion Website: www.wiley.com/go/barker

Much of the early evaluation work was done in the United States in an educational context, where it is known as *program evaluation*. It arose as a way of monitoring the federal money spent on large-scale social programs in the 1950s and 1960s, such as Head Start, a preschool educational intervention program (Rossi et al., 2004; Shadish, Cook, & Leviton, 1991).

This chapter departs from our chronological, research-process framework. We have so far concentrated on fundamental issues in research methods, which can be applied across different content areas of psychological research. This chapter draws on ideas from the groundwork, measurement, and design chapters, and applies them to the task of studying specific services in specific settings. Evaluation is a messy area in which sociopolitical and organizational issues are often as prominent as scientific ones (Cowen, 1978; Rossi et al., 2004; Weiss, 1972). The design compromises that we discussed in Chapter 8 become more acute here. Evaluation researchers often face a Hobson's choice: they can either collect inadequate data or no data at all.

We are devoting a separate chapter to evaluation both because it has its own distinct body of literature and because we anticipate that many readers will never conduct basic clinical research, but may well become involved in evaluation research. We argue that evaluation should be a routine part of clinical psychology: much clinical work is based on custom and practice rather than any formal knowledge base, and evaluating it is a way of seeing whether or not it lives up to its claimed benefits.

Planning an evaluation begins with two questions: "What is the service trying to do?" and "How will you know if it has done it?" The procedures used in evaluation research aim to answer these basic questions. This chapter looks at the practical issues in incorporating evaluation into working clinical services. Before that, however, we will examine some of evaluation research's basic concepts and specialized vocabulary.

What is Evaluation?

We defined "evaluation" above as a form of applied research. However, as we discussed in Chapter 2, the distinction between pure and applied research is better regarded as a continuum rather than a dichotomy. Evaluation, at the applied end of the continuum, differs from pure research in several ways (Hayes et al., 1999; Pawson & Tilley, 1997; Weiss, 1972)

- Its primary aim is to assist decision making, rather than to add to an existing body of knowledge. Thus it tends to be less concerned with theory and more with solving a particular setting's operational problems.
- It is done on behalf of a decision-maker, often a manager, who may be distinct from the evaluator.
- It takes place in a complex "action setting" (Weiss, 1972), as opposed to a more controlled academic research environment.
- Its participants are usually users of the service, rather than research volunteers, and their interests as clients are paramount.
- It is intended to be used soon, and is usually done under time pressure.

- It is often written up for purely local consumption, rather than for wider dissemination in professional journals. This may be because it may not meet exacting scientific standards, because the results are not generalizable beyond the particular service being evaluated, or because the time and effort needed to write up the findings for publication may be beyond the evaluator's resources. Sometimes, also, evaluators are unable to publish their findings because the people who commissioned the study do not want its results to be known by their competitors or by the general public.

Types of Evaluation

Evaluation has its fair share of jargon, and several terms are used to describe the type of evaluation being conducted.

Scriven (1972) classified evaluation into formative and summative approaches. A *formative* evaluation is typically used for internal program purposes, and feeds back its results to influence the service as it continues to develop (or form itself). A *summative* evaluation provides an overall summary, typically for administrative purposes; it is often done on a larger scale with its results delayed until after the end of the evaluation period. Formative evaluations thus lend themselves to evaluating new services, while summative evaluations lend themselves to well-established ones.

Donabedian (1980), a key figure in the quality assessment literature, distinguished three different foci of evaluation: (1) *structure* refers to the resources that are available for a service, such as staff, buildings, and equipment such as psychological tests; (2) *process* refers to the activities that constitute the service delivery—in psychology these are essentially a series of help-intended conversations or assessment procedures; (3) *outcome* is how the service affects the clients, for example, how they change psychologically as a result. The parallel concepts of input, activities, and output, which originated in economics, are also sometimes used (Fenton Lewis & Modle, 1982). The present chapter mostly addresses process evaluation: evaluation of structure is psychologically uninteresting (except from an organizational development point of view), and outcome evaluation overlaps considerably with our earlier discussion of design (Chapter 8).

The variables to be examined in an evaluation can also be conceptualized using Maxwell's (1984, 1992) widely cited list of six criteria for quality assessment: access to services, relevance to need, effectiveness, equity, social acceptability, and efficiency/economy. For example, Parry (1992) used this framework to address how psychotherapy services might be evaluated.

Evaluation, Audit, and Quality Assurance

The term *clinical audit* (or *service audit)* is current in the United Kingdom (Benjamin, 2008; Cape & Barkham, 2002; Crombie, Davies, Abraham, & Florey, 1993). Its history in medicine stretches back to the beginning of the 20th century (Lembcke, 1967; Young, 1982). Audit is a loosely defined term that refers to an intensive examination of one or more aspects of a service. For example, an out-patient psychotherapy service might audit the ethnic background of its referrals. An audit can be specific, as in this example, or it can be more wide ranging.

Definitions of audit tend to emphasize comparison against an agreed standard (e.g., Cape & Barkham, 2002; Crombie et al., 1993). For example, an audit of waiting

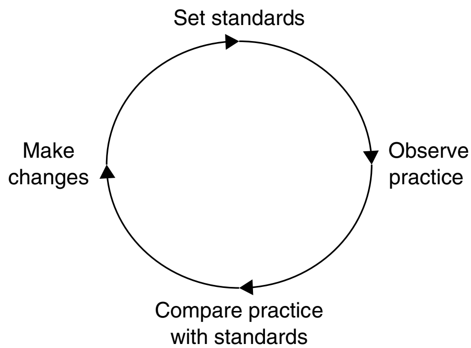


Figure 11.1 The audit cycle

times in an out-patient service might involve the standard that all clients should receive an appointment within six weeks of referral. Under this definition, simply monitoring practice prior to a standard having been set, or developing a standard itself, are important precursors to audit, but not audit proper.

Audit is often depicted as a circular process—known as "the audit cycle" (Figure 11.1)—which emphasizes using audit to make changes, either in clinical practice or in the standards governing practice (Benjamin, 2008). Audit thus involves a continuous process of evaluating, feeding back, making changes, and evaluating again.

Both audit and evaluation are closely linked to *quality assurance*, which emphasizes setting-up procedures to ensure that the standard of a service's work remains consistently high (Cape & Barkham, 2002; Green & Attkisson, 1984; Young, 1982). Quality assurance is related to various quality management methods originating from business and industry, such as total quality management, statistical process control, and continuous quality improvement (Cape & Barkham, 2002). Methods for quality assurance in clinical psychology could include establishing clinical practice guidelines (Parry, Cape, & Pilling, 2003), peer review, and systematic involvement of service users in monitoring delivery.

Audit and evaluation are retrospective, looking at the service after it has happened (although the results will naturally be fed back to help improve the service). Quality assurance, on the other hand, is essentially prospective, ensuring that no future problems occur in the service (although it is also retrospective in the sense of identifying problems and making sure that they do not happen again). To take a hypothetical example from manufacturing, where much of this language originated, evaluation (or audit, or quality inspections) will count the number of bugs in the baked beans; quality assurance procedures will try to stop them getting in there in the first place.

The Sociopolitical Context

It is vital never to underestimate the sense of threat that accompanies evaluation. Even people who feel largely positive about it will often be worried or irritated by it; other people may just pay lip service to enlightened attitudes about evaluation, but ultimately be defensive and obstructive. Some of the most important concerns are as follows:

- An oppressive sense of being continually scrutinized, that can feel like Orwell's "Big Brother is watching you."
- Resentment at having to take the time to provide the data for evaluation, since it leaves less time for client contact.
- Fear that the results of evaluation may provide ammunition for managers or other colleagues to attack the quality or quantity of work being done.
- Annoyance that the criteria used in evaluations do not capture the important aspects of a service's work. Evaluations may just focus on quantitative measures that are easy to collect, such as numbers of clients seen, rather than more valid but less tangible indicators of quality.

These are important objections. Even if you do not feel them strongly yourself, they will be undoubtedly felt, if not voiced, by a significant proportion of your colleagues. As we discussed in Chapter 3, this is an area where clinicians can use their skills to understand and possibly reduce the sense of threat. Clinical psychologists typically have had better training in this than other professionals involved in evaluation.

Internal versus External Evaluation

We have been tacitly assuming that you are evaluating a service that you yourself partly deliver: this is often called an in-house evaluation. Alternative possibilities are that an external evaluation consultant be used, or that an externally conducted inspection may be mandated by regulatory bodies. External consultants are usually less emotionally attached to the service and more able to weigh it dispassionately. On the other hand, external evaluators are usually more threatening, less knowledgeable about the service, and more expensive. For the rest of the chapter, we will assume that you are conducting an in-house evaluation, since that is the more common situation. However, psychologists are sometimes employed as external consultants to evaluate other services. External evaluations cover the same ground as in-house evaluations, but in addition they require the evaluator to possess specialized consultancy skills.

Our own view is that, despite its potential difficulties, evaluation of the services they deliver needs to become a routine component of psychologists' work, and that evaluation can be made more relevant if conducted by the psychologists themselves. The mental health field, in particular, is awash with poorly monitored programs and interventions. No one knows what their effects are, and there is often at best a lack of interest in, and at worst a contempt for, the views of the clients. Furthermore, the climate of managed care in the United States and clinical governance in the United Kingdom emphasizes evaluation, audit, and quality assurance (Cape & Barkham, 2002; Crombie et al., 1993; Lyons, Howard, O'Mahoney, & Lish, 1997; McSherry & Pearce, 2010). So the issue is not whether to evaluate, but how to. We believe that it is better to take control of evaluation yourself, than to have it imposed upon you.

What Stakeholders Want from Evaluation

The various *stakeholders* (evaluation jargon for someone who has an interest) in the service will each have different reasons for wanting the evaluation done (Rossi et al., 2004). These reasons are not necessarily mutually incompatible, but each stakeholder will attach their own weighting to each one. For example:

- People funding a service (e.g., managers or grant-giving bodies) may want to know whether it is doing what it is supposed to be doing, and whether it is using its resources effectively.
- Clinicians may want to test the effectiveness of an intervention or to compare it to other interventions. They may also want to know if their professional time is being used efficiently.
- Service planners may want to justify the development or continuation of a service, or to improve its delivery.
- Service users, or their parents, guardians, or carers, may be concerned about the accessibility, convenience, and effectiveness of the service.
- Community leaders may want to know if the service is reaching its intended target population.

Aside from these overtly expressed, rational reasons, there may also be some less legitimate, covert reasons for evaluating (Weiss, 1972). For example, evaluation may be used to delay making a decision, or as an empty public relations exercise, or as a way of generating information that can be used to justify closing down an awkward service. Evaluation is a complex political arena, in which some people do nasty things for nasty reasons, but rarely admit that they are doing so.

The next section examines the preparatory thinking that is needed to set up an evaluation. Then we will look at ways of monitoring the process of service delivery, and finally touch on evaluation of impact and effectiveness.

PREPARATION FOR EVALUATING A SERVICE

As we stated at the beginning of the chapter, the first question to address in evaluating a service is "What is the service trying to do?" This is usually followed by the subsidiary question "Why is the service trying to do that?" Before the evaluation proper can proceed, these preparatory questions must be addressed. We have adapted the comprehensive framework that Rossi et al. (2004) set out in their influential evaluation textbook. In practice, however, it is unrealistic to contemplate a full evaluation of a service: Their framework can be adapted to suit local needs. The process of answering these two questions can be broken down into the six steps shown in the box below.

Six preparatory steps for evaluating a service:

1. setting down the aims and objectives;
2. specifying the impact model;
3. specifying the target population;
4. estimating the extent of the target problem in the target population;
5. assessing the need for the service; and
6. specifying the delivery system design.

These tasks are all easier to do when you are setting up a new service, as building in evaluation is much easier at the planning stage when there is some flexibility. Moreover, addressing evaluation issues at the outset can help to define the service's goals and procedures. Specifying how a new service will be evaluated usually helps clarify what it is trying to achieve, and vice versa. However, these preparatory steps are also useful if you are evaluating an existing service.

Aims and Objectives

Aims and objectives are the *sine qua non* of evaluation, especially for new services. They articulate what the service is for. Without knowing what the service is trying to do, the evaluator has no benchmarks against which to measure its operation. People often confuse aims and objectives, or speak about them as though they are the same. However, there is a useful distinction to be made between the two terms.

Aims are global statements of the desired outcomes of the service, expressed in a general, often rather idealized way. For example: "The service aims to reduce depression in mothers of young children." *Objectives* are specific goals, ideally occurring within a specific period of time, that detail what the service is actually going to do to achieve its aims and that give specific targets to indicate whether or not the aims have been met. The objectives should be clear, simple, and, if possible, measurable, so that there will be no ambiguity about whether each one has been reached. Sometimes the acronym SMART is used, standing for Specific, Measurable, Achievable, Relevant, and Time-related. For example, "The service plans to set up three post-natal depression support groups for mothers of children under 2 years of age in the London Borough of Camden by the end of the current financial year."

Aims and objectives:

Aims are global statements of desired outcomes.
Objectives are specific goals, which:

- ideally occur within a specific time period;
- detail what the service will do to achieve its aims;
- indicate whether or not the aims will have been met; and
- are clear, simple, and, if possible, measurable.

The exercise of specifying aims and objectives often helps to clarify the goals of a service. Carrying it out within a clinical team usually results in the team members having a better understanding of each other's values and assumptions. Furthermore, without aims and objectives, team members may not know what they are supposed to be doing or may even be pulling in different directions or undermining each other. For example, in a community alcohol service, some members may emphasize prevention, others counseling, some individual work, others work with couples or groups, yet others research. While there is clearly healthiness in this diversity, the team also needs a sense of direction so that its energies are not spread too thinly.

The Impact Model

The impact model specifies the theoretical or empirical basis for each of the activities that the service is undertaking. It may never be formally specified, but thinking about each of its three components helps team members to plan an effective service. These components are:

- The *causal hypothesis,* which describes what causes or maintains the target problem(s) that the service is seeking to modify.
- The *intervention hypothesis,* which specifies how the proposed intervention will affect that causal determinants.
- The *action hypothesis,* which asserts that the proposed intervention will in fact reduce the target problem(s).

For instance, in our maternal depression example, the causal hypothesis is that depression in mothers of young children is partly caused by a lack of social support; the intervention hypothesis is that a support group will increase social support; and the action hypothesis is that the support group will decrease maternal depression. These three parts of the impact model are depicted in Figure 11.2.

Sometimes, however, it may not be possible or necessary to address the cause of the problem directly. For instance, with adult survivors of child sexual abuse we cannot alter the cause, because it occurred years ago. Furthermore, addressing the cause may not be the best strategy for alleviating the target problems: etiology does not necessarily determine treatment. The point of specifying the impact model is simply to make the rationale for the service's actions as explicit as possible.

The Target Population

Having specified the impact model, the next step is to identify the targets, direct and indirect, for the intervention. *Direct targets* are those people on whom the intervention is specifically focused, for instance, mothers of children under 2. It is important to define the unit of analysis, which could be individuals, families, or groups. *Indirect targets* are those people who may benefit indirectly from the service, for instance, the families of the above women. Including the indirect targets gives a full picture of the impact of the service. Ideally, the targets should be specified in the aims and objectives of the service.

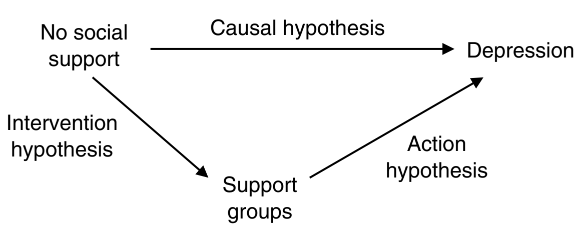


Figure 11.2 The impact model

Target boundaries should be clearly defined using both inclusion and exclusion criteria, for example, a specified geographical catchment area, and demographic and clinical characteristics of the client group. It is important to strike a balance between criteria that are overinclusive and those that are too restrictive. The following model, taken from a local drop-in service for people with severe mental health problems, is a good example of a target population description with both inclusion and exclusion criteria:

> To be in the Target Group, a person has to be: aged over 16 years; be living, staying or sleeping out in the South Camden sector of Bloomsbury Health District; have severe and enduring mental health problems; have positive or negative symptoms of psychotic illness; have had previous contact with mental health services; not be actively involved with other services; be experiencing severe social problems. People who meet these criteria but whose primary problem is due to the abuse of alcohol or drugs do not come into the Target Group. (Compass Project, 1989)

Estimating the Extent of the Target Problem in the Target Population

When planning a service, it is naturally important to estimate the extent of the target problem in the target population. Three epidemiological concepts are useful here. *Incidence* is the number of new cases during a specified time period, for example, the one-year incidence of flu. *Prevalence* is the number of existing cases, either at a specified time ("point prevalence"), or during a time interval. For example, the National Comorbidity Study Replication (Kessler, Chiu, Demler, & Walters, 2005) gives the one-year prevalence rates of psychological disorders in the United States.

Incidence and prevalence are related to each other by the duration of the illness: higher incidence or a longer duration will both increase the prevalence. Incidence is a more useful measure for illnesses of short duration such as flu; prevalence is more useful for those of longer duration such as Alzheimer's disease. With psychological problems, it is not always clear whether to measure the extent of the target problem in terms of incidence or prevalence. For example, in providing services for dealing with cases of child abuse, do you want to measure the number of new cases per month (incidence), or the total number of cases on the social services list (prevalence)? The issue is whether you are concerned with detecting and treating new cases as they appear or with knowing the number of existing cases in a population, whatever the time of origin.

The third concept, *population at risk*, is the subset of the general population that is more at risk of contracting a disease: intravenous drug users, for example, are a population at risk for HIV infection. It is particularly helpful to consider this target group for preventive projects.

There are several methods for estimating the extent of the target problem. There is a trade-off between their validity on the one hand and their complexity and cost on the other.

Surveys and *censuses* can be done in order to get the respondents' direct estimates of the size and severity of a problem. They generally yield the most valid data, especially if they include structured interview measures, but they are time-consuming and expensive to carry out. Two large national surveys of the prevalence of psychological disorders provide useful comparative data: the US National Comorbidity Study Replication (Kessler et al., 2005), and the UK National Survey of Psychiatric Morbidity (Bebbington et al., 2000a).

Rates under treatment. The size of the target problem in the target population can sometimes be estimated by looking at the rates under treatment in similar communities (if they exist). The number of people who seek treatment is usually a small fraction of the actual number of cases, but there may be ways of estimating the size of the untreated population, based on previous studies. For example, in the US National Comorbidity Study, only about a quarter of people suffering from a psychological disorder had received help from a mental health specialty service in the last year (Wang et al., 2005), and in the UK National Survey of Psychiatric Morbidity, fewer than 14% of people with a neurotic disorder were currently receiving any formal treatment (Bebbington et al., 2000a).

Indicators. This method uses statistical techniques, such as multiple regression, to predict the size of the target problem from nonclinical criteria. For example, one indicator of the number of heroin addicts in a community is the number of arrests for sale or possession of the drug (Hartnoll, Daviaud, Lewis, & Mitcheson, 1985).

Key informants. The researcher can use "networking" or "snowballing" sampling methods (see Chapter 10) to find knowledgeable people who might be able to help estimate the extent of the target problem. This is a simple and inexpensive method. In our experience, 20 or 30 respondents are usually sufficient. The advantage is that it develops the support of influential workers in the community; the drawback is the possible bias of the individuals surveyed. Qualitative and/or quantitative interviewing methods can be used.

Needs Assessment

Assessing the extent of the target problem in the target population is the first step in planning a service, as it gives an indication of what the volume of demand is likely to be. However, it is easy to assume that everyone suffering from the target problem needs or desires the service, which is not necessarily true. Needs assessments collect data that are more relevant to the service's operation: they are the health-care equivalent of market research.

