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Ways of Knowing

Competing Methodologies in Social and Political Research

Second Edition

Jonathon W. Moses and
Torbjørn L. Knutsen

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Preface and Acknowledgements

Nearly two decades ago, we were enlisted to collaborate on a social science methods course for incoming graduate students. Because our research interests and backgrounds were quite different at the outset, this took some doing. Indeed, this book is the remarkable product of a long-running collaboration – not only between we two authors but also between students and faculty at the Department of Sociology and Political Science at the Norwegian University of Science and Technology (NTNU). This journey we have taken together has been an enjoyable one, and we hope our pleasure and enthusiasm is evident in the pages that follow.

In the beginning, the course that inspired this text aimed to introduce students to the methods and methodology of what is generally (if somewhat polemically) called 'positivist' social science. While our students were already getting a strong introduction to statistical methods, the department felt it was necessary to provide more training for those who needed to employ other research methods. Thus the original focus of the course was on traditional philosophy of science issues, with the addition of comparative and case-study research methods.

Over the years, however, we began to realize that much of what our students were interested in did not fit very comfortably under the positivist rubric. Indeed, the term itself (positivism) began to grate on us. Even worse, because of the strongly positivist orientation of their methods education, many of our students (not to mention our colleagues!) were often misinterpreting the ways in which alternative methods were being employed by influential contemporary social scientists. In response to these challenges, we began to expand the course to include alternative approaches to social science. On doing this, we began to recognize a need to distinguish between the different ways that particular methods are employed in varying methodological contexts.

The result was *Ways of Knowing*. We have designed the book to cover and reflect on what we understand to be the two main methodological traditions in contemporary social science: naturalism (which corresponds to what we called positivism, above); and constructivism (which, as you will see, corresponds to what many people call 'interpretivism'). These two methodologies are juxtaposed with one another to emphasize the underlying differences in how scholars from each tradition see and understand the world they are studying. We then look at how particular methods are employed in different ways within each methodological tradition.

We were delighted by the success of this book's first edition and are excited about the invitation to revise it. In this second edition we have aimed to expand the discussions of the similarities between the two methodological traditions and to build stronger bridges between them. We have also added a number of new examples to illustrate the utility of this sort of bridge-building. We hope that our earlier readers are happy with the changes, and that newer readers might be attracted to a revised edition.

Given this book's long incubation period, it is difficult to acknowledge all the help and advice we have received. More than one late evening has been spent worrying that we might have forgotten to acknowledge an important source of inspiration or information. Perhaps our greatest partner has been time itself: we have benefited from being able to reflect on the experiences of the last decade, changing and refining the arguments each time we taught our course anew. As one debt often leads to another, we would like to thank our respective families for putting up with all the time we have devoted to this project.

Obviously, our approach has been greatly influenced by the critical attention of several generations of students. Indeed, many students have been subjected to rough drafts of this book as we experimented with different ways to present the material. To all of our students: thank you for your patience, help and support.

As our university has a very liberal sabbatical program, we have often found ourselves co-teaching the course with various colleagues. Through our collaboration, these colleagues have inevitably affected our thinking. As a result, Jennifer Bailey, Espen Moe and Stephen Swindle have all contributed in their own way to the final product. This second edition has also benefited from many readers who have written to us with comments on the first edition, and provided suggestions as to how it might be improved. In this regard, we would like to thank Einar Faanes, Tone Ceclie Faugli, Jo Jakobsen, Johan Modée, Alain Noël, and the anonymous referees at Palgrave.

We would be remiss if we didn't thank our commissioning editor, Steven Kennedy, who has both encouraged and badgered us about how this book should evolve. Steven is the model editor: well-informed, engaged and opinionated, with a well-trained eye for the market. He has stood by us from the start. There can be no doubt that our argument and this book have been greatly improved by Steven's careful reading and comments. It is also because of Steven that we have benefited from the very useful and detailed comments of Palgrave's anonymous referees. To all these readers, we are thankful.

Despite all the time we have taken, and the help we have received, we alone are responsible for any errors that remain. We do hope they are not many.

We close with a word of gratitude for Ola Listhaug, the patriarch of our department. Ola has been instrumental in allowing us the freedom and time to pursue these interests (and many others). It is for this reason that we have dedicated our book to him.

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Chapter 1

Introduction

> What we shall see is something like a battle of gods and giants going on between them over their quarrel about reality.
>
> Plato, *The Sophist*, 246

For as long as can be recalled, there have been arguments over ways of knowing. Gods, giants and even reasonable people cannot seem to agree about the nature of reality and how we can understand it. There are – quite simply – different ways of knowing.

When battles over the nature of reality are between gods and giants, we can expect sparks to fly. But the battles between mere mortals, or even scientists, can also generate a great deal of heat. As much as we like to pretend it is otherwise, the scientific process is not driven solely by the ideals of impartial and measured dialogue, drawing on empirical and rational support. Rather, presuppositions, aggressive rhetoric, economic and legal muscle, and authority all have a role to play in securing scientific knowledge. This book aims to explain some of the root causes of these heated exchanges. In particular, we introduce different ways of knowing and how these affect the methods we choose to study social phenomena.

Beneath any given research design and choice of methods lies a researcher's (often implicit) understanding of the nature of the world and how it should be studied. These underlying priors provide researchers with the philosophical ballast necessary to address important questions concerning the nature of truth, certainty and objectivity in a given project. These are very important issues, but they are receiving less and less of the attention they deserve from practising scientists. The reason for this is not difficult to discern. Contemporary social scientists have a plethora of new and more sophisticated methods at their disposal. As a consequence, they are devoting more time and energy to mastering these new methods. The result is predictable, if unfortunate: much of contemporary social science is driven by a given researcher's familiarity with particular methods. This preoccupation often comes with very little reflection about how a given method corresponds (or doesn't) to the researcher's underlying methodology.

Onr book aims to correct this nnfortunate shortcoming by focusing on the important ways in which methodologies and methods relate to one another. Toward that end we use this chapter to introduce two central methodological perspectives: naturalism and constructivism. These two methodologies can be said to constitute the main camps in the battle over reality in contemporary social science research: they are today's gods and giants. For this reason they provide the basic design of the book that follows: the first half is dedicated to how methods are employed in a naturalist methodology, while the latter half looks at the same methods as employed in a constructivist methodology.

Because these methodological traditions draw on different understandings of the nature of the social world, and on different ways of coming to understand that world, each of them employs common methods in different ways. For example, both naturalists and constructivists use comparisons, but they use them differently. Our primary objective is to highlight these differences so that students will better understand how their methodological priors affect the methods they choose and the ways in which they use them. To underscore these differences, the closing part of this introductory chapter provides an overview of the book's design.

But it would be a mistake to describe this battle between gods and giants only in terms of their differences: both methodological traditions are allies in the fight against ignorance and sloppy thinking. They share many common weapons and positions in this struggle, and it is just as important to embrace these similarities as it is to focus on the differences that separate the two traditions. After all, both naturalists and constructivists share an appreciation of honesty; an attention to detail and empirical accuracy; an embrace of reason and the utility of rhetoric; the need to address and minimize unwanted bias; and the desire to produce knowledge which can subsequently be reproduced by others who follow in their footsteps.

Ultimately, we hope to encourage students to become more aware of their own methodological positions and how these affect their research. We also hope to make students more aware of the various ways in which methods can be employed in social science projects. Most of us study social phenomena because we are fascinated by their depth and complexity. With this book, we wish to show how there is a corresponding degree of complexity and depth associated with the ways in which we can come to understand, and explain, these phenomena.

Methodological Foundations

Though they like to hide it from the world, scientists disagree about some pretty fundamental issues. Indeed, this book will depict social scientists differing on a number of these. For example: How do we understand the nature of the world we study? Is there only one type of scientific knowledge? What is the overall objective of scientific study? How should we assess which methods, data and evidence are appropriate? Amid all these differences, how do we assess competing claims? How do we know who is right? Is one side necessarily right, and the other wrong? How do we know?

To answer these difficult questions, we must begin by simplifying. We do this by suggesting that most work in social science can be grouped under two methodological rubrics, which will be described in much greater detail below. These two different methodologies incorporate radically different views of the world. As a consequence, each methodology employs similar methods in different ways – toward different objectives. It is our contention that many of the most significant differences and major disagreements in social science can be traced back to these methodological differences.

We distinguish between 'methodologies' and 'methods', viewing 'methodologies' as the basic and more comprehensive of the two terms. Thus we agree with Kenneth Waltz, who is worried that students

> have been much concerned with methods and little concerned with the logic of their use. This reverses the proper priority of concern, for once a methodology is adopted the choice of methods becomes merely a tactical matter. It makes no sense to start the journey that is to bring us to an understanding of a phenomenon without asking which methodological routes might possibly lead there. (Waltz, 1979, p. 13)

We concur. And we have written this book with an eye toward introducing the student to the ways in which methods and methodologies are related.

One useful way to consider this relationship is to think of methods as tools, and methodologies as well-equipped toolboxes. With this analogy, methods can be understood as problem-specific techniques. Thus we can expect electricians to view the world differently than carpenters (that is, they aim to resolve different types of problems). Each relies on a different mixture of tools or approaches to solve the problems he encounters. This is a good thing: when inappropriate tools are employed, a worker can inflict great damage. Thus we should not be surprised to find the electrician's toolbox filled with a different set of tools than those filling the carpenter's. On the other hand, we should not be surprised to find that the two people sometimes use identical tools for certain purposes.

Notice too that this analogy implies that the different occupations provide specialization, while complementing one another. After all, a well-built home needs both skilled electricians and carpenters, and the tools, toolboxes and skill sets of these different workers complement one another.

If this analogy is useful, it is alarming for a number of social scientists who use the term 'methodology' as a fancy word for statistical methods. Thus the central theme of John E. Jackson's (1996) overview of political methodology is the importation of econometric (read 'statistical') methods. For such scholars, it would seem, there is only one truly scientific method, and everything else is cold leftovers: having mastered the use of a hammer, the whole world around them can be understood in terms of nails. We hasten to note that this myopic affinity to a particular method is not restricted to statisticians: too many scholars, from a number of different methods backgrounds, are bound to a particular approach.

If we accept that methodologies imply real and important differences in understanding the world, then we can follow Hughes (1990, p. 11) in arguing that students should be aware of the methodological undergirdings of the social studies they read and (eventually will) produce:

> every research tool or procedure is inextricably embedded in commitments to particular versions of the world and to knowing that world. To use a questionnaire, to use an attitude scale, to take the role of a participant observer, to select a random sample, to measure rates of population growth, and so on, is to be involved in conceptions of the world which allow these instruments to be used for the purposes conceived. No technique or method of investigation (and this is as true of the natural science as it is of the social) is self-validating: its effectiveness, i.e. its very status as a research instrument making the world tractable to investigation, is, from a philosophical point of view, ultimately dependent on epistemological justifications.

In theory, this seems like a clear and reasonable statement. However, in practice it is hard to follow up. The methodological diversity of the social sciences can be confusing. For the new student of social science it may be helpful to know that 'methodology' often appears as one member in a trio from the philosophy of science, the two others being 'ontology' and 'epistemology'. These are the three musketeers of metaphysics – one of the more speculative fields of philosophy. *Ontology* is the most abstract of the three terms. It means the study of being – the study of the basic building blocks of existence. The fundamental question in the field of ontology is: 'What is the world really made of?' *Epistemology* is a more straightforward term; it denotes the philosophical study of knowledge. 'What is knowledge?' is the basic question of epistemology.

The third musketeer, *methodology*, is also a fairly straightforward term. It refers to the ways in which we acquire knowledge. 'How do we know?' is the basic question in methodology. Perhaps the easiest way to convey this is to break the word down into its component parts: methodology – that is, the study of methods, or the study of which methods are appropriate to produce reliable knowledge. This question of appropriateness covers both ontological and epistemological territory.

While methodology is a simple enough term, it is commonly wrapped in ambiguity, because 'methodology' is sometimes used as a fancy synonym for 'method'. Thus it is worth repeating that these two terms are *not* synonyms. In this book, method refers to research techniques, or technical procedures of a discipline. Methodology, on the other hand, denotes an investigation of the concepts, theories and basic principles of reasoning on a subject. The methodology of the social sciences, then, is to be understood simply as philosophy of science applied to the social sciences.

Ancient philosophical ghosts often frighten the new student investigating conflicting ontological, epistemological and methodological clues. Worse, modern methods courses (and their texts) often shelter students from their fears by assuming a single methodological, epistemological and ontological starting point. As we shall see in the chapters that follow, this often creates greater confusion later, when students observe how similar methods might be used in different guises toward different objectives, and under different ontological presumptions. It is our experience that the beginning social science student can be helped by a clear overview of how methodology and method choices relate to one another.

This book aims to provide that overview. Our objective is to supply the larger context into which more focused methods texts can be inserted and employed. In doing so, we hope to clarify some of the misunderstandings that students often encounter when they do not fully recognize the way in which one's choice of methods often (implicitly) reflects contentious methodological assumptions. Consequently, we hope to narrow the gap that now separates the implied ontologies and the methods employed by so many of today's social scientists (Hall, 2003).

In doing so, we raise some difficult and awkward questions about the relationship between the two main perspectives. Some authors – for example, Marsh and Furlong (2002, p. 17), argue that one's ontological and epistemological positions are like skins – once you've got one, you're pretty much stuck with it. We are not convinced. We would rather liken ontological and epistemological positions to jackets that you can put on and take off, depending on where you want to go and what you want to do. So too with methods and methodologies – these should be changed in accordance with the ontological and epistemological status of the question under study.

We think social science is better served by researchers who master several methods and methodologies, who can self-consciously choose among concepts and theories, and who command many basic principles of reasoning. In the text that follows, we provide several illustrations of how it is possible to move between methodological traditions – often with great success. Our aim is to provide students with enough methodological awareness that they can become informed and careful consumers of social studies. Though we shall touch on ontological and epistemological issues, we do so only lightly; we leave the ontological and epistemological proselytization to others.

This way of thinking about the world is perhaps most familiar to students of International Relations (IR). For generations, IR students have been taught to interpret the world through three disparate approaches, or ideological perspectives: liberalism, realism and radicalism (or Marxism). These students learn to recognize the different actors and levels of analysis associated with each approach, and are taught to understand the world from the vantage point of each perspective. Many of us were taught to think of these different approaches in terms of 'different-coloured lenses', which implies that the thing being studied is the same for all viewers, while the way it is viewed might vary from lens to lens. The objective of this common practice was *not* to find the one approach that 'best' fits the real world, but to emphasize the fact that the world can be perceived in different and contrasting ways.

This tradition might be compared with that of the modern (mainstream) economics tradition, which subscribes to a remarkably narrow ideological standard, steeped in a naturalist methodology. While this methodological commitment may be the reason that economics is known as the queen of social science, recent developments suggest that the empress has no clothes. In particular, the inability to predict the Great Recession of 2008 revealed a significant fissure among economists, where much of the discussion has been concerned with the problems of building social understanding on such a narrow ontological and methodological base (see, for example, Krugman, 2009).

We encourage social scientists to embrace a broader, more pluralistic approach to knowledge. As social scientists, we need to understand that there can be different types of knowledge, that knowledge can be accessed in a number of different ways, and that knowledge is not always unrelated to interest. As a consequence, we need to have access to different types of knowledge and ways of knowing.

This book is designed to introduce some methodological variety to those embarking on the study of social science. Different social scientists approach the world with different assumptions about the way it actually is, and how they should study it appropriately. As a consequence, scientists who come from different methodological traditions often use standard methods in different ways. While some of us will sympathize with one methodology more than another (and there is nothing wrong in that!), all of us must be aware of the existence of these differences and how they affect the ways in which methods are used.

Though we shall spend a great deal more time in subsequent chapters (Chapters 2 and 8, in particular) describing the basic philosophical components to various methodologies, we want to use this introduction to lay out briefly the methodological terrain as it appears to the practising social scientist. This terrain is dominated by two methodological traditions: naturalism and constructivism.

We are aware that philosophers of science may feel uncomfortable with such a simple depiction of the scientific world. But our intention is to help students understand the nature of contemporary social science research (not to outline the nature of contemporary philosophical debate), and we contend that this research is still strongly characterized by this simple methodological dichotomy. Indeed, we think that this methodological divide is the most important cleavage separating contemporary social scientists.

We hasten to add that we have created these methodological traditions as ideal types – they do not exist independently in the world. As is often the case in science, we are imposing a simple model that divides the complicated world of social scientists into two competing camps. Worse, since they are ideal types, individual scientists will not feel comfortable in either camp. For this reason, it may be more useful to think of these two methodologies as end points on an imaginary continuum, where individual authors find themselves at home some place in between them.

Indeed, scholars have recently embraced a new approach that attempts to fill the gap that separates naturalism from constructivism. In contrast to the first two methodologies, scientific realism can be seen as a distinct movement, to which philosophers and practitioners of science increasingly claim allegiance. Because it does not offer a unique or distinct ontological position, we only refer to scientific realism in our introductory and concluding chapters to show how it relates to the methodologies that still dominate the field.

Now that we have begun to throw in some pretty large and messy terms (naturalism, constructivism, realism), it is time to describe them in more detail.

Naturalism

How do we know? For most of the twentieth century, and onwards, the social scientist's answer to this question has been made with a nod to the natural sciences. In the push for scientific legitimacy, and the funding that follows in its wake, social scientists have quietly adopted a view of the world that was first articulated in the natural sciences. This view assumes that there is a Real World (big R, big W) out there, independent of our experience of it, and that we can gain access to that World by thinking, observing and recording our experiences carefully. This process helps scientists to reveal patterns that exist in nature but are often obscured by the complexities of life. Thus we call this methodology ***naturalism***, as it seeks to discover and explain patterns that are assumed to exist in nature.

In different academic contexts, naturalism is known by many different names. The most common of these is 'positivism', but 'empiricism' and 'behaviouralism' are also used to describe the same basic methodological position. As each of these terms, for a variety of reasons, has fallen into disrepute, or is used as a polemical epithet, we think it is useful to employ a more neutral and descriptive term to capture this methodology's essential characteristics.

Naturalists rely heavily on knowledge that is generated by sensual perception, such as observation and direct experience. For a naturalist, something is true when somebody has seen it to be true (and recorded it as such). As we shall see, naturalists also employ logic and reason. Ultimately, however, reason and logic need to be supported by direct experience if the naturalist is to rely on the knowledge that is produced.

From these core (ontological and epistemological) beliefs, naturalists have developed a rather narrow set of criteria for evaluating the reliability of the knowledge produced. In particular, social scientists have increasingly turned to falsification and predictive capacity as the standards for evaluating their knowledge. From here, mainstream social science has developed a hierarchy of methods that can be used to test our knowledge under different circumstances.

Though it is not easy to summarize a methodological tradition – and we shall examine the naturalist methodology in more detail in Chapter 2 – we might suggest that the naturalist's approach embraces the following six features:

- There exist regularities or patterns in nature that are independent of the observer (that is, a Real World).
- These patterns can be experienced (observed), and these observations can be described objectively.
- Observational or experiential statements (based on these regularities) can be tested empirically according to a falsification principle and a correspondence theory of truth.
- It is possible to distinguish between value-laden and factual statements (and facts are, in principle, theoretically independent).

- The scientific project should be aimed at the general (nomothetic) at the expense of the particular (idiographic).
- Human knowledge is both singular and cumulative.

Perhaps the easiest way to understand the ambitious nature of the naturalist project is to recognize it in the influence and success of Edward O. Wilson's (2003) *Consilience*. Wilson, a biologist accustomed to working with ants, believes that all knowledge is intrinsically unified and interlocked by a small number of natural laws. Using the natural sciences as his model, Wilson sketches an ambitious project: he aims to unify all the major branches of knowledge under the banner of (natural) science. Because there exists a Real World out there, independent of our experience of it; because we can know that World by careful thinking and observation in an objective and falsifiable manner; because such thinking and observations can uncover general patterns and laws that interact in a singular and cumulative project; then the scientific project is an enormous and singular one. This is an elegant and attractive vision, but one that would require a great deal more synthesis and agreement among scientists than exists today, or ever has existed.

Constructivism

Despite the naturalist view dominating modern social science, it has not escaped criticism, nor does it stand alone. Many social scientists are leery of accepting the naturalist's view of the world, as many of the patterns that interest them are seen to be ephemeral and contingent on human agency. For these social scientists, the patterns of interest are not firmly rooted in nature but are a product of our own making. Each of us sees different things, and what we see is determined by a complicated mix of social and contextual influences and/or presuppositions. It is for this reason that we refer to our second methodology as *constructivist*: it recognizes the important role of the observer and society in constructing the patterns we study as social scientists.

As with other methodological positions, constructivists are known by a variety of names, many of which are not particularly endearing. The most common of these is probably 'interpretivism', but constructivism also corresponds to 'Gadamer's hermeneutics, Habermas's Critical Theory ... French deconstructionists, post-structuralists, and other similarly suspicious continental characters' (Ball, 1987, p. 2). This methodology is described in more detail in Chapter 8, and the latter part of the book shows how constructivists employ traditional methods. For now, we wish to briefly introduce constructivism and show how it differs from naturalism and why we use it as its methodological counterweight in the overall design of the book.

At the bottom of the differences separating naturalists from constructivists is the recognition that people are intelligent, reflective and wilful, and that these characteristics matter for how we understand the world. Constructivists recognize that we do not just 'experience' the world objectively or directly: our perceptions are channelled through the human mind – in often elusive ways. It is in this short channel between the eye and the brain – between sense perception and the experience of the mind – that we find many challenges to naturalism. When our scientific investigation is aimed at perceptions of the world, rather than the world 'as it is', we open the possibility of multiple worlds (or, more accurately, multiple experiences).

Consequently, constructivists recognize that people may look at the same thing and perceive it differently. Individual characteristics (such as age, gender or race) or social characteristics (such as era, culture and language) can facilitate or obscure a given perception of the world. Recognizing the wilfulness of human agency complicates any attempt to try to capture it in simple, law-like terms (as is common in the naturalist world). Once a social 'law' is known to human actors, they start to exploit it in ways that can undermine its law-like features (Popper, 2002a).

To make matters even more complicated, human agency creates things that have a different ontological status than the objects studied by natural scientists. As Max Weber (1949, p. 81) noted: 'We are cultural beings, endowed with the capacity and the will to take a deliberate attitude towards the world and to lend it significance.' This capacity gives rise to a class of facts that do not exist in the physical object world: *social facts* (such as money, property rights or sovereignty) depend on human agreement, and typically require human institutions for their very existence (Searle, 1995, p. 2).

Because they recognize such ontological diversity and complexity, constructivists tend to draw on more diverse sources and on different types of evidence. While constructivists recognize experience and reason as useful epistemological devices, they also realize that both of these can be influenced by the above-mentioned contextual factors – undermining any claims to their being objective transmitters of truth. Because social contexts are filled with meaning, constructivists find utility in a much broader set of epistemological tools, including empathy, authority, myths and so on.

Given the fact that constructivists focus on the reflective and idiosyncratic nature of knowledge, the overall objective of constructivist science is quite different from its naturalist counterpart. If we follow Quentin Skinner (1975, p. 216), we could say that constructivists try to understand action

> not in causal and positivist terms as a precipitate of its context, but rather in circular and hermeneutic terms as a meaningful item within

> a wider context of conventions and assumptions, a context which serves to endow its constituent parts with meaning while attaining its own meaning from the combination of its constituent parts.

Rather than uncovering a true account, constructivists seek to capture and understand the meaning of a social action for the agent performing it (as well as for the scholar studying it). If something appears meaningful or real to a social agent, then it may affect her behaviour and have real consequences for the society around her.

While naturalists try to uncover singular truths in a falsifiable manner that corresponds to one true reality, constructivists embrace the particular and use their knowledge to expand our moral sympathies and political understandings. For the constructivist, truth lies in the eyes of the observer, and in the constellation of power and force that supports that truth. As even our descriptions of events are not free from the biases that surround us, constructivists hold little hope of securing an absolute truth: the best we can do is to be honest and open about the way in which our contexts (and those of our subject matter) frame the way in which we come to understand. This is not to say that constructivists are all relativists: there can be better and worse constructivist accounts. Rather, constructivists are more hesitant to claim truth as their own.

With an eye to symmetry, we might list some of the qualities of constructivist research, as a reflection of the naturalist approach:

- The world we study is not singular and independent of the observer: the world includes social facts.
- Observations and experience depend on the perspective of the investigator; they are not neutral and not necessarily consistent across investigators.
- Observational statements can contain bias and can be understood in different ways.
- Even factual statements are value-laden.
- Knowledge gained by idiographic study is embraced in its own right (not as a necessary part in a larger nomothetic project).
- There is value in understanding, and there can be more than one way to understand.

If Edward O. Wilson's (2003) *Consilience* can be seen as an exemplary text in the naturalist tradition, we suggest that Bent Flyvbjerg's (2001) *Making Social Science Matter* can play a similar role for constructivists. Rather than mimic the approaches that have been developed by natural scientists who study the natural world, Flyvbjerg suggests that social scientists should leverage the strength that comes from its rich, reflexive analyses of social facts, value and power. He prioritizes practical, applied knowledge over general, nomothetic, knowledge; promoting what he calls 'phronetic social science' in order to connect knowledge to power and to contribute to practical reason. In short, he hopes to:

> transform social science [in]to an activity done in public for the public, sometimes to clarify, sometimes to intervene, sometimes to generate new perspectives, and always to serve as eyes and ears in our ongoing efforts at understanding the present and deliberating about the future. We may, in short, arrive at a social science that matters. (Flyvbjerg, 2001, p. 166)

Scientific Realism

In recent decades a new philosophy of science has arisen to challenge the dominance of naturalism. In stark contrast to both naturalism and constructivism, scientific realism constitutes a self-conscious school, where scholars pride themselves on their membership (though the name of the club tends to vary by neighbourhood). They are known by many different names – including 'transcendental realists', 'relational realists', 'critical realists' and 'empirical realists' – but most commonly as 'scientific realists'. They are philosophers of science on a mission: they offer a full-fledged metaphysical position by blending some of the most attractive features of both the naturalist and constructivist approaches.