The concept of *need* is often used in a technical sense, defined as a problem for which there is a potentially effective intervention (McKillip, 1987; Stevens & Gabbay, 1991; Thornicroft, 2001). Under this somewhat counterintuitive definition, need is assessed by professionals, rather than by the users themselves. It is not determined by the severity of the problem, but by whether something effective can be done about it. In contrast, *demand* is defined as what people ask for and *supply* as what is provided.

Stevens and Gabbay (1991), in an article nicely entitled "Needs assessment needs assessment," discuss the relationship between need, demand, and supply. They depict the relationship of the three concepts using a Venn diagram (see Figure 11.3). The diagram helps conceptualize and label the areas where the concepts overlap, for example, need that is supplied (areas 6 and 7) is called "met need" and need that is not supplied (areas 1 and 4) is called "unmet need."

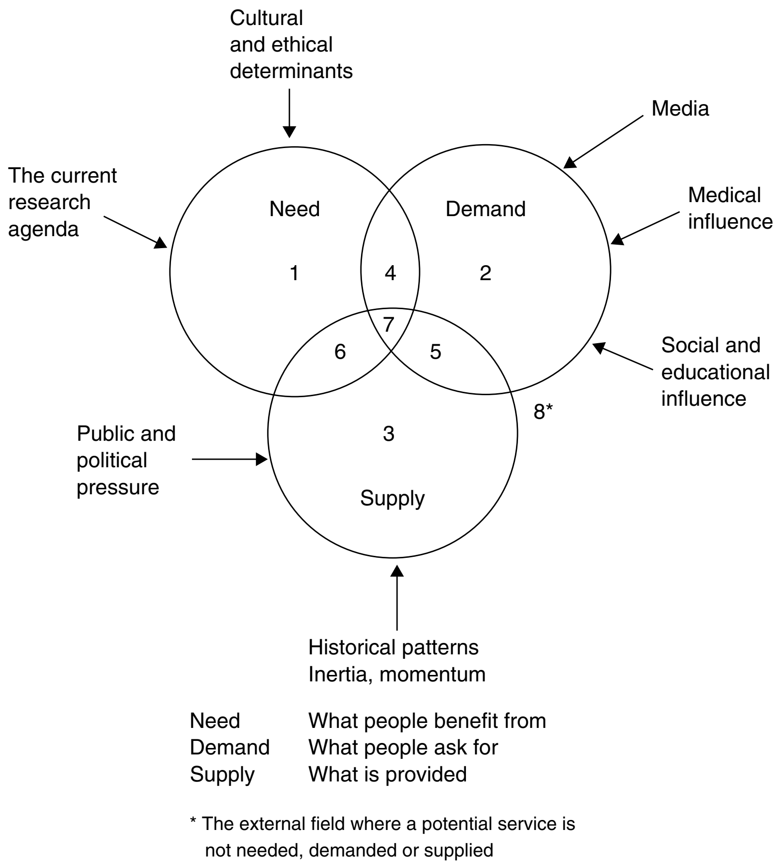


Figure 11.3 Need, demand, and supply: influences and overlaps.

Needs and demands can be assessed using the methods described above for assessing the extent of the target problem. However, such studies are not always popular with health-service managers and politicians, as they often imply spending further resources to satisfy whatever unmet needs are identified.

Delivery System Design

With new services, the foregoing are the preliminary steps in establishing the likely need and demand. The final step is to design the service itself. The delivery system design, which is ideally set out in the form of an *operational policy document*, specifies how the clinical team will go about delivering the service. It includes the organizational arrangements, such as procedures and activities, and structural aspects such as the physical setting, staff, and materials that are required to provide the service. The discussion needed to produce an operational policy document, and the existence of the document itself, may help anticipate some common problems in newly established clinical teams.

Once it has decided on its operational policy, the team can use role-plays or simulations to try to predict whether things will work smoothly in practice (e.g., what exactly will happen when a client walks in the door or when someone makes a telephone referral). Larger scale services may use *operations research methods* (a set of scientifically based procedures to aid decision making) to see if the services are planned in an optimal way, for example, whether the staffing levels at different sites are appropriate for the anticipated workloads (Taha, 2010).

MONITORING THE PROCESS OF SERVICE DELIVERY

Monitoring the process of service delivery:

- The central question is "Who does what to whom?"
- Monitoring service delivery can be divided into monitoring coverage and implementation.
- Monitoring coverage asks the question "Who is the service reaching?"
- A service is biased if it favors certain subgroups of its target population at the expense of others.
- Coverage can be assessed from service records, by surveys, and by examining dropouts.
- Monitoring implementation asks "What service is being given?"
- Implementation can be assessed by observation, service records or databases, and surveys.

Having gone through the above preparatory steps, the evaluation now focuses on what kind of service is being delivered: the process of the service, in Donabedian's (1980) terminology. Monitoring the process of service delivery means asking "Who does what to whom?" It also addresses such questions as "Is this service being delivered in the best possible way?" and "Is it accessible to its consumers?" (Maxwell, 1984, 1992). This differs from outcome evaluation, covered below, which assesses whether users benefit from the service.

There are two main targets of monitoring delivery, coverage and implementation (Rossi et al., 2004). Monitoring coverage asks the question "Who is the service reaching?", whereas monitoring implementation asks "What service is being given?" In addition, there is financial monitoring (to make sure that the funds are being properly used) and legal monitoring, to make sure that the service operates within the relevant laws (e.g., equal opportunities, health and safety). These latter areas are specialized activities, being the province of accountants and lawyers respectively, and we will not cover them here.

Coverage and Bias

Coverage is defined as the extent to which the service reaches its intended target population: is it reaching everyone it is supposed to, or just a certain subgroup of the target population, or even mainly people outside the target population? The related concept of *bias* is defined as the extent to which subgroups of the target population participate differentially, that is, the degree to which some subgroups receive greater coverage than others. Bias may arise from several factors:

- Self-selection, for example, if only the more motivated people come to a drop-in service.
- Program actions, for example, if staff favor some service users at the expense of others. In particular, there may be *creaming*: that is, a bias towards the more advantaged subgroups of the target population. For instance, when Community Mental Health Centers were first set up in the United States in the 1960s and 1970s, they tended to see a large proportion of better functioning people who were easier to work with, neglecting older people and people with severe and enduring difficulties (Grob, 2011). Other examples of program bias are where services do not adequately cater for the needs of physically disabled users or of users from certain ethnic groups (possibly because of unconscious racism).
- Unforeseen influences, such as where the service is located, for example, if it is poorly served by public transportation (strictly speaking, this is an aspect of structure rather than process). These factors may again reflect unconscious program bias.

Undercoverage occurs when some people in the target population have unmet needs. This is often a problem in face-to-face psychology services, as there are usually many people in the community who need the service but do not get it (Bebbington et al., 2000b; Wang et al., 2005). *Overcoverage* occurs when some inappropriate targets are served. For example, in health promotion campaigns, for example, to reduce smoking or to promote safer sex, material may inevitably be directed at some people outside the target population. This is usually not a significant problem.

Assessing Coverage

Several methods can be used to assess coverage:

- *Service records* are the most obvious and commonly used method. Most psychology services keep records of basic client information. They can be analyzed according to demographic characteristics, for example, client gender, age, or ethnicity, and possibly also according to clinical characteristics such as presenting problem or referral source.
- *Surveys* can be used when services are not targeted at defined groups of individuals, but at an entire community. They are more appropriate for preventive, health education or health promotion services. For instance, Barker, Pistrang, Davies,

Shapiro, and Shaw (1993) assessed the coverage of a BBC television series on preventive mental health. Although it was viewed by a national audience, the series was primarily aimed at certain subgroups of the population, that is, those people who were experiencing psychological problems themselves or who had a friend or relative who was. A national survey was used to estimate the nature of the viewing audience and their reactions to the series.

- *Analysis of dropouts* can be used to assess bias, by comparing people who participate fully in a service with those who drop out before the end. A high dropout rate clearly indicates that something is wrong with the service. It may reflect client dissatisfaction, or conditions in the community that prevent full participation (e.g., stigmatization of service users). Data on dropouts can come from service records or from surveys designed to find nonparticipants. Such data help identify subgroups of the target population who are not receiving the service. It may be possible to ask nonusers about their perceptions of the service and then to use their opinions to redesign the intervention to be more suited to their needs (an example of the formative use of evaluation).

Service Implementation

Monitoring service coverage focuses on whom the service is reaching; monitoring service *implementation* or delivery focuses on what kind of service the users are getting. Is the service's delivery consistent with its design specifications, that is, is it delivering what it is supposed to be delivering, according to its aims and objectives? You can look at both descriptive aspects, to label what components of the service are given, and quality aspects, to describe how well they are given. Implementation can be assessed by:

- *Observation* (qualitative or quantitative) in the clinical setting.
- *Service records*, for example, in antenatal care, to ensure that the right number of visits was made and the correct things done at each one. Standard clinical records can be augmented by asking clinicians to complete a checklist of activities. They can be given a standard form to tick off each procedure as it is completed, for example, in HIV pre- and post-test counseling, or an audit team can review the case note files at regular intervals to make sure that they are complete and that proper procedures are being followed.
- *Management information systems* and computerized case registers can keep track of the type of service each client received at each visit.
- *Service-user surveys* may be desirable when it is not possible to obtain user data routinely as part of service activities, or when the size of the target group is large and it is more efficient to do a sample survey than to obtain data on all participants. You can ask the clients about what kind of service they actually received. A natural step if you are doing this is also to ask them about their satisfaction with the service and what its impact was, which leads into the final area, outcome evaluation.

OUTCOME EVALUATION

- Outcome evaluation examines the impact of the service.
- One key area of impact is the extent of clinical benefit.
- Necessity often dictates using a simple one-group pretest–posttest design.
- Outcome is assessed using measures linked to the objectives of the service.
- Studies often include measures of user satisfaction.
- It is important to address economic variables in addition to psychological ones.

Outcome evaluation examines the impact of the service. It asks the crucial question "Do users benefit from this service?" Benefits may be manifest in the form of an improvement in the target problem (sometimes known as "health gain") or in the form of changes in attitude about the problem, so that the client, or other stakeholders such as parents or carers, experience it as less problematic.

It is also important to bear in mind the possibility of negative outcomes. Many programs, especially in the political arena, are subject to the *law of unintended effects*. In other words, policy changes that are intended to make matters better actually end up by making them worse. Or they make improvements in one area at the expense of deterioration in another. An example within clinical psychology is "psychological debriefing" for victims of traumatic incidents. Such programs were established with the plausible rationale that having counseling immediately after a trauma might prevent the development of post-traumatic stress disorder. However, research has shown that, far from helping many victims, psychological debriefing may often make them worse off than if they had had no intervention at all (Mayou, Ehlers, & Hobbs, 2000).

Assessing outcome involves applying the research methods that we have discussed in previous chapters, in so far as it can be done within the constraints of the clinical setting. The first step is to choose outcome measures that capture the key objectives of the intervention, for example, an intervention aimed at helping anxious adults might use the Generalized Anxiety Disorder scale (Kroenke et al., 2007). The second step is to select a research design that will assess any changes in those measures and, if possible, enable such changes to be attributed to the intervention itself rather than to other variables (Cook & Campbell, 1979; see also Chapter 8). Of course, in many working services, this is a counsel of perfection, and the evaluator may have to be content with drawing inferences from less than adequate designs or measures.

Recall the efficacy versus effectiveness research distinction, which we examined in Chapter 8. (Efficacy research uses randomized designs, often in highly controlled research clinic settings; effectiveness research uses nonrandomized designs to study interventions as they actually happen in real-world settings.) Evaluation research is by definition effectiveness research, and will often use a simple design, such as the one-group pretest–posttest design, although this may be augmented using *benchmarking* (Leach & Lutz, 2010), that is, comparing outcomes with those from other similar services.

Naturalistic field research of this sort is clearly imperfect from the point of view of internal validity. The issue here, however, is whether it is good enough to draw plausible conclusions that can aid practical decisions (Seligman, 1995). Managers and policy makers are often more convinced by research conducted in their own service, even if it is scientifically flawed, than by a methodologically sound piece of research published in a reputable scientific journal, which was conducted in another setting by other investigators.

Client Satisfaction Surveys

One important area of study is client satisfaction research (Lebow, 1982). Before any more sophisticated evaluation designs or outcome measures are deployed, it is important to ascertain that clients find the service acceptable and beneficial. If not, they will not attend and the service will not receive funding. Clients' views of the service they have received are usually assessed via standardized self-report instruments such as the Client Satisfaction Questionnaire (CSQ: Larsen et al., 1979), which can be adapted to most clinical services.

Because client satisfaction surveys rely on retrospective self-report methods, their validity is open to criticism (Shadish et al., 2002; Seligman, 1995; see also Chapters 6 and 8). Some mental health professionals can be dismissive of clients' views: "patients say any treatment is helpful, even useless treatments like aromatherapy, so what is the point of asking for their perspective." However, it seems wrong to use these validity problems to dismiss the whole enterprise out of hand (Lebow, 1982). Professionals' views of the effectiveness of the services they deliver also suffer from validity problems; service evaluation ideally needs to take both perspectives into account, and ideally that of third parties (e.g., family members) as well. Positive response sets in clients' reports can be avoided to some extent by asking clients explicitly to list any problems with or complaints about the service (Parry, 1992), and validity threats can be taken into account when interpreting the findings.

One published example of client satisfaction research is the *Consumer Reports* study (Seligman, 1995), which we discussed in Chapter 8. It used a large, though unrepresentative, sample to gather post hoc consumer views from people who had had psychological therapy, looking, for example, at their satisfaction with different therapeutic modalities and orientations. Seligman (1995), while acknowledging the problems with this approach, highlighted its usefulness in terms of examining how people experience therapy as it is actually conducted in the real world.

Patient-focused Research and Outcomes Management

One distinctive approach to evaluation research in clinical services is *case tracking* or *patient-focused research* (Barkham et al., 2010; Lambert, 2001). This sets out to evaluate the outcome of individual clients, in contrast to the groupings of clients that are the usual focus of evaluation research. The basic procedure is to compare each client's progress throughout the therapy against the trajectory that would be expected, given that client's initial clinical status. Such trajectories can be established via normative research, often involving thousands of clients. If the client's progress departs from the trajectory, that can be noted and acted upon. If normative data is unavailable, in practice it is sufficient simply to note instances in which clients show reliable improvement or deterioration on the particular outcome measure (see Chapter 12). It is also possible to use idiographic (individualized) measures as well as nomothetic ones (Sales & Alves, 2012).

Feeding back to clinicians information on each client's outcome as the therapy progresses, particularly if the client is doing less well than would be expected, has been shown to enhance the final clinical outcome (e.g., Shimokawa et al., 2010). Furthermore, this information on the progress of individual clients can be used to initiate improvements in the service delivery system, an approach known as *outcomes management* (e.g., Lyons et al., 1997).

Cost-effectiveness

A final issue to consider is *cost–benefit* or *cost-effectiveness* evaluation (Krupnik & Pincus, 1992; Mangen, 1988; Rossi et al., 2004). This compares the service's costs with its outcomes, in order to ensure that its funds are being well used. In economic terms, it compares inputs to outputs. This kind of evaluation has become more prominent in both the United States and the United Kingdom, as purchasers of health-care services (in the United States, health maintenance organizations or insurance companies; in the United Kingdom, clinical commissioning groups) must decide what to spend their limited resources on. Their decisions will be based on which services they think will give the greatest outcome per unit of resource employed.

There are clearly problems in measuring both input and output. At the input end, costing must take into account both direct costs, principally psychologists' contact time, and overheads, such as the cost of buildings, equipment, and support staff (Cape, Pilling, & Barker, 1993).

The output end of the calculation is even more problematic, since there is no universally agreed upon measure of effectiveness or of benefit. Different healthcare services (e.g., heart surgery compared to psychiatric in-patient treatment) use different criteria to measure outcome. One possible solution, derived from health economics, is to combine quality of life and life expectancy into *quality adjusted life years,* or "QALYs." Thus an outcome of a treatment, say an operation for cancer, may give a person a high quality of life for a short time or a medium quality of life for a longer time. These outcomes would be considered equivalent in terms of QALYs. Such an approach, although it fulfills the economists' goal of giving a single index upon which to base resource allocation, clearly makes a number of problematic assumptions about how to weight quite different clinical outcomes (Cox et al., 1992) and is also difficult to apply to psychological interventions.

Another approach is to attempt to measure the economic burden of illness or psychological disorder in terms of lost productivity, increased social services expenditure, and increased use of medical services (e.g., general practitioner consultations, emergency room visits, or in-patient hospitalization). Then the outcome of a psychological intervention can be partly assessed by the savings made in terms of increased productivity and reduced social services and healthcare expenditure—often referred to as "cost offset"—which may represent a substantial financial return in relation to the expenditure on the psychological intervention (Krupnik & Pincus, 1992; Layard, Clark, Knapp, & Mayraz, 2007).

An example of a cost-offset study is Humphreys and Moos's (2001) analysis of the value of encouraging patients with substance abuse to participate in self-help groups as an adjunct to their treatment. Using a large all-male sample drawn from inpatients in U.S. Veterans' Administration hospitals, the study compared inpatients in substance abuse programs that emphasized self-help groups with patients in programs that emphasized a traditional cognitive-behavioral approach, in terms of their subsequent health-care costs in the year following discharge. Humphreys and Moos found that the patients who were in the cognitive-behaviorally oriented programs received post-discharge health care costing an average of $12,100 per year, whereas patients in the self-help oriented programs received post-discharge care costing an average of $7,400 per year, thus demonstrating a significant cost offset for those programs that featured self-help groups as part of the therapy.

A simple form of cost-effectiveness analysis, and one with direct relevance to practitioners, is to compare practitioner input, measured in terms of number of sessions, with client output in terms of clinical improvement. All therapists must ask themselves, implicitly or explicitly, whether it is better to give one client 20 sessions or two clients 10 sessions (or 10 clients two sessions). Cost-effectiveness evaluation attempts to make the basis of such decisions explicit. Howard, Kopta, Krause, and Orlinsky's (1986) analysis of dose-response relationships in psychotherapy falls under this heading. They used the statistical technique of probit analysis on a data set drawn from 15 published studies to estimate the improvement rate of clients after a given number of sessions. They estimated, for example, that 53% of clients had improved by eight sessions and that 74% of clients had improved by 26 sessions. However, to be a true cost-effectiveness analysis, the input must then be expressed in monetary terms: that it costs so many dollars to produce such and such an outcome.

CHAPTER SUMMARY

Evaluation is applied research that aims to assess the worth of a specific service. It includes the areas of clinical audit, quality assurance, needs assessment, and outcomes evaluation, although each of these areas has its own distinct literature. Evaluation studies are conducted for the benefit of the various stakeholders in the service, although different stakeholders often have different priorities for the evaluation. Organizational and political issues are crucial, both in deciding the priorities of the evaluation, and in addressing the sense of threat that often accompanies it. To start with, the evaluator asks two central questions: "What is the service trying to do?" and "How will you know if it has done it?" Answering these questions is simpler if the service has explicit, agreed-upon aims and objectives, and a well-thought-out rationale for why it is doing what it does.

Evaluations can address the process or the outcome of a service. Process evaluation examines who is coming to the service and what services they are being given. It is important to ensure that the service delivery does not unfairly favor certain subgroups of the population at the expense of others. Outcome evaluation examines the impact of the service—whether users benefit or not. In addition to examining clinical outcome, evaluators may look at client satisfaction, and at economic indicators of costs and benefits.