Because of its relative youth, and because it was born in the thin and rarified air of metaphysics, scientific realism has yet to make a noticeable impact on the everyday practice of social science. Still, scientific realism is an approach with much promise, and for that reason it is important to introduce it to the reader. Also, it provides another perspective, from which we can leverage our understanding of both naturalism and constructivism.

In a practical sense, scientific realism straddles the ontological positions of naturalism and constructivism. This, in itself, is worth some reflection, as it helps us to understand the nature of the difference that separates our two main methodological positions. At its ontological core, scientific realism comes closest to naturalism. Scientific realists recognize that there exists a Real World independent of our experience. At the same time they embrace Weber's famous constructivist maxim, that man is an animal suspended in webs of meaning he himself has spun. Scientific realists realize that there can be many layers to the reality they study, and that their access to the one 'Real World' is highly complicated. The more complicated the picture, the closer scientific realists come to the constructivist's point of view. Yet they never let go of the naturalist foundation.

The scientific realist's position is akin to the famous Eastern guru who tells his disciples that the world rests on the back of a tiger, and that the tiger is supported by an elephant, who in turn stands on a giant turtle. When a disciple timidly asks what the giant turtle, in turn, stands on, the guru quickly replies: 'Ah, after that there are turtles all the way down!' In a sense, scientific realism provides a convenient way of avoiding the problem of two different and irreconcilable ontologies. After all, we doubt that there are many constructivists who are willing to reject outright the possibility that a Real World might exist out there, buried deep, deep down, or in significant areas of human endeavour. After all, engineers and physicists are able to send rockets to the moon (or to drop them on terrorist compounds). The relevant (and practical) questions to ask are: How deeply buried is this Real World? How far does it extend into our social experience? Does it make sense to employ research methods that assume it lies just beneath the surface and all around us?

While scientific realists recognize many layers of truth, and share with constructivists a realization that the social world is filled with complexity, they believe that the best way to uncover these buried truths is, ultimately, by way of scientific (read naturalist) approaches (Wendt, 1999). Thus, Ian Shapiro (2005, pp. 8–9) has summarized the core commitment of scientific realism as the 'twofold conviction that the world consists of causal mechanisms that exist independently of our study – or even awareness – of them, and that the methods of science hold out the best possibility of our grasping their true character'.

But the similarities with naturalism tend to stop there. Scientific realists avoid references to 'universal laws' and hypothetic–deductive approaches to explanation. They are critical of those who use falsifiability as a means of distinguishing between science and nonsense. They even question the neutrality of the scientist (and her language!).

In short, scientific realists focus on 'necessity and contingency rather than regularity, on open rather than closed systems, on the ways in which causal processes could produce quite different results in different contexts' (Sayer, 2000, p. 5). Compared to naturalists, scientific realists are willing to open up the scientific project by recognizing the possibility that powers can (and do) exist unexercised. In other words, scientific realists recognize and appreciate the open-ended nature of human exchange.

Where does this discussion lead us? As will soon become apparent, we have much in common with scientific realists. This is especially true with respect to the role of methods. We concur with scientific realists in recognizing that good science should be driven by questions, not by methods.

> Compared to positivism [naturalism] and interpretivism [constructivism], critical realism endorses or is compatible with a relatively wide

> range of research methods, but it implies that the particular choices should depend on the nature of the object of study and what one wants to learn about it. For example, ethnographic and quantitative approaches are radically different but each can be appropriate for different and legitimate tasks – the former perhaps for researching, say, a group's norms and customs, the latter for researching world trade flows. Perhaps more importantly, realists reject cookbook prescriptions of method which allow one to imagine that one can do research by simply applying them without having a scholarly knowledge of the object of study in question. (Sayer, 2000, p. 19)

We agree. We have written this book to help students recognize how methods and methodologies relate, and, consequently, how methods can be employed in a number of different ways and open up to various ways of knowing. More important, we hope that this recognition will help students to realize the utility of tailoring their choice of methods to the problems that interest them (rather than tailoring their problems to the methods they have learned).

Where we differ from scientific realists is in the perceived need to define a new unifying scientific tradition. Scientific realism introduces itself as an approach for those constructivists who feel a need to enter into the scientific fold. Following Lane (1996, p. 364): 'it has now become possible to qualify as a scientist without being a positivist'. In short, scientific realism offers a new universal approach – one that can straddle the natural and social sciences as well as the naturalist and constructivist traditions. It is a great synthesis of the two main methodological traditions in contemporary science, as described above.

We are leery of such ambitions. By contrast, we wish to encourage students to be sensitive to the ontological and methodological priors of social scientists, and to become more conscious and aware of how these priors affect our work (and how it should be evaluated). In short, we are sceptical of universal narratives. We do not proselytize for any given methodological position, or claim that one position provides better answers to all of life's difficult questions. Ours is a call for methodological pluralism, not methodological conformity.

Chapter Outline and Logic

This book aims to provide an approachable introduction to the main methodologies and methods employed in the social sciences. In contrast to existing methods textbooks, which aim to provide cookbook-like sketches of particular methods under a single methodological rubric, we aim to survey the broad horizons of contemporary social science research. To do this, we employ a simple, symmetrical outline that allows students to compare and contrast the way in which methods are employed in different methodological contexts.

As a result, our discussion of applied methods is necessarily brief. We encourage students to delve deeper into particular methods once it is determined that a given method is appropriate for the question at hand. We offer a broad survey or overview of the methods available, so that students can find their way more easily through the sometimes dense methods terrain.

The body of the book is divided into two methodological alternatives: one naturalist, the other constructivist. The ontological and epistemological backgrounds to each methodology are presented as an introductory chapter for each section. Thus, Chapter 2 provides an introduction to the naturalist methodology, while Chapter 8 provides an introduction to the constructivist methodology. Because of the material covered in these two chapters, they are necessarily denser than the others. For this reason, we ask for the reader's indulgence and patience when reading them. We believe that this investment of time and energy will pay off when we begin the methods chapters that follow.

By organizing our presentation in terms of two methodological alternatives, we do not intend to suggest that students and authors cannot (or should not) swap epistemological and ontological positions. We are simply proposing two ideal types for the purpose of clarifying different ontological and epistemological approaches (and their relationship to methods). Also, we think that a simplified (two-pronged) approach to methodology provides some pedagogic utility in that it can be used to deliver a relatively symmetrical depiction of the methods available to social scientists. In this way, we hope that the student will find it easier to remember the various ways in which methods are applied under different methodological contexts. In particular, we argue that each methodology appears to have its own hierarchy, or pantheon, of methods.

This hierarchy is clear (and most explicit) when we discuss the naturalist methodology. From this naturalist perspective, the scholar expects to find natural patterns in the world, and careful applications of methods are used to uncover these patterns. This ontology lends itself to an empiricist epistemology, where the collection of empirical evidence is used to persuade and predict.

From this point of departure, naturalists have developed a clear hierarchy of methods. At the top sits the experimental method. This is the ideal method for naturalist explanations because of its ability to control and order causal and temporal relationships. When the experimental method is not a realistic alternative, then naturalist social scientists prefer statistical approaches. Below statistical approaches lies the third-best alternative (when there are too few observations to run reliable statistical queries): small-N comparative approaches. Finally, at the bottom of the naturalist's hierarchy of methods lie case studies, interviews and historical approaches. Social scientists with a naturalist inclination are expected to employ these narration-based methods only when faced with a paucity of data or relative comparisons.

In contrast, constructivist scholars see the world of study as being socially constructed, so they do not expect to see objective (and verifiable) patterns of social phenomena existing naturally in the social world. For the constructivist, motivations and presuppositions play a central role in accessing this world, and the objective of social study is to interpret and understand, not to predict. As a result, the constructivist can draw from a much broader epistemological stable.

Given these ontological and epistemological starting points, we should not be surprised to find that constructivists have little faith, and find little utility, in the naturalist's hierarchy of methods. They advocate an alternative hierarchy, a flatter and less clear ranking than that of the naturalists – but a hierarchy none the less. This hierarchy reflects less a ranking of approaches in terms of their ability to access the truth, and more a hierarchy in terms of the popularity of the given approach/method. As constructivist scholars depend on maintaining the 'constitutive' context of a given phenomenon, they abhor methods that manipulate, dissect or reconstitute the setting in which relevant 'data' are embedded. Given this point of departure, narrative approaches such as discourse analysis and process tracing are the constructivist's methods of choice. These types of narrative approaches allow constructivists to dwell on the particulars and on the contexts that provide them with understanding and insight.

This is not to suggest that constructivists do not rely on comparative methods. Indeed, comparisons are as important to constructivists as they are to naturalists. After all, comparisons play a central (if often implicit) part in the hermeneutic tradition. But constructivists use comparisons in a radically different way. Rather than trying to uncover nature's underlying patterns, constructivists use comparisons to develop associations which can leverage our understanding over particular events, or to understand the reasons why we see the patterns that attract our analytical attention.

These opposing hierarchies are used to structure our presentation of the most common methods used in the social sciences today. Thus, after an introduction to the philosophy of naturalist social science in Chapter 2, we use the subsequent chapters to introduce the hierarchy of naturalist methods in the following preferred order: at the top is experimental (Chapter 3); followed by statistical (Chapter 4); then comparative (Chapter 5); and finally, in Chapter 6, case-study methods.

At this point we reach the book's fulcrum, in Chapter 7, where we pause to examine the problems of naturalism and the utility of an alternative methodological approach. In particular, we question the assumption that methodological holism serves the social sciences – in other words, the notion that there is a Real World beyond our senses, and that observation and language can be used to depict that Real World objectively. These shortcomings are used to introduce different methodological approaches to social phenomena – one of which is constructivist in nature.

The second part of the book describes the constructivist approach. Chapter 8 mimics Chapter 2, in that it provides the ontological and epistemological counterweights to the mainstream (naturalist) tradition. From the constructivist perspective, the human world is seen as being socially constructed; motivations and presuppositions play a central role in accessing this world; and the objective of social science is to interpret and explain the nature of those social patterns, rather than to predict outcomes. As a result, the subsequent chapters illustrate the utility and application of different methods, in the context of constructivism. Thus we begin with an introduction to narrative methods (Chapter 9), and follow this with a sketch of comparative (Chapter 10), statistical (Chapter 11) and experimental methods (Chapter 12). In this second part of the book we see how constructivists can employ identical methods to those used by naturalists, but how these methods are prioritized differently and used in different ways, toward different ends.

By organizing the book in this symmetrical fashion we are emphasizing the utility of *balancing* these two approaches. We begin with the naturalist approach because it is the dominant and the most familiar methodological approach in contemporary social science. And by concluding with a description of constructivist approaches we are not suggesting that the latter supersedes the former. Indeed, we think that the best scholarship in social science draws from both methodological sources: good work in the naturalist tradition is sensitive to constructivist concerns, and vice versa. We cannot emphasize this enough: our aim is to encourage methodological pluralism, not to advocate one approach at the expense of the other.

For fear of encouraging a new cleavage in social science, and with the aim of emphasizing the complementary nature of these two methodological approaches, our concluding chapter emphasizes the utility of building bridges that can link naturalist and constructivist approaches.

Given this design, it occurs to us that there are several different ways that the reader might approach the text. We have designed the book in a way that emphasizes the two distinct methodological traditions, so that each particular method can be understood in light of an author's particular methodological commitments. But it is entirely possible for the reader to jump around the book by comparing approaches on a particular method. For example, those with an interest in philosophy of science issues might begin by reading (and comparing) Chapters 2 and 8. Alternatively, those readers who have a soft spot for comparative approaches might begin by reading and comparing Chapters 5 and 10. In short, we hope that the book's logic and symmetry make this sort of individual reading both accessible and useful.

Recommended Further Reading

As mentioned in the text, readers might compare and contrast Edward O. Wilson's (2003) *Consilience* and Bent Flyvbjerg's (2001) *Making Social Science Matters* to fathom the remarkable variance that separates naturalist and constructivist approaches to social science. The founts from which much critical realism flow are Roy Bhaskar's (1997 [1975]) *A Realist Theory of Science* and his (1998 [1979]) *The Possibility of Naturalism*; and a thorough introduction to critical realism can be found in Margaret Archer *et al.*'s (1998) *Critical Realism: Essential Readings*. The practicing social scientist may find it easier to access scientific realism by way of Andrew Sayer's (2000) *Realism and Social Science*, or through its application – as in, for example, David Marsh *et al.* (1999) *Postwar British Politics in Perspective*. For those who would like to learn more about the philosophical foundations of contemporary social science, Patrick Baert's (2005) *Philosophy of the Social Sciences* is highly recommended.

Chapter 2

The Naturalist Philosophy of Science

The origins of modern science can be traced back to the early spring of 1610, to a slim book entitled *The Starry Messenger*. Today's readers would have to search long and hard for excitement or provocation in this book, as it largely describes the night sky. Yet, in the early 1600s, *The Starry Messenger* was capable of triggering condemnations, angry reactions and even calls for its author to be burned at the stake.

The author was Galileo Galilei (1564–1642). His controversial observations were enhanced by a new instrument, the telescope, which enabled him to describe and draw pictures of configurations in the night sky. The telescope also enabled Galileo to see things that traditional science had not prepared him to expect – including mountains on the moon (which orthodox churchmen considered impossible), and three moons or satellites that circled Jupiter in a steady orbit. The latter was not only impossible, it was clearly in violation of Church doctrine, which held that the Earth was handmade by God and placed at the centre of an equally divinely crafted universe. The Earth was encased in eight perfectly circular crystal spheres, to which the sun, the moon, the planets and the stars were attached (and pushed across the sky by angels). If moons orbited Jupiter, as Galileo said, this would break the crystal sphere to which Jupiter was attached.

The Church was in a quandary over what to do with the book (and its author). In a sense, Galileo made things easier for them by blatantly stating that any discrepancy between his observations and those of Aristotle must be the result of Aristotle's shortcomings. As Church scholarship rested almost entirely on Aristotle's authority, Galileo's rumblings could not be ignored. If Aristotle had been wrong, then a thousand years of established knowledge would tumble down around the ears of scholars everywhere.

The Starry Messenger is a milestone in the history of science. It is often seen as the first true application of the scientific method – of a process that involves systematic observation, scrupulous note taking of things and patterns observed, and thoughtful efforts to make sense of it all. The book represents a different approach to knowledge than that advocated by Church scholars. According to Galileo, the traditional approach did not further the cause of knowledge; rather, it inhibited new discoveries. The traditional approach to knowledge was weighed down by excessive reliance on established authorities, and it hampered human beings' observation of nature. In Galileo's view, only free and independent scholars could observe nature impartially and gain new insights about its regularities.

This view gained Galileo many opponents among clerics, who argued that he was rejecting tradition and authority – including the authority of God and the Church. The situation was untenable and the match uneven: in one corner was Galileo; and in the other, Aristotle, the Church, God and 2,000 years of accumulated knowledge. The situation was also dangerous; because Galileo persisted in his observations, his speculations and his disrespectful comments, the Inquisition charged him with heresy in 1633. Faced with a possible death sentence, Galileo agreed that cosmic questions were not 'legitimate problems of science' and publicly withdrew some of his claims. The Church, for its part, commuted his sentence to life imprisonment.

About the same time, Galileo's fellow stargazer, Johannes Kepler (1571–1630), found himself in a similar situation. He too broke with traditional science and struck out on his own. Like Galileo, he spent years observing planets and stars, and accumulated vast piles of notes (both his and those of the great Danish astronomer, Tycho Brahe). After a long and careful analysis of these notes, Kepler also drew conclusions that clashed with the established knowledge of the Church. First, he suggested that Aristotle was wrong (Aristotle had claimed that each planet travels in a perfect circle around the earth, whereas Kepler proposed that they orbit the sun in an elliptical pattern and that the speed of each planet is not uniform throughout its orbit; rather, planets travel faster when their orbits are closest to the sun). Kepler expressed this orbit, including its curious variance, in the precise language of mathematics.

Isaac Newton (1642–1727) would later draw on the observations of both Galileo and Kepler to take the next great leap in human knowledge. He identified regularities in the sky and on Earth, and argued that bodies attract each other according to a constant principle. Newton's supreme achievement was to bring Galileo and Kepler together, and to demonstrate that Galileo's laws of motion on Earth and Kepler's law of planetary motion in the heavens were, in fact, two aspects of the same great regularity. Newton's *Mathematical Principles of Natural Philosophy* (1968 [1687]) explained persuasively why the universe behaved according to clockwork-precise patterns of perfectly repeated movements in space.

The Birth of the Philosophy of Science

The above sketches, from the history of astronomy, provide a common story of the birth of modern science. It is a story of individual risk-takers who relied on empirical observation to combat the myths of the past and liberate themselves from the interpretive contexts of their time. Related to this story is another, which provides us with the epistemological support needed to understand Galileo's, Kepler's and Newton's success. Sir Francis Bacon (1561–1626) – lawyer, politician and scientist – played a central part in this story.

Galileo had openly criticized Aristotle's *Physica*, thereby triggering a controversy with the Church that produced a new methodology – a controversy that very nearly cost him his life. Bacon objected to another of Aristotle's great books, the *Organon*, and ignited a similar revolution in ontology and epistemology. In the same way that Galileo's work was followed up by astronomers such as Kepler and Newton, Bacon's work was followed up by philosophers of science – men like John Locke and David Hume.

Galileo and Bacon were both part of a critical movement that contributed to the secularization of human knowledge about the world. They both questioned traditional ways of knowing. They both challenged the Church-sanctioned idea that God had granted man 'natural reason', which could be accessed to understand the world, and that this approach alone could secure reliable knowledge. And they both found themselves in conflict with the Established Church authorities – though Galileo suffered more seriously than did Bacon.

Francis Bacon and the Method of Induction

By profession, Francis Bacon was a lawyer and a politician – eventually becoming Lord Chancellor under King James I of England. By inclination, he was a tinkering jack-of-all-trades. One might even say that Bacon was more of a handyman than a scientist – indeed, he had more respect for handymen than for scientists, whom he referred to as 'spiders who make cobwebs out of their own substance' (Bacon, 1994 [1620], p. 105).

Bacon admired the skills of craftsmen. By watching them work, he came to grasp a new way of obtaining knowledge about the world. In contrast to the sterile debates of Aristotelian philosophers of science, Bacon argued that the practical methods of craftsmen could generate new knowledge, informed by nature. When he sat down to write a book to introduce his new method, he began with a head-on attack on Aristotle's method (and with it, the method of Church scientists). His ambition was to write a book that superseded Aristotle's authoritative *Organon*; so Bacon called his book *Novum Organum* (1994 [1620]).

Novum Organum introduced an approach to acquiring knowledge that differed greatly from the methods used by traditional scientists. Traditional scientists followed Aristotle's advice and started with a general proposition. They began with generally accepted truths or axioms and would use these to illuminate particular observations. By doing this, Bacon explained disparagingly, traditional scientists were unable to produce new knowledge; the approach simply drafted observations to serve already established truths. For science to proceed, Bacon continued, it was necessary to follow a different procedure – one that combined deduction and induction; a procedure that was a matter of routine among skilled craftsmen.

Unlike the scientists of the day, craftsmen did not start with general truths. They began by assessing the particular object or situation at hand. Craftsmen were employed to produce different things under different circumstances – a carpenter was ordered to fix a roof by one patron, build a table by another, and repair a hayloft or a stable by a third. This variety of tasks necessitated an active, improvising and experimental approach, harnessing inductive procedures. From his observation of craftsmen in action, Bacon argued that the scientist must begin with systematic observation. He must then build his argument from a large number of single observations toward more and more general truths. The craftsman and the scientist both begin with the particular and '[call] forth axioms from the senses and particulars by a gradual and continuous ascent, to arrive at the most general axioms last of all' (Bacon, 1994, p. 47f).

This active way of engaging the objects of the world stood in stark contrast to the passive contemplation of the Church philosophers, who, in their observations of objects, plants and animals, too readily relied on preconceived notions and on the facts that supported them. The philosopher begins at the wrong end, Bacon charged; he begins with axioms or general truths, and seeks to understand the particulars in light of them. These different approaches are described in Figure 2.1.

Bacon is seconding a critical point that Galileo had already hurled at traditional Church scientists: their main problem was that they engaged in deductive exercises based on authoritative texts. While Bacon

Figure 2.1 *Classic deduction and induction*

Deduction builds on true and accepted claims (axioms). Deduction starts with general truths and proceeds through established rules of reasoning toward explanations of single events. As such it can be understood as a top-down approach, where lofty, more general, theories guide the empirical studies below.

Induction builds on sensory observations (sight, smell, touch and so on). Induction starts with empirical particulars on the ground, and generates more general theories at a higher level. Consequently, induction can be seen as a bottom-up approach.

preferred to take his clues from craftsmen, he recognized that they had shortcomings of their own. One was that they had no texts. The experience of craftsmen was handed down orally and practically from master to apprentice. The substantial knowledge and the pragmatic methods of a craft were kept alive as praxis, but they remained largely unrecorded. For Bacon, hope lay in combining experience with record-keeping: when 'experience has learned to read and write, better things may be hoped' (Bacon, in Mason, 1962, p. 142). Craftsmen, in other words, must learn to record their observations. Their notes could then be checked and tested in a way that would provide an empirical basis from which new knowledge could be generated.

When Bacon explained this procedure, he justified it by two important claims: (i) only direct observations supply us with statements about the world; and (ii) true knowledge is derived from observation statements. In other words, Bacon not only rejected the deductive method of the old philosophers; he protested the faith in God-given insights and made himself the champion of sense perception. In effect (if a little unjustly), Bacon became history's spokesman for the inductive method.

The old logic of deduction relied on reason alone and was applied by philosophers who followed 'the way of the spider'. No new knowledge could come from such men, who endlessly 'spin webs out of themselves'. Against this method of the spider, Bacon contrasted the logic of induction – the logic of craftsmen who relied on trials and experiments and their faculties of observation. Craftsmen followed 'the way of the ant' by collecting material from the world and using it to construct larger edifices. In this way, they could produce new knowledge. This was a great advantage, but it had to be tempered by the realization that this new knowledge was not necessarily true.

Despite Galileo and Bacon agreeing that systematic observation of the world could produce new knowledge, Bacon's argument had a darker edge to it. He saw that the human senses could not always be trusted, and that the world might not always be as it appears. An observer could not trust his senses blindly; he must fortify them with 'common sense' and reason. In the end, then, Bacon recommended that science could not rely exclusively on either the 'way of the spider' or the 'way of the ant'. Science must rely on both – 'the middle way':

> The middle way is that of the bee, which gathers its material from the flowers of the garden and field, but then transforms and digests it by a power of its own. And the true business of philosophy is much the same, for it does not rely only or chiefly on the powers of the mind. Nor does it store the material supplied by natural history and practical experiments untouched in its memory, but lays it up in the

> understanding changed and refined. Thus from a closer and purer alliance of the two faculties – the experimental and the rational, such as has never yet been made – we have good reason for hope. (Bacon, 1994, p. 105)

Locke, Hume and the Modern Philosophy of Knowledge

At the end of the seventeenth century, John Locke (1632–1704) built on Bacon's empiricist foundations in *An Essay Concerning Human Understanding* (Locke, 1984 [1690]). Locke set out to discuss the 'extent of *human knowledge*, together with the grounds and degrees of *belief*, *opinion* and *assent*' (p. 63, italics in original). He repeats Bacon's argument that knowledge should rely on sense perception, and defends it in a way that has since played a decisive role in modern science. Locke's defence had an enormous influence on subsequent British philosophy and has furnished the modern notion of empiricism with its basic claim that all knowledge is empirical in origin.

Locke did not deny the Christian axiom that humans are God's creation, fashioned in God's image. However, he did deny the medieval notion that God had endowed human beings with innate (or *a priori*) ideas. For Locke, a human being was born with a mind that resembles a blank slate (a *tabula rasa*): there is no such thing as *a priori* knowledge. For this reason, knowledge of the world cannot be gained by turning our attention inward in an introspective search for a 'natural reason', divinely endowed by an omniscient God. For Locke, all knowledge is a posteriori – in other words, it can only be derived from sense experience. Knowledge enters the human mind through the organs of sense in the form of sense impressions; these are stored in the memory as single ideas and may be retrieved and recombined by the imagination.

Even fanciful ideas that have no correspondence to the Real World – a unicorn, for example – are arrived at through simple sense perceptions. Thus, we perceive simple phenomena, such as a horse and a rhinoceros, and we store these in our mind in the forms of simple ideas. By rearranging and recombining these simple ideas, the mind can form new, more complex ideas. Out of the single idea of a horse and the single idea of a rhinoceros, the mind can produce the complex idea of a unicorn.

In order to gain knowledge about the world, then, we must first gain impressions about the world – through our senses – and store these in our minds. We can then process these sense impressions in systematic ways, according to established rules of logic, 'justified by a sufficient and wary induction of particulars' (Locke, 2004, §13). Note how Locke follows Bacon in being aware of the potential biases inherent in inductive approaches.

Locke's concrete and commonsensical style, his practical tone and his warnings against unverifiable speculations combined to secure him a wide circle of readers and followers. As a result, his book was immensely influential. Indeed, when David Hume (1711–76) resolved to write an epistemological essay of his own half a century later, he could confidently assume that his audience was already familiar with Locke's argument.