FURTHER READING

Rossi et al.'s (2004) text, which we have drawn on extensively here, gives a comprehensive framework for conducting a program evaluation. Shadish et al. (1991) present the conceptual background, and discuss the ideas of the major figures in the modern American program evaluation movement. Weiss (1972), in one of the founding texts of this movement, gives an excellent discussion of the rational and irrational feelings about evaluation. Patton's (2008) *Utilization-Focused Evaluation* is also appealing for its broad, practical approach. Seligman's (1995) *Consumer Reports* study and the subsequent commentary on it (in the October 1996 special issue of *American Psychologist*) give an airing to many of the issues raised by using imperfect research designs to answer practical questions. The quality assurance and audit literature as applied to clinical psychology are reviewed by Cape and Barkham (2002).

QUESTIONS FOR REFLECTION

1. Apply program evaluation concepts to an intervention related to your research interests. For example, what would your research look like if it were to be implemented in real-world, community agency settings? What would the aims and objectives of the intervention be? What is the target population? How might you assess the level of need?
2. Individual case-tracking (i.e., patient-focused) methods are controversial, for example, Lambert's use of early outcome data to designate an individual client's treatment as "Off track," "On track," and "Cured" or "Nonclinical." Some have criticized these methods as an unwarranted intrusion into psychotherapy and have argued that insurance companies or mental health services will inevitably misuse these methods to deny services to clients. What do you think?
3. Cost-effectiveness and cost–benefit methods have also been controversial. Do you think that it is possible to properly attach monetary values to psychological distress costs or treatment benefits? Assuming that we could do it, should we?

12

Analysis, Interpretation, and Dissemination

KEY POINTS IN THIS CHAPTER

The final stage of the research process involves making sense of your data, first for yourself, then for a wider audience.

- Analysis means establishing what the findings are and how they answer the research questions.
- Qualitative analysis is an inductive process of developing a set of themes or a conceptual framework that captures the main ideas in the data.
- Quantitative analysis can be exploratory or confirmatory, depending on the research questions.
- The concepts of statistical conclusion validity, effect size, and clinical significance are used to evaluate the strength and meaningfulness of quantitative findings.
- Interpretation involves understanding the psychological meaning of the results, and their scientific and practical implications.
- Dissemination means communicating both the findings and your understanding of them to other people.

Having collected the data, the final stage of the research process consists of making sense of the findings, first for yourself, then for a wider audience. This stage can itself be broken down into three parts: analysis, interpretation, and dissemination. Analysis means establishing what the findings are and how they answer the research questions, interpretation means understanding the findings in terms of their broader implications, and dissemination means communicating them to a wider audience. Analysis is typically

Research Methods in Clinical Psychology: An Introduction for Students and Practitioners,
Third Edition. Chris Barker, Nancy Pistrang, and Robert Elliott.

Companion Website: www.wiley.com/go/barker

reported in the Results section of a research paper; interpretation is reported in the Discussion section. As always, the components overlap and intermingle: an interpretation of the findings might suggest a further analysis of the data, or presenting the study at a conference might lead to new ideas about its interpretation. However, for simplicity, we will cover the three components as though they were distinct and sequential.

The goal of the analysis is simply stated: to use the data to answer the research questions. We will look at the steps undertaken in the qualitative and quantitative approaches. However, the specific techniques of data analysis, both qualitative and quantitative, involve specialized methods that are beyond our present scope. Here we will focus on the general strategies that researchers use when analyzing their data.

QUALITATIVE DATA ANALYSIS

- Qualitative analysis can take either a within-case (idiographic) or a cross-case approach.
- The first step is usually to transcribe the data (paying attention to confidentiality issues).
- It is important to become familiar with the data, by listening to recordings and rereading transcripts.
- Qualitative analysis is an inductive procedure, which typically involves three related processes: identifying meaning, categorizing, and integrating.

As described in Chapters 6 and 7, qualitative data come in various forms, such as transcripts from interviews, field notes from observations, or other kinds of texts. It is usually unstructured and often voluminous. Qualitative researchers are faced with finding ways of systematically analyzing these collections of words. This problem is compounded by the well-known issue of "qualitative overload": the fact that qualitative investigations, even ones with a small sample size, usually generate vast quantities of data. This abundance of data needs to be analyzed and represented to the reader clearly and accurately.

Much has been written about qualitative data analysis. Key references include Braun and Clarke (2006, 2013) on thematic analysis, Corbin and Strauss (2015) on grounded theory, Potter and Wetherell (1987) and Potter (2012) on discourse analysis, and Smith et al., (2009) on interpretative phenomenological analysis, as well as some more general approaches (e.g., Creswell, 2013; Miles et al., 2014; Patton, 2002; Pope et al. 2000). There are also several computer programs now available to assist with qualitative analysis (e.g., Atlas-ti and nVivo), and the online app Dedoose (http://www.dedoose.com/). Some of these tend to favor a particular analytic approach (e.g., nVivo is based on grounded theory analysis), others are more flexible.

Here we will attempt to sketch out some general principles in analyzing qualitative data. However, it is worth noting that different qualitative orientations use different methods of analysis, and also tend to use different vocabulary to describe similar procedures.

Different orientations, and the goals of particular studies, also vary in the depth of interpretation or inference. Some approaches keep to a fairly descriptive level, without much interpretation, but most go beyond description: they attempt to explain the meanings, causes, structures, or patterns in the phenomenon being examined. The degree of interpretation partly depends on the goal of the particular investigation.

Flexibility is required in all phases of qualitative research, including the analysis. It is important to adapt the analytic method to the data, to the research question, and to your own cognitive style and talents. However, it is also important to be explicit about your procedures. In other words, flexibility does not mean vagueness or sloppiness.

Within-case and Cross-case Analysis

Regardless of one's general approach to qualitative research, the data may be analyzed either within or across cases. Although the vast majority of studies report cross-case analyses, they normally start out with analyzing each individual case separately.

Within-case analysis is idiographic in nature, concentrating on understanding the features of a single case, or a small number of cases. The material may be presented with minimal interpretation, for example by organizing it chronologically into a narrative. In this descriptive, narrative approach, the researchers restrict themselves to arranging the material into a story, which is allowed to speak for itself. Such presentations can be an excellent way of demonstrating the existence of a phenomenon. A classic example is Bogdan and Taylor's (1976) study of "Ed Murphy," which demonstrates the existence of perceptive self-awareness in a young man labeled as being "retarded" (in UK terminology, a person with intellectual disabilities).

However, even when qualitative researchers focus on individual cases, they usually go beyond the descriptive level. This approach is consistent with clinical psychologists' interest in understanding the meaning of individuals' situations. For example, Varvin and Stiles (1999) studied a political refugee's experience of therapy, using a particular theoretical framework (the "assimilation model") to interpret the data.

Cross-case analysis looks across individuals in order to identify common themes about the question being studied, aiming to see which aspects are shared across participants. Usually the researcher is also interested in describing variations within the phenomenon, that is, themes or patterns that characterize only some participants' accounts. For example, Knox , Edwards, Hess, and Hill (2011) studied 12 trainee therapists' perceptions of self-disclosure by their supervisors. They identified several categories of the consequences of supervisor disclosure, both positive and negative. Some were mentioned by most supervisees (e.g., normalizing their experience) and some were less common (e.g., the supervisees themselves became more confident in disclosing in supervision).

Preliminaries to Qualitative Data Analysis

Data Preparation

The first step in qualitative analysis is to prepare the data. In interview studies or studies using recorded interactions, this involves transcribing the recordings—usually a laborious and time-consuming process.

Transcription is in fact a form of analysis (Riessman, 2008), because of the many theory-guided decisions that must be made along the way. For example, decisions must be made about how to break the speech or interaction into units, and how to record nonverbal and paralinguistic elements of speech (e.g., speech rate, loudness, pausing). Even the seemingly straightforward task of writing down the spoken words turns out to be complicated by repetitions and irrelevancies, as well as our unconscious tendency to edit what we hear, ignoring things that don't make sense and hearing what we expect to hear. Clearly, there is no single correct method of transcription; rather, the transcription method should be chosen for the task at hand.

For transcribing a qualitative interview, where it is mostly the content, rather than how things are said, that is of primary interest, the best approach is the simple method illustrated in the sample interview presented in Chapter 6 (also see box below for guidelines). However, for research requiring the careful analysis of moment-by-moment interaction, many researchers use Jefferson's system, presented, for example, in ten Have (1999), which measures pauses and the exact beginning and end of interruptions, among other things (see also Mergenthaler & Stinson, 1992, for widely used standards for transcribing therapy sessions).

Before beginning the analysis, it is vital to check the transcripts for accuracy, and also to ensure that participants' anonymity is preserved by removing or disguising names and other identifying information. In research using multiple perspectives and sources of information (e.g., Comprehensive Process Analysis: Elliott et al., 1994), the different types of data must also be collated.

Guidelines for transcribing qualitative interviews

- The principal requirements are accuracy and readability. The transcript should convey the words and quality of the actual speech: what was said and how it was said.
- Prefer short sentences to long ones. Use commas to indicate subclauses (as consistent with normal punctuation).
- Indicate special emphases by question marks, exclamation marks and italics.
- Use quotation marks to indicate that the participant is reporting the speech of other people.
- Use round parentheses to give paraverbal features of the interaction (laugh, sigh, etc) or to indicate inaudible material: (inaudible).
- Use standard spellings as much as possible, even for nonstandard pronunciations.
- Spellings of some common paraverbal utterances: mm, mm-hm, um, er.
- Use square brackets to enclose names omitted to preserve confidentiality: she went to talk to [her sister].
- Also use square brackets to include back-channel overtalk that doesn't interrupt the speaker's flow: Participant: I want to go [Interviewer: Yes] back to work.
- Indicate silences of 5 seconds or more in brackets: (10 sec silence).
- If a speaker is interrupted, so that their flow of speech is cut off by the following speaker, use two slashes//

Adapted from Auld & White (1956), Mergenthaler & Stinson (1992), Potter & Wetherell (1987), Stiles (1992).

Immersion

Before the formal analysis, it is important to immerse yourself in the data, by listening to the recordings and by rereading the transcripts. This gives you an overall feel for the data's scope and meanings, although you may not be able to articulate your understanding at this stage. Systematic understanding is a product of the formal analysis.

Processes in Qualitative Data Analysis

Qualitative data analysis (particularly thematic analysis approaches) can typically be thought of as involving three generic sets of processes, which we will call *identifying meaning*, *categorizing*, and *integrating*. Naturally, this division is a simplification: the processes represent related activities and the boundaries between them are not always distinct. Furthermore, the processes are not linear: researchers often cycle back and forth between them. Different approaches to qualitative analysis make use of different forms and mixes of these activities. However, they all describe an inductive process, which is common to most qualitative research, of going from the raw data to ideas about the data.

It is usually unwise to begin by analyzing the transcript as a whole: global analysis encourages ignoring data that do not fit one's expectations or emerging understanding. For this reason, most approaches adopt some type of microanalysis, paying close attention to words and phrases. Some approaches divide the data into units before coding, the most common division being the *meaning unit*, which consists of material on a single point in the participant's description, often approximating a verbal sentence. Its use is largely a practical strategy; its exact definition is not critical and may vary among researchers and among studies with different research objectives.

Identifying Meaning

The researcher begins the formal part of the analysis by going through the data and trying to identify the ideas that are being expressed. In this first stage, the researcher attaches tentative labels, often called initial codes, to the text. The codes are attempts to sum up the essence of what is being said in each phrase, sentence, or other portion of text. In the second stage, the codes will then be integrated into larger themes or categories, but the initial task is one of identifying meaning. However, texts can be understood in many ways, and different approaches to qualitative research (as described in Chapter 5) use different ways of highlighting the meaning of the data.

Codes are not necessarily mutually exclusive; that is, a particular unit of data may be assigned to a number of different ones. For example, consider one statement from a peer supporter in the Pistrang et al. (2013) Women Helping Women study, which we used as an example in Chapter 6: "At the beginning, it was just a fine balancing act of making sure she knew that I really had gone through it [the illness] and that I was very willing to talk about anything she wanted to talk about. But, at the same time, that it was about her." When first encountered early in the analysis, this statement was given two different codes: "Reciprocity: a two way process" and "Self disclosure—striking a balance."

One issue is how closely the codes stay to describing explicit meaning, as opposed to interpreting implicit meaning. Most approaches tend to stay fairly close to the text at this point, attempting to condense what is expressed into a brief phrase or label (e.g., "a balancing act"), but the coding may involve some translation into psychological language. Other, more interpretive approaches, may focus on bringing out what is said implicitly "between the lines." Some interpretive and critical approaches take this a step further and may read beyond the speaker's apparent awareness, based on some interpretive theory (e.g., feminist theory or psychoanalytic theory). This approach is necessary when the analyst is dealing with apparent self-deception on the part of the respondent; however, it may not sit well with readers who do not share the analyst's interpretive framework.

Phenomenologically oriented researchers may take a stance of *psychological reflection*, which Wertz (1985) has described in terms similar to therapeutic empathy. In particular, he describes a process of "entering and dwelling," in which the researcher attempts to immerse themselves in the participant's world. The researcher tries to slow down the story and to dwell on its details and meanings, setting aside (bracketing) the assumption that they already understand what is being described. At the same time, the researcher tries to step back from the description, attending to meanings rather than matters of truth or falsity. Often, this process incorporates a dialogue with the data, in which the researcher asks such questions as, "What is really meant here?", "What kind of thing is being described?", and "How does this relate to the phenomenon I'm trying to understand?" This method is both descriptive and interpretive in that it tries to stay close to the data while still generating a deeper understanding through drawing out the participant's implicit meanings and assumptions. Such approaches are not simply inductive but often involve a process of creative reading of the data that Rennie (2012) and others refer to as *abduction* (see Chapter 2).

Although different approaches to qualitative analysis emphasize different ways of reading data, in actual practice there is much overlap. In carrying out a qualitative analysis, you may want to consider making flexible use of a range of such activities, as appropriate to your data and research questions.

Categorizing

All forms of qualitative analysis engage in some form of theme generation, in which the researcher groups together important concepts or ideas. The previous ("identifying meaning") phase usually results in a tentative set of labels corresponding to the set of ideas in the data; the task now is to organize them conceptually.

The process of giving initial labels leads to the identification of key concepts, often referred to as *categories* or *themes* (we will use the terms interchangeably). Such themes usually consist of a word or phrase that captures an essential meaning, often at a more abstract level (e.g., "normalization," "unblaming the self," "fall from grace"). The theme names may derive from numerous sources, including the respondent's own words, the research literature, or metaphor. The development of themes is an interpretive process that involves both close attention to the meanings expressed in the data and the researcher's own ideas.

There is rarely one definitive set of themes: it is more often the case that there are several possible ways to categorize the data. Each researcher will have their own perspective, partly depending on their epistemological leanings, but in practice there is often a common core of overlap between the themes produced by different analysts (Madill et al., 2000). What is important is that the analysis be transparently grounded in the data.

Sometimes, the data are first organized into large *domains* corresponding to major aspects of the phenomenon being studied. These domains do not provide answers to the research questions, rather they are ways of dividing up the data in order to make it easier to analyze. They represent divisions that could have been made before the data were collected. Domains may be structured in different ways, for example, corresponding to topics addressed by the research questions, or following a broad narrative structure (e.g., "background," "event," and "aftermath").

The analysis begins at an individual level, taking each case sequentially. As further cases are added, the researcher attempts to identify patterns across the cases. The *constant comparative method,* originally articulated by Glaser and Strauss (1967), is valuable here. It essentially involves exploring similarities and differences between the emerging themes. As new themes are identified, they are compared to the other ones in the current set. If an idea is similar to an existing theme, it is added to that theme and may help to clarify or elaborate it. If it is judged to differ from existing themes, it is noted as a possible new theme.

The process of categorization continues until *saturation* is reached; that is, until themes are no longer added or elaborated. As the analysis proceeds, the researcher will discover that fewer and fewer themes are added, and the analytic process becomes more and more one of coding data into existing themes.

Integrating

As themes begin to be identified in the data, the researcher attempts to make connections between them. The aim is usually to create some sort of conceptual framework, rather than a list of unrelated themes. Often a hierarchical structure can be identified, with lower order categories, often called subthemes, setting out the constituent parts of higher order themes. For example, in a study of the benefits of mutual support groups for parents of children with disabilities (Solomon, Pistrang, & Barker, 2001), three broad categories were identified: "Control and agency in the world," "Sense of belonging to a community," and "Self-change." Each of these subsumed several lower order categories: for example, "Sense of belonging" included "Being understood," "Sharing emotions," and "Friendships." In the grounded theory approach, researchers attempt to identify a single, higher order, unifying category—often referred to as a *core category.* It is intended to capture the essence of the phenomenon, the headline of the story. For example, in Solomon et al. (2001), the core category of "Identity change" captured the essence of the three broad categories listed above.

The process of integrating is cyclical and interpretive, involving refining and linking concepts, going back to the original data to check for accuracy, and revising the categories and the framework.

It is often helpful in the analytic process, and in the final presentation of the results, to depict the themes and their connections in a visual format. This takes different forms in different approaches. Grounded theorists, for example, often use tree diagrams that depict hierarchical category structures with the core category at the top. Others may use a mind map or concept map style of diagram (Eppler, 2006); many others simply set out the thematic structure in a table. The aim is to provide the reader with a clear and coherent picture of the findings.

Good Practice in Qualitative Analysis

As described in Chapter 5, much has been written about ways of evaluating qualitative research. Several of these criteria pertain specifically to analysis. For example, it has been widely recommended that researchers implement procedures for checking the credibility of their analysis: these include using several analysts, having another researcher audit the data trail, and establishing *testimonial* (or *respondent*) *validity* by checking back with the original respondents or others like them (Elliott, Fischer, & Rennie, 1999; Mays & Pope, 2000; Stiles, 1999; Yardley, 2000).

It is important for the researcher to consider whether the research questions have been thoroughly and clearly answered. What remaining ambiguities are there? How could the analysis have been continued? Is the analysis coherent and integrated, without oversimplifying the data? Does the analysis illuminate the phenomenon? These questions, which are also suggested in guidelines for evaluating qualitative research (see Chapter 5), need to be addressed by researchers throughout the analysis.

QUANTITATIVE DATA ANALYSIS

Steps in quantitative analysis:

- data entry
- data checking
- data reduction
- exploratory analyses
- statistical significance testing for answering the research questions
- analyzing the strength and clinical significance of effects.

Before formal quantitative analyses or hypothesis testing can be carried out, researchers need to prepare their data and explore its general properties.

Data Entry

If the data were collected using a written questionnaire or quantitative interviews, they are usually entered manually into a spreadsheet, such as the SPSS data editor, or Microsoft Excel. If they were collected using a computerized interface, they can usually be downloaded directly into Excel. Prior to data entry, the variables must all be named and defined, and any labels or codes for various values entered (e.g., the sex of the participant might be coded as 1 = male, 2 = female).

In the common case of a multiple-item scale, it is usually better to enter the scores on all the items, rather than just the total scale score, in order to reduce scoring errors and to allow for the possibility of analyzing the scale's reliability or factor structure. Any reverse-scored items need to be recoded so that their values are consistent with the rest of the items in the scale. This can be done manually before the data are entered, but it is usually simpler and more reliable if the computer performs the recoding, creating new, reversed variables.

Data Checking

Data errors can arise either from typing mistakes at data entry or from incorrect computer commands. It is important to check for both possibilities. In order to ensure that the data have been entered correctly, it is a good idea to proofread the entries by asking someone to read them aloud from a printout and checking them against the original source. Rosenthal (1978) estimated that, on average, about 1% of data points are wrongly entered. Sometimes computer scan sheets can be used in order to eliminate typing errors, but these also need to be checked to ensure that they have been properly filled in. Data entered via a computerized interface, such as with online questionnaires, avoids data entry errors by the researchers, but not, of course, by the participants.

To check that the data are being processed correctly, some simple descriptive analyses can be performed (Tabachnik & Fidell, 2013). For nominal scale data, frequency analyses can be used; for interval scale data, summary descriptive statistics are also useful, including the mean, the standard deviation, minimum and maximum values, and the number of valid observations. These also provide some basic statistics that you will probably need for the Results section of your research report. For some descriptive studies, for example, opinion surveys or consumer satisfaction research, knowledge of the frequency distributions may be all that is required to answer the research questions.

Descriptive analyses also help you to check that missing values are being handled properly, whether there is any systematic pattern to the missing data, and that there are no out-of-range values (e.g., "56" entered for a variable that is supposed to range from 1 to 7). Missing data can be estimated statistically (Schlomer, Bauman, & Card, 2010), using a procedure such as multiple imputation, which is available in SPSS.

Examining the frequency distributions of the variables also enables you to check their distributions, particularly whether or not they are approximately normally distributed and whether there are any outlying observations that will distort the subsequent analyses. Discrepancies in standard deviations (i.e., ones much smaller or larger than other variables of the same type) often indicate problems with unreliability, outliers, or restricted ranges, which may suggest the elimination of cases, items, or measures before further analyses are carried out. If the data are not normally distributed, it may be possible to transform them to bring them closer to normality (Tabachnik & Fidell, 2013). Otherwise, nonparametric statistical tests may need to be used.

Data Reduction

Data reduction involves condensing the data, so that it is more manageable and easier to analyze. One obvious approach consists of simply dropping some of the variables from the data set. Researchers are often overambitious in the beginning stages of a project and then realize at the start of the analysis that they have more variables than they know what to do with. Such planning errors can often be corrected by eliminating variables from the analyses. It is usually better to focus your energy on thoroughly analyzing a few important variables, rather than struggling to analyze everything that you optimistically included because you thought it might be interesting to look at.

Once the basic variables have been decided upon, the data set can be reduced by summing or averaging the items of any multi-item scales to provide a total score or subscale scores (e.g., by using the SPSS Compute command). With a new scale, it is important first to conduct an item analysis (e.g., using the SPSS Reliability procedure), as the averaging process assumes that the items are parallel (see Chapters 4 and 6). Item analysis will identify bad items, that is, items which do not hang together with the rest of the scale, and will also show whether the scale as a whole has a high enough internal consistency to warrant its use as a homogeneous measure. Once these analyses are done, the individual item scores can be dropped from the data set or simply ignored.

A third method of data reduction is factor analysis (Floyd & Widaman, 1995; Tabachnik & Fidell, 2013), a multivariate statistical technique that is designed to determine the structure of a set of variables. It is often used as a step in measure development research (see Chapter 6), to investigate the number of underlying dimensions of a new measure or set of measures. Factor analysis can also be used for data reduction, when the researcher wants to represent most of the information in a large number of variables by a small number of independent factors.

Item analyses tend to be regarded as preparatory analyses and are usually reported in the Method section of the research paper, whereas factor analyses tend to be regarded as proper analyses in their own right and are usually reported in the Results section.

Data Exploration

The final preparatory step is to get a feel for the patterns in your data. Even if you are working within a hypothesis-testing framework, it is still a good idea to look at the data from other angles to see what else they can teach you, if only to generate ideas for future studies. Scientific advances often come from unexpected findings that purely confirmatory procedures may fail to pick up (Merbaum & Lowe, 1982). It is worth trying to develop a playful attitude to the analysis, looking at things from different angles, so that you end up feeling that you know the data inside out.

Several statistical techniques have been developed to assist this process. Tukey's (1977) *Exploratory Data Analysis* is the standard reference volume; a briefer account is given by Velleman & Hoaglin (2012). Exploratory data analysis (usually abbreviated to EDA) methods emphasize displaying the data graphically, and, in line with its spirit of taking a more playful stance towards one's data, they often have appealing names, such as "stem and leaf plots" or "box and whisker plots." Many can be done within SPSS.

It is also useful to explore correlations between the main variables, particularly among all the independent variable measures and among all the dependent variable measures. Such analyses usually reveal patterns in the data that help you to understand subsequent results. For example, if one criterion measure performs differently from the others, it is useful to have studied its patterns of correlations with the other variables. Similarly, repeated confirmation of hypotheses is less impressive if the variables in question are strongly interrelated, suggesting that they are really measuring the same underlying construct.

There is a dilemma between, on the one hand, the desire to get the maximum mileage out of the data, by conducting many analyses, and, on the other hand, the need to avoid the common error of overanalyzing the data, of trying to relate everything to everything else (known as "fishing expeditions" or "data dredging") and thereby capitalizing on chance associations, leading to high rates of spurious findings (Simmons, Nelson, & Sinonsohn, 2011). As we discussed above, you need to be ruthless in prioritizing the most important variables, and associations between them, that you want to focus on and then omitting the rest.

Statistical Significance Testing for Answering the Research Questions

For some discovery-oriented research, exploratory statistical analyses may be all that is required. Broad research questions are addressed with exploratory analyses, most of which may not be precisely planned in advance; instead, the researcher will follow up interesting leads as the analysis progresses. On the other hand, more focused research questions or hypotheses call for *confirmatory* analyses. These are aimed at testing prestated hypotheses, with specific planned tests corresponding to each one. In other words, exploratory data analysis is inductive (and abductive), whereas confirmatory analysis is deductive (Tukey, 1977).

In either case, you need to select statistical tests that are appropriate to your research questions and design. The complexity of the design and the nature of the research questions will determine the complexity of the analysis. For some designs, simple descriptive or correlational statistics will suffice; others may require complex multivariate methods.

The choice of inferential statistical methods lies outside the scope of this text. Detailed treatments can be found in the standard statistics texts (e.g., Field, 2013; Howell, 2010; Siegel & Castellan, 1988; Tabachnik & Fidell, 2013; Winer et al., 1991). It is also worth seeking advice from psychologist colleagues or from statisticians: even experienced researchers need help for more complicated analyses (although statisticians prefer to be consulted before the data are collected, in order to have some input into the design).

Analyzing the Strength and Significance of Quantitative Effects

The final step in analyzing the findings of a study is to evaluate the strength and significance of the findings: are the results substantial or are they trivial? This might seem to be a matter of interpretation, but statistical methods for addressing these issues have continued to emerge, especially over the past 25 years. In addition to statistical significance (based on probability), these include effect size (based on amount of covariation), and clinical significance (based on practical impact).

It is helpful first to return to Cook and Campbell's (1979) four validity types (see Chapter 8). We have previously examined construct validity and internal validity.

The other two validity types are statistical conclusion validity, which we examine next, and external validity, which we touched on in Chapter 10 but will address further in the Interpretation section of this chapter.

Statistical Conclusion Validity

As we discussed above, statistical analysis will often demonstrate that two variables covary, that is, that they are associated with each other. For example, therapist empathy may be found to be associated with client improvement. The assessment of statistical conclusion validity asks whether conclusions about covariation are sound. It is a preliminary step before making causal inferences (which are covered under internal validity). Three questions need to be addressed (Shadish et al., 2002).

First, was the study sensitive enough to permit reasonable inferences about covariation? Greater sensitivity is obtained either by larger sample sizes or by reducing the amount of error, both of which give greater statistical power (see Chapter 10). Error can be reduced both by selecting measures that are more reliable and by choosing a research design that controls for extraneous variation (for example, by using a repeated-measures design), by selecting a homogeneous sample (see Chapter 10), or by incorporating an individual difference variable as an extra factor in an experimental design (see Chapter 8).

Second, if the study was sensitive enough, do the variables in fact covary? Here the issue is whether the right statistical tests were performed. Did they meet the assumptions behind them (e.g., for a normal distribution)? Was an appropriate error rate (alpha level, i.e., the critical value of p) set? How likely is it that the results were due to chance variations? "Fishing expeditions," that is, conducting a lot of significance tests in a large data set until something interesting turns up, will produce spuriously significant results. For example, if you correlate 10 variables with 10 other variables, you will have 100 distinct correlation coefficients. Suppose that 10 of them are statistically significant at a conventional alpha level of $p<0.05$. In this case, your best guess is that half of these 10 correlations (i.e., 5 out of the 100) are significant by chance alone, but you will have a difficult time telling which correlations are probably genuine and which are most likely spurious.

Of course, if you have to conduct multiple statistical tests, the alpha level at which the tests are performed should be made more stringent (Howell, 2010). Unfortunately, this will reduce your power to detect any real effects, sometimes drastically, because, in clinical research, samples are generally small and difficult to obtain.

Third, if the variables do in fact covary, how meaningful is that covariation? This seemingly straightforward question opens up a number of difficult issues about how to measure the significance of the findings. There are three ways to do this, which we will examine in turn: statistical significance, effect sizes, and clinical significance.

Statistical Significance

Statistical significance defines the meaningfulness of an effect in terms of how improbable it is that it would occur by chance alone. However, psychologists have long argued that statistical significance in itself does not reveal much, and that the whole null hypothesis testing framework is mistaken (e.g., Bakan, 1966; Cohen, 1990, 1994; Lykken, 1968; Rodgers, 2010). Cohen (1994) memorably entitled his paper "The earth is round ($p<.05$)", which neatly sums up his main argument.

One issue is the arbitrariness of the conventional criterion of $p<.05$, in other words that a result has to have a probability of less than 1 in 20 of occurring by chance in order to be counted as being statistically significant. A result with a p of .049 is barely stronger than one with a p of .051, yet the first will be reported and the second will not. There is no logical reason why 1 in 20 was settled on, the convention could just as well have been 1 in 25 or 1 in 18 (although see Cowles & Davis, 1982, for the historical background to the .05 value).

A more serious issue is that, given a large enough sample size, any effect will become statistically significant at whatever alpha level has been set, since no null hypothesis is ever exactly true (Meehl, 1978). Thus a result may be statistically significant but practically trivial. For example, in testing a new therapy for depression, a mean difference of two points on the Beck Depression Inventory between the experimental and control group may reach statistical significance with a large enough sample, but it would be clinically irrelevant, especially if both groups remained severely depressed.

A third issue is whether it is ever valid to conclude that two groups are equivalent. This issue has implications for hotly debated questions such as whether different therapies have equivalent outcomes. It is a truism in statistics that failing to show a difference between two groups does not mean that they are equivalent. In other words, "you can't prove the null hypothesis." However, various methods have recently emerged for testing the approximate equality of two conditions in a trial. These include *equivalence analysis* (Rogers et al., 1993), *noninferiority trials* (Pocock, 2003), and Bayesian significance testing (Dienes, 2014). These approaches use confidence intervals to allow researchers to demonstrate the equality, within specified limits, of two or more experimental or control groups (see Elliott et al., 2013, for an application in psychotherapy research).

A fourth issue is that the traditional null hypothesis testing framework is answering the wrong question. What we really want to know is: given the data, how likely is it that our hypothesis is true? However, null hypothesis testing addresses a different question: given the null hypothesis, how likely are our findings? Two statistical approaches provide researchers alternative ways of examining significance. The first is to use confidence intervals to give probabilistic parameter estimates (American Psychological Association, 2010b; Cumming & Finch, 2005); the second, more radical, alternative is to work within a Bayesian, rather than a frequentist, statistical framework (Dienes, 2011, 2014).

Effect Sizes

One potential solution to the problems of statistical significance is to also evaluate the meaningfulness of findings in terms of the amount of covariation, that is, the *effect size* (see our discussion of statistical power analysis in Chapter 10). There are a number of different effect size measures, depending on the statistical comparison being carried out (Cohen, 1992; Vacha-Haase & Thompson, 2004). The basic principle is to create an index of the strength of the relationship between two variables that is independent of the sample size. In general, effect sizes should always be reported along with statistical significance tests, as they provide important information for interpreting findings.

The calculations involved are best illustrated by considering a simplified one-group pretest–posttest comparison. In this case, the most appropriate effect size measure is the difference between the pre- and the post-therapy mean scores, divided by the pooled standard deviation for pre- and post-therapy. For example, in a study of virtual reality exposure therapy for fear of flying (Rothbaum, Hodges, Smith, Lee, & Price, 2000), the mean pre-treatment score on the Fear of Flying Inventory was 105.85, the mean post-treatment score was 86.14 and the pooled standard deviation can be calculated as 36.65. The effect size therefore is (105.85 - 86.14)/36.65, which comes to 0.54 (Cohen, 1988, classifies 0.50 as a medium effect: see Chapter 10). Thus, using effect-size measures, we can say that these clients showed, on average, a moderate improvement in self-reported fear over the course of the therapy.

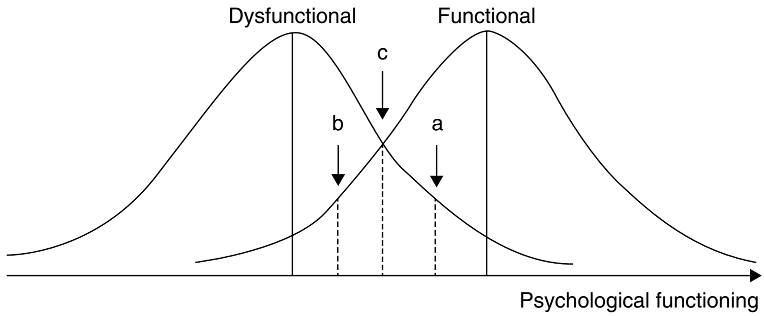
Effect-size metrics have an important additional advantage: they make it possible to compare the strength of findings across different studies, using meta-analytic procedures (see Chapter 3). Their relevance to analyzing the strength of findings from a single study is twofold. First, it is often useful to use meta-analytic methods to compare and combine results within a study: for example, when you have several different outcome measures, effect sizes can be used to characterize the largest or smallest areas of change, and also to provide an overall summary index for the study (mean effect size). Second, effect sizes can be used to compare the results of a single study with the findings from other similar studies in the literature. This involves calculating the effect sizes and comparing them with the corresponding effect sizes obtained in other studies or from meta-analyses, thus revealing how the findings fit in with the rest of the literature.

Clinical Significance

One problem with effect sizes is that they compare differences in mean scores against the standard deviations of the groups, rather than against any absolute standard. Although effect sizes are more meaningful than *p*-values, the presence of a large effect size still does not guarantee that a result is clinically meaningful. For example, in a two-group experimental design, a large effect could be due to small standard deviations in the experimental and control groups, rather than a substantial difference in the means themselves. A pre–post study of a psychological intervention could have a large effect size, but the clients may not feel much better after it.

The search for a way of capturing which findings are clinically important and which are trivial has led to the development of indices of clinical significance, most closely associated with the work of Jacobson and his colleagues (e.g., Jacobson, Roberts, Burns, & McGlinchey, 1999; Jacobson & Truax, 1991). Such indices are now routinely incorporated into studies where clinical change is assessed. In contrast to both statistical significance and effect sizes, clinical significance methods examine change at the level of each individual, rather than averaging across groups of individuals. This enables one to address the commonsense question about a therapy—what proportion of its clients get better?

These ideas are again best illustrated in the context of psychotherapy outcome research. The first thing is to ascertain whether there is *reliable change*, that is, whether the observed change is greater than the fluctuations that might be expected to arise from unreliability in the measuring instrument. Jacobson and Truax (1991) present a formula for a *Reliable Change Index (RCI),* which involves calculating the cut-off value that any pre-post change has to exceed in order for it to be considered as reflecting more than just



a. The area to the right of this line depicts the scores corresponding to criterion 1.
b. The area to the right of this line depicts the scores corresponding to criterion 2.
c. This line represents the mid-point between the means of the dysfunctional and the functional groups.

Figure 12.1 Three criteria for clinical significance. From Jacobson & Truax (1991), © 1991 the American Psychological Association. Adapted by permission.

random error of measurement (see http://www.psyctc.org/stats/rcsc.htm for details on carrying out the calculations). However, reliable change is clearly just an initial requirement: the more important issue is whether any change is clinically meaningful.

The concept of *clinical significance* attempts to encapsulate quantitatively what we usually mean when we say that an intervention with an individual client was successful, which is that the client's level of functioning (in terms, say, of a depression, anxiety, or self-esteem score) has substantially improved after the intervention (Kraemer et al., 2003; Lambert & Ogles, 2009).

Substantial clinical improvement normally implies one of three things: (1) that the client is no longer in the abnormal range, (2) that they are back in the normal range (sometimes known as "high endstate functioning"), or (3) that they are at least half-way between the two. These different ways in which a successful outcome may be conceptualized can be expressed more formally (Jacobson & Truax, 1991; Lambert & Ogles, 2009):

1. That the client's post-intervention score no longer represents abnormal functioning, that is, it has moved outside the range of the dysfunctional population. Outside the range is usually defined as more than two standard deviations away from the mean of the dysfunctional population.
2. That the client's post-intervention score represents a return to normal functioning, that is, it has moved inside the range of the functional population. Inside the range is usually defined as being within two standard deviations of the mean of the functional population.
3. That the client's post-intervention score is more likely to be in the functional than the dysfunctional population. This is usually defined as being closer to the mean of the functional population than the dysfunctional population.

These three criteria are illustrated in Figure 12.1 (adapted from Jacobson & Truax, 1991), which locates the cut-off points for each of the three possible criteria on the distributions of the dysfunctional and functional groups. Which of the three criteria to adopt in any given study depends on which of the three ways best fits your conceptualization of a significant outcome of the intervention that you are researching. Criterion (c) represents a possible compromise, since it is defined in terms of where the "weight of the evidence" falls. In measures like the CORE Outcome Measure (Barkham et al., 2001), criterion (c) falls about 1.25 standard deviations above the mean for the normal population, where it provides a useful cut-off between "normal" and "moderately distressed."

INTERPRETATION

Interpretation is the subject of the Discussion section of a research report and involves:

- understanding the meaning of the findings, in terms of previous research and theory, and how they contribute to knowledge;
- assessing the strengths and limitations of the study;
- considering the scientific and practical implications of the findings.

Analysis yields the basic findings of the study; interpretation attempts to spell out their implications or broader meanings. Within the quantitative tradition, analysis is often a technical exercise, which follows set rules and requires expertise more than inspiration, whereas interpretation requires imagination and insight into the psychological meaning of the phenomena. It is a broader conceptual task, aimed at bringing the results of the study to bear on the issues that initially inspired it. In qualitative research, the distinction between analysis and interpretation is not so clearly drawn; nevertheless, there is still room for taking a broader view of one's findings or representations of the data.

Interpretation is the main topic of the Discussion section of a research article; it consists of three related parts, which we will cover in the order that they usually occur in that section. The first is to understand the meaning of the findings, in terms of previous research and theory. The second is to assess the strengths and limitations of the study, to see whether it can really support the interpretations that you bring to it. The third is to address the scientific and practical implications of the findings: what research needs to be done next and how might the findings inform clinical practice and possibly also social policy.

Contributions to Knowledge: Understanding the Meaning of the Findings

Having examined the strength and significance of the findings, the next step is to understand what they mean. This task involves relating the findings back to the literature: the research, theory, or conceptual model that the study was based upon. How do the data answer the research questions? Do they support or contradict the underlying theoretical model? How do you explain any discrepancies between your expectations and the findings? What is the study's contribution to knowledge: how does it change our understanding, and what is new or important about it? (These are all questions that journal reviewers will ask if you submit your study for publication.)

You may also wish to speculate more imaginatively about what the findings mean. Speculation is quite acceptable if labeled as such: that is, if you warn your readers that you are not claiming that your speculations are securely grounded in the evidence.