Hume begins his *An Inquiry Concerning Human Understanding* (1983 [1748]) where Locke stopped. Like Locke, Hume agreed that all human knowledge comes from sense experience, and that the mind preserves sense impressions in the form of simple ideas. But Hume refined Locke's argument by probing the two faculties of the human mind (memory and imagination) in greater detail. Through this discussion, Hume refined some of Bacon's more troubling insights about the fallibility of the human senses and things not being what they seem. From this scepticism Hume fashioned one of the most consequential arguments in modern epistemology: he began to doubt the universal validity of induction. This led him to wonder whether causal analysis was in fact possible at all – a doubt that still shakes the very foundations of modern philosophy of science.

Hume the Empiricist: The Philosophy of Human Understanding

Like Locke, Hume claimed that we use *memory* to preserve and arrange the simple ideas we have stored in our minds. In fact, he held that we preserve these ideas in the exact order in which they entered the mind. He then suggested that we use *imagination* to rearrange and recombine these simple ideas into complex ones. This delegation of responsibilities within the brain raises an important point: since ideas are sequenced by the order they entered the mind, simple ideas cannot be rearranged in any desired manner. In other words, the mind does not function in a random way: human imagination arranges ideas in ordered clusters or sequences. Thus Hume believed that ideas are strung together by a principle of association or attraction. He argued that the identification of associations is common to all scientific endeavours. His discussion of the relationship between association and causation contains some of the most basic insights of modern philosophy of science. And the implications he drew sparked a debate about cause and effect that continues undiminished today.

Whenever we see two events that appear together, we immediately begin to discuss cause and effect, argued Hume. This, however, raises a dilemma for empiricists, as causality cannot actually be perceived. We can observe that A and B occur concomitantly, or simultaneously; but we cannot observe causality itself. It is our imagination, not our perception, that provides the actual (causal) link between A and B. Hume held that our imagination does this because it is our custom or habit to link events, and because the imaginative properties of our minds are capable of providing logical explanations for why B must occur in the aftermath of A.

At the core of Hume's argument lies a psychological claim: namely, that human beings are pattern-finding animals, and the human mind is capable of devising theories, which it then imposes on the world (Popper, 1989, pp. 42ff). At this point, Hume's training as a sceptic comes in with full force.

Hume the Sceptic I: Doubting the Inductive Road to Knowledge

Hume sympathized with Bacon's two claims: (i) that observations supply us with statements about the world; and (ii) that scientific knowledge could be derived from such observation statements. He also shared Bacon's doubts about human beings' frail faculties of observation. The more he turned these doubts around in his mind, the more sceptical he became of the way that scientists often used observation statements as springboards for bold and unwarranted conclusions. He concluded that no number of observation statements, be it ever so large, can produce reliable generalizations. Whereas Bacon had considered general statements to be the reliable children of reason, Hume revealed them as bastards of custom and imagination.

Human knowledge is a flimsy phenomenon, and because of its flimsiness, Hume argued, science needs to treat causal claims with great caution. Strictly speaking, science should not try to explain facts; it should be content with describing them and demonstrating their regular appearance. The reason is obvious: patterns and regularities can be observed, while causality cannot! We can observe facts. We can observe that first one fact (A) appears and that another fact (B) then appears. We can observe that the two facts always appear together. But our senses cannot observe any mechanism by which one fact causes the other. Our imagination, however, can easily enough conjure up some such mechanism, and our reason can make a causal connection credible. Following Hume, we must recognize that causal explanations are nothing more than imaginary. We make them up.

This is not to suggest that all observation is relative: for the naturalist, a Real World does exist. Rather, our perception of this Real World is held together by imaginary notions. John Passmore (1987) provides an example of how we can understand Hume's argument when he asks us to imagine a baby – an exceptionally bright child – whose parents have always given him soft cotton toys to play with. The baby has often dropped these toys out of his crib and they have fallen to the floor with a soft thud. One day his uncle comes to visit and gives the baby a rubber ball. The baby smells it, tastes it, feels it and then drops it out of his crib. Instead of landing softly on the floor, the ball bounces around. The baby is surprised and confused, and begins to cry. For all his careful investigation, the baby's experience with toys is limited to those that land softly on the floor when dropped; he has no possible way of predicting the bouncing behaviour of the ball. This example serves to illustrate Hume's first point: that just by examining a thing, we can never tell what effects it might produce.

To illustrate Hume's second point, Passmore changes the parallax from the baby to the uncle. When he sees the baby drop the ball, the uncle expects the ball to bounce. If you ask him what caused the ball to bounce, the uncle might reply: 'Balls bounce. Rubber balls have the power to bounce when tossed. My nephew tossed the ball and caused it to bounce.' Asked to elaborate, the uncle might say: 'There is a necessary connection between a ball's being dropped and its bouncing. ...' It is at this point that Hume asks his profound question:

> What experience has the uncle had that the child lacks? The uncle makes use of such general concepts as 'cause', 'power', 'necessary connection'. If these are not just empty words, they must somehow refer back to experience. Well, then, what, in the present case, is his experience? How does the uncle's experience differ from his nephew's experience? (Passmore, 1987, p. 147)

Habit is the only difference Hume can find. The uncle has different expectations than the child because the uncle has observed, in many different contexts and over a large number of cases, that rubber balls bounce when dropped. His expectations are hardly conscious, but are derived from custom or habit. The baby is too young to have had such experience.

This explanation seems to answer the question as to why the uncle has different expectations than the child. But it raises another, much more serious, problem: it implies that these habits of the mind are not trustworthy because they do not produce certain knowledge. Habits are merely unthinking products of our minds. If induction is the foundation of science (as, for example, Bacon insisted), then science (Hume implies) rests on a foundation whose stability and carrying capacity are impossible to demonstrate. This implication has baffled philosophers of science ever since. Indeed, throughout the nineteenth century and the first half of the twentieth, it may be fair to say that Hume's argument was the prime skeleton in the naturalist's closet.

Hume the Sceptic II: Ground Rules of Science

If induction cannot produce certain knowledge, and causal explanations are nothing more than habits, justified by human beings' fertile imaginations, how in the world can we perform science? Hume's answer was: very cautiously. Scientists should lower their ambitions. They should not yield to the temptation of trying to explain too much. They should refrain from imposing causal explanations on the world. Science should, in fact, avoid causal claims completely; it should restrict itself to identifying and observing regularities in the world. In short, scientists should focus on correlations. They should identify and map factual correlations – that is, correlations among facts that are directly observable by the human senses.

To explain the realm of science more carefully, Hume drew a basic distinction between two types of knowledge: that based on *facts* (empirical knowledge) and that based on *values* (normative knowledge). *Empirical knowledge* is based on fact, and is the foundation of science. It consists of knowledge about the observable world. It is accessible to all human beings via sensory perception. And all sensible people are in agreement about the basic properties of this observable world. This is the core element of what we have called the naturalist methodology: a Real World characterized by natural patterns that are observable to us (in other words, that we can experience). Over time, humankind has collected much common knowledge about the world from a vast number of simple sense impressions. In contrast, *normative knowledge* is a type of knowledge based on values and beliefs. It can provide no basis for science, because we can say nothing certain about it. It is subjective, since different individuals tend to entertain varying values and beliefs.

This distinction between facts and values – between empirical knowledge and normative knowledge – remains important in naturalist science. It implies that science is based on facts, not on norms. This should not be interpreted to suggest that Hume felt that values and beliefs were unimportant or unworthy of scholarly investigation. His simple point was that they fall outside the purview of science proper. Science can help us to answer questions formulated about empirical events, but it cannot settle normative disputes – these must be left to theologians and philosophers (who, after 2,000 years of debate, still appear to be far from in agreement).

All members of the community of naturalist science will, when push comes to shove, agree with Hume's proposition that science must be based on facts and not on values. Still, few of them would choose to formulate this claim in the draconian terms with which Hume concluded his *An Inquiry Concerning Human Understanding*. If we should reassess human knowledge, if we should:

> run over libraries, persuaded by these principles, what havoc must we make? If we take in our hand any volume – of divinity or school

metaphysics, for instance – let us ask, *Does it contain any abstract reasoning concerning quantity or number?* No. *Does it contain any experimental reasoning concerning matter of fact and existence?* No. Commit it then to the flames, for it can contain nothing but sophistry and illusion. (Hume, 1983 [1748], p. 173)

The Basic Assumptions of the Naturalist Methodology

Francis Bacon, John Locke and David Hume provide us with the basic framework for a modern philosophy of scientific knowledge. In their work, subsequent thinkers have found support for the claims that the world is real; that it consists of independent particulars; that these particular components interact in regular and patterned ways; and that human beings can experience these interactions by way of sense perception. To the basic conceptual frame built by Bacon, Locke and Hume, modern naturalists have added planks and boards of their own. Their additions, however, have hardly altered the basic design of these Founding Fathers, whose main contributions are listed in Figure 2.2.

For example, subsequent naturalists have interpreted Locke and Hume to mean that there *is* a Real World 'out there' – a Real World that exists independently of our senses. This world exists whether human beings are there to observe it (or not); and it may be experienced through systematic sense perception. Such experience and observations can, in turn, be communicated from one naturalist to the next through the reliable medium of language – that is, through clear and precise observation statements. From this, naturalists can access a clear and simple definition of 'truth': a statement that accurately corresponds to a state of affairs in the Real World. This is the famous 'correspondence theory' of truth, which is today often associated with Karl Popper (1994): a 'theory or a statement is true, if what it says corresponds to reality' (p. 5).

Figure 2.2 *Some founding fathers of the naturalist methodology and their main contributions*

Galileo Galilei	1565–1642	*The Starry Messenger* [1610]
Francis Bacon	1561–1626	*Novum Organum* [1620]
John Locke	1632–1704	*An Essay Concerning Human Understanding* [1690]
David Hume	1711–1776	*An Inquiry Concerning Human Understanding* [1748]

Subsequent naturalists have found in Hume an impetus to uncover the regularities of nature and document them as accumulated associations. John Stuart Mill's (2002 [1891]) magisterial *A System of Logic* is typical in this regard. For Mill (1806–73), science involves two propositions. First, knowledge about the laws of nature is acquired through the identification of associations (or, in more modern terms, variable correlations). Second, human knowledge grows over time through the accumulation of observation statements, of tested and true correlations, and of logical argument. New scholars rely on the disseminated texts of their predecessors, using the arguments of their elders as vantage points for their own. In this way, knowledge grows through the generations.

Finally, naturalists have relied on this empiricist epistemology to define a 'theory' as a set of (verified) correlations, logically or systematically related to each other. In the naturalist tradition, 'theory' hinges on a statement which says that one phenomenon (or one class of phenomena) is connected in a certain way with another phenomenon (or class of phenomena). For the naturalist, a theory is a map of associations. Galileo's observation statement that the planets revolve around the sun would be the core of his theory of planetary orbits.

On Doubt and Reductionism: The Cartesian Revolution

The empiricist philosophy that evolved in seventeenth- and eighteenth-century England had parallels elsewhere. In France, for example, René Descartes (1596–1650) shared the basic attitudes of the empiricists of his age. He was an opponent of traditional, scholastic philosophy, and shared with Galileo and Bacon a number of attitudes and new insights about the world and how we can come to know it. Indeed, Descartes pushed to its extreme the idea that the world is a material reality; that human observers can gain knowledge about the world through their senses; and that knowledge can be spread by communicating it to others in crisp and clear language. His *Meditations on the First Philosophy* (Descartes, 1993 [1641]) is an excellent example of this. Not only does he set his own observations before the reader, but he also tries to make the reader engage with the facts. He wants his readers to do more than just passively absorb the information he provides: he cleverly engages them to ensure they understand the importance of the question and then to follow the twists and turns of his argument.

Descartes did not question the key empiricist claim that sense experience is the basic component in knowledge acquisition. Indeed, he sought to capture it more accurately by arguing that sense experience belonged to a world of its own – an outer world of extension that could be captured in geometrical terms. This world of the senses was separate from the inner world of the mind. Descartes elaborated on this distinction between an observable world of extended matter on the one hand and an invisible world of spirits on the other, and these elaborations have gone down in the history of philosophy as Descartes' distinction between body and mind, or the doctrine of Cartesian dualism (Descartes 1993 [1641]). It created a great deal of trouble for Descartes and his adherents, because they knew it was impossible to rely on sense experience alone. Descartes shared Bacon's concern that the human senses are not trustworthy; they must be harnessed by Reason. In fact, the famous 'Cartesian method' is not far removed from Bacon's 'way of the bee'. The difference between the two is often exaggerated (it is commonly claimed that whereas Bacon stressed the importance of induction, Descartes emphasized the importance of deduction); it is important to note that theirs is largely a difference of emphasis – both of them found a place for inductive as well as deductive procedures. Both Descartes and Bacon claimed that the business of science was to produce general statements, cultivate main features and produce simple models of the world.

Descartes, like his contemporaries Galileo and Bacon, assumed that the world ultimately *was* simple. If one could penetrate below the blooming, buzzing complexity of the superficial world, one would find the serene and simple mechanisms of a streamlined design. To arrive at this world, Descartes recommended two epistemological principles: systematic doubt, and reductionism.

The most famous explication of systematic doubt is set out in his *Meditations*. Here, Descartes begins by asking what it is possible to know. But before he begins to build his argument about human knowledge, he argues that we must first cleanse our mind of all former beliefs, because many of these are bound to be false. This claim created an enormous stir in scholastic circles, and members of the Church accused Descartes of wanting to destroy truths, morals and decency. (Sound familiar?)

Descartes responded to the charges with an analogy: he who is worried about rotten apples in a barrel will be well advised to tip out all the apples and then replace each one carefully, inspecting every single apple for damage and rot. Only when he is certain that an apple is sound should he put it back in the barrel. If he makes a single mistake, the entire barrel may be spoiled. Descartes' point is that all claims should be treated as if they were false. We should only add a claim to our stock of knowledge if we are certain that it is true; if we are in the slightest doubt about a claim's veracity, we should reject it.

In 1637, Descartes published his famous book on the scientific method: *Discourse on Methods for Conducting Reason and Seeking Truth in the Sciences* (1973 [1637]). Here he expanded on his second epistemological principle of science: reductionism. This principle holds that you should always build your investigation from the bottom up, beginning with propositions that you know to be absolutely true. Descartes' principle of reductionism is intimately connected to his principle of systematic doubt: begin your investigations into a subject by dividing every extant argument into its component propositions. Ask of each and every proposition: how do I know that this is true? Then, reject every proposition that you cannot verify without the shadow of a doubt – as if they were bad apples. By this process, in due time, you will have reduced the number of propositions about your subject to a few, true, core claims. These few, indubitably certain components will serve as the solid foundation upon which you can then build an argument.

How, precisely, do you build this argument? Descartes summarized his method with three pieces of advice. We have already learned of the first: divide each problem into its smaller, constituent parts. His second piece of advice was to proceed in an orderly and logical way: 'always beginning with the simplest objects, those most apt to be known, and ascending little by little, in steps as it were, to the knowledge of the most complex'. And third, learn from geometry! Look at how the geometricians proceed from a few indubitable axioms and build their arguments step by step, with clear logic and discipline. Observe, writes Descartes (1973 [1637], p. 20; our translation), the 'chains of perfectly simple and easy reasonings by means of which geometricians are accustomed to carry out their most difficult demonstrations', and deduce one thing from another.

Descartes believed that his method of systematic doubt – whose procedures are so well captured by his apple barrel analogy – was the best way to clear the cluttered growth of everyday sense perception and lay bare the simple, basic structures of the Real World underneath. He also believed that this process could be aided by the logical procedures of geometry and algebra. His principles of systematic doubt, reductionism and cool analysis are still basic rules of thumb in the naturalist methodology. Not only do they increase the certainty of an argument, but they also help to make it lean and efficient in form. By eliminating all dubious assumptions, a scientist is left with a simple set of axioms upon which a rational argument can rest logically. It is, in other words, possible to cultivate simplified versions of the world. Indeed, it is not merely possible; it is the only proper way. The only way to penetrate the complexity of the superficial world (and identify the streamlined design of the universe) is to remove superficial details and unnecessary clutter; to reduce the world to a simplified model of essential principles.

There are clear differences between the English philosophers of science and their continental colleagues. To some, these differences are large enough to warrant different labels: whereas Britain's seventeenth- and eighteenth-century philosophers of knowledge are commonly called *Empiricists*, their French contemporaries are often referred to as *Rationalists*. For us, the parallels between these schools of thought are more striking than their differences. Both schools assumed that the Real World is a material fact. Both assumed that this World is orderly and streamlined. Both argued that scientists have access to this world through sense perception. Descartes, who is often identified as a rationalist par excellence, quarrels with none of these key assumptions. The procedures of 'Cartesian doubt' and 'Cartesian reductionism' were adopted by empiricists everywhere – and developed into potent instruments of modern science. The immense analytical powers they represented were greatly augmented by the addition of mathematical techniques – which Descartes also pioneered, and which subsequent scientists such as Sir Isaac Newton applied with immense success.

In the naturalist tradition, this rationalist legacy is clearly evident in today's rational choice approaches. In effect, Descartes planted an intellectual seed that lay dormant for a century and a half, while remaining fertile all the while. Then, with the protection and sustenance offered by David Ricardo (1772–1823), a deductive approach began to take root. From Ricardo (and the modern study of economics) grew rational choice approaches, which have spread rapidly to neighbouring fields of social science.

Rational choice theorists formulate their argument on the basis of axioms. An axiom is a statement for which no proof is required. Because of this, axioms form an important premise to an argument – but they do not, in themselves, furnish a conclusion. Common axioms in rational choice approaches include perfect rationality, transitivity and non-satiety – axioms that are necessary for deriving inference curves that are convex to the origin.

Upon these axiomatic premises lies the logic imbedded in mathematics. It is these rules of logic that allow the modeller/analyst to deduce consequences. In short, the method involves establishing basic axioms that are either true by definition or 'self-evident', and using deductive logic to derive theorems that are not self-evident. In other words, the main role of deductive approaches is to guarantee consistency. The use of logic, the set of rules that preserve the truth of an argument, guarantees that an argument is consistent.

This deductive arsenal is today employed as part of a mind-numbing (shock and awe!) display of formal models and game-theoretic approaches to social behaviour. At their root, these approaches tap into the underlying patterns inherent to nature, as revealed by reason. Naturalists embrace rationalism as an integral part of their effort to explain the social world; they employ rational arguments in the form of theory. These theories are then used to generate testable hypotheses, which the naturalist subsequently tests on the Real World. But for the naturalist, the real proof still lies in the pudding: the explanation that results must correspond with those measurable patterns that are evident in the world.

In pursuing this rationalist/deductive lead we have gone too far ahead of our story. It is time now to return to our earlier focus on the (empiricist) way in which methods are designed to map out, or guide us through, the patterned social world. To do this, we turn to one of the first scholars who sought to carve out an academic field devoted to the scientific study of human society: Auguste Comte (1798–1857). He called this new field 'sociology'.

Post-Cartesian Developments: From Comte to Vienna

Comte's *Cours de philosophie positive (Course of Positive Philosophy)* (1949 [1830–42]) popularized terms such as 'positive perception' to indicate the type of knowledge that was acceptable for science. For Comte, the social and natural sciences shared two important features: the same epistemological form, and both needed to be freed from metaphysical speculation (read deductive approaches). Toward that end, Comte coined the term 'sociology' to designate the science that would synthesize all positive knowledge about society and guide humanity in its search for the 'good society'.

Comte's sociological method hinged on two arguments: one epistemological, the other historical. His epistemological argument involved two simple claims. The first repeated the basic claim of earlier empiricists: that all scientific knowledge about the Real World flows from empirical observation – from sense perception or, as he called it, from 'positive perception'. Comte's second claim was a radical application of Hume's distinction between fact and value – between empirical and normative knowledge. In particular, Comte held that knowledge which does not originate in positive perception – that is, which is not fact-based and empirical – is not knowledge about the world, and therefore falls outside the purview of science. Comte derived his two claims from observing how research was done in the natural sciences, and he saw a logical continuity between the investigation of natural and social phenomena. Knowledge about the social world, he argued, will also accumulate until it slowly arrives at general statements and fundamental insights.

The second argument that sustained Comte's sociological method elaborates on this notion of slowly accumulating knowledge and involves historical evolution. It held that human thought and science has evolved through various 'ways of knowing'. In particular, he mapped three historical phases. The first was a mystical, theological stage – a primitive phase during which human beings tried to understand the world in religious terms. One of its key characteristics was the notion that the world was created by divine beings. The second phase was metaphysical, when humanity tried to understand the world in abstract terms. Its key notions involved abstract principles and ultimate causes. Finally, knowledge proceeded to a scientific or positive phase. Here the search for ultimate causes is abandoned, and humanity instead tries to establish laws. The only way to search for these laws is through systematic, empirical observation.

Émile Durkheim (1858–1917) fully agreed that the purpose of social science was to search for laws in the social world through systematic, empirical observation. He carried Comte's project into the twentieth century with respect to the need to develop more rigorous, empirically-grounded scientific methods. In addition, Durkheim agreed that society is a part of nature, and that a science of society has to be based on the same logical principles as those that characterize the natural sciences. Durkheim – like Comte – longed to cut social science free from the metaphysical tendencies that dominated social thought in the nineteenth century. Toward that end, Durkheim went to great lengths to encourage sociologists to move away from the study of concepts and to focus on the study of things – most particularly, 'social facts'.

Durkheim did this most evidently in his *The Rules of Sociological Method and Selected Texts on Sociology and Its Method* (1964 [1895]). In this he lamented the lack of discussion among sociologists about the proper approach to social phenomena. To address this problem, he suggested that we must start the journey anew, and used the first two chapters of his book to trace these initial steps.

In particular, Durkheim argued that '[t]he first and most basic rule is: *Consider social facts as things*' (1964 [1895], p. 14, emphasis in original). Social scientists need to establish social facts: things that are independent of, and constrain, individuals. For Durkheim, '[a] social fact is to be recognized by the power of external coercion which it exercises or is capable of exercising over individuals' (Durkheim, 1964 [1895], p. 10). Defined in this way, social facts are not reducible to other disciplines – for example, they are not biological or psychological facts; they are socially constructed and collectively maintained constraints (for example, norms, rules, laws, economic organizations, customs and so on). On this premise Durkheim made the case for sociology as an autonomous social science.

For sociology to be a science, Durkheim argues, it has to start with sense perception. To this he adds that senses are not always trustworthy. In doing so, he begins by merely retracing the thoughts of Bacon and Hume on the problems of perception. Then, however, he adds a new concern: the epistemological problems that haunt the natural sciences are multiplied in the social sciences. Social facts, Durkheim continues, are more difficult to observe than natural facts. Social facts do not just appear to our senses; on the contrary, what appears directly to our senses is often illusory or mistaken. For this reason, the layperson is often deluded about the nature of social reality: she often substitutes the 'representations' of social facts for the real thing.

To crack this nut, the sociologist needed to break away from popular perceptions and approach the social world as if for the first time. Here Durkheim follows Descartes' lead in two ways. First, he embraces Descartes' call for reductionism by advising the sociologist to start anew, and build his scientific edifice on sturdier, empirical foundations. Then he makes an explicit reference to Descartes' systematic doubt to explain that the first step in social research is to turn away from all preconceptions and turn attention toward the facts (Durkheim, 1964 [1895], p. 22).

> In the present state of knowledge, we cannot be certain of the exact nature of the state, of sovereignty, political liberty, democracy, socialism, communism, etc. Our method should, then, require our avoidance of all use of these concepts so long as they have not been scientifically established. And yet the words which express them recur constantly in the discussions of sociologists. They are freely employed with great assurance, as though they corresponded to things well known and precisely defined, whereas they awaken in us nothing but confused ideas, a tangle of impressions, prejudices, and emotions. (Durkheim, 1964 [1895], pp. 65–6)

Consider Durkheim's concern with the precision and clarity of language. In the above extract he sounded a loud klaxon to warn against the use of ambiguous terms such as 'freedom', 'democracy', socialism' and so on. Underneath this warning lies the correspondence theory of truth as a bedrock assumption: scientific discussions must be conducted in terms that correspond to phenomena in the Real World – to things well known and well defined. Consider also his famous investigation on suicide. Durkheim's entire argument is built around the empiricist notion that a 'theory' involves a proposition in which one social fact (or class of phenomena; in this case 'suicide') is connected in a certain way with another social fact (or another class of phenomena; in this case 'individualism').

With his *Rules of Sociological Method*, Durkheim sought to provide a sound methodological footing for sociology in particular, and for the other new social sciences in general, but with mixed success. On the one hand, he provided sound advice – such as when he insisted on relying on facts, and using concepts that corresponded to things well known and well defined. On the other hand, he introduced concerns that complicated his task. His distinction between the natural sciences and the social sciences is a case in point. When he argued that the social sciences were different from the natural sciences in terms of the objects observed, he opened up a Pandora's Box in the philosophy of the young social sciences. His distinction was embraced by advocates of more constructivist approaches and used in a vast metaphysical debate that shook the social sciences at the time, and which has since been regularly resurrected by new generations of social scientists.

Durkheim provoked some scholars to wonder whether natural-science ideals were appropriate for the emerging social sciences, and to advocate more humanist and interpretive approaches. These sceptics happily embraced Durkheim's distinction between natural and social objects: they sought to prise the social and natural sciences apart and to sever totally the methodological links with the natural sciences. As we shall see later, some of these sceptics will return to play a larger role in subsequent chapters of this book.

In some ways this was a curious denunciation, as never before had science been able to claim so much progress in so short a time. 'As the century drew to a close, scientists could reflect with satisfaction that they had pinned down most of the mysteries of the physical world: electricity, magnetism, gases, optics, acoustics, kinetics and statistical mechanics, to name just a few, had fallen into order before them' (Bryson, 2003, p. 153). There are reasons to argue that the humanist critique of the naturalist approach was not driven exclusively by academic concerns. The methodological debate that exploded around the fledgling social sciences in the final years of the nineteenth century took place in a turbulent environment. Scientists had produced great feats, but they had also produced great fears. The whole world clanged and chuffed with the machinery that modern science had produced, and societies were changing rapidly as a result; there was a widespread fear that order and morality were unravelling, and that the West was descending irretrievably into a deep crisis. There was also a growing concern that ambitious dictators might harness the insights of modern science for their own nefarious purposes. This latter worry would erupt on a grand scale with the advent of an unprecedented war between the Great Powers of Europe: a war that would engulf the West in a destructive, all-consuming struggle.

Logical Positivism

The First World War brought with it a reaction against all things Prussian – including the Prussian-based philosophy of knowledge. One of the most significant of these reactions emerged among German academics themselves. The result was a leaner and meaner version of empiricism. In the wake of the Great War, in the Austrian capital of Vienna, a small group of German expatriates introduced a tighter and more focused philosophy of knowledge. The members of the so-called Vienna Circle were critical of the abstract and arid nature of metaphysical quarrels, and they strongly opposed what they considered to be the woolly idealism of Germany's philosophy of knowledge (as represented, for example, in the work of Georg Wilhelm Friedrich Hegel's idealistic followers) and the relativism that was increasingly dominating many fronts of human knowledge.

The founder of the Circle, Moritz Schlick (1882–1936), proposed to create a new approach that could provide science with more solid logical foundations. A German physicist, Schlick had moved to Vienna in the wake of Germany's defeat in the First World War. There he was joined by another German expatriate, Rudolf Carnap. These two men were the Circle's driving figures. In addition, Kurt Gödel, Otto Neurath, Herbert Feigl, Philipp Frank, Hans Hahn, Victor Kraft and Friedrich Waismann were all associated with the Vienna Circle and with its philosophical journal, *Erkenntnis*. Finally, it is also necessary to mention Alfred J. Ayer, a young student from Oxford's Department of Philosophy, who went to Vienna in 1932 and sat in on the meetings. He synthesized the discussions in a brilliant little book, *Language, Truth and Logic* (1952 [1936]), through which he became the Circle's most important ambassador in the English-speaking world.

The members of the Vienna Circle were not much interested in metaphysics or in the history of philosophy. Their arguments tended to echo those of David Hume and Auguste Comte. In that sense, their arguments were not particularly revolutionary in content. What was most revolutionary, however, was the form and extreme fervour of their position.

In terms of form, the Vienna Circle insisted on using logic as the primary tool of positive (or naturalist) science. Its members developed a more far-ranging logic, a logic that provided very powerful tools of analysis that the Vienna Circle wanted to turn toward the philosophy of science. In terms of fervour, the Circle tightened and focused the positivism of Comte and Durkheim. Among other things, its members sharpened Comte's already narrow interpretation of Hume's distinction between fact and value.

The fundamental question of the Vienna Circle was: When is an argument scientific? Deeply disturbed by the many ideologues, nationalists, mystics and faith healers who invoked science to support their arguments, members of the Circle searched for a specific and explicit criterion that could distinguish scientific from pseudo-scientific – or 'metaphysical' – arguments. *Fin de siècle* Vienna was one of the most energetic and academically exciting places in Europe – if not the entire world. It was a city of extraordinary talents in the fields of literature, music, art, philosophy and science. City life was famous for its 'nervous splendour', its heady mix of gossip and intellectual brilliance. Among the many topics of Viennese conversation were new academic theories – such as those of the young patent-office clerk, Albert Einstein, who apparently argued that Galileo, Kepler and Newton were mistaken; and those of the smooth and charming young doctor, Sigmund Freud, who claimed he could interpret dreams. The Vienna Circle wanted to know whether these arguments were scientific or not: Was Dr Freud a brilliant doctor or an influential quack? Was Albert Einstein a true scientist?

Moritz Schlick, deeply inspired by the young Austrian philosopher, Ludwig Wittgenstein, imagined that he could settle controversies such as these by identifying a proper *demarcation principle* – that is, a criterion that could distinguish scientific from pseudo-scientific arguments. With such a principle in hand, Schlick hoped he could cut away the intellectually gangrenous tissue of the ailing body of science. Traditional philosophies of knowledge had stressed the role of empirical observations and logic as such demarcation principles. But Schlick was all too aware that pseudo-scientists could also use logic and muster empirical evidence to support their claims. Besides, scientists would inevitably err, while charlatans might stumble across occasional truths. Schlick and his colleagues wanted to hone the arguments of positivism and logic into even sharper tools. They referred to their approach as 'logical positivism'.

The logical positivists subscribed to a single demarcation principle: the *principle of verification*. They argued that all scientific statements had one particular quality in common: that they were meaningful – which meant that they could be subjected to tests that would identify them as true or false. (Statements that could *not* be subjected to such tests were, in contrast, non-scientific or meaningless.) If the Vienna Circle had a basic, founding principle, it was this principle of verification. Using it as their main stick, Circle members beat contemporary scholarship in ways that sent shock waves through the scientific communities, pronouncing Einstein's claims to be scientific while ridiculing Freud's as meaningless drivel.

Karl Popper

Logical positivism's critics came in all shapes and sizes. The young Michael Oakeshott rejected the positivist notion of a unified science as early as 1933, and remained a fierce critic of positivism for the rest of his life. Robin G. Collingwood (1962 [1940]) rejected, almost without reservation, the approach of Ayer and the logical positivists. Collingwood was especially irritated by their short-sighted calls for the elimination of metaphysics, and hurled at them the claim that you can have no knowledge without foreknowledge – as we shall see in subsequent chapters. However, the most significant critic of logical positivism was probably Karl Popper.

Popper lived in Vienna in the early 1930s, but was not a member of its illustrious Circle of philosophers; he taught in a secondary school. Yet, in 1934 he published *The Logic of Scientific Discovery* (Popper, 2002b [1934]), a thick book that levied two objections against logical positivism: one criticizing inductivism, and the other rejecting the verification principle.

Popper was critical of the role of inductivism in the positivist project. He leaned heavily on David Hume: not on 'Hume the empiricist', but on 'Hume the sceptic'. For empiricists, science begins with sense perception and proceeds through systematic observation and the rules of induction toward the development of general laws. Sceptics, however, hold that this argument suffers from a problem of justification: on the basis of observed regularities alone, one cannot use the past to infer any certain knowledge about the future. From the accumulated experience that the sun rises each morning, most people infer the general law that the sun always rises in the morning – and deduce that it will also rise tomorrow. However, this cannot be a logically conclusive inference, because there is no absolute guarantee that what we have seen in the past will persist in the future. The 'law' is ultimately based on an illogical leap of faith – or, to use Hume's expression, on 'habit'.

Popper illustrated this with a simple example using swans. He begins by noting the universal observations (and claims) of European ornithologists that swans are always white (Popper, 2002b, p. 4). However, this inference would be sabotaged by any tourist to the Antipodes who happens to observe the native *Cygnus atratus*: the Australian black swan. The existence of a single black swan is enough to falsify the universal claim that all swans are white.

This argument enabled Popper to launcb a second criticism at the logical positivists: Schlick was wrong in thinking that the verification principle can provide a solid basis for knowledge. The world is simply too vast and varied for anyone to demonstrate a general claim to be accurate and true. On the other hand, Popper continued, it is easy to demonstrate that something is materially false. Rather than a verification principle, Popper argued that science could be defined with reference to a *falsification principle*.

Popper was especially critical of Marxism and used it to illustrate his larger point: for young Marxists in the wake of the Bolshevik revolution, the world was filled with verifications of Marxist theory: 'A Marxist could not open a newspaper without finding on every page confirming evidence of his interpretation of history; not only in the news, but also in its presentation – which revealed the class bias of the paper' (Popper, 1989 [1953], p. 35).

This falsification principle led Popper to criticize another aspect of the logical positivist project: he claimed that they quietly assumed that scientific observation was in itself objective, whereas, in reality, most people tend to see what they want to see. Consequently, any systematic observation of the world is already affected by theory – if it were not, the observation could not be systematic. In light of this argument, the central claim by logical positivists – that a scientist could observe the world and systematically induce general statements from these observations – was impossible. Without theory, we fumble helplessly around in the thicket of trees that is the empirical forest.

Popper has made a deep impression on twentieth-century empiricism and its naturalist methodology. Contemporary philosophy of science still reverberates with at least three of his major arguments: (i) his claim that empirical observation is theory-dependent; (ii) his criticism of inductivism; and (iii) his rejection of the verification principle. These three contributions sank logical positivism and left such a profound impression on twentieth-century science that it is worth looking more closely at their implications.

On Theories

One way of illustrating Popper's argument about the theory-dependence of sense perception is via Sir Arthur Conan Doyle's fictitious detective, Sherlock Holmes, whose stated method of discovery bore an uncanny resemblance to the logical positivists' view of science. Holmes goes out into the world to collect pieces of information. He compares and contrasts facts in order to identify a pattern that constitutes the truth. His findings always astonish his faithful sidekick, Dr Watson, who invariably wonders how Holmes arrives at his conclusions. Holmes' answer is always the same. First, you have to acquire all the necessary facts. Then you must combine them in various ways. Finally, you systematically compare each of the various ways against the events of the Real World and eliminate, one by one, those that are not supported by the evidence. In the end, 'when you have eliminated the impossible, whatever remains, however improbable, must be the truth' (Doyle, 1930, ch. 6).

If Holmes' behaviour is observed more closely, however, there are reasons to think that he is pulling the wool over his good friend's eyes.

Consider, for example, the famous case of *Silver Blaze*, which involved a missing racehorse and the murder of its trainer. Doyle (1927, p. 343) describes how Holmes discovers a key piece of information:

> Holmes took the bag, and, descending into the hollow, he pushed the matting into a more central position. Then stretching himself upon his face and leaning his chin upon his hands, he made a careful study of the trampled mud in front of him. 'Hullo!' said he suddenly. 'What's this?' It was a wax vesta, half burned, which was so coated with mud that it looked at first like a little chip of wood.
>
> 'I cannot think how I came to overlook it,' said the inspector with an expression of annoyance.
>
> 'It was invisible, buried in the mud. I only saw it because I was looking for it.'

In this description, Holmes' approach is not at all a careful, open, methodical survey of the Real World. Rather, he obviously has a theory, and that theory tells him what to look for – a wax vesta – before he throws himself on the muddy ground to begin his search. Holmes saw the wax vesta because he was looking for it. But how would Holmes have known what to look for if he hadn't already got a theory?

On Induction

Popper's notion of the theory-dependent nature of observation was an outcome of his thoughts on 'Hume's problem'. As we have already seen, David Hume had begun to ask the first, awkward questions about whether observations could yield general statements, such as theories and laws. Already by the mid-eighteenth century Hume had pointed out that a number of individual observations – however many – could not logically sum to a general statement that was indubitably true.

The sun may have risen every day in the past, but there is no guarantee that it will also rise tomorrow. A pragmatic physicist might brush this claim aside as idle speculation and retort that we *can*, in fact, be pretty sure that the sun will rise tomorrow. Indeed, by our understanding of the laws of physics and astronomy, it is possible to predict the precise time at which the sun will rise tomorrow. Hume would answer the pragmatic physicist twice over. First, the fact that the laws of astronomy have held good in the past does not logically entail that they will continue to hold good in the future. Second, the laws of astronomy are themselves the outcome of many individual observations of the heavens; they are, in short, general statements produced by induction. Attempts to justify induction by appealing to general statements – which are themselves produced by induction – constitutes a tautology, not a valid argument.

For Popper, then, science is not about finding the ultimate truth. It is a process; it builds on general statements. But where these statements come from is not important. We do not evaluate a theory on the basis of where it has come from; it is evaluated on the basis of its explanatory power. Which, of course, raises the question: how do you *do* that?

On Explanation

Popper's answer is that, first, you have to devise an explanation; that is, you have to make a particular kind of statement that identifies the cause of an event. Second, and more to the point, you invoke a universal law and establish a deductive link between the statement and the law:

> To give a *causal explanation* of an event means to deduce a statement which describes it, using as premises of the deduction one or more *universal laws*, together with certain singular statements, the *initial conditions*'. (Popper, 2002b [1934], p. 38, emphasis in original)

Why did the rope break when we lifted the anchor? If we know that the anchor weighed 25 kilograms and, after some investigation, found that the rope had a tensile strength of 20 kilos, we can easily fashion an explanation. This explanation will contain two kinds of statements: first, we have a statement of universal character (or a law) which says that 'whenever a rope is loaded with a weight that exceeds its tensile strength, it will break'. Then we have singular statements (in this case, two), each of which applies only to the specific event in question: (i) 'The weight that can be sustained by the rope is 20 kilos'; and (ii) 'the weight of the anchor is 25 kilos'. From the universal statement (or law) in conjunction with singular statements (which characterize the specific event and which Popper therefore calls specific or 'initial conditions') we can deduce the cause of the rope breaking.

This way of looking at scientific explanations was made famous by the German-born philosopher, Carl Gustav Hempel (1905–97). Hempel (1965, 1969) recognized that there are inductive as well as deductive types of explanations, but all explanations shared the same general characteristics: they invoked a general law and include descriptions of relevant conditions under which the law is valid. Together, these two components first identified by Popper – the general law and the initial or relevant conditions – constitute the premises (the *explanans*) from which an explanatory statement (*explanandum*) could be deduced (Hempel and Oppenheim, 1948). Together, these components constitute Hempel's definition of science, as presented in Figure 2.3. This view, that an event can be explained by invoking a universal law, is commonly referred to

Figure 2.3 ***Hempel's definition of science***

$$E = f[(C_1, C_2, \ldots C_n), (L_1, L_2, \ldots L_n)]$$

- C_1, C_2, etc. represent 'conditions' or partial facts – that is, statements concerning the conditions under which the law holds true. In the text's example there are two such conditions: the tensile strength of the rope is 20 kilos; and the anchor weighs 25 kilos.
- L_1, L_2, etc. indicate a 'law' – that is, some regularity in nature that can be captured, for example, by the expression 'whenever a rope is loaded with a weight that exceeds its tensile strength, it will break.'
- E represents the explanandum event – the thing to be explained. E, then, is a function (f) of the laws and conditions under which the laws hold true: it results 'from the particular circumstances specified in C_1, C_2, ... C_n, in accordance with the laws L_1, L_2, ... L_n'.

Source: Based on Hempel (1969 [1962], p. 81).

as the 'Popper–Hempel covering theory of explanation', or simply as 'Hempel's *covering law*'.

One of the intriguing characteristics of Hempel's covering law is that explanation and prediction share an identical logical structure: the logic of the law can be used on past events (for which it is an explanation) or to forecast events in the future. From the universal law which says that 'whenever a rope is loaded with a weight that exceeds its tensile strength, then it will break', in conjunction with the initial conditions that (i) 'the rope can sustain 20 kilos' and (ii) 'the weight of the anchor is 25 kilos', we can predict that the rope will break if we try to lift the anchor by using the rope.

Post-Popper

Popper provides us with a justification for keeping our eye on the empirical terrain, but he does so with a firm reminder of the need to position our empirical inquiry in an explicit theoretical framework. By employing a rigid falsification criterion, scientists are encouraged to maintain a critical attitude toward their research object, and to prepare themselves for the possibility of unintended outcomes.

Subsequent work in the philosophy of science has questioned the utility of relying on a simple, or naïve, falsification criterion, as theories can still maintain much explanatory power, even in the face of aberrant facts. While it is an exaggeration to suggest that this is what Popper meant, his position was often interpreted in too stark a manner, with scientists being expected to jettison a theory as soon as it encountered falsifying evidence, and replace it with a new and better theory. As theories can remain strong and viable even in the face of much evidence to the contrary, a simple nod to the facts can never settle theoretical differences. Consequently, scientists have needed to develop more flexible relationships toward facts, theories and demarcation principles.

One prominent approach, associated with the Hungarian philosopher of science, Imre Lakatos, is linked to the concept of 'research programmes'. Lakatos (1999 [1970], p. 115) pointed out that science was not just a two-cornered fight between a particular theory and a deviant fact. It is a fairy tale to believe that a single fact can murder a reigning theory by the simple thrust of falsification. In practice, there are always rival theories waiting in the wings – pretenders to the throne, as it were. Solid science requires that we consider them all; that we assess how all theories, princes and pretenders alike, relate to the facts – how strong is the supporting proof and how damaging the dissenting evidence? In practice, Lakatos argued, the progress of science is a complex tug of war for factual support between a reigning theory and its rivals. To secure the crown, a theory needs stronger support than that for its rivals; it has to be able to explain more than any of the others; and it cannot be killed by a single deviant arrow. As Lakatos explicitly recognizes, this is a significant amendment to Popper:

> *Purely negative, destructive criticism, like 'refutation' or demonstration of an inconsistency does not eliminate a programme.* Criticism of a programme is a long and often frustrating process and one must treat budding programmes leniently. One can, of course, *undermine* a research-programme but only with dogged patience. It is usually only *constructive criticism* which, with the help of rival research programmes, can achieve major success; but even so, dramatic, spectacular results become visible only with hindsight and rational reconstruction. (Lakatos, 1969, p. 183, emphasis in original)

For Lakatos, a research programme consists of contending theories, each trying to make the most elegant sense of a universe of unruly facts; all gathering around what he called a 'hard core'. Scientists in a given research programme circle around this hard core and protect it from falsifying facts by fashioning a protective belt of auxiliary hypotheses. Thus the battle for science occurs between competing research programmes, not between individual facts, theories or hypotheses:

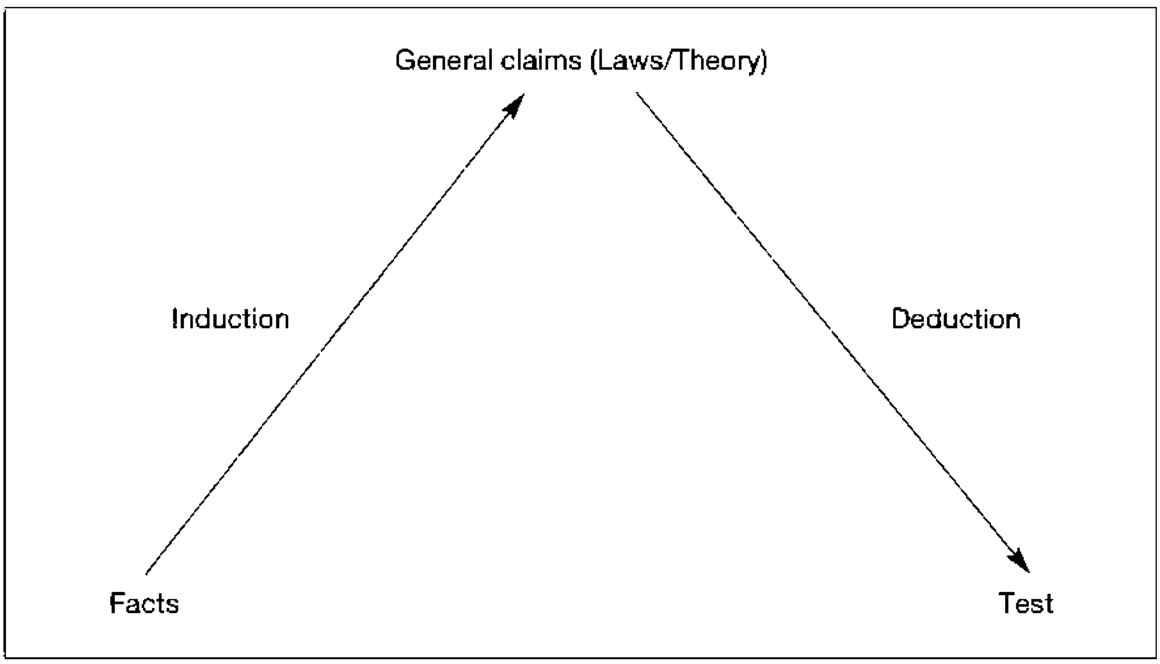
> Newton's theory of gravitation, Einstein's relativity theory, quantum mechanics, Marxism, Freudianism, are all research programmes, each with a characteristic hard core stubbornly defended, each with its more flexible protective belt and each with its elaborate problem-solving

> machinery. Each of them, at any stage of its development, has unsolved problems and undigested anomalies. All theories, in this sense, are born refuted and die refuted. (Lakatos, 1978, p. 5)

Lakatos leaves the modern social scientist on guard. No longer can we wield simple facts and theories in the name of clear truths. Theories do not fall with a single blow from a hard fact. Research programmes are so heavily defended that they lie beyond the reach of a single theoretical or empirical attack. Consequently, the modern social scientist aims to develop arguments in an open-ended fashion. Arguments need to be exposed to the possibility of falsification, and aimed at engaging testable hypotheses that are generated by dominant research programmes. In short, the social scientist needs to employ both falsification and verification in a subtle, nuanced and reflective way.

If Lakatos provides us with the most sophisticated philosophical grounding for the contemporary naturalist approach, most practicing social scientists in this tradition have a simpler understanding of the relationship between facts and theories. This understanding can be depicted in terms of a triangular relationship, but this triangle balances inductive and deductive approaches under a single theoretical rubric. This commonplace approach is depicted in Figure 2.4, where a particular research project is usually engaged with either an inductive (left-hand side) or deductive (right-hand side) component, and where the projects

Figure 2.4 ***Inductive–deductive model***



are usually seen as distinct contributions, like two sides to the same coin, or as iterations over time.

In distinguishing between the upside and downside of this triangular endeavour we are consciously promulgating the myth – 'sired by Kant, foaled by the Vienna School, and raced past us in our statistics textbooks' (Stinchcombe, 1978, p. 4) – that one can fruitfully separate the theoretical from the empirical parts of the research design. We do this because this myth continues to play an absolutely central role in the world view of naturalist social science. In practice, of course, even the most dyed-in-the-wool naturalists recognize that it is impossible to begin an empirical study without theoretical expectations, or a theoretical study without empirical experience – a modest combination of both ingredients is necessary before the researcher can even begin.

In short, the naturalist methodology of modern social science reflects the conceptual history sketched above: it mixes the salvageable parts from Logical Positivism, Popper, Hempel and Lakatos. In describing this development we have attained the tools and vocabulary of the modern naturalist scientist, who goes out into the world in search of patterns and regularities that reside in nature.

The naturalist scientist engages the world with a basic hypothesis in mind – something that needs explaining. (Where this hypothesis actually comes from is not easy to explain, as it involves a complicated juggling process that includes both deductive and inductive processes as depicted in Figure 2.4.) This thing in need of explanation is called the *dependent variable*, and is often denoted as Y. The things that explain changes in the dependent variable are called *independent variables*, traditionally referred to as X.

It has been a long-standing habit among philosophers to depict the relationship between such variables by means of a causal arrow: $X \rightarrow Y$. Naturalist social scientists have depicted the relationship differently, however. Influenced by modern mathematics, they have captured it as a simple equation. Here, the dependent variable is placed on the left side of the equals sign and the independent variable placed on the right. Since reality is complex and a phenomenon we want to explain tends to have many causes, modern scientists must allow for many independent variables (X_1, X_2, ... X_n). Thus modern social scientists tend to depict their propositions in an algebraic expression, like this:

$$Y = \alpha + \beta_1 X_1 + \beta_2 X_2 + \varepsilon$$

Here the dependent variable (Y) is put on the left side of the equation, while the independent variables (X_1 and X_2) are listed to the right. The coefficients (β_1 and β_2) work as a multiplier to depict the relative strength of the corresponding independent variable in explaining observed variation in the dependent variable. In this equation there is also a constant term (α) and an error term (ε). The role that these variables play in explanation will be elaborated on in Chapter 4. For now we need only note that this algebraic expression implies a linear relationship between the dependent and independent variables. This is a very common (if often unrealistic) assumption among naturalist social scientists, but it is not a necessary feature of the methodology itself. It is tradition and the maths-processing skills of social scientists (and their computers) that limit this approach, not the methodology itself.

Recapitulation: The Naturalist Way of Knowing

The founding fathers of modern science have provided us with a powerful philosophy of knowledge. They have also provided a legitimizing philosophy; naturalists gain an argument that they can use to justify their approach. Locke and Hume, in particular, provide the philosophical foundations for the naturalist approach to social science, to which subsequent naturalists have added boards and planks. The next section will examine these foundations and the component elements – the supporting joists – of the naturalist approach.

The Broad Joists of the Naturalist Methodology

Naturalist social science builds on three broad joists – all of them hewn from the trunk of traditional natural science: one is ontological, another is epistemological, and the third is methodological in nature. These are presented briefly in Figure 2.5.

First, there is the ontological joist. Subsequent naturalists found in Locke and Hume an atomistic *ontology* – a clear notion that the Real World consists of independent particulars. They interpreted Locke and Hume to mean that there *is* a Real World 'out there' – a Real World that exists independently of our senses. This world exists whether human

Figure 2.5 ***The three basic joists of naturalist social science***

- An *ontology* of independent particulars.
- An *epistemology* which relies on an idea of accumulated *a posteriori* knowledge of associations (or correlations).
- A *methodology* which seeks to identify regularities in the Real World.

beings are there to observe it or not. Subsequent naturalists have built on this ontological joist a simple definition of 'truth': a statement is true if it accurately corresponds to a state of affairs in the real world. This definition is known as the *correspondence theory of truth*.

The second supporting joist is epistemological. Subsequent naturalists entertain the same *epistemology* as their forebears about the regularities of nature and the drive to document these regularities as accumulated associations. This involves two things. First, it means that knowledge about the regularities of nature is acquired through systematic observations of associated phenomena. Knowledge about the laws of nature is, in other words, acquired through the identification of associations (or variable correlations). This suggests that the ultimate purpose of science is to uncover these regularities and to re-state them as (natural) laws. This knowledge can be gained by reason and deduction, but it must ultimately be confirmed by empirical evidence. Second, the empirical epistemology means that human knowledge grows over time through the accumulation of confirmed correlations. This accumulation is reflected in the growth of increasingly accurate theories.