"The facts are friendly"

Although it is much easier said than done, it is worth reminding yourself to approach this task with an open mind. Try not to be defensive or dogmatic about your theories, but allow the data to speak for themselves. Carl Rogers used to say "the facts are always friendly" (Kirschenbaum, 1979: 205); in other words, do not fight the results, even if they cause you discomfort. The opposite of the attitude of openness is to deny the validity of the results if they conflict with your preconceived ideas: rather than adjusting your theories, you adjust reality instead. Research may force us to rethink our ideas, which can be a painful process, but if we are unwilling to revise our views, what is the point of doing the research in the first place?

In practical terms, this attitude of openness to your data means that you should not give in to the temptation to skim through your results attending only to the findings that confirm your hypotheses. In fact, the results that deviate from your expectations are worth at least as much reflection and discussion as those that confirm them, because they are the key to revising your understandings of what you are studying, as well as improving your design, data collection, and analysis methods.

Finally, researchers also need to consider the weaknesses of their chosen theoretical explanation. Could the findings be explained in other ways than in terms of your pet theory? Are they compatible with other frameworks than your own? How would a neuroscientist or a psychoanalyst view them? Often these questions lead on to developments or refinements in the original theoretical model, which then lead on to ideas for further research (much like the cycle of inquiry that we described in Chapter 2).

Methodological Issues: Strengths and Limitations of the Study

All studies have their strengths and limitations. The task for researchers (and readers) is to weigh up the relative merits and flaws of any study. Some weaknesses are trivial and can be mentioned in passing; others will be more serious but may—or may not—be offset by particular strengths (Rozin, 2009).

The seriousness of a study's weaknesses determines how much credence can be given to its findings. Researchers need to ask whether there are any problems with their study that might have influenced the results: they owe it to themselves and their readers to make these explicit. (In qualitative research this process is sometimes called discounting.) The physicist Richard Feynman forcefully expressed his belief in this aspect of scientific honesty:

> It's a kind of scientific integrity, a principle of scientific thought that corresponds to a kind of utter honesty—a kind of leaning over backwards. For example, if you're doing an experiment, you should report everything that you think might make it invalid—not only what you think is right about it: other causes that could possibly

> explain your results. Details that could throw doubts upon your interpretation must be given, if you know them. You must do the best you can—if you know anything at all wrong, or possibly wrong—to explain it. (Feynman, 1985: 341)

It is useful at this point to return to the various methodological frameworks relevant to appraising particular types of research. For example, Cook and Campbell's (1979) framework of validity threats in experimental research is useful not only for planning a study, but also for making sense of its findings and reflecting on its strengths and limitations. A review of the lists of validity threats is often useful for statistical conclusion validity issues, internal validity problems, construct validity confounds, and external validity limitations. Was the statistic power adequate? (A perennial issue.) Are there plausible third variables that might account for your findings equally well? In what other ways could your findings be explained, in addition to the variables that your research has focused on? How much conceptual overlap is there between predictor and criterion variables? In what ways do you think your sample might be unrepresentative of typical clinical populations, such as by being volunteers or early adopters of new treatments (see the following section on external validity)?

Similarly, if your study was qualitative, it is useful to revisit the guidelines for good practice described in Chapter 5. For example: Have you described your commitments and expectations that may have affected the results? Was there enough data to reach saturation of categories? Did you use auditing procedures, member checks or multiple qualitative analysts?

External Validity

An important aspect of assessing the strengths and limitations of a study is to ask to what extent its findings can be generalized beyond its immediate context. This is the external validity question (Shadish et al., 2002; see also Chapter 10). It examines the representativeness of the study: its range of application across persons, settings, and times. Any peculiarities of the sample, procedures, setting, or timing will reduce the external validity.

A dilemma for researchers is that the demands of external validity and those of statistical conclusion validity and internal validity often conflict. For example, one way to reduce error, and therefore to increase the statistical conclusion validity of the study, is to draw the sample from a homogeneous target population. However, this will make the sample less representative and thus lower the study's external validity. Furthermore, randomized controlled trials are designed to have high internal validity, but this may be at the expense of having procedures unrepresentative of normal clinical practice (see Chapter 8). As frequently happens with decisions in research, there is no clear-cut answer here.

The role of external validity in qualitative research is controversial (Braun & Clarke, 2013). Some qualitative researchers reject the whole notion of representativeness, arguing that they are attempting to develop particular contextualized understandings of particular cases, and that generalizability is not an issue. Others argue that the representativeness of their sample (referred to as "horizontal generalization") is not the issue, but what is important is whether the theoretical ideas that are generated are capable of broader application ("vertical generalization"). Yet others argue that external validity is important in qualitative research, and can be obtained by careful sampling in order to draw a qualitatively representative sample, that is, a sample that includes the important variations and aspects of the phenomenon being studied. In any case, possible sample limitations need to be considered when drawing conclusions from a qualitative study, if that study claims to be interested in developing general knowledge about a phenomenon.

Replication

The best way to increase external validity is via replication. The more that you can reproduce the initial findings under diverse conditions, the more convincing will they become. Lykken (1968), drawing from Sidman (1960), distinguished three types of replication: (1) *literal* replication is an exact duplication of the study conducted by the original researchers using identical procedures; (2) *operational* replication is carried out by other researchers, using the methods published by the authors of the original study; and (3) *constructive* replication replicates the basic idea of the study, but uses different methods, for example, a different population or alternative measures of the same constructs. If the results of a study hold up under several constructive replications, we can begin to develop an understanding of the range of situations and persons to which its results generalize.

Research programs often begin with laboratory studies which have low external validity, since it may be a good idea to start out by a simple test of one's theories in such a setting, rather than in an expensive and time-consuming field study. For example, early behavior therapy studies used college student volunteers who had spider phobias for which they had not sought help. However, if the first laboratory or analog studies prove to be successful, then the researcher needs to conduct more ecologically valid studies to give the findings credibility. On the other hand, a researcher with primarily applied interests might want to begin with small-scale field studies, thus avoiding the initial laboratory work.

Psychology in general is currently undergoing a "replication crisis," that is, a concern that many published findings are one-off instances, often caused by false positives, that will not be able to be replicated (Pashler & Wagenmakers, 2012; Simmons et al., 2011). The problem partly stems from journal editorial policies of being reluctant to publish null findings (e.g., failures to replicate) and also preferring to publish original research rather than replications. Added to this are recent high-profile cases of fraudulent publication in social psychology (see Pashler & Wagenmakers, 2012). The Nobel Laureate, Daniel Kahneman, has said, with respect to research on social priming, that he fears a "train wreck looming" unless researchers get their collective house in order and start replicating each other's work (see Yong, 2012).

Scientific and Practical Implications

Scientific Implications

As we discussed in Chapters 1 and 2, research is often a circular process, in that the data may only partially answer the research questions. Often the study reveals, with the benefits of hindsight, that the research questions could have been better formulated, that the theory on which it was based was inadequate, that there were measurement or design weaknesses, or perhaps that the approach shows promise and could be expanded or applied more broadly. All of these conclusions have scientific implications, in that they will lead naturally on to a plan for future research in the area.

Practical Implications

Finally, in clinical research it is important to consider what implications your findings have for professional practice. What would you suggest that practitioners (e.g., therapists, teachers) do, or not do, differently on the basis of your study? For example, the Dimidjian et al. (2006) RCT that we used as a running example in Chapter 8 has the implication that practitioners attempt behavioral activation with their severely depressed clients.

Laying out the possible practical implications of your study often feels intimidating, as it requires you to extrapolate what would follow from your results if they held true for the clinical situations and populations that you were interested in when you started the research. A useful starting point is to ask yourself a question that examiners like to ask candidates at defense or viva exam meetings: How has your research changed your own practice? What if anything have you learned from your study that you have put to use in your work with clients? Sometimes this is a direct result of having your hypotheses confirmed or disconfirmed, but often it is more subtle learning, such as an increased awareness of certain client issues. Both obvious and more subtle learnings are worth noting as practical implications.

Consideration of the practical implications of the study leads naturally on to the final section, dissemination, which considers how the findings and their implications will be made known to people who might use them.

DISSEMINATION

- Dissemination is the last step in the research process: it is often difficult but ultimately rewarding.
- Research may be disseminated through a variety of outlets, from academic journals and conferences through to more popular media.
- It is worth paying close attention to your writing, to tell your story as clearly and economically as possible.
- Research does not always translate directly into wider use: several factors influence whether findings are acted upon.

In the end, research is basically a public activity. In the midst of it, you may feel as though you are doing it for yourself alone, but ultimately your goal is to communicate your findings to others, often with the aim of achieving some kind of change. Usually this communication involves a written report or published article, but it may also involve presentation in the research setting, at conferences, or for policy-makers in government departments.

Writing up

Writing up the project can seem a mountainous task. It is easier if you start the process early on and think of the write-up as accumulating progressively by a series of successive approximations over the course of the project. Ideally, it is worth starting to plan the report and writing a first draft of the Introduction and Method sections while you are collecting your data.

Having said that, many people resist writing up, for various reasons. For some, putting pen to paper, or rather fingers to keyboard, is the hardest step of all. It is arduous and intellectually demanding, since it forces you to present your ideas in a clear and watertight way. Fear of criticism—one's own or others'—can lead to procrastination. Also, the workload and the emotional stresses of many clinical jobs can make it hard to find the time for writing.

Good research reports are usually simple. They tell the story of the research project, sticking to the main themes without bogging the reader down in irrelevant detail. It is worth continually bearing in mind the one or two key questions that guided the investigation (or at least the part of it that you are writing up) and to structure your report around those themes. This is often hard to do at the end of a study, because you have often lost perspective on what is important, and cannot see the wood for the trees. Try to step back and distance yourself from your study (this is not easy if you have just spent months fretting over it), attempting to see it through the eyes of a general informed reader, as though it were done by someone else. Get criticism from trusted colleagues. Presentations at seminars or conferences are often a good way of shaping up your work and getting other people's reactions to it.

Writing Style

Not only should you try to tell a simple and clear story, but also try to tell it in simple and clear prose. The novelist, Philip Pullman, expresses this nicely:

> The aim must always be clarity. It's tempting to feel that if a passage of writing is obscure, it must be very deep. But if the water is murky, the bottom might be only an inch below the surface—you just can't tell. It's much better to write in such a way that the readers can see all the way down; but that's not the end of it, because you then have to provide interesting things down there for them to look at. (Pullman, 2002)

Journal articles in psychology, and in the social sciences generally, are notorious for using incomprehensible jargon or overelaborate sentence constructions. Much psychology writing is impenetrable or pretentious. Oppenheimer (2005) parodied this tendency in the title of his paper "Consequences of erudite vernacular utilized irrespective of necessity: problems with using long words needlessly" and his findings showed that the strategy of using long words backfired, in that writers using complex expressions were judged to be less intelligent.

George Orwell's much quoted spoof rewrite of a Biblical passage (see box) is a paradigmatic example of the contrast between vigorous writing and psychological waffle. Many style guides are available to help combat the tendency to write like Orwell's second paragraph. We recommend Lanham's (2007) amusing and idiosyncratic *Revising Prose*, which presents a 10-step method for editing one's own writing, Strunk and White's (2000) miniature gem *The Elements of Style*, and Williams's (2007) *Style: Lessons in Clarity and Grace.*

George Orwell's parody of psychological writing.

Here is a well-known verse from Ecclesiastes:

> I returned and saw under the sun, that the race is not to the swift, nor the battle to the strong, neither yet bread to the wise, nor yet riches to men of understanding, nor yet favor to men of skill; but time and chance happeneth to them all.

Here it is in modern English:

> Objective consideration of contemporary phenomena compels the conclusion that success or failure in competitive activities exhibits no tendency to be commensurate with innate capacity, but that a considerable element of the unpredictable must be taken into account.

Orwell (1946/1968: p. 156); reproduced by permission of the estate of the late S. M. B. Orwell.)

Psychology journals have complicated stylistic requirements of their own, which can deter novice authors. The APA *Publication Manual* (American Psychological Association, 2010b) is comprehensive and detailed, ranging from general issues about layout and style to minutiae about where to put commas in the reference list. It also includes a helpful section on how to use inclusive language that does not express prejudice about gender, ethnicity, etc. Sternberg and Sternberg (2010) summarize the stylistic guidelines and provide useful advice on how to write up a project.

Publication

It is always worth considering publishing your research, even though your primary aim may be to write it up for a course requirement or part of a local evaluation. The initial research report itself may not be very useful. Dissertations and theses are often long, formalized, and indigestible, and evaluation reports are usually geared to the interests of a local audience. On the other hand, research reports in professional journals at least aim for brevity and comprehensibility. If the study does not meet the exacting methodological standards of the APA or BPS flagship journals, consider less demanding outlets, often those attached to specific divisions, sections, or interest groups of a professional body. You may also want to present your work at conferences, which is often a good stepping stone to publication.

The process of submitting an article for publication is as follows:

1. Identify the target journal that you are aiming for, as different journals have different requirements, both in terms of content and style. The anticipated readership of the journal will partly determine what material to include and how to present it.
2. The journal website will describe the topics and kinds of articles that the journal publishes, and give a link to tables of content of recent issues. It will also have a link labeled something like "instructions for authors," giving the journal's stylistic rules and instructions for submitting manuscripts.
3. Once submitted, if your paper does not meet the broad requirements of the journal or it is considered to be seriously flawed, the editor will send it straight back without additional review. Otherwise, it will be sent out to reviewers, who are usually blind to the authors' identity. Reviewers are normally asked to return their reviews to the editor within a month, but they often take longer. On the basis of the reviews, the editor then makes one of the following decisions: accept the paper immediately (this is very rare), accept it subject to specified amendments, request revisions and resubmission, or reject it. Mainstream journals tend to have high rejection rates: for example, in 2013, the *Journal of Consulting and Clinical Psychology* had a rejection rate of 79% (American Psychological Association, 2014).
4. The editor will email their decision to you with copies of the reviewers' comments. You then have to decide how to proceed, based on your perception of the reviewers' and editor's views of your work. Bad reviews, both in the sense of negative ones or sloppy ones, can be upsetting. The whole process may be somewhat arbitrary, in that different reviewers do not always agree (Fiske & Fogg, 1990). Even if you get unfavorable reviews, it is important not to give up. A thick skin is needed: if your paper is rejected, it is better not to take it too personally, and to move on to resubmitting it elsewhere. At least try a couple of other journals before concluding that your work is not worth publishing. The potential pleasure and professional recognition that comes from getting your work in print repays some investment of effort and emotional energy.

Authorship Issues

If the research has been done as part of a team (or if your supervisor has had a major input), the issue arises of who will be listed as authors and in what order.

Authorship issues can often arouse feelings of competition or resentment in the research team and so it is helpful to start discussing them early on in a research project (see Chapter 3). However, sometimes everyone's contributions can only be evaluated once the study is completed.

To be listed as an author, an investigator should have made a substantial scientific contribution to the paper, for example, a major contribution to the formulation or design (American Psychological Association, 2002, Standard 8.12; British Psychological Society, 2011; Fine & Kurdek, 1993). Minor contributions that do not merit authorship, for example, help with interviewing or data analysis, or a senior doctor's permission to study patients under his or her care, should be mentioned in the acknowledgements section. The order of authorship should reflect each person's contribution.

Utilization

Ideally, research should serve a purpose, not just be an empty exercise. You can increase the likelihood of its having an impact if you actively promote your findings. Articles in academic journals are only read by a select few, so it is worth putting effort into disseminating your work more widely. This can be done informally, by discussing the research with people who might use it to make decisions (e.g., managers, policy-makers, government officials). Or it could be done by writing more accessible articles for the general public, for example, in newspapers, magazines, blogs or specialist websites. You may even want to write a press release in order to interest radio, television, or print journalists in your findings. Many institutions have a press or media office to assist researchers with dissemination. Email listserves and social media can also be used to alert people to your study.

The process of how research findings get taken up, if at all, is not always clear. Academic research can feel narrow and inward looking, with researchers writing primarily for other researchers, but sometimes findings do permeate through to influence practice. Weiss (1972, 1986) has developed models of research utilization, the thrust of her work being that the relationship between research and policy is nonlinear and complex (for further discussion, see also Patton, 2008; Humphreys, 2003; Shadish et al., 1991). The diffusion of innovation model (Rogers, 2003) examines how innovations in general, such as technologies or public health programs, are taken up (or not) over time. The naive idea that research has a direct influence upon policy is rarely borne out in practice. Often research is ignored, misapplied, or used to buttress only one side of an argument. There is now an academic specialty, with its own journal *Implementation Science*, devoted to issues of translating research into policy or practice (Madon, Hofman, Kupfer, & Glass, 2007).

However, there are cases where, for better or worse, research findings appear to strike a chord and become incorporated into far-reaching policy decisions. The studies carried out in the 1950s and 1960s, demonstrating the dehumanizing effects of long-stay institutions, provide an example of research influencing practice. Although this research may have been used simplistically, and possibly only as a cover for cost cutting by governments, it also made forceful points that have lasting resonance about the psychological damage done by institutionalization.

THE END

By the time you have finished writing up your study—particularly if it is a student project—you may be thoroughly fed up with it and painfully aware of its flaws. You may even be thinking of giving up research altogether. Although we understand that reaction, having felt it many times ourselves, we hope that you will give yourself a well-earned break and that after you have recovered you will return to do more research. Consider going back to the drawing board and using your hard-won wisdom to start the cycle again by designing a better study. The field of clinical psychology needs to strengthen its knowledge base through high-quality research. Psychologists who can draw on both research and clinical skills—whom we would call good scientist-practitioners—are central to this process.

CHAPTER SUMMARY

The final stage of the research process consists of three interrelated parts: analysis, interpretation, and dissemination. Analysis means establishing what the findings are, and in particular how the data answer the research questions.

Qualitative analysis is a largely inductive procedure. Although different qualitative orientations use different approaches to analysis, some general principles can be outlined. Analysis starts with preparing and organizing the data (usually involving transcribing interviews). The analysis itself involves three related processes: identifying meanings in the data, grouping ideas into categories or themes, and integrating the themes into a conceptual framework. Qualitative analysis can be done using either a within-case (idiographic) or a cross-case approach.

Quantitative analysis may be either exploratory or confirmatory, depending on the type of research questions. It involves preparing the data, exploring its properties, conducting statistical tests to address the research questions, and evaluating statistical significance. The concepts of statistical conclusion validity, effect size, and clinical significance are typically used to evaluate the strength and meaningfulness of the findings.

Interpretation and dissemination are the final stages of the research process. First, the researcher needs to evaluate the meaning of the results, including their implications for scientific knowledge, their methodological strengths and shortcomings, and their practical, professional, and policy implications. Dissemination involves communicating both the findings and your understanding of them to other people. It is often a difficult but rewarding task, as it involves painstaking attention to the detail of what you are trying to communicate. Research may be disseminated through a variety of outlets, from academic journals and conferences through to popular media or social media. It does not always translate directly into practice: a complex set of factors influences whether research is ever acted upon.

FURTHER READING

Robson (2014) covers the fundamental steps in both quantitative and qualitative analysis, and also discusses the dissemination of research. Statistical methods are explained in the standard texts, for example, Field (2013) and Howell (2010) for a general coverage, Siegel and Castellan (1988) for non-parametric methods, Winer et al. (1991) for analysis of variance, and Tabachnik and Fidell (2013) for multivariate analysis (and also a good account of the preliminary steps in data preparation).

There are a number of sources for qualitative analysis. Some present general approaches (e.g., Miles et al., 2014; Patton, 2002) or give an overview of a range of approaches (e.g., Camic et al., 2003; Creswell, 2013; Willig, 2013); others present one specific approach in detail. Among the latter are Braun and Clarke (2006, 2013) on thematic analysis, Corbin and Strauss (2015) on grounded theory, Smith et al. (2009) on interpretative phenomenological analysis, Hill et al. (2011) on consensual qualitative research, and Potter and Wetherell (1987) and Potter (2012) on discourse analysis.