Finally, there is the methodological joist. Subsequent naturalists have found in Hume a confirmation of the *methodology* of Galileo, Bacon and others. In particular, these authors maintain that the world is filled with many kinds of repetitions and regularities, and the main purpose of naturalist science is to identify these regularities. This means that regularities are observable by the systematic use of human sense perception, and that such observations are communicable.

The Naturalist Hierarchy of Methods

Naturalist science sets out to discover and chart the regularities of the world. Naturalist scholars observe the world, painstakingly collect empirical evidence, then analyse and order it so that they are able to reveal and accumulate knowledge of the regularities of the world. From these tasks, naturalist social scientists seek to account for individual events in the past and predict events in the future. This understanding of the nature of the Real World, and the appropriate way to uncover its truths, has resulted in a firm hierarchy of methods within the naturalist approach to social science.

Francis Bacon and Galileo Galilei rank among the major thinkers in naturalist science. Despite their inductive procedures and experimental designs being probed and amended over the centuries, their basic designs still offer valid models for naturalist ventures. Popper and his followers have not strayed far from these models. Indeed, the experimental design introduced by Galileo and Bacon lies at the very core of the methods preferred by contemporary naturalists. Modern philosophies of naturalist social science are fully congruent with the experimental designs of Bacon and Galileo.

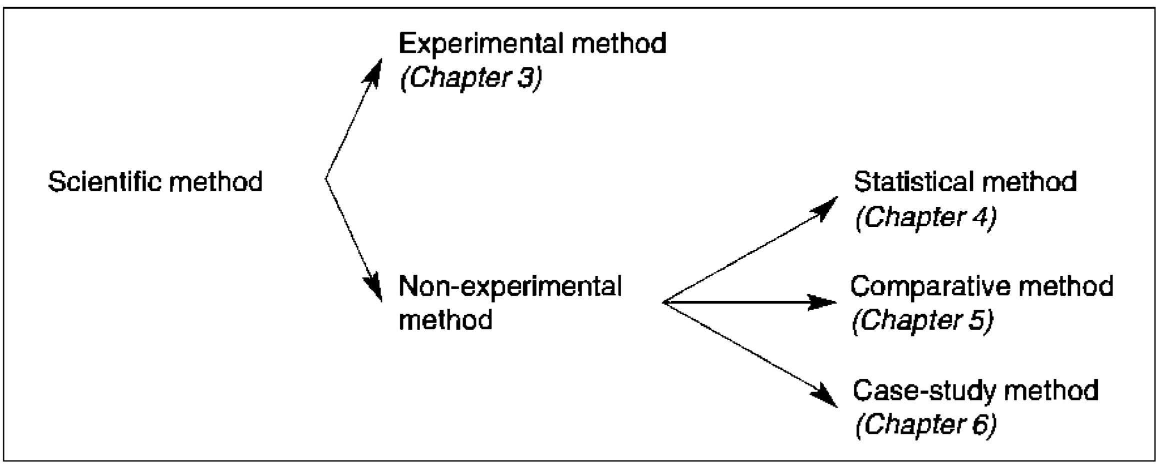
For naturalists, in other words, the experimental method is the ideal – which other methods strive to emulate. This method is ideal because of its ability to control and order causal and temporal relationships. Other methods are less suitable in these regards. Consequently, the experiment ranks as the one true scientific procedure; other methods are deemed to be less accurate or powerful and rank lower on the naturalist scale of preferred methods in social science.

Of course, experiments are often not practical, affordable or ethical. When experimentation is not a realistic choice, naturalist social scientists tend to fall back on the second-best approach: the statistical method. This method tries hard to emulate the basic design of experiments. However, because of a lack of data, even the statistical method can prove impractical, so the social scientist may find it necessary to use a comparative approach designed for a smaller number of observations. In the worst-case scenario, when a research question cannot even be pursued through systematic comparisons, the social scientist may be forced to resort to the case-study or historiographic method, which lies at the bottom of the naturalist's hierarchy of methods. Naturalist social science is expected to employ this method only when faced with a yawning paucity of data.

The existence of such a hierarchy of methods is a commonly entertained notion in the naturalist social sciences. Arend Lijphart (1975) has given this notion a classic expression, as depicted in Figure 2.6.

We employ this hierarchy as a pedagogic device because we wish to emphasize the different roles that methods can play when placed

Figure 2.6 ***The hierarchy of methods in the naturalist tradition***



Source: Based on Lijphart (1975, p. 162).

in different methodological contexts. But it is also interesting to note the different roles that each of these methods can play in investigating different types of causal relationships. For example, Bennett and Elman (2006, p. 457), referencing Brady's (2002) work, note how statistical analyses lend themselves to examining neo-Humean regularity theories of causation, experimental approaches are consistent with counterfactual and manipulation-based theories of causation, while case studies can be used to map out the particular causal mechanisms we associate with more process-oriented understandings of causation.

The first half of the book that follows is organized with Figure 2.6 in mind. Thus Chapter 3 – discusses the ideal, experimental, method. Subsequent chapters will then introduce other methods in descending order of usefulness to the naturalist social scientist: Chapter 4 discusses statistics, Chapter 5 comparisons, and Chapter 6 will describe case studies and historical methods.

Recommended Further Reading

Readers who want to trace the philosophical roots of the naturalist tradition should return to the original: David Hume's *An Inquiry Concerning Human Understanding* (1983) [1748]). The classic formulation of logical positivism is Alfred Ayer's *Language, Truth and Logic* (1952). Karl Popper's (1989 [1953]) *Conjectures and Refutations: The Growth of Scientific Knowledge* is the best portal for accessing his immense influence on contemporary social science. For a more up-to-date introduction to larger philosophy of science issues, read Martin Hollis's *The Philosophy of Social Science* (1994).

Chapter 3

The Experimental Method

In closing the previous chapter we introduced a hierarchy of methods associated with the naturalist approach, an approach that assumes the world is inherently characterized by regularities or patterns. These patterns are made accessible to the naturalist by the systematic use of particular methods or techniques. The most important of these are *control* and *comparison*. Control is used to isolate the cause–effect relationship from other potential explanatory variables, while comparison is used to map regularities with the aim of discovering general laws or patterns. By means of control and comparison, the scientist is able to identify, isolate and explore regularities in the world. This is done – as Hume and Mill insisted – by the systematic observation of that world.

Methods vary in their ability to deal with this type of control and comparison, but none are better at this than the experimental method. No method is better at securing knowledge about causal relationships. It obeys a simple logic and involves straightforward procedures. No wonder it is the naturalist's premier means for obtaining knowledge about the world. Many naturalists – Nagel (1961) and Lijphart (1975) foremost among them – hold that, because of its superior ability to control and compare, the experimental method represents the scientific ideal and constitutes the only truly scientific method.

What is the simple logic at its core? Why is it so effective? And if it is so good at identifying causal relationships, why do we need other methods? Why don't we use it all the time? These are some of the questions raised in this chapter. Let's begin with the most basic question: What *is* this method, precisely?

The Logic at the Core of Experiments

In essence, the experimental method involves two operations rolled into one: a demonstration that when an independent variable (X) is present, then its dependent associate (Y) is also present; *and* that when X is absent, then Y is also absent. This is the core logic of the experiment.

In other words, experiments allow us to focus on particular associations, or correlations/co-variations.

This focus on associations or correlations, as we have already noted, is central to all scientific endeavours. But the prime reason why the experimental method is so effective is that it allows us to control the environment in which the correlations are probed and the causal relationships are tested. This, in turn, reassures us that the relationships discovered are real and direct, and not the result of some accidental (contextual) influence. The simplicity and control inherent in experimentation are the very reasons why it is taken as a model for other methods in the naturalist social science tradition.

Indeed, this method is so central to the naturalist approach that it is difficult to appreciate any naturalist method without first fully understanding the logic of experimentation. This is the view of Ernest Nagel (1961, p. 425f), according to whom 'every branch of inquiry aiming at reliable general laws concerning empirical subject matter must employ a procedure that, if it is not strictly controlled experimentation, has the essential logical functions of experiment in inquiry'.

It is likely that Nagel had the natural or physical sciences foremost in mind, because the social sciences present a number of moral and practical hindrances for experimental research. While we can assume that many generals long for a better understanding of the nature of war, and many ministers of finance would like to find the causes of large-scale recessions, it would be neither cheap nor appropriate to explore these topics through research projects that apply the experimental method.

But resistance to experiments is not limited to ethical concerns. Some methodological traditions are wary of those very qualities of experiments that naturalists embrace: their ability to manipulate contexts with an eye to developing firm knowledge about specific causal relationships. After all, the experiment is an artificial construct: creating an experiment means creating an artificial (and controlled) context. Worse (from the constructivist perspective), the experimenter employs this context in a very mechanistic and manipulative fashion.

This is not a criticism anchored in ethics, or even generalizability (what we shall refer to below as external validity). This is an ontological argument about the nature of the things we study: is the world of social science made up of atomistic, interchangeable parts (like a clock), or is it an organic whole, where the very context provides it with meaning (and where manipulating the context will change its meaning)? While social scientists in the naturalist tradition boast about the great strides that have been made in the design and application of the experimental method in recent years, constructivists tend to claim that its cavalier and ultimately destructive attitude toward context makes it an unacceptable tool. We shall return to this issue in Chapter 12, when we discuss the merits of experiment from a constructivist point of view.

Despite the constructivists' scepticism, the social scientists' use of experimental methods is on the rise. Just a few decades ago, experimentation was largely confined to narrow and applied research agendas (for example, within social psychology and management studies). Furthermore, its practitioners were almost always on the defensive – a posture that might be explained in part by a general recognition of the ethical and practical problems associated with social scientific experimentation, and an intellectual context in which social scientists were more critical of the sorts of damage that experimental control does to the constitutive context of social behaviour.

By contrast, experimentation today has become increasingly mainstream and receives broad support – both academic and financial. The main reason for this lies in the fact, noted above, that experiments provide a strong (perhaps the strongest) proof of causal relationships. When properly conceived, the experimental design provides us with a phenomenally strong basis for inferring causal relationships between variables. Not only are experiments designed to produce secure knowledge about causal relationships via control and comparison, but this design fits perfectly with the empiricist's reliance on observational evidence. After all, experimentation is 'experience carefully planned in advance' (Fisher, 1953, p. 8). Given this compatibility, is not surprising that experiments have been granted a leading role in the naturalist's pantheon of methods.

This chapter aims to explain this important role. In doing so, we have two main objectives. First, we examine the design of the experimental method, with an eye to explaining how it provides internal validity. Second, we aim to examine the accepted strengths and weakness of this method, in light of the design features described in the first section and a small number of influential examples.

Historical and Definitional Preliminaries

Since experiment involves a practical, tinkering element, we might return to one of history's greatest tinkerers: Francis Bacon. He conducted a classic experiment to demonstrate the effect of heat. He began by selecting two iron balls of equal size – just big enough to pass through the hole in an iron sleeve. He heated up one of the balls and noted that it no longer passed through the hole. He observed that the other ball, which had not been heated, still glided through the sleeve. Bacon then made two observations before he drew a general conclusion. First, he observed that the two balls were equal in all respects, except that one had been exposed to heat. Second, he observed that the heated ball did not pass through the hole in the iron sleeve – though it had done so before it was heated – and that the other ball, similar but unheated, still passed through. The general conclusion? That the heated ball had expanded and that the heat was the cause of the expansion.

Bacon's procedure – his selection of objects, his systematic manipulation and observation, and his comparative logic – conforms to the modern experiment in its simplest form. We shall discuss these design details below, as this is the primary objective of the chapter that follows. But it is equally important for us that you think of Bacon's experiment in light of his larger methodological argument. Experiments can provide us with observations about the world, which can then be used to make more general statements. In Bacon's experiment, the objective was not to increase the size of an iron ball, but to understand the general relationship between solid objects and heat. Hence his general conclusion, that solid objects expand when heated.

Galileo's Design

A more famous experiment is associated with Galileo Galilei, who claimed to have dropped different-sized balls from the top of the Leaning Tower of Pisa. Galileo was interested in testing Aristotle's claim that objects of a different weight fall at different speeds. To do this, he developed an experimental process in three neat steps. The first step involved *setting up the conditions* – in other words, selecting the proper objects and arranging them in ways that allowed for manipulation (that is, he selected a set of different-sized balls and carried them to the top of the Tower of Pisa). The second step involved the *systematic observation* of the phenomenon at hand; in other words, throwing the objects off the tower and observing their fall very closely – carefully noting their gathering speed and carefully measuring the time it took for each ball to land. His main observation was that different-sized balls fell to the ground together 'with not so much as a hand's breadth between them'. Galileo's third step was to *analyse his results*. After much careful consideration (where he twisted and turned his observation statements in ways that made them yield the information they held), Galileo came to the conclusion that Aristotle had been wrong: all objects fall at the *same* speed (in principle, if not always in practice).

In general, experimentation is a research procedure that sets up a representation of the world: it involves the isolation of component parts in terms of *conditions* and *variables*. Experiments then manipulate the variables so as to observe (and record) the relations between them. Experimentation allows the observer to control claims made about an object; it allows the observer to check systematically – by wiggling and poking – that the claims made about an object are, in fact, correct. At the most general level, the investigations of both Bacon and Galileo square nicely with G. H. Zimney's general definition of experimentation as an 'objective observation of phenomena which are made to occur in a strictly controlled situation in which one *or more* factors are varied and the others are kept constant' (Zimney, 1961, p. 18, emphasis in original).

But this is not the only reason we have begun our discussion of experiments with Galileo and Bacon. We also wanted to return to history's greatest inductivists to illustrate the important role of hypothesis testing (and hence theory) in experimental designs. As Galileo's example illustrates, an experiment does not begin by setting up a representation of a particular part of the world; it begins with a good reason for doing so!

Experiments, then, start with a proposition, an educated guess, a hunch, an argument or a theory; in short, they begin with hypotheses. Indeed, Bacon was known to criticize his forebears and colleagues for not using hypotheses as a guide in their experimental work. Thus experimentation helps us to answer questions that are inspired by theoretical concerns.

The Classic Design

Like Galileo and Bacon, modern scientists use experiments to better understand the world. Naturalists find utility in this method because it rests critically on an ontological assumption about the existence of naturally occurring patterns in the Real World. It is, after all, these patterns that the experimenter intends to capture. Experiments allow us to construct representations of a particular part of the world, isolate its component parts in terms of conditions and variables, and manipulate these variables in order to observe (and record) any changes in the relations between them.

Since Galileo, the experiment has developed a more formal and explicitly comparative design. The researcher distinguishes between two equivalent phenomena. He then exposes one phenomenon (the *treatment group*) to a stimulus (X), but not the other (the *control group*, which remains unexposed) – as when Bacon applied two iron balls and exposed one of them to heat but not the other. The two phenomena are then compared. Since they were identical before the treatment was administered, any difference between the two must be attributed to the treatment. This method can help the scientist to identify the presence of a distinct cause-and-effect relationship. When done correctly, an experiment can provide a clear understanding of the causal relationship between variables.

There are two main (and related) features of the classical experimental design: control and random assignment. It is these two features that allow the experiment to produce such strong knowledge about the nature of hypothesized relationships. *Control* refers to the ability of the analyst to operationalize both independent and dependent variables, and to measure the impact of a given treatment or stimulus. *Random assignment* refers to the ability of the experimenter to control all extraneous factors – known and unknown, plausible and implausible – that may be linked to the phenomenon of interest. This combination of control and random assignment is critical for securing firm knowledge about causal relationships. For this reason, variations of control and random assignment are employed by all methods in the naturalist tradition. A hypothetical example may prove useful at this point to clarify what we mean by control and random assignment.

Imagine that we have developed a new way of teaching social science methods, and that we want to gauge the effectiveness of this new pedagogy. To test its effectiveness, we can experiment on a group of incoming students to the course. This can be done by dividing the class in half (making sure that this 'division' is purely random). We wouldn't want to divide the group by simply drawing a line down the middle of the classroom, because friends, of similar levels of intelligence, may be sitting next to one another. In addition, we would want to make sure that age, sex, class, income and so on, were randomly distributed across the two sample groups (because these characteristics might influence the outcome). The easiest way to do this may be to flip a coin for each student, and let the coin distribute students randomly between the two groups within the class. In this way, random allocation is used to ensure that the results of our experiment are not caused by some extraneous factor in the sample (such as age, sex, friend-cohort, income and so on).

Once the class has been divided into two equal groups, each is given a test to assess their initial competence in social science methods. This is often referred to as the *pre-test*. The pre-test will give us a baseline from which we can evaluate the effects of the given treatment (in this case, our new approach to teaching methods). We then spend the semester teaching each group of students in a different way: one half is taught using the new technique (this group is the treatment group), while the other half is taught the old way (this group is the control group). When teaching both groups, we make sure that the only difference separating the control and treatment groups is the method of teaching (the stimulus, *X*, or treatment). At the end of the semester we again test each group (a *post-test*) and compare scores. In this way, a control is used to ensure that any observable difference in test results can be attributed to the treatment (the new teaching method).

This example illustrates the basic design of the experimental method, and it is captured schematically as the fourth example in Figure 3.1 (see page 63). It also illustrates the potential explanatory power of the experimental method. The ability of the experimenter to select control and treatment groups with an eye trained on random assignment provides him with a high degree of ***internal validity***. Internal validity refers to the scientist's control over context, such that he can be certain of the causal relationships among them. In the example above, the experiment has internal validity if students in the treatment group score significantly better (or worse) than those in the control group at the end of the semester, and there is no reason to believe that this effect is due to something other than the different teaching methods employed. For social scientists, the provision of strong internal validity is the 'crown jewel of experimentation' (McGraw, 1996, p. 772).

The reason for this lies, rather uncomfortably, in our understanding of causation, which is anchored in Hume. Because we cannot observe causation itself, we must use counterfactual analyses to confirm causal effects. In other words, to distinguish causation from correlation, the experimenter is forced to engage in a counterfactual thought experiment. If two variables are causally related to one another, the experimenter assumes that the absence of the (causal) factor would lead to the absence of an effect. In non-causal correlations, the experimenter does not expect this counterfactual to hold. Though experimenters often neglect to admit it, the internal validity of their experiments depends critically on counterfactuals.

While it is easy to admire the experiment's provision of internal validity, it is just as easy to exaggerate this feature. After all, experimental design still cannot provide us with information about the underlying processes that link treatment and outcome. As we shall discuss with respect to statistical methods (in the next chapter), confirming causal relationships requires that the social scientist considers the mechanisms, or mediators, by which treatment variables cause outcomes. Indeed, while internal validity is clearly the strongest asset of experimental designs, one of the most famous examples of experiments in social science illustrates a major difficulty associated with applying this method to thinking subjects. We are referring to the set of management experiments conducted in Hawthorne, Illinois, in the late 1920s.

In the interwar period, the Western Electric Company was eager to employ new developments in social science techniques to increase the productivity of its workers. Toward that end, the company hired Elton Mayo, a psychologist at Harvard University, to examine whether minor changes in the plant's environment could enhance worker productivity. In 1927, Mayo and his associates travelled to the company's Hawthorne plant near Chicago, and proceeded to set up an experiment. The research group began by randomly segregating workers into two rooms: one containing the treatment group, the other the control group. They then began to introduce a number of treatments to the first room to gauge the effect of these changes on worker productivity. For example, they might have improved the lighting, introduced paintings on the walls, music playing in the room, and so on.

Mayo predicted that worker productivity would increase in the treatment room as new treatments were introduced. He was therefore surprised to note that productivity increased in the control room as well, despite the absence of a treatment there. Worse (for Mayo), it seemed as though productivity at the Hawthorne plant was increasing whether Mayo's team was introducing new treatments or not. Indeed, when Mayo dimmed the lighting in the treatment room and left the workers in semi-darkness, their productivity still increased (every plant manager should be so lucky!). After many sleepless nights, it dawned on Mayo that the workers were not responding to the changes he had so cleverly designed and so systematically introduced. Instead, they were responding to being observed. In other words, the workers reacted to being observed by improving their productivity, regardless of whether they were working in the treatment room or the control room.

This phenomenon has gone down in the lore of management studies as 'the Hawthorne effect'. While familiar to students of behavioural science, it is also familiar to the general public by way of Gary Larson's cartoon of the panicky members of an indigenous tribe trying to hide their microwaves and TVs while yelling, 'Anthropologists! Anthropologists!'

The Hawthorne effect illustrates one of the main problems with the experimental method in social science: when the researcher delves into the world, in order to isolate the features that most interest her, she also alters the nature of that world. To minimize this effect, social science experiments often try to avoid any physical separation of the treatment group and the control group. Thus, in medical research – for example, in experiments designed to gauge the effectiveness of a new cold medicine – the participants themselves do not know the group to which they belong. All participants receive a pill – half of them receive the actual medicine, whereas the others receive a harmless placebo.

Experiments lend themselves to securing strong knowledge (based on sensory perception and observational statements) about the nature of causal relationships. For this reason, they play a vital and central role in the scientist's toolbox of methods. By manipulating the context of a relationship between variables, the experimenter can generate the conditions for studies with very strong internal validity. But this method's very ability and willingness to manipulate the environment means that the knowledge generated by experiments cannot easily be generalized beyond the controlled environment. This leads us to the issue of validity.

Many researchers think of validity as 'truth'. We do not. Hammersley (1990, p. 57), for example, defines validity as 'truth: interpreted as the extent to which an account accurately represents the social phenomenon to which it refers'. There are two reasons why we are not too fond of this. First, we are doubtful of the notion that the goal of science is to produce truth; we rather think its primary goal is to find explanations (at least for naturalists). Second, the above idea of 'truth' is already covered by Popper's 'correspondence theory of truth' (presented in Chapter 2). We don't need two different terms for the same phenomenon. The experimental method offers a more precise meaning to the notion of 'validity' – in fact, it offers two: one meaning that concerns the internal procedures of experiment itself (internal validity), and another that pertains to the relationship between the experiment and the world at large (external validity).

Internal validity means control – it refers to a control of variables so tight that we can confidently say that correlation equals causation. *External validity* means generalizability, or the degree to which we can trust that the lessons learnt from experiments 'in the laboratory' are extendable to the real world. If internal validity is the crown jewels of experimentation, external validity is its Achilles' heel (Iyengar, 1991). Indeed, the very qualities that make an experiment produce tests with strong internal validity (that they are contextually specific), undermines their capacity to generalize: we have no way of evaluating the effects of non-controlled variables once the experiment leaves the laboratory.

Some of this tension can be resolved by employing different types of experiments. For example, it is common to distinguish between field experiments and laboratory experiments. Field experiments occur in a natural situation – Galileo's dropping of balls from the Leaning Tower of Pisa being a famous example. The natural setting allows the researcher to manipulate the relevant independent variables; however, it confines him to contextual variables that can only be controlled in a loose fashion. This assures the field experiment a high degree of external validity, but it also makes it more difficult to control intrinsic and (especially) extrinsic, or prior factors.

Laboratory experimentation is clearly the most controlled method of data collection. A laboratory setting allows the researcher to control certain features in the natural environment as well as to manipulate independent variables in order to observe the effects produced. These types of experiments tend to have a high degree of internal validity, but a fairly low degree of external validity. For an example of the problem of external validity, consider the popular resistance and scepticism toward genetically modified (GM) crops. While the consuming public is fully aware that genetically modified food crops have undergone a phenomenally large barrage of experimental tests to attempt to evaluate (and minimize) their negative effects on human health, they remain sceptical that the lessons learned in the laboratory will continue to hold once these crops are introduced into a natural environment. The very complexity of the natural world makes it impossible to control for all contingencies. Theories help the natural scientist to test the most likely interactions, but consumers are sceptical of the scientists' ability to consider all contingencies, or to generalize safely from the lessons learned in the laboratory.

In the social sciences, the problems of both internal and external validity are complicated by the inability of the analyst to use random assignment at will. Not even the strongest proponents of social science experiments are willing to downplay the ethical and practical difficulties associated with conducting experiments on people, communities and nations. Because of these very real and serious difficulties, social scientists often have to develop alternatives to true experimental design, or what Cook and Campbell (1979) have referred to as *quasi-experiments*.

These alternative designs can be illustrated by returning to the hypothetical teaching example introduced above. Instead of finding a truly random way of dividing our class into two groups (one control, one treatment), we might use non-comparable groups, or groups whose composition is not strictly controlled. For example, instead of dividing one class into two, we might teach the new approach to this year's class, and compare it to the results generated from the traditional teaching approach used in a class from the previous year. Obviously, this approach is not optimal in that there may be several important differences separating the two years – differences that might affect the outcome (independent of the 'treatment').

An alternative quasi-experimental approach could build on strong theoretical expectations about what kinds of students tend to do well in a class of research methods. We might use these expectations to ensure an even (no longer random) distribution of important individual characteristics across the two groups. For example, if we know that women tend to do better than men in methods training, we would want to make sure that each group had an equal distribution of women and men. In this way, we use theory to help us control for expected variation (for example, to make sure that the sample is equally distributed with respect to sex, age, income, class background and so on).

To illustrate the differences between real and quasi-experiments, we can draw on a colleague's graphical depiction. Kristen Ringdal (2001, p. 217) introduces four types of experimental designs, each with a single causal factor (X). The two examples in the right-hand column are real experiments; the two in the left-hand column are quasi-experiments. Within each group the researcher distinguishes between pre-test and post-test designs. The researcher's comparison is reproduced as Figure 3.1.

In the first design (i) we find a single group (with no control), which is only tested after the treatment (X) has taken place. In this design, it is difficult to control for a number of alternative explanations, so the level of internal validity is relatively low.

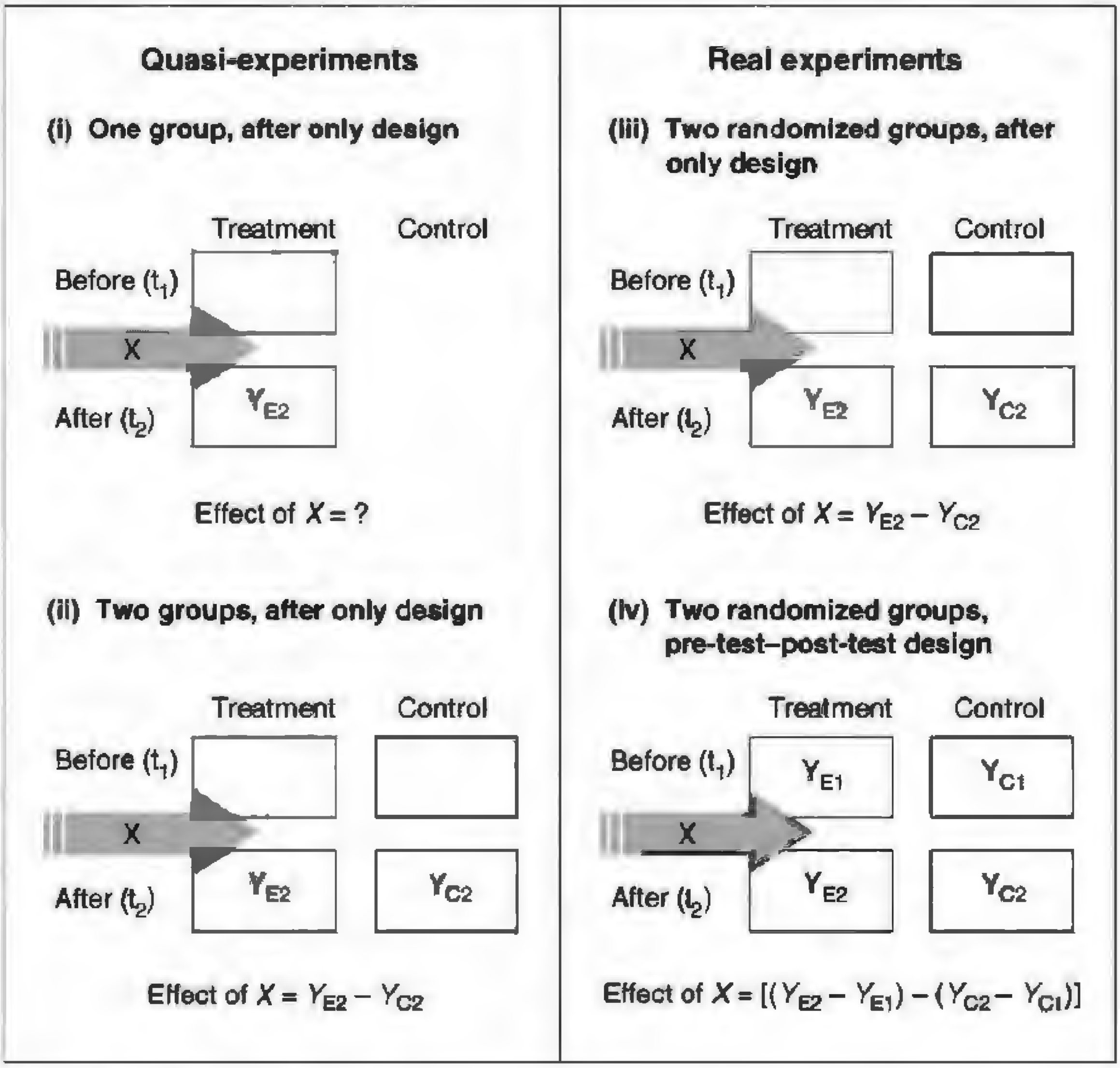
In the second design (ii), the researcher has access to both a treatment and a control group, but the group members are not randomly chosen (this is what distinguishes it from a real experiment). In this design, the first group is affected by the treatment variable (X), but the second (control) group is not. The effect of the treatment is then measured by comparing the difference in outcomes between the control (C) and treatment (E) groups (in other words, $X = Y_{E2} - Y_{C2}$). This design was used in our quasi-experimental example above (where we tested the effect of our new methods teaching approach on one year's students and compared it with the results from the previous year's students).

In the third design (iii) we find the first of two experimental designs where there is a truly random distribution of group members. As this design protects against selection bias, the researcher can be more confident that the different post-test outcomes are caused by the treatment variable (X). On the surface, this design appears very similar to the quasi-experiment design (ii); the only difference is the experimenter's ability to ensure that the control and treatment cases are exactly similar, apart from the introduction of the treatment variable.

Finally, the fourth design (iv) is the most common, as it provides a strong defence against alternative explanations or bias. Not only is there random selection, but the existence of both a pre-test and post-test helps to define, locate and test real causal factors. This is the design that lay behind our initial hypothetical example, where X can be understood as our new approach for teaching research methods, and the effect of X is measured by comparing the change in test scores (post-test minus pre-test) in the treatment group, with those in the control group (in other words, $X = [(Y_{E2} - Y_{E1}) - (Y_{C2} - Y_{C1})]$.

As we shall see in subsequent chapters, other comparative approaches rely on the same sort of design logic as that found in modified or quasi-experiments. Researchers aim to control for alternative sources of variation to be certain that the observed variation is the only one (and hence its cause). By employing quasi-experimental designs, the experimenter accepts a lesser degree of internal validity (relative to a true experimental design). In doing so, however, the researcher can avoid some of the most difficult practical and ethical problems associated with experimentation when employed in the social sciences.

Figure 3.1 ***Some examples of experimental design***



Source: Ringdal (2001, p. 217).

Exemplary Illustrations

The experiment is being employed with increased frequency and recognition across the spectrum of social science. Still, some academic disciplines have proved to be more comfortable than others with experimental designs.

Psychologists have a long history of relying on experiments, and the knowledge generated by these has made significant inroads into neighboring social sciences. In the first edition of this book, we discussed the fascinating experiments that Norman Maier (1949) conducted on rats to develop his frustration–aggression hypothesis, which was subsequently used by Ted Gurr (1970) to explore the reasons behind political rebellions.

While experiments in the disciplines of Psychology and Management Studies might be better known (as the examples from Mayo and Mayer illustrate), political scientists, too, rely on experiments. Some rely on them indirectly – such as Gurr, for example – by borrowing the results of experiments conducted by others and applying their lessons to explore questions in their own fields, while others conduct their own experiments.

Ideological Innocence

One of the best-known experiments in Political Science is the influential 'question wording experiment' conducted by Sullivan *et al.* (1978). Their experiment was a direct response to Philip Converse's (1964, 1970) thesis about 'ideological innocence'. As was common in much of the early (1950s) research on public attitudes and opinion in the USA, Converse held that Americans were innocent, even ignorant, of ideological concepts, and that they lacked true opinions on most policy questions. In the mid-1960s, this concept of ideological innocence came under increasing criticism, with several authors suggesting that American public opinion had become more sophisticated and ideological in its assessments of issues, parties and candidates. These new arguments were largely based on evidence from changing responses to questions in the National Election Study. In particular, after 1964, it would appear as though respondents were becoming more ideologically sophisticated. As the 1964 election was a hotly contested ideological campaign, it made good intuitive sense that voters had become more ideologically aware.

Because of their familiarity with National Election Study questions, Sullivan and colleagues devised an alternative explanation for the (apparent) change in public attitudes. They thought that the changes did not reflect underlying attitudes, but (rather) a change in the way that the questions were framed (after 1964) to gauge ideological competence. To check the validity of their hunch, and to challenge the growing evidence of more ideologically sophisticated American voters, they developed a classic experimental design, where respondents were divided into two groups. Half of the respondents were given pre-1964 questions concerning ideological competence and the other half were posed questions in the new, post-1964, format. The results of their experiment showed convincingly that the observed change in attitude was not related to any real change in the electorate, but rather to changes in the survey questions themselves. This example is one of the most elegant (and most referenced) demonstrations of a cause-and-effect relationship in the social sciences, and it is one that would have been difficult to demonstrate in a non-experimental form.

Media Influence

A second example is provided by the work of Shanto Iyengar and Donald Kinder on the role of the media in influencing public opinion. Iyengar and Kinder (1987) used a series of well-designed experiments to show how the presentation of news affects public opinion in a number of subtle ways. In contrast to much of the (then) conventional wisdom about the minimal effects of the media, Iyengar and Kinder concluded that television news shapes the American public's conception of political life in pervasive ways. Their book, *News that Matters*, offered 'more persuasive evidence than parallel work in the critical, rhetorical, content-analytic, or even correlational schools' (Chaffee, 1989, p. 277).

Iyengar has also used experiments in subsequent studies on the effects of mass media. As described in his 1991 book, *Is Anyone Responsible?*, Iyengar divided respondents into two groups. One group was shown a videotape that included an *episodic* news report on a particular issue problem, while a second group was shown a *thematic* report on the same problem. The issues of crime, terrorism, poverty, unemployment, racial inequality and the Iran–Contra affair were included in the experiments. After their exposure to the videotape, which contained seven news stories including the story that was subjected to experimental manipulation, participants completed a post-test questionnaire that included open-ended questions about the causes and treatment of the problem at issue. A comparison of the episodic and thematic treatment groups revealed that the episodic group's response usually contained more individualistic and punitive attributions and fewer societal attributes.

In yet another piece, Iyengar collaborated with Stephen Ansolabehere and others (Ansolabehere *et al.*, 1994) to construct an experiment that could gauge the effect of negative campaign advertising on voter turnout. By manipulating the tone, but holding all the other relevant aspects of the political advertisement constant, the authors were able to show that exposure to attack advertising decreased voter engagement and participation significantly.

In this experiment, 1,655 actual voters were placed before a 15-minute video clip of a local newscast on an election day, covering several different themes and campaigns (for example, the California gubernatorial race, California Senate races, the Los Angeles mayoral race). Embedded in these clips were 30-second advertisements by real candidates. These clips were identical in every respect, except that the tone and the candidate sponsoring the advertisement were changed in the treatment case (Ansolabehere *et al.*, 1994, p. 830). From the experiments we learn that a person's intent to vote dropped by 5 percent when she was exposed to negative advertising. These findings were then collaborated by a statistical study which showed how voter turnout in the 1992 US Senate campaigns was significantly lower in states that experienced negative campaigning.

Media-related issues lend themselves to experimental design, if only because they are fairly easy to replicate under controlled conditions. Many social scientists have easy access to a deep (and cheap) reservoir of experimental subjects (read students). Better still, it is fairly easy to entice these subjects to sit and watch a short item on television, after which they exchange their impressions (answer a questionnaire) for money. Experiments of this kind are helping us to understand the important role of the modern media in shaping political, economic and social attitudes.

Collective Action

Media-related questions are especially suitable for experimental design. But experimental studies are also becoming more commonplace in other fields of social research – such as in the study of voting behaviour and election turnout, in committee and jury decision-making, and in studies of coordination and cooperation as well as in various bargaining strategies (for surveys, see McDermott 2002; Palfrey 2009; and de Rooij *et al.* 2009). Across the social sciences, experiments are filling the gaps where existing methods of inquiry have produced inconsistent or contradictory results. They are often linked to other methods (as shown in Ansolabehere *et al.*, 1994) to triangulate on specific processes and relationships.

One of the more exciting new areas of experiments concerns work done on the limits to collective action theory. Such theories aim to explain whether and how individuals overcome collective action (or social) dilemmas – for example, in creating or maintaining a public good (Olson, 1965; Ostrom, 1998). In voter studies, these problems are often formulated in the form of a paradox, where voting (or political participation, more generally) is seen as irrational, because the costs of voting for a rational, self-interested voter will normally exceed the expected benefits (Downs, 1957). Still, individuals continue to vote. The question experimenters have tried to answer is: Why?

Behavioural and formal theories of voter turnout tend to explain the paradox in terms of the voters' sense of civic duty. This has never been a very satisfactory explanation for those that tend to embrace rational choice approaches. But a number of lab experiments have shown that individuals tend to cooperate much more than theories of collective action would have us believe. Much of the causal focus has been on the role of face-to-face communication (Sally, 1995; Ostrom, 1998).

More recent field experiments have shown that the way in which voters are mobilized can have an effect on election outcomes.

For example, Gerber *et al.* (2008) used field experiments to find that greater social pressure encouraged people to vote. They did this by sending out a number of mailers to groups of potential voters during an election. One group received a mailing that reminded them that voting was a civic duty; a second group received a mailing that informed them that researchers would be studying their turnout based on electoral records; a third group received a mailing that showed the turnout record for voters in the household; and a fourth group received a mailing that documented both the household's voter turnout, as well as their neighbours' turnout (Gerber *et al.*, 2008, pp. 33–4). The authors found that social pressure (via a neighbour-surveillance effect) increased voter turnout, and that a reminder of civic duty alone was less effective than the real threat of social pressure to increase voter turnout. In the doing the study, the experimenters cast new light on collective action theories and focused attention on the role of surveillance and social sanctions in affecting voting behaviour. While the authors are not 'advocates of shaming tactics or policies, their cost-effectiveness makes them an inevitable development in political campaign craft, and social scientists have much to learn by studying the consequences of making public acts more public' (Gerber *et al.*, 2008, p. 42).

For many social scientists, the appeal of experiments may be dampened by their apparent need for large research budgets (necessary to acquire the requisite computer simulation equipment and data generating processes, to construct relevant laboratory facilities, or simply to pay for willing subjects). But good experimental designs can be simple and cheap. Our favourite example in this regard is the important experimental work done on preventing winter falls (important, that is, for anybody that is crazy enough to live above 60 degrees latitude). Lianne Parkin *et al.* (2009) conducted a simple experiment, published in the *New Zealand Medical Journal*, to test whether socks worn over normal footwear improved traction on icy downhill footpaths. Their study of 30 pedestrians concluded that '[w]earing socks over normal footwear was associated with a statistically significant improvement in traction; the difference in mean self-reported slipperiness scores between the control (n = 15) and intervention [read treatment] (n = 14) groups was 1.3 ...' (Parkin *et al.*, 2009, p. 31). Just as important, '[t]he only adverse events were short periods of indignity for some members of the intervention group' (ibid.). For the social scientist, who is accustomed to more than just short periods of indignity, it would seem that the costs of experimentation need not be prohibitively expensive.

Conclusion

The power of experiment in the naturalist methodology can be traced to its relationship to observation. Observation and observation statements are the premier epistemological devices used by naturalists; experiments place these devices centre stage. More important, observations are seen to be most useful when carried out in a systematic way, and experiments provide this systematization. Experiments allow the scientist to control and compare relevant variables (and contexts) in order to secure knowledge about posited relationships. In those experimental designs where researchers have the most control (for example, in laboratory experiments), the researcher is able to produce remarkably strong and dependable knowledge about specific causal relationships.

This is what attracts many naturalists to the experimental method. But this very characteristic is what makes it such a problem for some constructivists. Traditional experimental designs harvest information at the expense of the context from which the information was originally derived. Because the experimental method is the most invasive and destructive with respect to original context, it is often shunned by scholars in the constructivist tradition. For them, the experiment can seem like an extreme choice of method. Other constructivists, however, have employed experimentation to document the social and political nature of the patterns we study as social scientists, as we shall see in Chapter 12.

Of course, researchers can develop experimental designs that are more realistic, but this gain in external validity tends to come at the expense of internal validity. However, many social scientists are willing to make this trade-off, and sophisticated field experiments are becoming increasingly common. This should not surprise us, as field experiments still allow us to develop remarkably solid knowledge about specific causal relationships. This is because field experiments, like their laboratory brethren, allow the scientist to control and manipulate variations in the most relevant variables.

Still, there are many areas of social life that do not lend themselves to experimental design – whether in the laboratory or out in the field. In some cases, experiments would violate norms of ethical conduct. For example, one does not distribute cigarettes to children to see if they develop cancer later in life. In other cases, experiments would involve such complex, large and expensive preparations as to be practically impossible. For example, an experiment designed to establish the causes of economic development in poor countries would prove terribly difficult to conduct. In yet other cases, experiments would be both practically unfeasible and morally reprehensible. Clearly, for example, we would not want to identify the causes of war through experimentation.

To avoid awkward situations such as these, social scientists have found it necessary to develop alternative tools which try to mimic the experiment in design. In these situations, statistical analysis appears as the next best choice of method, as its access to large numbers of independent observations allows the scientist another (yet similar) means of controlling and monitoring variation. It is to this method that we now turn our attention.

Recommended Further Reading

There are several good introductions to experimental designs in the social sciences. A good place to begin is with Ronald Fisher's classic *The Design of Experiments* (1953). Donald Campbell's work – with Julian Stanley, *Experimental and Quasi-Experimental Designs for Research* (1966), and with T. D. Cook, *Quasi-Experimentation* (1979) – may be particularly useful. For a recent overview of experimentation in political science, see Morton and Williams' (2010) *Experimental Political Science and the Study of Causality.*

Chapter 4

The Statistical Method

Naturalist social scientists agree that their task is to identify patterns and regularities in nature. Applied methods of comparison, or what John Stuart Mill (2002 [1891]) referred to as 'experimental methods', are used to flush out these patterns. While Mill's methods of experiment refer mainly to what we call the comparative method today, they have been elaborated on by statisticians in ways that have secured statistics a very high status in the pantheon of naturalist methods.

While naturalists are able to agree on the importance of identifying regularities in the world, there is a tension among them as to how much we can infer about the nature of these observed relationships. As we saw in Chapter 2, David Hume distrusted causal explanations and cautioned scientists against their use. For Hume, scientists should limit their activities to identifying, observing and charting the regularities of the world.

By contrast, J. S. Mill's faith in the uniformity of nature allowed him to see the Real World as being held together by intricate webs of causal relationships. Despite acknowledging that causality cannot be observed by the naked eye, Mill suspected that some kind of cause will be lurking nearby whenever a co-variation is identified. For Mill, then, co-variation and cause are different things; yet the two always appear together. The presence of co-variation can indicate the presence of a cause – in the same way that the eager fly-fisherman who observes rings on a lake can be alerted to the presence of a trout. The task of the scholar begins by observing the co-variation; he moves below the surface of mere appearances; and concludes by capturing the causal mechanism at work, deeper down.

In short, there is an important ontological difference separating Mill from Hume, and we intend to exploit this difference to distinguish between the two main ways in which statistical methods are used by scholars in the naturalist tradition: *descriptive* and *inferential*. Descriptive statistics are used most frequently to supplement narratives and illustrate claims; as such they are a conventional tool in the naturalist's toolbox of methods. But, as we shall see later, descriptive statistics can also be welcomed by the constructivist scholar. Inferential statistics, however, are a much more ambitious project: they extend the inductive enterprise to infer about the characteristics of a population, in order to generate predictions, provide explanations and test hypotheses. This type of statistical approach is most at home among naturalists, as it replicates many of the design features of the experimental method (examined in Chapter 3).

Descriptive Statistics

Statistics involves the systematic collection of quantitative information along lines specified by the rules of inductive logic. Its etymology is revealing: the term 'statistics' literally referred to information about the 'state' – it was quantitative information for statesmen, about the inhabitants of the country (for example, their numbers, sexes, ages and so on), and those of their enemies. From time immemorial, rulers have tried to assess the number of people over whom they exercise authority. Recall, for example, that Jesus Christ was allegedly born in a Bethlehem stable because King Herod ordered a gigantic census (which required all his subjects to return to their place of birth). Throughout the millennia, the Christian Church has kept baptismal registers, cemetery registers and confirmation books. When these numbers are collected in order to derive some other information – for example, by a ruler to calculate the tax returns of his lands, or to assess the military strength of his nation – this sort of bookkeeping can qualify as statistics.

Pioneers: Graunt, Petty and Conring

While the collection of statistics has been around for a very long time, its modern application can be traced to the seventeenth century. John Graunt (1620–74) was one of the first people to apply numbers in the systematically inductive way that we now recognize as 'statistics'. Though Graunt was by occupation a haberdasher, he seems to have had a morbid preoccupation with death, and a brief account of this preoccupation may help to convey the essence of the method he helped to develop.

Graunt processed death records that had been kept by the London parishes. It was in grouping and regrouping these records according to the various causes of death, that he discovered how large numbers displayed patterns and regularities that were not evident in smaller numbers. He noted, for example, that the proportion of suicides remained remarkably constant over time, and that fatal diseases and accidents (events that seemed to be triggered by pure chance) possessed a surprising regularity. He discovered that the death rates in towns exceeded those in the countryside, and noted that the population was divided equally between the sexes (despite the fact that the birth rate of boys was greater than that of girls – suggesting that the greater birth rate of boys was offset by a greater mortality rate for males later in life).

In essence, Graunt applied various bookkeeping techniques to group facts and statistical records. He collected facts, invented categories and taxonomies for them, counted up the entries (or 'scores') of their different categories, and applied simple arithmetic techniques. These bookkeeping techniques allowed him to describe the general characteristics of a set of data and to derive 'some truths and not commonly believed opinions' (Graunt, 1996 [1662], preface, §3).

Thus from his infamous 'Table of Casualties' in the 1662 edition of his *Natural and Political Observations ... upon the Bills of Mortality*, we learn that the most common causes of death in London at that time were 'Ague and Fever'. We also learn that the least common causes of death during the period surveyed were 'Shingles', 'Stitch' and (our favourite) 'Fainted in Bed'; deaths of this nature occurred only once over a twenty-year period. The *Observations* showed readers how many of the varied causes of death (accident, suicide and various diseases) remained remarkably stable over time, but it also illustrated how the incidence of certain diseases varied greatly over time. Graunt recognized that these diseases were likely to have very particular causes, and he argued that lives could be saved if these causes could be found and removed. On the strength of this argument, Graunt set about creating a system to warn of the onset and spread of bubonic plague in the city.

Graunt died in London – reportedly of jaundice and liver disease – in 1674, but his statistical legacy was propelled by a friend and supporter, Sir William Petty (1623–87). An army physician and professor of anatomy and music, Petty had neither the morbid inclination of his bookkeeping friend, nor his patience for note-taking and systematization. However, Petty did have a scientifically-trained mind and a capacity to marvel at Graunt's discoveries. Thus endowed, he began to speculate about the practical and scientific implications of them. Over time, Petty came to the conclusion that Graunt's method was the *only* viable method for investigating medical, economic and political subjects. He eagerly demonstrated the application of this new method to his friends and colleagues at the newly established Royal Society (of which he was a founder member). Naming this method 'Political Arithmetic', Petty defined it as 'the art of reasoning by figures upon things relating to government' (Pearson, 1978, p. 2).

Petty and Graunt compiled information, sifted through it, classified it, and grouped it in various ways in an attempt to uncover the world's uniformity and hidden patterns. In this they were not alone. Around 1650, Herman Conring (1606–81), at the University of Helmstädt, had introduced a system that allowed him to collect quantitative information about countries and compare them according to size and structure. In addition, he elaborated on the kinds of inferences that could be drawn from descriptive facts concerning the rules of conduct for responsible statesmen – a skill that earned him a profitable reputation among German princes, many of whom hired him as an adviser.

These men instigated a remarkable revolution – but its effect was slow and muted. The eighteenth century saw comparatively few efforts to pursue the scientific promise contained in the works of Graunt, Petty and Conring. Still, there was some activity on the ground, and it was not insignificant. In particular, the early eighteenth century saw new Dutch and English insurance companies using statistics to gauge the probabilities of accidents at sea (in order to establish premiums for ships and cargoes). In France, academic gamblers began to develop more formal theories of probability – first, by systematically observing games of chance; and later by extending their observations to problems of economics, insurance, warfare, politics and medicine.

One of the main reasons for this hiatus in interest may have been resistance to the use of statistics within the scientific community itself. This resistance can be seen in an early attempt to bring statistical methods under the umbrella of British science. In 1830, when it was first proposed that a statistics section of the British Association for the Advancement of Science be formed, the Association found it necessary to appoint a committee to evaluate whether statistics was a proper branch of science. Chaired by Thomas Malthus, this committee soon became divided (as was the entire scientific community at the time). While they could agree that the collection and orderly tabulation of data was consistent with scientific objectives, they were sceptical about whether the statistical interpretation of results was scientifically respectable.

This sceptical view was clearly evident in the motto of the Statistical Society of London (later the Royal Statistical Society), which was formed in 1834. Indeed, their motto – *Aliis exterendum* – can be translated literally as: 'Let others thrash it out' (Cochran, 1976, p. 8)! As shown in Figure 4.1, this motto appears on a binding ribbon around a fat, neatly bound sheaf of wheat. This, presumably, was meant to represent a collection of abundant, well-tabulated data. In short, the scientific community's embrace of statistics was limited to its descriptive capacity. The data collected would be 'objective'; its interpretation would be 'thrashed out' by others.

Basic Concepts and Examples

To get a feel for the power of the statistical approach we need to begin by describing some of its component parts. In particular, we want to look at two subsequent innovations that transformed modern statistics: the

Figure 4.1 ***Emblem of the Statistical Society of London***

Source: The Royal Statistical Society.

explicit phrasing of social science questions in variable terms, and the construction of arithmetical and mathematical formulae designed to capture such variable relationships.

Variable Analysis

As we noted in this chapter's introduction, John Stuart Mill believed it was possible to use inductive approaches to capture causal relations. To do this, the scientist needed to break down the chaos that appears on the world's surface, and distil it into single, well-defined, facts. When this is done, each fact can be related to other facts – one, or two, or a few at a time. Through systematic observation of relationships, and meticulous mapping of co-relations of facts, the uniformity of the world can slowly be uncovered. This is possible, averred Mill (2002 [1891], p. 248), because every observed fact has a cause and this cause will be found in another fact which immediately precedes it. Once a scholar identifies a clear co-variation between two facts – *X* and *Y* – she knows that there are only two simple ways in which this co-variation can logically be understood: either *X* causes *Y*, or *Y* causes *X*.