Shadish et al. (2002) are, as always, a key source in thinking generally about interpreting findings. It is worth reading the first few pages of Jacobson and Truax's (1991) classic paper on clinical significance, and there is a further discussion of effect size and clinical significance in Kraemer et al. (2003) and Lambert and Ogles (2009). Finally, Carol Weiss's (1972, 1986) seminal work on the uptake of research by policy makers is always worth revisiting.

QUESTIONS FOR REFLECTION

1. To get a feel for qualitative analysis, make an attempt at analyzing the main themes in a short extract from an interview, either one you or a colleague have conducted, or you could use the brief extract given in Chapter 6.
2. What do you think about the practice of estimating missing data? Is it cheating? When do you think it would be justified? When might it be a bad idea?
3. In your own research, what would each index of significance (statistical significance, effect sizes, and clinical signficance) tell you?
4. It's not too early: Spend a few minutes contemplating the possible real-world applications or implications that your results might have: (a) if you find what you expect/hypothesize; or (b) if you fail to find what you expect/hypothesize. Write these down now rather waiting until you're too exhausted in the last stage of writing up.
5. Surveys show that most psychologists don't publish their thesis or dissertation. Why do you think this is?
6. Writing up research can be difficult. At a personal level, what beliefs or feelings get in your way? How can you minimize their impact?

13
Epilogue

> "Thus scientific methodology is seen for what it truly is—a way of preventing me from deceiving myself in regard to my creatively formed hunches which have developed out of the relationship between me and my material." (Rogers, 1955, p. 275)

We started this book by comparing a research project to telling a story. Our own narrative of how a research project progresses is now drawing to a close. The book has been structured around the four basic stages of the research process, that is, the steps that researchers go through when they are carrying out a project (although they do not usually go through them in the neatly ordered sequence that we have depicted). These stages are: (1) groundwork, (2) measurement, (3) design, and (4) analysis, interpretation, and dissemination. Separating out the important issues according to the stage in which they are prominent is helpful both in planning and also in reading research.

This brief final chapter brings together some central ideas that run through the book: methodological pluralism (matching the method to the problem), appraising research, and combining research with practice. Finally, we end with some images of research.

Methodological Pluralism

Our central theme has been methodological pluralism: that no single approach to research is best overall, rather, what is important is that the methods be appropriate for the questions under investigation. It can also be labeled appropriate methodology, by analogy with the catchphrase "appropriate technology" (although strictly speaking the word methodology should only be used in its precise meaning of the study of methods). No single research method is inherently superior to any other: all methods have their relative advantages and disadvantages.

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However, we want to make it clear that methodological pluralism is not equivalent to methodological anarchism. Unlike Feyerabend (1975: see Chapter 2), we are not saying that "anything goes." Quite the reverse, we have tried to outline methodological rules and principles within the context of each method. Some of these principles are common to all approaches; some are relevant only to specific research approaches or genres.

Wherever they are applied, the central purpose of these methodological rules is, as Carl Rogers aptly said in the quotation forming the epigraph to this chapter, to prevent one's deceiving oneself and others by drawing conclusions that are not supported by the data. In terms of the simple model of research that we presented in Chapter 2, the essence of the research attitude is finding ways to test your ideas against your experience of the world.

This book has covered both traditional, quantitative methods and the more recent (at least within psychology), qualitative methods. Although the proponents of each approach have sometimes represented warring factions, we believe that the debate has often been too polarized and that it is possible, indeed desirable, to combine multiple methods within a single study or research program.

Our message is not that knowing about research methods enables you to produce the perfect piece of research. However, we do hope that, having read this book, you will be better able to make informed choices in your own research—or at least informed compromises. As we have said throughout, there are always compromises and trade-offs in research. However, although we are saying that there is no one right way to do it, there definitely are wrong ways. Consideration of how research might be done badly leads in to the next section, on appraising research.

Appraising Research

We have aimed to give readers the conceptual tools needed to become both better producers and better consumers of research. Throughout the book we have pointed to issues that need to be considered in evaluating studies, whether from the standpoint of the researcher or the reader. Here, we will focus on the consumer's perspective, and address how the concepts that we have raised in the context of planning and executing research can be applied when you are reading and evaluating research reports.

The more you know about research methods, the more you are able to recognize the problems in a piece of research. However, this does not mean that appraisal equals negative criticism. It is easy to criticize research. Psychology training often does a good job of teaching students how to pull studies apart; it is usually less good at giving them a sense of perspective. Our own students are often quick to find numerous flaws (many of which we consider to be trivial) in research papers, but they are less able to take a broader view and to see things in balance.

Thinking about the different stages of the research process helps you to conceptualize some of the important issues to consider when evaluating a piece of research. We have discussed general criteria that apply to all types of research as well as some that apply to specific approaches. We present these general criteria below, organized roughly according to how they fit within each of the stages in the research process. Although we have tried to make them as generally applicable as possible, not all of the criteria will apply to all pieces of work, nor will each criterion have equal weight.

Criteria for evaluating research

Groundwork. The first question to ask is "Who cares?" In other words, is the topic worth researching, has it been done before, does the study have the potential to add useful knowledge or to develop theory?

Is the literature review relevant and up to date? Does it cover empirical, methodological, and theoretical issues, and does it place the study in the context of scientific research in the area? Is the rationale for the study clearly articulated (possibly including an indication of the conceptual model linking the variables under investigation)? Are appropriate research questions or hypotheses clearly formulated?

Measurement. How are the main constructs defined and measured? Do the measurement methods (used in a broad sense to cover both quantitative and qualitative methods) adequately capture the constructs of interest? Are the methods appropriate to the research questions? If quantitative measures are used, do they have acceptable reliability and validity? If qualitative methods are used, have they been placed in the context of the researcher's theoretical orientation?

Design. Are the procedures described in sufficient detail to enable the reader to understand what was done and, if necessary, to be able to replicate the study? Is the design of the study appropriate to the research questions? Will it enable the desired inferences to be drawn? In quantitative studies, what are the threats to internal, external, construct, and statistical conclusion validity?

Is the sampling procedure clearly specified? Are the size and composition of the sample appropriate to the research questions and data analysis? Does the study conform to the relevant professional and ethical standards?

Analysis. Do the analyses address each of the research questions? Are the data presented clearly and coherently? (Any tables and figures should be both understandable and informative.) In quantitative studies, are data reduction techniques and statistical tests used correctly? In qualitative studies, is the analysis grounded in examples, and have procedures been included to check the credibility of the analysis?

Interpretation. Are the findings interpreted in the context of the research questions and the wider theoretical context in which the work was carried out? Are interpretations supported by the data? (However, speculations are in order if labeled as such.) Are competing explanations for the findings considered? Is the generalizability of the findings assessed? Are weaknesses of the study addressed? Are the scientific and practical implications of the findings discussed?

Presentation. Is the paper readable? (Consider its general prose style, use of jargon and sexist or other offensive language.) Is the paper's length appropriate for its content?

In arriving at an overall evaluation of a research paper, take into account that strengths in some areas may compensate for weaknesses in others (Rozin, 2009). Rather than simply listing the flaws of a study, try to estimate how they distort the conclusions of the research. Some technical flaws in the procedure may have a negligible impact on the results. The more important or innovative the topic or methods, the more forgivable should be any shortcomings: it is relatively easy to do methodologically sound but trivial research; it is harder to do innovative research that makes a scientific or practical impact.

Combining Research with Practice

As we hope to have made clear throughout this book, we believe there are benefits to be gained from the scientist-practitioner approach in its various expressions. Research helps advance practice by developing and testing new procedures; practice helps advance research by providing a source of, and a testing ground for, new ideas and methods, and by giving a reminder of the complexity of human behavior that helps counterbalance the simplifying tendency of much research. Researchers and practitioners have not always seen eye to eye: they have different needs and live in different worlds—even when they are the same person. Despite these difficulties, we believe that the relationship between the two activities can ultimately be mutually enriching.

However, although a scientist-practitioner approach is good for the field as a whole, it does not follow that combining the two activities is right for everyone. We recognize that actually carrying out research may not be everyone's cup of tea. As we discussed in Chapter 2, although there are many positive reasons for becoming involved in research, there are also several reasons why combining research and practice is problematic. Different individuals will weigh up each of these reasons differently, and decide to what extent, if at all, they want to be involved in conducting research. We do maintain, however, that at a minimum, practitioners need to be sufficiently informed about research methods to be able to understand and appraise research, even if they are not actually doing it themselves.

In the past, the scientist-practitioner role has been identified with a narrow conception of research, which has put many psychologists off attempting to do their own research. It is certainly true that some types of research are prohibitively complicated, costly, and time-consuming for the individual practitioner: randomized comparative therapy outcome research being the prime example. It is also unrealistic to expect that most practitioners would have the resources to conduct the kind of research that meets the exacting requirements of the major scientific journals. However, it is possible to work within a broader conceptualization of the scientist-practitioner model and generate practice-based evidence. We have tried to outline some possible methods that can be adopted by practitioners working on their own (especially the small-N approaches outlined in Chapter 9 and the evaluation methods in Chapter 11).

Another strategy to increase one's involvement in research is to make it a group endeavor. For us, one of the central pleasures of research is the process of working with students and colleagues. Teamwork provides stimulation through discussing mutually interesting ideas and struggling to resolve disagreements or differences in perspectives. It also brings support to what can otherwise be a lonely enterprise.

We would strongly echo Hodgson and Rollnick's (1996) advice to form a research team if at all possible.

We hope to have conveyed the message that research need not be as forbidding a process as is often imagined. Some of the old rigidities are now dissolving; it is widely recognized that there are many different approaches to research. It is possible to work in a genre of research that suits your own values, abilities, and ways of thinking. However, research is not to be undertaken lightly. It requires time and effort and it can be an intellectual struggle: rigorous thinking does not usually come easily. But we hope that ultimately the potential enjoyment and stimulation that research gives will encourage at least some of our readers to consider becoming involved in it themselves.

Some Images of Research

We began this book with the metaphor of research as a story. We will end with three additional images of the research process.

First, research can be understood as a *need*, a modern expression of the innate, universal human need to understand and master one's environment. This is how Cook and Campbell (1979) see research, with the framework of "evolutionary epistemology," that is, as part of what has made it possible for human beings to survive and prosper. This may be seen as an aspect of the growth tendency (Rogers, 1961), or as a biologically adaptive, inherited trait of curiosity, or simply as the "joy of knowing." In any case, research is one of the primary contemporary vehicles for living out this basic part of what it means to be human. We are not saying that research is always fun (much of it is drudgery!), but only that it is marked by moments of understanding and accomplishment which make it all worthwhile.

Second, research is a *journey*, a metaphorical going out on a voyage of exploration, like Odysseus or Jason in Greek mythology, or Darwin's voyage of discovery on the *Beagle*. This voyage begins with optimism and excitement but often veers toward danger, risk, and disappointment, sometimes almost crashing on the rocks of rigid methodology or running the danger of being sucked down into a whirlpool of confusing alternatives (Kvale, 1996). Sometimes, we make it home with a research project, and sometimes it fails and is never heard from again, buried at the back of a filing cabinet. But generally, if we persist, we will return safely with a story or two to tell about the adventure.

Third, research is a cultural *tradition*, a means of developing and communicating important ideas between people and across time. The methods and concepts we use are part of the syntax and vocabulary of this tradition; we use them as a modern rhetoric (Rennie, 2012), to establish ourselves and our work as credible and persuasive. But this also means that research is not complete until it is communicated to others. Viewing research as a cultural tradition also suggests that we can expect our field's research methods to continue to evolve and change, and to produce new forms and contents. This is simply what it means for something to be a living tradition: if it were set in stone, it would be dead.

Finally, being part of a tradition means that as you proceed, you first borrow (with credit) some of the tradition's voices; then you master the techniques, which means that you make some of these voices your own; and then you allow the tradition to speak through you. In the end, perhaps the best goal for a researcher is this: to become part of the tradition of knowledge and method in your field, adding your voice to those who have gone before, speaking to and through those who come after you, even when they no longer even recognize that it is your voice, among many others, that speaks in them. In this book, we have tried to provide a basic foundation in the tradition of clinical research, and we have invited you to step inside and find your own voice within it.

QUESTIONS FOR REFLECTION

1. Throughout this book, we have advocated methodological pluralism. Do you think that it is possible to carry this too far, or that such an approach might ask too much of fledgling researchers? What reasonable alternatives to such an approach might there be?
2. Find a study that is close to your topic of interest and apply the list of "Criteria for Evaluating Research" to it. How easy or difficult was it to do that? What did you learn?
3. Our sense of what scientific research means to us can change over time and is very individual. This chapter contains several common metaphors or images of the research process, but there are many more, such as mining/archeology, tree/plant, pushing a boulder uphill, and so on. What metaphors or images for the research process fit your experience?
4. Having finished reading this book (or just this chapter), what questions are you left with? What have we left out or not given enough attention to? What else would you have liked to have heard about? (Please consider emailing us to let us know your thoughts.)

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Author Index

Abraham, S.C.S., 200
Acarturk, C., 41
Adair, J.G., 149
Adams, M., 169
Addison, R.B., 10, 34, 87
Adorno, T.W., 116
Ahulwalia, M.K., 90
Ainsworth, M.D.S., 120, 129
Albury, D., 18
Alcock, C., 90
Allport, G.W., 26, 127, 136, 165
Alves, P.C.G., 173, 214
American Psychological Association, 20, 48, 196, 229, 238–9
Anderson, R.J., 10
Angel, E., 77
Angrosino, M., 122, 136
Ansaldo, F., 93
Antaki, C., 87, 115
Armistead, N., 145
Arnett, J.J., 182
Attkisson, C.C., 61, 201
Auld, F. Jr, 220
Avdi, E., 88
Ayers, S., 38

Baer, B.A., 173
Baird, S.C., 176
Bakan, D., 228
Bakeman, R., 132, 135–6
Barbour, R.S., 91
Barker, C., 84, 91, 93–4, 103, 133, 210, 214, 223
Barker, L.S., 120
Barker, R., 120, 132
Barkham, M., 23, 89, 94, 127, 151, 173, 177, 200–202, 213, 216, 232
Barlow, D.H., 20, 163–5, 168, 170, 176
Baron, R.M., 142
Barrios, B.A., 135
Barron, K.E., 142
Barry, D., 118
Bartlett, J., 9
Bass, D., 155, 183
Battle, C.C., 173
Bauman, S., 225
Baumann, E., 93
Baumeister, R.F., 120
Bebbington, P.E., 207, 210
Beck, A.T., 143, 164
Beidel, D.C., 69
Belenky, M.F., 102
Bellack, A.S., 129
Benjamin, A., 200–201
Benjamin, L.T., 28
Bennun, I., 168
Berger, P.L., 18, 82

Research Methods in Clinical Psychology: An Introduction for Students and Practitioners, Third Edition. Chris Barker, Nancy Pistrang, and Robert Elliott.

Companion Website: www.wiley.com/go/barker

Bergin, A.E., 163
Bernstein, I.H., 60, 63, 64, 70–71, 113–114, 116
Bersoff, D.M., 188–9, 194–6
Bersoff, D.N., 188–9, 194–6
Bhaskar, R., 10
Biernacki, P., 186
Billings, J., 33
Birnbaum, M.H., 187
Bishop, D., 49
Black, N., 40, 158
Blackburn, I.M., 133, 175
Blehar, M.C., 120
Blythe, R., 75
Bogdan, R., 45–6, 49, 88, 95, 101–2, 122–6, 136, 219
Bolger, A.E., 80
Bond, T.G., 59, 69, 114–15
Boot, W.R., 155
Borckardt, J.J., 175
Borenstein, M., 40
Borkman, T.J., 31, 43
Bourke-Taylor, H.M., 114
Bower, P., 156
Boyatzis, R.E., 87
Bradburn, N.M., 98, 109, 111, 116, 119
Bradley, C., 156, 192
Braun, V., 87, 95, 218, 234, 241
Braunholtz, D., 185
Breuer, J., 76, 168
Brewer, L., 86
Brewin, C.R., 156, 192
Bricmont, J., 83
Brinkman, S., 97–8, 102, 107
British Psychological Society, 48, 196, 239
Brock, D., 133
Bromley, D.B., 165
Brooks, N., 44
Brown, D.R., 144
Brown, G.K., 143
Brown, G.W., 101, 139, 161
Bruner, J.S., 11, 87, 95
Brunetti, D.G., 117
Bryant, C.G.A., 55
Bryman, A., 55, 57, 74, 95
Buchner, A., 184
Budd, E.C., 116
Burns, S.B., 230
Burns, T., 90
Butcher, J.N., 109, 119
Butts, M.M., 70

Camic, P.M., 90, 91, 93–4, 241
Campbell, D.T., 6, 11, 36, 45, 64, 67–8, 72, 93, 133, 137, 140–141, 144–9, 153, 159–61, 166, 174, 180, 185, 196, 212, 227, 234, 247
Campbell, N.C, 159
Cape, J., 200–202, 214, 216
Card, N.A., 225
Carlson, R., 2, 102
Castellan, N.J., 227, 241
Causey, A., 90
Ceci, S.J., 194
Centre for Reviews and Dissemination, 39
Chadwick, P.D.J., 168
Chalmers, A.F., 13, 17–8, 28
Chambless, D.L., 158, 176
Chapman, J.P., 152
Charmaz, K., 86
Chiu, W.T., 206
Choi, K., 148
Christensen, L.B., 160
Christian, L.M., 98
Chui, H., 93
Churchill, S.D., 96
Clark, D., 215
Clark, D.M., 151
Clarke, V., 87, 95, 218, 234, 241
Clinchy, B.M., 102
Cochran, S.D., 33
Cochrane Library, 39, 158
Cohen, J., 59, 63, 183–4, 196, 228–30
Cohen, R.L., 103
Comer, J.S., 157–8, 160
Compass Project, 206
Cone, J.D., 135–6
Connell, J., 151
Connelly, B.S., 97
Conrad, E., 103
Cook, T.D., 6, 11, 36, 45, 64, 137, 140–141, 144–9, 153, 159–61, 166, 174, 180, 185, 196, 199, 212, 227, 234, 247
Cooper, H., 40, 49
Corbin, J., 31, 85, 186–7, 196, 218, 242
Costello, C.G., 164
Cowan, P., 188–90, 197

Cowen, E.L., 45–7, 199
Cowles, M., 229
Cox, D.R., 214
Crawford, J., 164
Creswell, J.W., 35, 93, 218, 241
Crombie, I.K., 200, 202
Cronbach, L.J., 57, 67, 72, 93, 138, 145, 170, 185
Crowne, D.P., 116–17
Cuijpers, P., 41
Cumming, G., 229

Daviaud, E., 207
Davidson, P.O., 164
Davies, H.T.O., 200
Davies, S., 210
Davis, C., 229
Davis, M.I., 102
Davison, G.C., 170, 193–4
Dawis, R.V., 109, 119
de Graaf, R., 40
de Vellis, R.F., 109, 119
Demler, O., 206
Department of Health and Human Services, 188
Derogatis, L.R., 64, 173
Dienes, Z., 7, 185, 229
Dilke, O.A.W., 55
Dillman, D.A., 98, 109, 111, 117–19, 182, 187
Dimidjian, S., 143–4, 148, 153–4, 156, 161, 236
Dixon-Woods, M., 41
Donabedian, A., 200, 209
Dooley, D., 106
Douglas, B., 38
Downs, S.H., 40, 158
Driver-Linn, E., 2, 16
Dryden, W., 38
Dukes, W.F., 163, 165, 170, 186
Duncan, B.L., 155
Dunnington, R.E., 132
Dyer, J.F.P., 59