As an example, let us return to our proposed new approach for methods teaching. In Chapter 3 we showed how an experimental design could prove the effectiveness of our new teaching approach by separating students into control and treatment groups. A statistician might approach the same question from a slightly different angle (as she does not have the ability to actually create control and treatment groups, for whatever reason). The statistician might begin by noting that this new teaching method can only be effective if students actually attend lectures. She then might consider the behaviour of two students: Aurora and Bruce. Both students attend lectures regularly, and both tend to get very good grades. On the basis of this observation our statistician may wonder if there is a more general co-relation at work here. To explore this hunch further, she begins to observe other students to find out how often they attend lectures. Later, she finds out what grades these students get, and searches for the hypothesized co-relation between 'lectures attended' and 'getting good grades'.

The statistician proceeds by ranking the students according to how often they attend lectures. In doing so, she notes that the course consisted of ten lectures and included seven small tests, so that the best possible student score was seven good grades (where 'good' grades are defined as an 'A' or a 'B'). She then compares the attendance and grade scores to see if there is any systematic co-relation across cases. She notes her observations in a data matrix, presented as Table 4.1.

A data matrix is a composite of three different things: units, variables and values. In this example, the *units* of analysis are the people who are observed – in other words, the twenty students (Aurora, Bruce,

Table 4.1 ***Good grades and lectures attended***

Observation	*Students*	*Number of lectures attended (X)*	*Number of good grades (Y)*
1	Aurora	8	6
2	Bruce	7	5
3	Carol	6	3
4	Dina	5	3
5	Elisabeth	3	3
6	Freddy	3	1
7	George	2	0
8	Harry	1	0
9	Irene	2	1
10	Jon	4	2
11	Kim	4	3
12	Lorraine	6	4
13	Mike	8	6
14	Nomsa	9	6
15	Oprah	9	7
16	Peter	10	6
17	Quincy	10	7
18	Robert	10	7
19	Shelly	10	7
20	Thandeka	10	0

Carol and so on). There are two *variables* in this example, 'Number of Lectures Attended' and 'Number of Good Grades' – denoted by variables *X* and *Y*, respectively. As we observe each of the units in turn, we allocate observation *values* to each unit on each of the variables. For example, as we observe that Aurora attended eight lectures, we give her a value of eight (8) on variable *X*; since Harry doesn't have a single good grade, he is given a value of zero (0) on variable *Y*. 'Units', 'variables' and 'values' are some of the most common terms in the modern naturalist trade. Consequently, it is important to know these terms in order to follow discussions in the naturalist approach to social science.

The relation between the two variables in Table 4.1 – number of lectures attended (*X*) and good grades (*Y*) – is clearly visible in this matrix, since high attendance values in column X are associated with high grades in column Y. There is, in other words, a positive relationship between variables *X* and *Y*. (Regarding the anomalous values for observation 20, Thandeka, see the discussion on Figure 4.4 later on in this chapter.)

The central actor in this familiar story is the 'variable'. A variable is something that varies: it is a phenomenon that assumes different (varying) values according to different cases (for example, grades for each student). In the experimental method we can make values vary by manipulating reality. We could, for example, keep some students away from the lectures and compare their grades with students who did attend. But sometimes experiments are not possible: for example, we cannot artificially change the sex of a person, increase his or her age, and so on. We therefore need to create variation by observing many different cases with different values, according to a number of properties (variables).

Capturing Variable Analysis: On Peas and People

The initial establishment of the Statistical Society of London reflected a renewed growth of interest in statistical approaches. Few individuals played a more important role in that resurgence than Sir Francis Galton (1822–1911). Taking a page from the books of Graunt and Petty, Galton began to investigate the distribution of attributes among human beings.

Graunt had measured the world, but he had measured it one variable at a time. His contributions – such as the measures of central tendency and dispersion – were designed to capture the shape or form of a data set collected along a single variable; they pertained to univariate statistics. Galton elaborated on and systematized Graunt's univariate devices – he captured the logic of central tendency and dispersion in statistical formulae; he elaborated on Graunt's notion of 'the average' and refined it by distinguishing between three measurements of central tendency: the 'arithmetic mean', the 'median' and the 'mode'. These contributions, and other central conceptions in statistics, are spelled out in Figure 4.2.

Figure 4.2 *Central concepts in statistics*

- **Arithmetic mean** is a simple calculation for an average measure – the sum of the values of all observations, divided by the number of observations. The mean is commonly denoted as $\bar{x}$, and can be summarized by the formula: $\bar{x} = \Sigma x/N$. If Bob earns £100, Doug earns £150, Sam earns £150, Ed earns £250 and Lucky Eddie wins £650 in the lottery, their total income equal £1,300 and the arithmetic mean equals £260 (i.e. [100 + 150 + 150 + 250 + 650]/5).
- The **mode** is the most common value in a distribution – or, more formally, the value with the greatest frequency (in the example above, £150, because it is the only value to appear twice).
- The **median** is that value which divides a distribution exactly in half – or, more formally, that value above and below which one half of the observations lie (that is, £150).
- The **standard deviation** is denoted by the Greek letter σ (or *sigma*), and is defined as follows: $\sigma = \sqrt{1/N\ \Sigma(x_i - \bar{x}_i)^2}$. The standard deviation is a measure of dispersion, used to capture the spread of scores in a distribution of scores. In the example above, because of Eddie's incredible luck, the standard deviation is a whopping £201, or $\sqrt{1/5\ \Sigma\ [(100 - 260)^2 + (150 - 260)^2 + (150 - 260)^2 + (250 - 260)^2 + (650 - 260)^2]}$.
- The **correlation coefficient** is designed so that it will vary between the values of +1 and −1. A correlation of 1 indicates a perfect positive correlation (so that when one variable is large, the other is also large); when one variable rises (or falls) the other does the same. A correlation of −1 indicates a perfect negative correlation (so that when one variable is high, the other is low); when one variable rises, the other falls. A correlation of 0 means that there is no association (that the variation of one variable has nothing to do with the variation of the other).
- The **dependent variable** is the variable to be explained, usually denoted as *Y*, on the left-hand side of the algebraic equation. Also known as the *response variable*.
- The **independent variable** explains variation in the dependent variable. It is usually denoted as *X*, on the right-hand side of the algebraic equation, and is commonly accompanied by a coefficient (usually denoted β). Also known as the *predictor variable*.
- **Degrees of freedom** is a measure of the number of values in the final calculation of a statistic that are free to vary. This is calculated most commonly as the number of cases, minus the number of independent variables, minus one.
- The $\mathbf{R^2}$ is a summary statistic, varying between 0 and 1, used to denote how well an equation fits the data. When $R^2 = 1$, then all of the variation in the dependent variable is explained by the model being tested (this only occurs in the dreams of experienced statisticians).
- A **spurious relationship** is one in which two or more variables are found to be statistically related (they co-vary), but they are not in fact causally linked. This co-variation is usually a result of coincidence or because of a third (lurking) variable.
- A relationship is said to be **significant** when it is unlikely to occur by chance. This does not mean that the variable is important or meaningful. There are several measures of statistical significance; the most common is the p-value, or the probability of observing data at least as extreme as that observed, given that one's (null) hypothesis is true. The smaller the p-value, the more strongly the test rejects the hypothesis being tested.

Subsequent calculation of the standard deviation built on a concept Galton had pioneered – that of the 'normal distribution', which he defined as a curve in which the mean, the median and the mode coincide. He conceived of it as an ideal pattern for the distribution of attributes in a population. In addition, he elaborated on univariate techniques by expressing his data in terms of figures, and made important contributions to bivariate analysis. In fact, it was in the field of bivariate statistics that Galton made his most significant contributions.

To put a little meat on this skeleton, we can take a closer look at one of Galton's interests: peas. Galton was the original pea counter. His contribution to modern social science techniques had its humble beginnings in 1875, when he sorted sweet peas of different sizes into seven envelopes, marked them K, L, M, N, O, P and Q, and distributed them among his friends. Each envelope contained ten peas of exactly the same size. His friends planted their peas and dutifully tended the plants. In the autumn they harvested the new generation of peas, returned them to the marked envelopes and gave them back to Galton. He, in turn, carefully measured the diameter of each pea down to a hundredth of an inch and noted the results, which are reproduced in Table 4.2. Finally, he compared the notes of these new measurements with the notes he had already made about the sizes of the peas he had distributed earlier among his friends.

Galton summarized his results in a matrix, several pages of drawings and a graph illustrating his main conclusion: that the mean diameter of filial seeds from a particular diameter of parent seeds approximately

Table 4.2 ***Parent seeds and their produce***

Packet name	*Diameter of parent seed (in/100)*	*Mean diameter of filial seed (in/100)*	*Diameter of filial seeds (%)*							
			Under 15	*15*	*16*	*17*	*18*	*19*	*20*	*Above 21*
K	21	17.5	22	8	10	18	21	13	6	2
L	20	17.3	23	10	12	17	20	13	3	2
M	19	16.0	35	16	12	13	11	10	2	1
N	18	16.3	34	12	13	17	16	6	2	0
O	17	15.6	37	16	13	16	13	4	1	0
P	16	16.0	34	15	18	16	13	3	1	0
Q	15	15.3	46	14	9	11	14	4	2	0

Source: Galton (1889, p. 226).

described a straight line with positive slope of less than 1 (see Pearson, 1930, vol. III, p. 3). What he meant was this: that big peas tend to produce other big peas. Galton had, by all appearances, taken a fairly uninteresting topic and made it difficult too! Charles Darwin seemed to agree. In a letter to Galton (dated 7 November 1875), Darwin admits: 'I have read your essay with much curiosity and interest, but you probably have no idea how excessively difficult it is to understand. I cannot fully grasp, only here and there conjecture, what are the points on which we differ – I daresay this is chiefly due to muddle-headiness on my part, but I do not think wholly so' (cited in Pearson, 1930, vol. II, p. 187).

Galton got more attention when he began to count and measure people. In 1886, he published a paper based on measurements of the height of 1,000 people: 500 men and their grown-up sons. This study was designed using the same logic as his pea study. The conclusion was that big men (like big peas) tend to produce big offspring. Not the most surprising of conclusions, yet Galton's argument reverberated through the scientific community and occasioned no less than a revolution in the social sciences.

This reaction might be explained by the fact that a study of people is more interesting than a study of peas. But the reaction was also, in part, because of the *technique* that Galton developed for his second study. Indeed, Galton introduced a new way of thinking about social-science phenomena – a way that allowed him to visualize his two observations in spatial terms. Furthermore, he expressed his new vision in an algebraic formula, termed the 'correlation coefficient' (see Figure 4.2). Galton's correlation coefficient provided the social sciences with a standard measure, according to which its practitioners could assess the strength and direction of a co-relation (or co-variation or correlation) between two variables. This technique, and the way of thinking that undergirded it, allowed social scientists to demonstrate the patterned variations of their units of analysis in new and convincing ways.

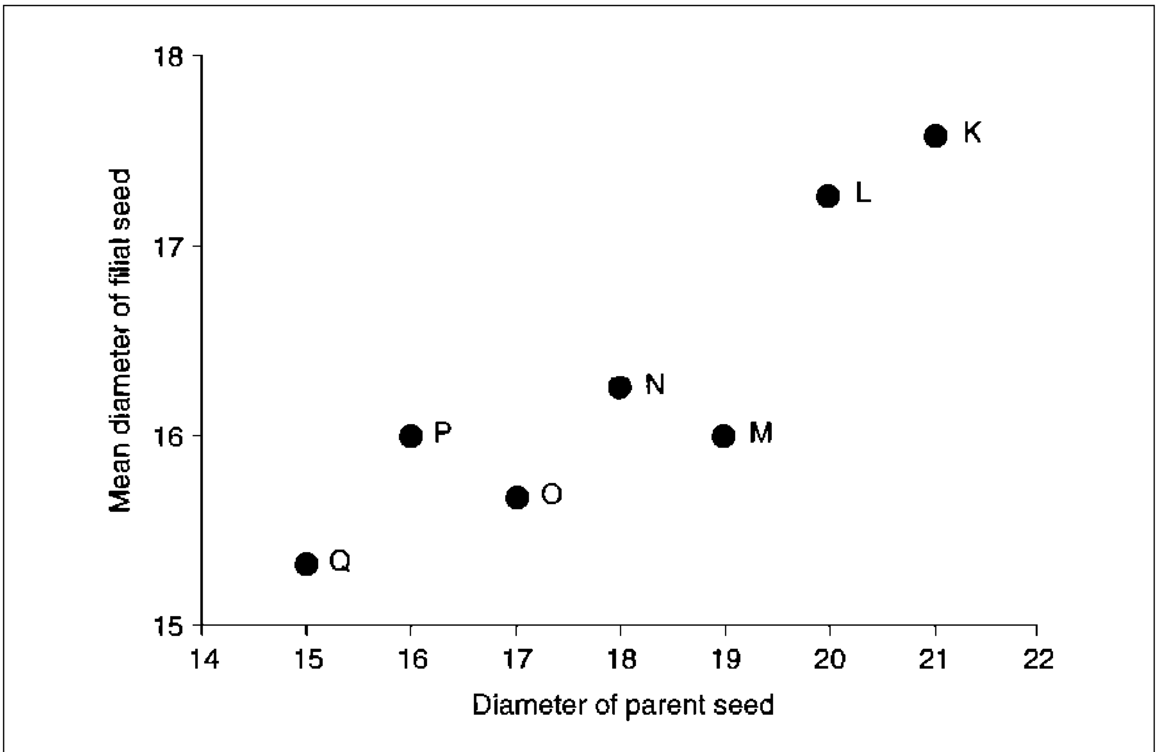
Galton's new visualization technique is easy to follow if we apply it to his earlier data on seven packets of sweet peas rather than on the more complicated set of 1,000 fathers and sons. The observations he made of his peas were summarized in Table 4.2 above, reproduced from Galton's 1889 book *Natural Inheritance*. This table offers hours of excitement for any devoted pea counter. He will, upon the sight of it, rub his hands in joy and immediately begin to draw the distribution curves of these packets of seeds – individually and in various combinations, calculate their spread, their central tendency, their standard deviation, and so on.

For our present purpose we shall limit our focus to the first two columns in Galton's table: the columns labelled 'Diameter of parent seed' and 'Mean diameter of filial seed'. If we study the numbers, it seems pretty obvious that big parent peas tend to produce big filial peas. We see that the largest parent seeds (in packet K, whose peas measured 21/100 inches in diameter) produced the largest filial seeds (17.5/100 inches as an average); and that the smaller the parent seed – from packets L, M, N and so on, in descending order) produced filial seeds of a steadily declining average size. At the smallest end we find parent seeds (in packet Q with a diameter of 15/100 inches each) which produced the smallest filial seeds (with an average diameter of 15.3/100 inches).

Exciting as this is, we can do more. In addition to describing one variable or comparing single variables, we can co-relate them. Such a co-relation is depicted in Figure 4.3, where the column 'Diameter of parent seed' is measured along the horizontal axis, and the column 'Mean diameter of filial seed' is measured on the vertical axis. The figure shows very clearly how big parent peas produce big filial peas. This graph is, in effect, Galton's invention. He developed it by making two clever moves. The first was to transform the values from a data matrix into a set of coordinate points. The second move was to plot these points into a Cartesian graph. Presto! Galton had invented the 'scatter plot'.

To appreciate more fully the brilliance that lay behind Galton's two moves, it is necessary to return briefly to René Descartes. Descartes'

Figure 4.3 ***Parent seeds and their produce***



Source: Based on Galton (1889, p. 226).

Discourse on Method had suggested to Galton the true importance of converting his data matrix into a spatial graph. When Galton read Descartes, he was amused by the author's story of how he once lay ill in bed and watched a fly walk on the ceiling above him, a ceiling that consisted of square tiles. As Descartes watched the fly's movements, he was struck by the thought that he could describe the position of the fly by considering each ceiling tile as a coordinate point – in other words, as a point where a horizontal line (or row) of tiles crossed a vertical line (or column) of tiles. On the strength of this idea, Descartes developed the concept of reference lines and coordinate points. Galton pursued Descartes' logic and applied the notion to pairs of variables. Galton's original presentation of this material was made before the Royal Institute and published in *Nature* (1877). Actually, this publication does not include a scatter plot of the pea data. However, from Karl Pearson (1930, vol. III, pp. 3–4) we know that Galton had used the pea data to produce not only a scatter plot, but also the world's first regression line. At any rate, ten years later – in 1886 – we find its graphical presentation in a paper on people (instead of peas). Galton had collected data on the heights of fathers and their eldest sons and plotted all the individual values on to a Cartesian graph. Galton ended up with a scatter plot which showed him that big fathers (like big peas) produce big offspring. But because his later study included a Cartesian graph, Galton had acquired a more powerful tool of analysis and he could perform a more penetrating analysis.

In this history lie the roots to modern regression, so it is worthwhile to recap. In his early work with peas, Galton used measurement techniques such as arithmetic mean and standard deviation to show that big parent peas tend to produce big filial peas. Ten years later, he used the correlation coefficient and a scatter plot to demonstrate that tall men tend to have tall sons. In addition, he showed that very tall men tend to have sons who are fairly tall, but not as tall as themselves, whereas very small men tend to have sons who are bigger than themselves. With his new Cartesian tool in hand, Galton could formulate this insight in a new, simple and revolutionary way: filial size regresses toward the mean of the race. Galton had discovered *regression analysis*, the workhorse of modern social-science statistics. We shall return to this workhorse in the next section.

Galton's statistical techniques are today universally applied; they are included in the analytical armoury of every serious social science student. Galton's subsequent influence rests on a number of factors, but we shall focus briefly on three of these.

First, he popularized statistical measurements, such as the correlation coefficient, on which his fame deservedly rests. Second, he made scholars critically aware of the dangers of comparing fundamentally similar units. In 1889, when Galton was president of the Royal Anthropological

Institute, he attended a talk by Edward Tylor, who had collected historical information on marriage and descent for 350 cultures and claimed to see in the data a similar evolutionary pattern across cultures. Galton objected to his findings, and challenged Tylor to demonstrate the independence of each unit – to which Tylor could not respond. Galton argued that the similarity between cultures might be the result of borrowing, or common descent, or some other common factor. Without controlling for borrowing and common descent, Tylor could not make valid inferences regarding evolutionary development. The dangers associated with comparing similar units have since gone down in social science lore as the eponymous *Galton's Problem*.

Finally, Galton taught others. He recruited and taught other men who, in turn, contributed further to the development of modern statistics. Foremost among them was Karl Pearson (1857–1936), who pioneered the study of frequency curves, elaborated techniques for measuring correlations – such as the 'chi-squared "goodness-of-fit" test' – and coined important terms in the statistician's working vocabulary (for example, 'standard deviation'). Pearson continued the statistical work of his mentor, recruited talented students and gave them projects to work on. Many of Pearson's students, in turn, pioneered new methods and techniques. One of them was W. S. Gossett (1876–1937). Better known by his pseudonym, 'Student' (as in '*Student's t*'), Gossett worked as a chemist for the Guinness brewery in Dublin in 1899, and developed methods for measuring the quality of ingredients on the basis of small samples. This *t* distribution is particularly important for interpreting data gathered from small samples when the population variance is unknown.

This early application of statistical methods was aimed mainly at *describing* relationships. But Pearson's developments, in particular, began to push statistical studies in a more inferential direction. While these developments have had an enormous impact on the way statistics are used as part of a larger, inferential, project, Pearson himself was quite clear about the limitations to his 'scientific' approach: 'Science of the past is a description, for the future a belief; it is not, and has never been, an explanation, if by this word is meant that science shows the necessity of any sequence of perceptions' (Pearson, 1892, cited in Sayer 1992, p. 193).

Inferential Statistics

By the end of the nineteenth century, developments in statistical techniques were propelling the method into new, more explanatory, realms of science. No longer was statistics confined to simple Political Arithmetic, or numerical descriptions of the world. Statistics gradually became more connected with characterizing (and implicitly, explaining) the relationship between two (or more) variables. Consequently, the role of modern statistics is increasingly associated with attempts to infer beyond the data to something (laws, theories, hypotheses) that is not directly observed.

This new mode of describing the world was quickly seized on by social scientists. Among these was the French sociologist, Émile Durkheim. While Durkheim didn't develop any new statistical techniques, he placed statistics at the centre of social scientific activity.

Durkheim's *Suicide* serves as a useful example. The study begins by demonstrating how different countries in Europe have different rates of suicide. For example, Durkheim established that the suicide rate in England was twice as high as in Italy, and the rate in Denmark was four times the English rate. From these observations, Durkheim demonstrated that suicide is unevenly distributed across countries. In addition, he found that the suicide rate remains fairly stable in any given society from year to year. Suicide, then, 'is not simply a sum of independent units, a collective total, but is itself a new fact *sui generis*, with its own unity, individuality, and consequently its own nature' (Durkheim, 1952, p. 46). Suicide is, in effect, a 'social fact'.

If we acknowledge that suicide is a patterned phenomenon, how can we account for its pattern? Durkheim argued that, if we systematically investigate the various European societies with an eye to *other* patterned phenomena, we should, sooner or later, be able to identify co-variations between suicide rates and other patterned phenomena. Thus Durkheim was struck by the evident fact that suicide co-varies with religion – and he demonstrated (through the use of statistical tables) how the suicide rate was systematically low in Catholic countries, while being systematically high in Protestant countries (and that countries with mixed populations of Catholics and Protestants tended to have rates in between these extremes). Religion, then, must have something to do with the patterned distribution of suicide.

At this point, Durkheim no longer used statistics for descriptive purposes alone; he also used it to develop explanations. He actively engaged statistical findings to probe arguments and build theories: since Catholicism and Protestantism condemn suicide with equal severity, it is unlikely that the character of the doctrine or beliefs affect a country's rate of suicide, Durkheim reasoned. However, since the two religions differ systematically in social structure, this might provide a clue, he argued. In Protestantism, the individual is alone with God; but in Catholicism the individual has a priestly hierarchy between himself and the deity. Thus, whereas Protestantism is severely individualistic, the Catholic Church represents a 'more strongly integrated' social hierarchy.

The degree of social integration of the Church, then, can account for differences in suicide rates across European societies. With this proposition, Durkheim established a causal generalization linking suicide to social solidarity in churches. In particular, the suicide rate of a religious community is inversely related to the level of social integration in that community – the more strongly integrated the religious society, the lower its rate of suicide.

This claim led Durkheim to suspect that the connection between social integration and suicide can be extended further – that the principle of social solidarity applies not only to religious communities, but also to communities more generally. For example, he noted that the suicide rates of unmarried people were generally higher than those of married people of comparable age. Marriage may involve burdens and responsibilities that single people do not have, yet marriage is also a small community with integrative mechanisms of its own that have a protecting influence against suicide.

Durkheim's work offers a good bridge from descriptive to inferential statistics, as he used national suicide statistics to conjure up a more general explanation about the social foundations for suicide. It is this sort of inferential application of statistics that has become one of the hallmarks of modern naturalist social science. Indeed, one of the most influential recent texts in social science methods, King *et al.*'s *Designing Social Inquiry* (1994, p. 8), argues that 'the key distinguishing mark of scientific research is the goal of making inferences that go beyond the particular observations collected'. While we find this to be a rather narrow and unsatisfying demarcation principle, King and his colleagues believe that there is a single logic of explanation common to all empirical social science research, and that this logic is statistical. Their intent is to proselytize small-N social science researchers to adopt the logic of statistical inference.

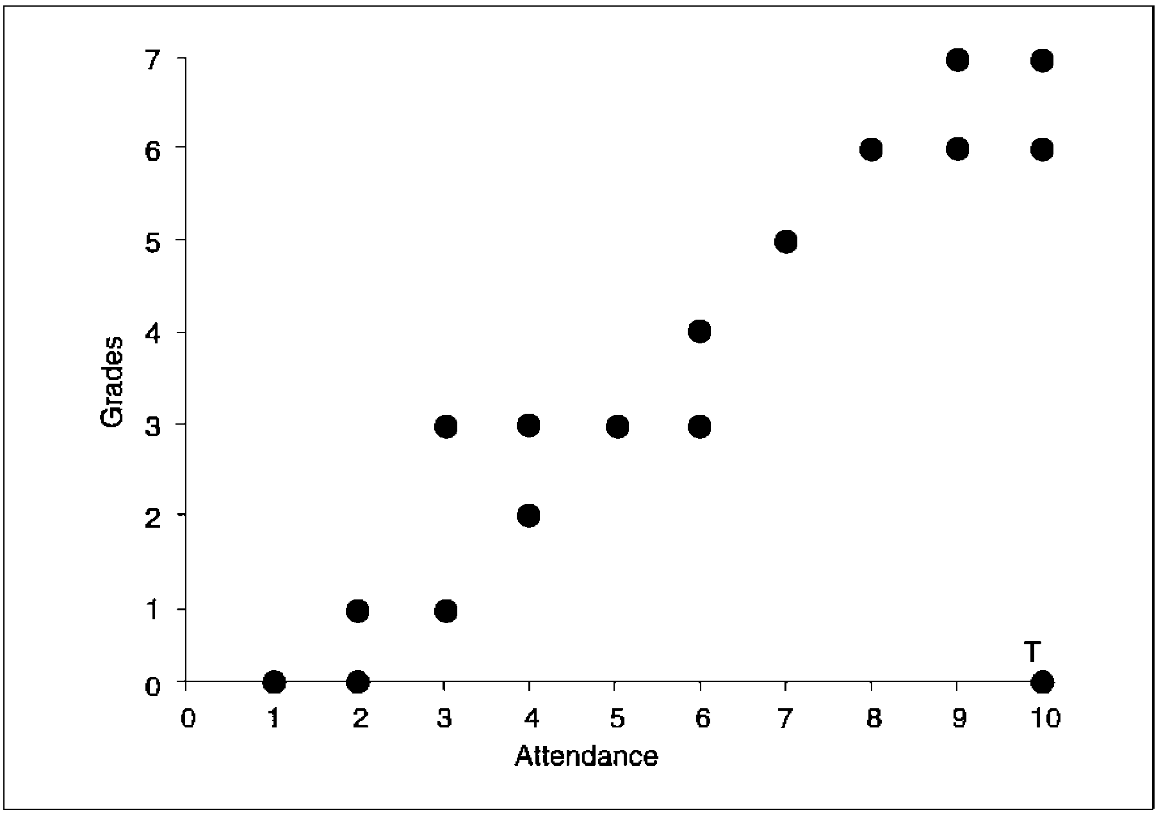
The workhorse of modern statistical inference is regression analysis. Regressions allow us to predict the value of a dependent variable (the '*Y*', or the variable to be explained), given the value of an independent variable (the '*X*', or the explanatory variable). Generally speaking, regression analyses are of two types: bivariate and multivariate. Bivariate regressions, like correlational analysis, provide a depiction of how changes in the level of a single independent variable are related to changes in a dependent variable. Multivariate regressions allow us to expand on the number of independent variables.