Eagleton, T., 95
Edwards, A.L., 117
Edwards, L.M., 219
Edwards, S.J.L., 185
Ehlers, A., 212
Elkin, I., 123
Elliott, R., 6, 8, 10, 18–19, 21, 26, 33, 59, 69, 91–2, 94, 114, 118, 131–2, 141, 154, 157, 159, 164, 172–5, 177, 220, 224, 229
Emerson, R.M., 90, 122–4, 136
Endicott, J., 59, 133
Eppler, M.J., 224
Erdfelder, E., 184
Erhart, D., 132
Ericsson, K.A., 98
Erikson, E., 127
Evans, C., 99
Evans, E.M., 118
Evans, J.J., 164
Evans, R., 33
Eysenck, H.J., 16, 117
Eysenck, S.B.G., 117

Fairburn, C.G., 151
Fanshel, D., 126, 128
Farquhar, J.W., 151
Faul, F., 184
Feit, A., 98
Feldman, G.C., 174
Fenton Lewis, A., 200
Fewtrell, W.D., 130
Feyerabend, P., 5, 17, 27, 244
Feynman, R.P., 233–4
Fidell, L.S., 64, 140, 225–7, 241
Field, A., 144, 153, 160, 227, 241
Finch, S., 229
Fine, M.A., 239
Firth, J., 94
Firth-Cozens, J., 201, 218
Fischer, C.T., 10, 78, 91, 224
Fisher, K., 133
Fiske, D.W., 67–8, 72, 93, 239
Fiske, S.T., 8, 97–8
Fleiss, J.L., 59, 63, 135
Flick, S.N., 157
Florey, C. du V., 200
Flowers, P., 77
Floyd, F.A., 64, 226
Fogg, L., 239
Fonagy, P., 175, 177
Fox, C.M., 59, 69, 114–15
Frazier, P.A., 142
Freeman, H.E., 149

Freire, E., 157
Frenkel-Brunswick, E., 116
Fretz, R.I., 124
Freud, S., 76, 122
Funder, D.C., 120

Gabbay, J., 207–8
Gallhofer, I.N., 109, 111, 113, 119
Garfinkel, H., 78
Gast, D.L., 164
Gaynor, S.T., 176
Geertz, C., 74, 94
Georgaca, E., 80, 88
Gergen, K.J., 2, 11, 18, 80, 83
Gessler, S., 103
Gesten, E., 45–7
Ghannam, J., 59
Gillaspy, J.A., 175
Giorgi, A.P., 77, 86, 102
Giorgi, B.M., 86, 102
Glaser, B.G., 85
Glass, D.V., 16, 40
Glass, R.I., 240
Glenton, C., 93
Gleser, G.C., 67, 84
Gloster, K., 127
Godin, G., 53
Goffman, E., 76, 123, 136
Goldberger, N.R., 102
Goldfried, M.R., 56, 129
Golombok, S., 18, 71
Goncalves, M.M., 88
Goodman, G., 47, 106
Gorham, D.R., 133
Gosling, S.D., 187
Gottman, J.M., 134, 175
Gottschalk, L.A., 84
Gough, B., 75, 82
Grace, R.C., 53
Graham, S., 182
Green, R.S., 201
Green, S.B., 113–14
Greenberg, L.S., 129, 136, 157, 174
Greenhalgh, T., 40
Greenson, R.R., 79
Grimby, G., 115
Grob, G.N., 210
Guba, E.G., 11, 74, 80, 95
Guerin, D., 152
Gurd, J., 38
Gynther, M.D., 113

Haaga, D.A.F., 157
Hafez, H., 105
Haig, B., 16
Hamilton, M., 143
Hamlyn, D.W., 10
Hannah, M.T., 154
Hardin, L. (aka Moondog), 17
Harding, C.M., 105
Hardy, G.E., 23, 46–7
Hargreaves, W.A., 61
Haridi, C., 91
Harper, D.J., 80–81, 128
Harris, T., 101, 139, 161
Hart, E., 136
Hartmann, D.B., 135
Hartnoll, R., 207
Hatcher, R.L., 175
Hathaway, S.R., 117
Hayes, A.M., 174
Hayes, S.C., 20–21, 24, 26, 28, 129, 162, 166, 168–9, 171, 173, 176, 185, 199
Haynes, R.B., 22
Haynes, S.N., 71, 129, 136, 141, 165, 182, 196
Heath, C., 90
Hedges, L.V., 40
Heiby, E.M., 129
Heine, S.J., 183
Helman, C.G., 76
Henggler, S.W., 8
Hengst, J.A., 122
Henker, B., 132
Henrich, J., 183
Henwood, K.L., 85
Hepburn, A., 89
Hersen, M., 129, 166
Hess, S.A., 219
Higgins, J.P.T., 40
Hill, C.E., 86, 93, 135, 219, 242
Hinshaw, S.P., 132
Hoaglin, D.C., 226
Hobbs, M., 212
Hodges, L., 230
Hodgson, R., 42, 49, 247
Hofman, K.J., 240

Hogan, R., 65
Hollon, S.D., 158
Hollrah, J.L., 117
Honderich, T., 28
Horne, D. J. de L., 115
Horowitz, L.M., 173
Horowitz, M.J., 32, 35
Hoshmand, L.T., 24
Howard, K.I., 157, 202, 215
Howell, D.C., 33, 59, 144, 153, 160, 227–8, 241
Hoyle, R.H., 140
Hsu, L.M., 157
Hubble, M.A., 155
Huberman, A.M., 100
Hufford, M.R., 98, 134
Hughes, C., 103
Hughes, J.A., 10
Humphreys, K., 20, 88, 215, 240
Humphries, L., 125
Hunsley, J.D., 77
Hunter, J.E., 40, 149, 159

Imber, S.D., 188, 191
Iwata, B.A., 134

Jackson, J., 185
Jacobson, N.S., 230–231, 242
Jahoda, M., 8
James, W., 2, 127
Jason, L.A., 102, 179
Jay, Z., 103
Jennings, J.L., 77
Joffe, H., 84, 130
John, O.P., 187
Johnson, B.A., 11, 55, 57, 74
Johnson, R.B., 160
Jones, E.E., 59, 97
Jordan, A., 80
Josselson, R., 98, 102, 119

Kahneman, D., 8
Kahng, S.W., 134
Kamin, S.J., 18
Karpiak, 19
Kazdin, A.E., 22, 155, 162, 166, 171, 174, 176, 183
Kelly, G.A., 8, 81, 101
Kendall, P.C., 157–8, 160, 197
Kennedy, P., 133
Kenny, D.A., 142
Kent, R.N., 56, 129
Keppel, G., 154
Kessler, R.C., 206–7
Keys, C.B., 102
Kiesler, D.J., 163, 173
Kimble, G.A., 2
King, M., 156
Kiresuk, T.J., 173
Kirschenbaum, H., 129, 233
Kischka, U., 38
Kitzinger, J., 102
Klein, M.J., 94
Knapp, M., 215
Knoblauch, M., 90
Knox, S., 219
Koch, S., 57
Kopta, M., 215
Korchin, S.J., 188–90, 197
Korman, M., 24
Korotisch, W.J., 134
Kraemer, H.C., 70, 184, 231, 242
Kraus, M., 215
Kraut, R., 187
Kreppner, J.M., 150
Krippendorff, K., 84, 130
Kroenke, K., 59, 99, 147, 172, 212
Krupnik, J.L., 214–15
Kuhn, T.S., 5, 16–7, 27–8
Kupfer, L., 240
Kurcias, J., 127
Kurdek, L.A., 239
Kurz, D.E., 127
Kvale, S., 97–8, 102, 107, 119, 247

Labov, W., 126, 128
Laing, R.D., 77
Lakatos, I., 17
Lambert, M.J., 38, 169, 213, 231, 242
Lambie, J., 16
Lampé, F., 156
Lance, C.E., 70
Lang, A.-G., 184
Lanham, R., 237
Larkin, M., 77
Larsen, D.L., 61, 173, 213
Lather, P., 89–90
Laurenceau, J.P., 174

Law, M., 115
Layard, R., 215
Lazarus, A.A., 170
Leach, C., 173, 212
Lebow, J., 213
Lee, J.H., 230
Lembcke, P.A., 200
Leudar, I., 90
Levinson, D.J., 116
Levi-Strauss, C., 82
Leviton, L.C., 199
Levy, K.N., 67
Lewin, K., 34
Lewin, S., 41, 49, 93
Lewis, J., 85
Lewis, R., 207
Lewontin, R.C., 18
Lieberman, P., 105
Lilford, R.J., 185
Linacre, J.M., 115
Lincoln, Y., 11, 74, 80, 95
Lipsey, M.W., 40–41, 49, 149
Lish, J.D., 202
Lissitz, R.W., 114
Littel, J.H., 8
Llewelyn, S.P., 101, 136, 173, 175
Loken, E., 67
Löwe, B., 99
Lowe, M.R., 9
Luborsky, L., 157
Lucas, R., 168
Luckmann, T., 18, 82
Luria, A.R., 164
Lutz, W., 173, 212
Lykken, D.T., 228, 235
Lynham, S.A., 74
Lyons, A.C., 89
Lyons, J.S., 202, 214
Lyotard, J.-F., 82

MacKinnon, D.P., 152
MacLeod, C., 121
Madill, A., 75, 80, 82, 89, 94, 127, 223
Madon, T., 240
Mahrer, A.R., 33
Malinowski, B., 122
Mangen, S., 214
Manolov, R., 164
Marlowe, D., 116–17
Marmot, M., 25
Marsden, P.V., 109, 119
Marshall, J., 38
Matthews, A., 121
Mattis, J.S., 90
Mattison, V., 102–4, 224
Maxwell, R.J., 200, 209
May, R., 77
Mayou, R.A., 212
Mayraz, G., 215
Mays, N., 41, 85, 91, 224
Mays, V., 33
McCabe, R., 90
McCormack, H.M., 115
McFall, R.M., 22, 24, 28
McGlinchy, J.B., 230
McGrath, J.E., 11, 55, 57, 74
McGuire, W.J., 145
McKillip, J., 207
McKinley, J.C., 117
McLeod, J., 82, 87–8, 94, 105, 172–3, 177
McMillan, D., 176
McSherry, R., 202
Meehl, P., 57, 66, 67, 72, 73, 145, 186, 229
Mellor-Clark, J., 23, 151
Melton, B.B., 8
Meltzoff, J., 35–6
Merbaum, M., 9
Mergenthaler, E., 220
Meyer, G.J., 98
Michels, K.M., 144
Michels, L.C., 70
Midgley, N., 93
Miles, M.B., 100, 218, 241
Milgram, S., 191
Miller, G.A., 152
Miller, P.J., 122
Miller, R.B., 19, 172, 174
Miller, S.D., 155
Milne, D., 12
Minke, K.A., 182, 196
Mintz, J., 173
Mischel, W., 129
Mitcheson, M., 207
Modle, W.J., 200
Monahan, P.O., 99

Moos, R., 215
Moran, G.S., 175, 177
Moras, K., 135
Morgan, D.L., 165, 168, 170, 176
Morgan, R.K., 165, 168, 170, 176
Morley, S., 169, 176
Morrow, S.L., 91
Morrow-Bradley, C., 8, 19, 26
Murray, H.A., 165
Murray, M., 87, 165
Musgrave, A., 17

Nanda, H., 67
Nay, W.T., 69
Nazareth, I., 156
Neimeyer, R.A., 11, 81
Nelson, L.D., 227
Nelson-Grey, R.O., 20, 134, 176
Neuendorf, L.A., 130
Newton, R.R., 49
Nguyen, T.D., 61
Nicholls, C.M., 85
Nigg, J.R., 59
Nisbett, R.E., 8, 97
Nock, M.K., 164, 166
Norcross, J.C., 19, 155
Norenzayana, A., 183
Norman, G.R., 109, 119
Novotny, C.M., 22, 159–60
Noyes, J., 41, 49
Nunnally, J.C., 60, 63–4, 70–71, 113–14, 116

Oakley, A., 102–3
O'Brien, G., 9
O'Brien, W.O., 129, 136, 141, 165
Ogles, B.M., 231, 242
O'Mahoney, M.T., 202
Ones, D.S., 97
Oppenheimer, D., 237
Orford, J., 25
Orlinsky, D.E., 215
Ormston, R., 85
Orne, M.T., 53, 149
Orwell, G., 237–8
Ostrov, J.M., 136
Otto, R.K., 38
Overall, J.E., 133
Oxman, A.D., 93

Packer, M.J., 10, 34, 87
Pallant, J.F., 115
Parry, G., 94, 172, 176–7, 200–201, 213
Pashler, H., 235
Patton, M.Q., 12, 32, 47, 93–5, 96, 102, 136, 159, 186, 196, 216, 218, 240–241
Paul, G.L., 154
Paulhus, D.L., 117
Pawson, R., 199
Payne, S.L.B., 112
Peacock, R., 40
Pearce, P., 202
Pearson, E., 133
Peirce, C.S., 16
Pell, J.P., 156
Peplau, L.A., 103
Peräkylä, A., 90
Perdices, M., 164
Perrucci, A., 87
Peters, D., 194
Peterson, D.R., 24
Phillips, J.P.N., 59, 164, 173
Phillips, J.R., 85
Piaget, J., 11
Piasecki, T.M., 98, 134
Pidgeon, N., 85
Pilling, S., 201, 214
Pincus, H.A., 214–15
Pinsof, W., 129, 174
Pistrang, N., 33, 84, 93–4, 103–4, 107–8, 136, 186, 210, 221, 223
Plotkin, J., 194
Pocock, S.J., 229
Polkinghorne, D., 24, 54, 71, 87
Pollner, M., 78
Popay, J., 41
Pope, C., 41, 49, 85, 218, 224
Popper, K.R., 5, 11, 14–15, 17, 27–8, 33, 142, 186
Potter, J., 89, 126, 136, 218, 220, 242
Price, L., 230
Priebe, S., 90
Prilleltensky, I., 82
Proctor, R.W., 28, 55, 74, 95
Pullman, P., 237

Quarturo, G.K., 85
Quera, V., 132, 135–6
Quizon, C.A., 90

Rajaratnam, N., 67
Rapley, M., 87, 115
Rapp, B., 164
Raven, R., 91
Rayner, R., 76, 177
Reason, P., 8, 102, 179
Reichenbach, H., 10
Reicher, S., 81, 91
Reise, S.P., 69
Rennie, D.L., 10, 16, 22, 85–6, 91, 93, 224, 247
Rhodes, J.E., 93
Richardson, W.S., 22
Riessman, C.K., 11, 88, 219
Riger, S., 102
Ritchie, J., 85
Roberts, L.J., 230
Robertson-Nay, R., 69
Robson, C., 186, 241
Rodgers, J.L., 228
Rogers, C.R., 2, 6, 17, 53, 57, 79, 129, 141, 164, 176–7, 243–4
Rogers, E.M., 240
Rogers, J.L., 229
Rollnick, S., 42, 49, 247
Rook, D.W., 102
Rorer, L., 116
Rose, S., 18
Rosen, A., 66
Rosenberg, S.E., 173
Rosenberg, W., 22
Rosenhan, D.L., 76, 125–6, 189
Rosenthal, R., 149, 225
Rosnow, R.L., 149
Ross, L., 8, 97
Rossi, P.H., 149, 152, 156, 159, 199, 202–3, 209, 214, 216
Rothbaum, B.O., 230
Rothstein, H.R., 40
Rowan, J., 8, 102, 179
Rowley, G.L., 67
Roy, A.K., 134, 174
Rozin, P., 233, 246
Rubin, H.J., 102, 119
Rubin, I.S., 102, 119
Rudestam, K.E., 49
Runyan, W.N., 128, 171
Russell, B., 9
Rust, J., 18, 71
Rutter, M., 150

Sackett, D.L., 22
Sacks, H., 89
Sacks, O., 164
Saldaña, J., 100
Sales, C.M.D., 173, 214
Sanford, R.N., 116
Santoro, S.O., 19
Sarbin, T.R., 2, 87
Saris, W.E., 11, 109, 113, 119
Savage, J., 90
Scarr, S., 191
Schegloff, E.A., 90
Schlottman, R.S., 117
Schlumer, G.L., 225
Schmidt, F.L., 40, 149, 159
Schoenrock, C.J., 116
Schoggen, M.F., 120
Schön, D.A., 19
Schwartz, J., 18, 57, 111, 127
Schwartz, N., 113
Schwartz, R.D., 133
Scott, A.B., 117
Scriven, M., 200
Sears, D.O., 145
Sechrest, L., 133
Seligman, M.E.P., 150, 158, 160, 213, 216
Seltzer, M.H., 148
Severns, J., 87
Shadish, W.R., 45, 46, 49, 141, 146–7, 150–151, 153, 157, 159–60, 199, 213, 216, 228, 234, 240, 242
Shallice, T., 163–4, 186
Shapiro, D.A., 94, 145–6, 154, 211
Shapiro, M.B., 21, 59, 164, 173
Sharrock, W.W., 10
Shavelson, R.J., 67
Shaw, I., 211
Shaw, L.L., 124
Sheather, S., 115
Sheatsley, P.B., 101, 111
Sherman, R.E., 173
Shiffman, S., 134
Shimokawa, K., 169, 173, 214
Shirley, C., 90
Shoham-Salomon, V., 154
Shrout, P.E., 63, 135

Sibbald, B., 156
Sidman, M., 185, 196, 235
Sieber, J.E., 195
Siegel, S., 227, 241
Sigelman, C.K., 116
Silberschatz, G., 161
Simmons, J.P., 227, 235
Simon, H.S., 98
Simonds, L.M., 90
Simons, D.J., 155
Simonsohn, U., 227
Skinner, B.F., 15, 164
Slatich, E., 173
Slife, B.D., 9, 28, 90, 140
Smart, D.W., 169
Smith, G.S., 156
Smith, G.T., 66–7, 71, 140
Smith, J.A., 77, 87, 93–4, 218, 242
Smith, J.D., 165, 169, 176
Smith, L.A., 8
Smith, M.L., 16, 40
Smith, S., 230
Smyth, J.D., 98
Snow, R.E., 154
Sokal, A., 83
Solhan, M., 98, 134
Solomon, M., 223
Spankel, C.L., 116
Spence, D.P., 171
Spencer, L., 85
Spinelli, E., 77, 79
Spitzer, R.L., 59, 99
Sprague, J., 89
Srivastava, S., 187
Stanley, B., 195
Stanley, J.C., 144, 146
Sternberg, J.C., 238
Sternberg, K., 238
Stevens, A., 207–8
Stevens, S.S., 53, 58
Stewart, D.W., 102
Stewart, I., 93
Stewart, R.E., 176
Stiles, W.B., 16, 23, 88, 91, 148, 151, 154, 157, 161, 175, 219–20, 224
Stinson, C.H., 220
Stone, A.A., 134
Stothart, C., 155
Strauss, A., 31, 85, 186–7, 196, 218, 242
Strauss, J.S., 105
Strauss, M.E., 66–7
Strawbridge, S., 38
Streiner, D.L., 109, 119
Stricker, G., 21
Strong, D.R., 69
Strunk, W., 238
Stuart, R.B., 193–4
Stutts, C., 155
Suarez-Balcazar, Y., 102
Sudman, S., 98, 182, 196
Sudnow, D., 127
Suzuki, L., 90
Svartberg, M., 148

Tabachnik, B.G., 64, 140, 225–7, 241
Taha, H.A., 219
Target, M., 93
Tarule, J.M., 102
Tashakkori, C., 11, 93
Taylor, R.R., 102
Taylor, S., 89
Taylor, S.E., 8, 97–8
Taylor, S.J., 45–6, 49, 88, 95, 101–2, 122–6, 136, 219
Teddlie, C., 11, 93
ten Have, P., 90, 220
Thiemann, S., 184
Thompson, B., 184, 229
Thompson, B.J., 86
Thompson-Brenner, H., 22, 159–60
Thornée, R., 115
Thornicroft, G., 207
Thornton, H.M., 189
Tilley, N., 199
Timulak, L., 41, 157
Tix, A.P., 142
Todd, D.M., 127
Tomarken, A.J., 140
Toms, D.A., 130
Tourangeau, R., 98
Trierweiler, S.J., 21
Trivedi, P., 43
Trower, P., 168
Truax, P., 242, 320–331
Trull, T.J., 98, 134
Tryon, W.W., 28
Tukey, J.W., 226–7

Turner, L.A., 160
Turner, S.M., 69

Ureno, G., 173
Urman, M., 173

Vacha-Haase, T., 184, 229
Valentine, J.C., 40
van Straten, A., 41
Varvin, S., 219
Vazire, S., 187
Vehviläinen, S., 90
Velleman, P.F., 226
Villasenor, V.S., 173
Vohs, K.D., 120
Vygotsky, L.S., 11

Wagenmakers, E.J., 7, 235
Waldorf, D., 186
Wall, S., 120
Waller, N.G., 69, 140
Walsh, R., 87
Walters, E.E., 206
Wampold, B.E., 155
Wang, P.S., 207, 210
Wang, S., 122, 187
Wansink, B., 98
Wasserman, R.H., 67
Waters, E., 120
Watson, J.B., 56, 76, 113, 157, 177
Webb, E.J., 133
Webb, N.M., 67
Weiner, D.N., 65, 117
Weiner, I.B., 38
Weiss, C.H., 46, 49, 199, 203, 216, 240, 242
Wertz, F.J., 102, 222
Wessely, S., 151
West, S.G., 157
Westen, D., 22, 98, 121, 156, 159–60
Wetherell, M., 89, 127, 136, 218, 220, 242
Wexler, M.M., 175
Whalen, C.K., 132
White, A.M., 220
White, E.B., 238
White, P.A., 140
Whyte, W.F., 76, 123, 136
Whyte, W.H., 116
Wickens, T.D., 154
Widaman, K.F., 64, 226
Wilkinson, S., 102
Williams, J.B., 59, 99
Williams, J.M., 238
Williams, J.M.G., 9, 90, 121
Williams, L.N., 86
Williams, R.N., 28, 140
Willig, C., 53, 80, 84–5, 93–4, 241
Willmott, P., 103, 107
Willott, S.A., 89
Wilson, B., 164
Wilson, D.B., 40–41, 49
Wilson, S., 15
Wilson, T.D., 97
Winer, B.J., 144, 153, 160, 227, 241
Winter, D.A., 59, 101
Wittgenstein, L., 56
Wood, D.D., 135
Woolfe, R., 38
Wright, B.D., 115
Wright, H.F., 120
Wright, J.D., 109, 119
Wright, K.B., 187
Wykes, T., 43

Yalom, I.D., 2, 123
Yardley, L., 84, 91, 93, 130, 224
Yates, S.J., 89
Yin, R.K., 177
Yong, E., 235
Young, H.H., 200–201
Young, M., 103, 107
Young, R.M., 57

Zeldin, T., 127
Ziebland, S., 85
Zimbardo, P.G., 191
Zittel, C., 98
Zuccato, E., 133

Subject Index

AB design, 166–7
ABAB design, 167–8
abduction, 16, 222
access to settings, 45–7, 124
acquiescence response set, 116
action research, 12, 102, 179
aims and objectives, 204
analysis, 4, 217–32, 245
  cross-case, 219
  of qualitative data, 218–24
  of quantitative data, 224–32
  in single-case designs, 169
anchoring of rating scales, 115–16
applied behavior analysis, 164
applied research, 12, 199
applied scientist model, 20–24, 27
appraisal tools, 40, 158, 244–6
appropriate methods, 3, 32, 243
aptitude-treatment interaction, 154
attrition, 157
audit, 200–201
authorship, determining, 48, 239

baseline measures, 165–6
Bayesian statistics, 7, 185, 229
behavioral assessment, 56, 129
behavioral observation, 54, 56, 129–36
benchmarking, 23, 212
bias
  attributional, 97–8
  in information processing, 7–8
  observer, 126
  in questionnaire items, 111–12
  in sampling, 182–3
  in service delivery, 210–211
Boulder model, 20, 24
bracketing, 34, 78–9, 222

case study
  narrative, 19, 23, 76, 164, 170–171
  systematic, 171–7
case tracking, 23, 173, 213
case-control study, 151, 163
causality, 36, 132, 138–152, 165–6, 171, 173, 175, 205. *see also* validity, internal
census, 99
central tendency, 115
changing criterion design, 169
checklists, 54, 99
classical test theory, 60
client satisfaction research, 61, 97, 139, 213
clinical audit, 200–202
clinical replication series, 168–9, 173
clinical scientist, 22–4
clinical significance, 184, 230–232

Research Methods in Clinical Psychology: An Introduction for Students and Practitioners, Third Edition. Chris Barker, Nancy Pistrang, and Robert Elliott.

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clinical trials, 93, 143–4, 152–61, 191–2
Cohen's kappa, 63, 135
coherence theory of truth, 10–11
comparison group. *see* control group
conceptual confound, 142
conceptual research, 9
confidentiality, 99, 192–3
confirmatory research, 2, 10, 33, 227
confounding variables, 148–9
consensual qualitative research, 86
consensus criterion of truth, 10–11
CONSORT statement, 158
constant comparative method, 223
construct, 52–7
construct validity, 57, 67–8, 145, 148–9
constructionism, 10–12, 97–83
constructivism, 11, 81
consumer satisfaction research, 61, 97, 139, 213
content analysis, 84–5, 130
control group, 144, 151, 154–6
  comparative treatment, 155–6
  ethical issues, 192
  expectancy and relationship, 155
  no-treatment, 155
  placebo, 155
  treatment as usual, 155, 192
  wait-list, 155
conversation analysis, 89–90, 116
correlation and causation, 140–142
correlational designs, 139–42
correlations, spurious, 141, 227
correspondence theory of truth, 10–11, 66
cost effectiveness
  evaluation of, 214–5
cost offset, 215
coverage, 210–211
creaming, 210
credibility checks, 91
criterion-referenced measures, 59
critical approaches, 89, 222
critical realism, 11, 53
Cronbach's alpha, 61–4, 135
cross-sectional designs, 140

data
  analysis (*see* analysis)
  checking, 225
  entry, 224–5
  exploration, 226–7
  reduction, 225–6
debriefing, 190
deception, 190
deconstructionist approach, 90
deduction, 14–15
demand characteristics, 53, 149
demand for services, 207–8
describing, 79
descriptive designs, 138–9
design, 4, 137–77, 162–77, 245
  analysis of covariance, 154
  blocking, 154
  Cook and Campbell's classification of, 146–53
  correlational, 139–42
  cross-sectional, 140
  descriptive, 138–9
  dismantling, 156
  experimental, 138, 142–61, 165–70
  factors, 153–4
  interrupted time series, 152
  longitudinal, 140
  multi-factorial, 154
  nonequivalent groups posttest-only, 149–50
  nonequivalent groups pretest-posttest, 151–2
  nonexperimental, 138–42
  nonrandomized, 146–52
  one-group posttest-only, 146
  one-group pretest-posttest, 147–9
  quasi-experimental, 146–52
  randomized, 152–61
  randomized groups pretest-posttest, 153–61
  repeated measures, 154
  single case, 162–77
  small–N, 162–77
  stratification, 154
  time-series, 152, 169, 175
discourse analysis, 89, 126–8
discovery-oriented research, 2, 33–4, 75, 93, 123, 227
dismantling study, 156
dissemination of the findings, 4, 239–44
double-blind study, 155, 158
Duquesne phenomenological approach, 86–7
duration recording, 133

ecological validity, 145
effect size, 41, 184–5, 229–30
efficacy *vs.* effectiveness, 153, 157, 212
empirically supported treatments, 18, 22, 159
empiricism, 2, 9, 55
endogenous change, 147
endstate functioning, 231
environmental measures, 133
epistemology, 10–11, 54, 74, 76
equivalence analysis, 156, 229
ethical issues, 26, 188–97
  in observation, 125–6
  in randomized designs, 155–5, 191–2
  in single-case experiments, 167
ethics committees, 46, 194–7
ethnography, 76, 90–91, 122
  focused, 90–91
evaluation, 12, 198–216
  aims and objectives in, 204
  of cost effectiveness, 214–15
  formative, 200
  of outcome, 200, 212–15
  of process, 200, 209–11
  of service implementation, 211
  structure, 200
  summative, 200
event recording, 132
evidence-based practice, 18, 22–4
expectancy effects, 149
experiential knowledge, 31, 43
experimental designs, 138, 142–61, 165–70
exploratory data analysis, 226–7
exploratory research, 2, 10, 33–4, 75
external validity, 23, 145, 169–170, 180–181, 234–5

factor analysis, 64, 110, 226
falsification, Popper's view of, 14–16, 186
feminist research, 26, 89, 102–3, 222
field notes, 46, 124–5
focus groups, 102
framework approach, 85
funding, 44–5

gatekeepers, 45–6, 124
generalizability theory, 67–8
generalization. *see* validity, external
gold standard in criterion validity, 66
grants, 44–5
grounded theory, 85–6, 123, 186, 218, 223
groundwork stage, 4, 29–49, 179, 245

harms and benefits of research, 191–3
Hawthorne effect, 149
hermeneutic approaches, 77, 87
hypotheses, 15, 32–4
hypothetico-deductive approach, 2, 15, 33, 55, 78, 143

idiographic approach, 59, 163–5, 219
impact model, 205
implementation of research, 240
implications of findings, 235–6
incidence, 139, 206
induction, 13–16
inferences, 55–6
informant, 97, 102, 179
  key, 123–4, 207
informed consent, 126, 189–97
institutional review boards, 194–5
intent-to-treat analysis, 157
interfering events, 148
internal consistency, 61, 63–4, 68, 110
internal validity, 23, 144–53, 172, 174, 213, 234
internet questionnaires, 118, 182, 187
interpretation of the findings, 4, 232–6, 245
interpretative phenomenological analysis, 77, 87
interrupted time-series design, 152
interval recording, 132
interval scales, 58–9, 62–3
interview, 99–109
  qualitative, 99–109
  schedule, 103–4
  semistructured, 102, 107
  skills, 105–9
intraclass correlation, 63, 135
introspection, 56
intuitive practitioner, 19
inventories, 99
item analysis, 226
item response theory, 68–9, 114–15
item wording, 111–13

language-based approaches, 88–91
leading questions, 105, 111
leakage, 157

life history research, 88
Likert scales, 113–16
limitations of the study, 233–4
literature review, 37–41
local clinical scientist, 21–4
locally intensive observation, 170, 185
logical positivism, 56
longitudinal designs, 140

manualization, 158
maturational trends, 148
meaning unit, 223
measurement, 4, 36–7, 50–72, 245
  combining qualitative and quantitative, 92–4, 118
  facets of, 68
  reliability of (*see* reliability)
  utility of, 69–70
  validity of (*see* validity)
measures
  of change processes, 174–5
  of client change, 172–4
  development of, 109–19
  repeated, 154
  of therapeutic process, 174–5
mediating variables, 142
meta-analysis, 16, 40–41
meta-synthesis, 41
method variance, 67, 110
methodological behaviorism, 56–7, 83, 129
methodological pluralism, 3, 17, 92–3, 176, 243–4
mixed methods research, 93–4
moderator variables, 142
multiple baseline design, 168
multiple single-case design, 168
multisystemic therapy, 8
multitrait-multimethod matrix, 67

naive enquiry, 8
narrative
  approaches, 11, 87–8
  case study, 123, 164, 170–171
  distortions, 172
  recording, 132
  smoothing, 171
natural experiments, 150
naturalistic case study designs, 170–171
needs assessment, 207–8

new paradigm research, 102, 179
nominal scales, 58–9, 62–3, 74, 135
nomothetic approach, 59, 163, 165
nonequivalent groups posttest-only design, 149–50
nonequivalent groups pretest-posttest design, 151–2
nonexperimental designs, 138–42
non-inferiority trial, 156, 229
nonrandomized designs, 146–52
nonspecific factors, 155
normal science, 16–17
norm-referenced measures, 59

observation, 54, 120–136
  advantages and disadvantages of, 121–2
  covert, 121, 125–6
  duration recording, 133
  environmental measures, 133
  ethical issues in, 125–6
  event recording, 132
  global rating scales, 133
  interval recording, 132
  narrative recording, 132
  partial interval sampling, 132
  participant, 76, 122–6
  qualitative, 122–8
  quantitative, 128–36
  reliability of, 126, 134
  sequential act coding, 133
  time sampling, 132
  validity of, 126, 134–5
  whole interval sampling, 132
observer drift, 134
one-group posttest-only design, 146
one-group pretest-posttest design, 147–9
open clinical trial, 144, 147
operant behavioral approach, 15, 164
operational definition, 53, 130–132
operational policy document, 208
operationalization, 52–3
ordinal scales, 58–9, 62–3
outcomes management, 213–4

paradigms, 16–17
parameter, 180
partial interval sampling, 132
participant observation, 76, 122–6
participants, 178–97

passive observation studies, 121, 139
path analysis, 140
patient-focused research, 213–4
performance indicators, 57
personal constructs, 8, 81
personal documents, 101, 127
personal questionnaire, 21, 59, 164, 172–3
personal reasons for doing research, 24–6
personal reasons for not doing research, 26–7
phenomenology, 76–9, 97, 139
  empirical, 86–7
philosophical issues
  in defining research and science, 6–18
  in qualitative research, 76–83
  in quantitative measurement, 55–7
pilot study, 44
placebo effects, 144, 149, 153–5, 192
politics of research, 17–18, 45–7, 201–3
population at risk, 206
positivism, 26, 55–7, 74, 80, 143
postmodernism, 11, 82–3
poststructuralism, 82
power, statistical, 183–5, 228
practice-based evidence, 23, 246
practitioner-scholar model, 24
pragmatist criterion of truth, 10
prevalence, 139, 206
privacy, 192–3
professional models of research, 18–24
psychometric theory, 58–72
psychotherapy outcome research, 93, 146, 153, 230
psychotherapy process research, 129, 174–5
publication, 48, 200, 238–9
pure research, 12, 199

qualitative analysis, 218–24
qualitative research, 2, 54, 73–95, 101–9, 122–8, 219–26
  categories in, 85, 221–4
  evaluating, 91–2, 224
  overload in, 100, 109, 218
quality assurance, 200–202
quantitative methods, 2, 50–72, 109–11, 128–136, 224–32
quasi-experimental designs, 146–52
questionnaires, 99, 110–119
questions, in self-report methods, 100–113

random assignment, 146, 152, 156–7, 192
randomized controlled (or clinical) trials, 93, 143–4, 152–61, 191–2
randomized design, 152–61
randomized groups pretest-posttest design, 153–6
Rasch analysis, 69
raters, working with, 134–5
rating scales, 113–17
rationalism, 2, 9–10, 77
reactivity of measurement, 53, 121, 126, 134, 148
realism, 10–12, 54, 80–81, 84
reflective practitioner, 19
reflexivity, 81–2
refutation, 14–16
regression to the mean, 148
reliability, 10, 60–64, 68, 110, 114–15
  equivalent forms, 61, 68
  internal consistency, 61, 68
  inter-rater, 62, 68
  in participant observation, 126
  in qualitative research, 91, 126
  in quantitative observation, 134–5
  split-half, 61
  standards for, 70–71
  statistics for, 62–4
  test-retest, 61, 68
reliable change, 230–231
replication, 11, 76, 158, 167–70, 181, 185–7, 235
research
  appraisal tools, 40, 158, 244–6
  combining with practice, 18–28, 162–3, 246–7
  definition of, 6–12
  design (*see* design)
  evolutionary roots of, 7, 247
  journal, 30–31, 46, 106
  process, 3–4
  proposal, 41–4
  protocol, 4, 44, 75, 98, 195
  questions, 30–45, 51–2
    confirmatory, 33–4
    discovery-oriented, 33–4
    exploratory, 33–4
  sociopolitical aspects of, 17–18, 45–7, 201–3
  as story, 1–2, 247

research (*cont'd*)
threatening aspects of, 46–7, 201–2
topic, 30–31
tradition, 247
utilization of, 240
researcher allegiance, 8, 41, 156–7
response modes, 106–7
response scales, 113–18
response sets, 116–17

sample
achieved, 179
convenience, 182, 187
definition of, 179
heterogeneous, 181
homogeneous, 181
intended, 179
random, 187
size, 183–5
unbiased, 182
sampling, 117–18, 178–87
alternative approaches to, 185–7
internet, 187
methods, 179–87
networking, 186
purposive, 186
snowballing, 186
surveys, 117–18
theoretical, 186–7
saturation, 187, 223
scales of measurement, 58–9
interval, 58–9, 63–4
nominal, 58–9, 62
ordinal, 58–9, 63–4
science, definition of, 12–18
scientific dishonesty, 7, 235
scientific revolutions, 16–7
scientist-practitioner model, 20–4, 246
secular drift, 148
selection bias, 152
self-monitoring, 134, 165
self-report methods, 54, 96–119
advantages and disadvantages of, 97–9
qualitative, 109–9
quantitative, 109–19
semantic differential scales, 116
sensitivity, 66
sequential act coding, 133
service-user consultation, 43
single-case designs, 165–70
experimental, 21, 165–70
in neuropsychology, 164
small-N designs, 162–77
social constructionism, 11, 18, 76–7, 79–83
social desirability, 116–17
Spearman-Brown prophecy formula, 64
specificity, 66
spontaneous remission, 16, 147, 155
stakeholders, 202–3
statistic, definition of, 180
statistical conclusion validity, 144–5, 228
statistical power, 183–5, 228
statistical significance, 227–30
statistics
analysis of covariance, 151–2, 154
analysis of variance, 144, 163
Bayesian, 7, 185, 229
descriptive, 139, 225
factor analysis, 64, 110, 226
for reliability, 62–4
in single-case designs, 169
subjects, 26, 43, 102, 178–9
survey, 99
systematic reviews, 39–41

target complaints, measure of, 173
target population, 179–82, 205–8
target problem, 206–8
testimonial validity, 91
text-based research, 126–8
thematic analysis, 84–7, 221–4
theory, role of, 34
theory-dependence of observation, 13–14, 53, 83
time sampling, 132
time-series designs, 152, 175
transcription, 219–20
triangulation, 11, 91, 93
truth, criteria of, 10–11, 66

uncontrolled selection, 150–151
uncontrolled trials, 144, 147
uniformity myths, 163
unobtrusive measures, 133
utilitarian criterion of truth, 10
utility of measures, 69–70
utilization of research, 240

Vail model, 24
validity, 10, 50, 64–72, 110, 144–6
  concurrent, 65, 68
  construct, 57, 67–8, 145, 148–9
  content, 65, 68
  convergent, 67–8
  Cook and Campbell's analysis of, 144–6
  criterion, 65, 68
  discriminant, 67
  ecological, 145
  external, 23, 145, 169–170, 180–181, 234–5
  face, 65
  internal, 23, 144–53, 172, 174, 213, 234
  in participant observation, 126
  predictive, 65, 68
  in qualitative research, 91
  of self-report, 97–8
  standards of, 70–71
  statistical conclusion, 145, 228
  testimonial, 91
  threats to, 146–52, 166, 174, 234
variable
  confounding, 148–9
  dependent, 144
  domains of, 52
  independent, 143
  mediating, 142
  moderator, 142
  operational definition of, 53, 130–132
  outcome, 144
verification, 14–5
visual-analogue scale, 113

whole interval sampling, 132
writing style, 237–8