In the name of simplicity and clarity, we shall begin with a simple bivariate example to describe the general logic of the method. We shall then add additional explanatory (independent) variables to illustrate how the mathematical manipulation of data allows the analyst to control for the effects of a variable that cannot be controlled in practice.

Let us return to the class attendance example from earlier in the chapter, as it can help to illustrate simple statistical relationships. Most of us have fairly strong prior experience of the factors that influence grades. Off the top of our heads, we can conceive of several possible factors that influence them: time spent in the library, time spent doing homework, class attendance, level of education, social status, gender and so on. As a first cut at the problem, we begin by examining how an individual's class attendance is related to grades. Of course, in framing the question in this way, we are ignoring other important causal influences (that is, the model is mis-specified, or it suffers from omitted-variables bias), but our primary purpose here is pedagogic, not scientific.

To test the relationship between attendance and grades, we first need to consider how to measure each variable, collect data on both, and then map them in a two-dimensional space. The first two steps have already been taken (in Table 4.1). For convenience, we can provide these variables with shortened names, such that the number of good grades is shortened to the variable name *GRADES*; and the number of lectures attended is abbreviated by the variable name *ATTENDANCE*. The third step is produced as the scatter plot in Figure 4.4, where each point in the diagram represents an individual in the sample. It is customary to place the dependent variable on the y (vertical) axis.

Figure 4.4 ***The spatial relationship between class attendance and good grades***



From this simple scatter plot, a trained statistician will see a clear relationship between attendance and grades. This relationship is captured by the apparent pattern in the scatter plot of individual observations: individuals who attend classes more often tend to do better in the class (that is, get more good grades). This is evidenced by the fact that the data are clustered in a line-like cloud that appears to stretch upward and to the right.

Before the statistician proceeds to quantify this relationship, however, she will need to deal with a particular observation in the scatter plot – one that doesn't seem to fit the general pattern. This observation, labelled 'T' (in the bottom right-hand corner of the graph) corresponds to the 20th observation in Table 4.1. For some inexplicable reason, Thandeka seemed to have attended all the lectures, but didn't secure any good grades. Before the statistician can proceed, she must decide what to do with this *outlier* observation.

After contacting us, the statistician discovers that Thandeka is the daughter of one of the teachers, and that she was forced to attend lectures every week (as the class was offered very early in the morning, and the teacher in question was not able to secure a babysitter). For this reason, Thandeka's attendance had been perfect, but she never delivered any work to be graded (hence her 'lack' of good grades). Because Thandeka's experiences are not directly relevant to understanding the relationship between attendance and good grades, the statistician can discard this observation from the subsequent analysis. Unit T can be deleted because it is understood to be an irrelevant outlier.

To generate an estimate of how many better grades might be secured by attending an extra lecture, we can develop a mathematical expression that captures this relationship. To do so, we need to think about the relationship in terms of interpreting Figure 4.4. For the sake of simplicity, we'll assume that the relationship is linear (in other words, that each additional lecture attended delivers the same payoff in terms of good grades). This assumption is not problematic when looking at these data (which 'line up'), but it might be very problematic if the data should reveal another pattern (or given alternative theoretical expectations). Unfortunately, this rather common assumption is a legacy of the limits to regression analysis in the pre-computer era. Contemporary statistical programs allow us to think of these relationships in much more sophisticated terms (for example, quadratic or cubic), but the weight of history bears down heavily on the shoulders of statisticians – at least in this particular case.

In the language of statistics, we can summarize the hypothesized relationship depicted in Figure 4.4 as:

$$GRADES = \alpha + \beta ATTENDANCE + \varepsilon$$

where α is a constant term (the grade a person can expect to earn without attending any lectures); β is the effect on grades (in number of 'good grades') of attending an additional lecture; and ε is an 'error' term which is used to capture the effect of other factors on grades. We use the Greek letters (α, β and ε) to remind us that these are estimates generated by the analysis – they are *not* directly observable. We only have observations of *GRADES* and *ATTENDANCE*. Because it does not make sense to speak about a negative number of good grades, or attending lectures a negative number of times, we can use this knowledge to set the lower limit, or baseline, to the relationship. In particular, we shall constrain the constant term (α) to zero.

In this relationship, the dependent variable (which we are aiming to explain) is *GRADES*; the independent (explanatory) variable is *ATTENDANCE*; and the β term is referred to as the coefficient (in this case, for our independent variable, *ATTENDANCE*). As the β coefficient is positive, we are assuming that the relationship between grades and attendance is positive (in other words, more attendance leads to better grades). If we expected a negative relationship (more attendance leads to poorer grades) we could capture this with a negative coefficient (for example, $GRADES = \alpha - \beta ATTENDANCE + \varepsilon$).

To generate the estimates for α and β, we begin by ignoring ε (in fact, we simply assume that it is, on average, equal to zero). We then try to fit a line that comes closest to all of the points in Figure 4.4. There are a number of ways to do this, but we shall focus on the most common (minimum sum of squared errors) approach. This line will intersect the y axis at a given point (this is represented by α, the constant), and the line itself will have a slope of β.

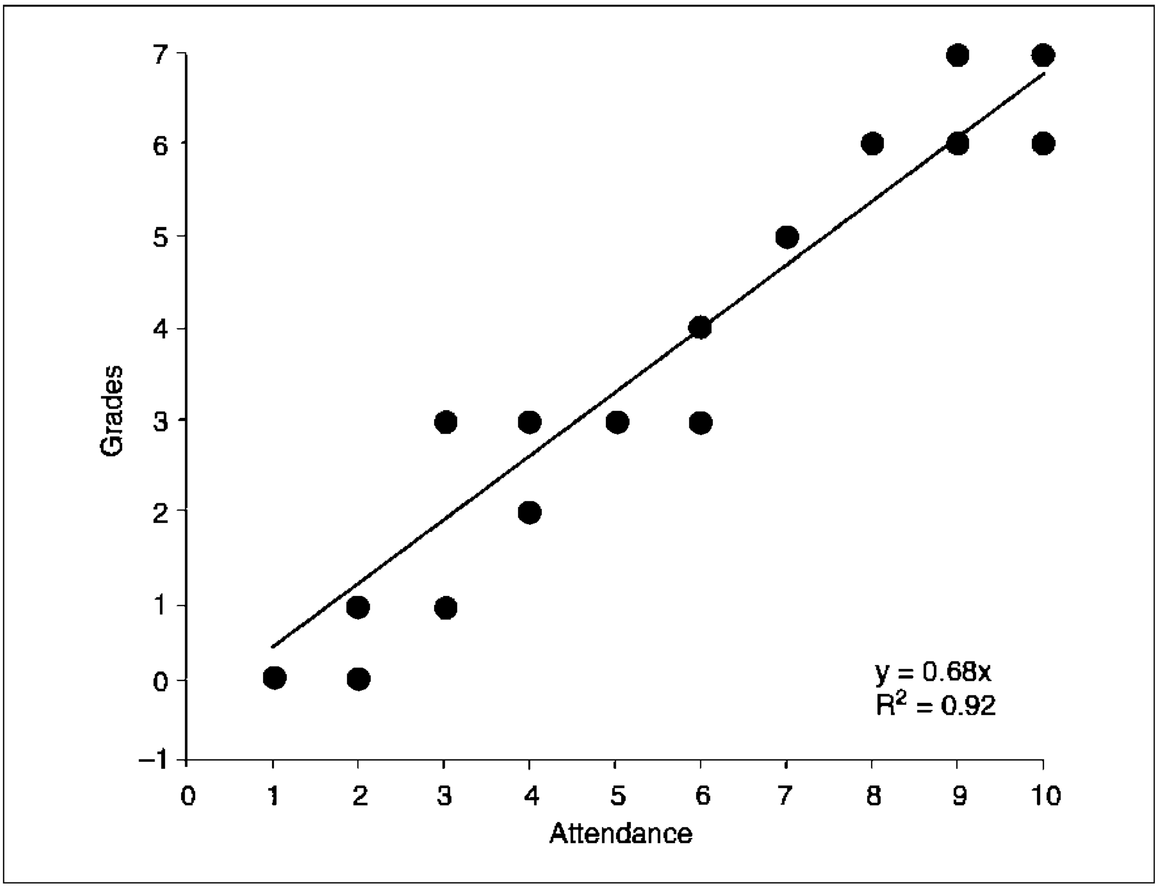
To generate this line, we simply ask a computer to find a line that minimizes the estimated vertical distance between each observation and the hypothesized line. We refer to this distance as the estimated error term associated with each observation. In practice, the computer starts with a hypothesized line, calculates the error estimates, then tries to minimize these by moving the line around. When it is satisfied that these errors have been minimized, the computer generates numerical estimates for α and β. This hypothesized relationship is depicted in Figure 4.5.

With this statistical summary we can predict how attendance, generally, affects grades. Traditionally, the relationship is depicted with an algebraic equation and statistic; in other words:

$$GRADES = 0 + 0.68\ ATTENDANCE$$
$$R^2 = 0.92$$

The first figure to the right of the equals sign, (0), was imposed on the equation so we would not have to deal with odd interpretations of the

Figure 4.5 ***Regression line on attendance/grades data***



data. If we had not set the constant to zero, and let it float freely, the equation would have become: *GRADES* = −0.71 + 0.77 *ATTENDANCE*; with an R^2 of 0.94. Note how the slope to this equation is steeper, and the strength of the explanatory model increases (from 0.92 to 0.94). But interpreting the constant term forces us to suggest that a student who hadn't attended any classes would get a negative number (−0.71) of good grades. This hardly makes sense (our teaching method is good, but not that good!).

The second coefficient (0.68) represents the slope of the line in Figure 4.5 (a positive number means the line slopes upward, from left to right; a negative number means that the line slopes downward). We can interpret this to mean that, for each additional class attended, a given student can expect 0.68 more 'good grades'. Not at all a bad return on his or her investment in time! The R^2 statistic (0.92) captures the model's degree of fit: that is, 92 per cent of the variance in grades is captured by this very simple model.

Among other things, the accuracy of this prediction depends on the sample's degree of representativeness. For example, we need to know if the sample is a good indicator of the population at large. If we were to find out that the sample included only men, and we know that that the relationship between attendance and grades varies between men and women, then we would not be able to generalize to the whole population from a study based solely on male subjects. There are other assumptions that could also prove problematic. For example, why should we assume that the relationship is linear? Is it reasonable to assume that attendance is the most significant influence on grades?

In this case, there is little justification for developing any generalizations from the observations in Figures 4.4 and 4.5. After all, the generalization is based on very few observations (only 19). We have purposely chosen a small number of observations to illustrate how these relationships can be captured empirically. And the data themselves are fictitious: we have constructed them to suit our purpose. Because of this, the estimates for α and β will necessarily be nonsense. However, if there was some empirical basis to the observations for *GRADES* and *ATTENDANCE*, and if we had more observations on which to draw, we could use these estimates to predict, exactly, what attending an additional lecture would yield in terms of better grades.

Multivariate Analysis

As we mentioned at the outset, there are good reasons to expect that other factors apart from attendance might influence a student's grade. To the extent that these factors are important, they can undermine the interpretive validity of the coefficients produced in the *bivariate* regression. Under these conditions, the analyst turns to a *multivariate* regression technique. As the name implies, multivariate refers to a relationship with more than one explanatory variable. The procedure for incorporating additional independent (explanatory) variables is very straightforward, but it is difficult for us to depict these developments in two-dimensional space. Conceptually, we begin to estimate planes instead of simple lines – but the logic is the same: we allow the computer to select a plane so as to minimize the sum of squared errors.

When we add more explanatory variables we can see why these are referred to as 'independent variables' by statisticians. Statistical inference proceeds on the basis of a number of simplifying assumptions about the nature of relationships in the real world. One of the most important of these is the assumption that the independent variables are independent of one another; in other words, that they are not capturing the same thing (see Galton's Problem, above). In short, when employing multivariate analyses we choose explanatory variables that are assumed to be unrelated to one another. If this assumption is violated, then the estimated coefficients can be misleading.

This is not a minor issue for social scientists, as many of the things we are interested in have common (and complex) causal backgrounds. Nor is this problem of interdependence limited to the right-hand side of the explanatory equation. A serious difficulty in much social science enquiry is the problem of endogeneity, where the relationship under study can also be understood in a more complicated and indirect way: both X and Y might be caused by a third (and hitherto unknown) variable, called a *lurking variable*.

Thus it is conceivable that, in the example above, both attendance and grades can be explained by a social situation. What we mean by this is that a student's social status might be the underlying explanation for both attendance and grades. For example, it is not unreasonable to expect an underprivileged student to find employment while studying, and work obligations can easily conflict with class attendance. It is also possible that an underprivileged student can grow up in an environment where academic performance is not encouraged or prioritized. In tbis situation, the relationship between attendance and grades is *spurious*, as both can be explained by another, endogenous, factor. While there are several empirical means for limiting the endogeneity problem (see, for example, King *et al.*, 1994, section 5.4), a sound theory is the most reliable defence.

Let us now consider the effect of sex *and* attendance on grades. As we mentioned earlier, there may be some reason to expect that women students tend to get better grades than men students. To test whether this is the case, we simply add sex observations (*SEX*) to our model, so that the computer will also produce coefficients (in this case β_2) for that variable. Because sex is a dichotomous variable (there are usually only two sexes), its coefficient will behave in a somewhat different way, but we hope that the choice of a dichotomous variable will clarify the conceptual procedure below. Thus, our new model can be depicted as:

$$GRADES = \alpha + \beta_1 ATTENDANCE + \beta_2 SEX + \varepsilon$$

When we run this equation, we ask the computer to estimate the nature of the relationship between attendance and grades, for both women and men. In short, the computer divides up the data into two groups: women and men. It then estimates the nature of the relationship between attendance and grades for each group. By comparing these differences, the computer can estimate the effect of sex. At the same time, the computer can divide the sample up into, say, three groups: high, medium and low levels of attendance. It then estimates the effect of sex on grades within each of these three subgroups. Here too, the computer compares estimates for men and women across each attendance subgroup. Given sufficient data, this process of adding additional (independent) variables can be extended to produce very complex models of the world.

At this point we might reflect on how control and comparison are being used here, in contrast to their use in the experimental method. After all, as we suggested in the previous chapter, it is possible to conceive of an experimental approach to study this relationship, but this requires that we physically manipulate our data and their contexts (for example, randomly dividing subjects into control and treatment groups). The statistical method allows us to bypass these difficulties. Instead of physically altering the context of our subjects to control for the influence of a particular variable, we can use the computer to virtually divide the sample into subgroups, and run partial correlations for each group. In doing so, we can estimate the effect of a given 'treatment' on an outcome. It should be clear that the analyst's demand for data increases significantly with the number of partial correlations.

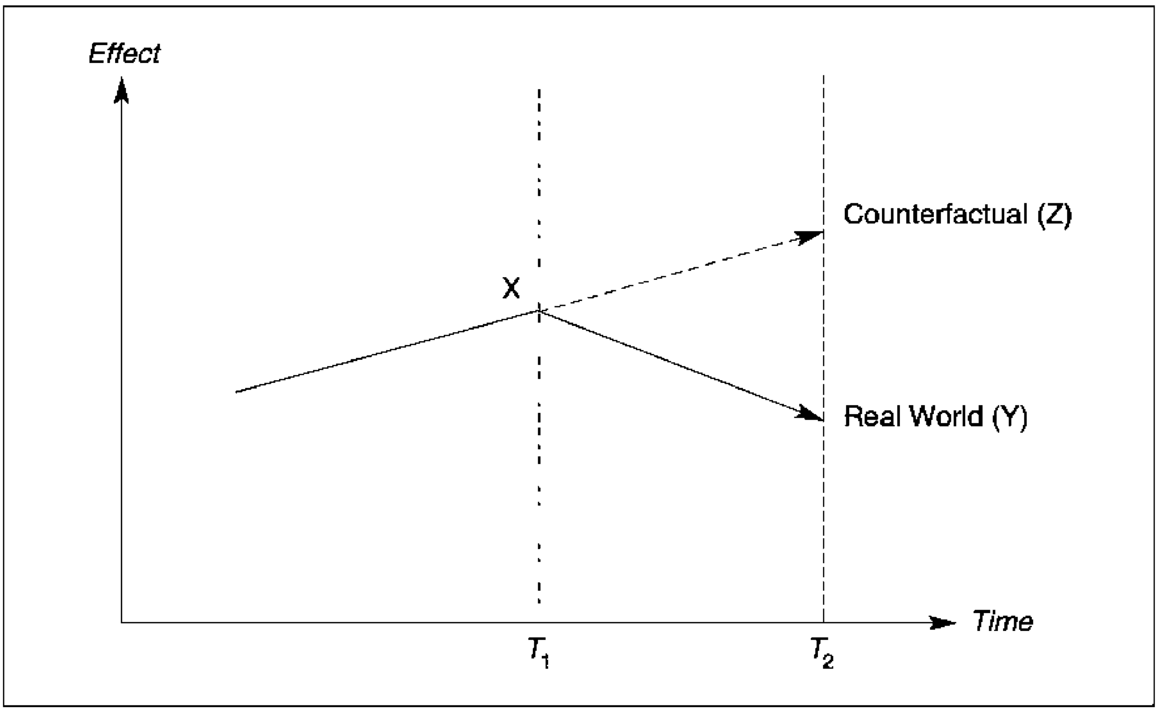
Regression analysis provides a remarkably strong foundation for making predictions. This predictive capacity relies heavily on an underlying naturalist ontology. The statistician (implicitly) assumes that it makes sense to divide up the social world into variables and to search for patterns among them. In addition, she assumes that the Real World patterns are so stable that we can expect them to hold beyond our narrow sample of observations. The statistical method allows us to manipulate data in ways that can uncover hidden patterns in the data. The predictive capacity of the regression analysis (for example, our ability to predict that a student who attends an additional lecture can expect to get 0.68 better grades) is based on this ability.

Perhaps these ontological assumptions are even more evident when we think about how statistical techniques are so conveniently used in counterfactual analyses. This is done schematically in Figure 4.6, where we ask you to consider the impact of a new policy (X) in a given policy space (the effect of which is measured on the y axis). Using the language of experiments, we can understand the effect of the introduced policy as a treatment variable introduced at time T_1. To measure the impact of this policy, we need to compare a real policy outcome (Y) with a counterfactual outcome (Z) at some time in the future (T_2). In this case, the counterfactual (Z) represents the way we expect the world to look in the absence of the posited treatment or policy change (X). To gauge the effectiveness of the policy in question, we cannot simply compare the pre-treatment score (at T_1, prior to X) against the post-treatment outcome (Y), as we cannot assume that time stopped in the absence of the new policy. In short, we have to compare the real (post-policy) and counterfactual outcomes. The counterfactual point of comparison is generated by using regression analysis to project a trend (based on pre-treatment data) into the future. This trend is depicted by the dotted line, XZ: it provides us with an empirically informed image of what the world would have looked like in the absence of the imposed policy change.

This is, in effect, what Robert Fogel – the 1993 Nobel Prize Co-Laureate in Economics – does. Fogel pioneered a research tradition, called *cliometrics*, which combines economic theory, quantitative methods, hypothesis testing, counterfactual analyses and more traditional techniques of economic history to explain economic growth or decline. He uses these techniques to ask difficult questions about fundamental tenets of American economic historiography; for example, that the railroad was an indispensable and driving force behind American growth in the nineteenth century (Fogel, 1964); or that American slavery was not as unprofitable as traditionally assumed (Fogel and Engerman, 1974). In doing so, Fogel's analyses build on naturalist assumptions about the nature of the Real World and exploit the patterns they offer to generate counterfactual histories that can probe and challenge deeply held assumptions (even truths) about economic history.

The basic regression model has become a staple tool in modern scientific analysis. Its influence has spread broadly across the social scientific landscape. Most developments in statistics since the 1980s have been aimed at extending this basic regression model to an ever broader set of problems (and to overcome an increasingly wide set of violations of the basic model). In particular, many developments in the specialized

Figure 4.6 ***Counterfactual depiction***



field of econometrics have spread to other social science disciplines. Among these are refinements of so-called 'structural equation models' that allow researchers to incorporate systematic hypotheses about measurement error and missing variables into a wide variety of models; innovative time-series approaches that have allowed statisticians to deal with data shortages in cross-national studies; and models with very complex (non-linear) parameter functions. In addition, specialized statistical applications (and software) have developed within each of the social science disciplines – making it nearly impossible to try to provide any sort of comprehensive overview of developments.

The problem with statistical approaches, even from a naturalist perspective, is their inability to examine causal mechanisms. Causality is, as Hume used to say, invisible. The statistical method prioritizes the collection of variables and correlations, and – in the doing – affects the underlying (natural) contexts in ways that make it difficult to find the causal relationships posited by the investigator's theory. By focusing on variables, we lose sight of the cases and the particular contexts in which these causal connections are embedded. For this reason, there is a growing tendency to combine statistical studies with comparative and case study approaches, to triangulate on causal connections.

Conclusion

It is easy to understand the desire to use statistical inference as the logical point of departure in social science study. For most social scientists, statistics is the closest alternative method they have to the experiment. Because statistics does not involve the physical manipulation of data, it is a method that lends itself to the study of social phenomena – where we tend to study events that have already occurred. Instead of manipulating the physical data itself, statistical approaches allow us to manipulate already existing data in a conceptual (or logical/mathematical) manner. For that very reason, statistical approaches cannot possibly control for all other variables – merely the other key variables that are known to exert influence.

This chapter has outlined the important role that the statistical method plays in contemporary social science from a naturalist perspective. We have divided the chapter in a way that emphasizes the role of descriptive as well as inferential statistics in this methodological tradition. While social scientists have come to prioritize the sort of knowledge generated by statistical approaches, its very logic depends heavily on that of the experiment. The utility of statistical analysis depends critically on the availability of data – in sufficient numbers. Unfortunately, not all that interests social scientists lends itself to statistical study – either because the objects of study are too few in number (for example, outbreaks of world wars) or because there is insufficient data already collected. For these unlucky scholars, the only option is to descend one step further down the hierarchy of naturalist methods: to the small-N comparative study.

Recommended Further Reading

We think that the best way to learn statistics is through history. For that reason, the curious student might begin by reading Stephen Stigler's (1986) *The History of Statistics* or by browsing through the *Journal of Statistics Education*. Good and influential statistics texts include Hanushek and Jackson's (1977) *Statistical Methods for Social Scientists*, and Michael Lewis-Beck's (1980) *Applied Regression*. For more fun and playful applications, see Davis Salsburg's (2001) entertaining book, *The Lady Tasting Tea*; Joel Best's (2001) *Damned Lies and Statistics*; and T. R. Knapp's (1996) *Learning Statistics through Playing Cards*. Finally, King *et al.*'s (1994) *Designing Social Inquiry* offers a broader methodological approach anchored in statistical inference. For those who are challenged (or feel threatened) by numbers, try Alex Bellos's (2010) *Alex's Adventures in Numberland*.

Chapter 5

The Comparative Method

Let us return to the basic philosophical components of the naturalist approach: that there is a Real World out there, independent of the observer; that this World is uniform and orderly; that observations and observation statements allow us to access this World; and that a careful process of induction and deduction can be used to identify the ordering principles of the World, so as to determine its component parts and their causal relations. This chapter describes how the comparative method is employed from this methodological perspective.

In one sense, of course, all scientific endeavours are comparative in nature. Francis Bacon used the comparative method in his laboratory to identify the optimal conditions for the sprouting of seeds. He steeped wheat seeds for twelve hours in nine different liquids: cow dung, urine, three different wines and four different water solutions. He then carefully observed the speed of germination and the heartiness of growth in each dish, and compared each sample of seeds carefully with all the others – as well as with a sample of unsteeped seeds. After doing this several times over, he drew two general conclusions: first, seeds steeped in urine are a sure winner, every time; second, seeds steeped in claret is a waste of good drink (Bacon, 1627, p. 109f).

Sometimes Bacon referred to this exercise as a systematic comparison, and sometimes he referred to it as an experiment. The label hardly matters, because an experiment always involves systematic comparison; and a comparative investigation is usually modeled after the experiment. Indeed, Talcott Parsons (1949, p. 743) made the same point when he noted that: 'Experiment is ... nothing but the comparative method where the cases to be compared are produced to order and under controlled conditions.' When John Stuart Mill explained the main variations of the comparative method – and he is, as we shall soon see, the major authority on the subject – he did this in a chapter entitled 'The Four Experimental Methods'.

For Arend Lijphart, the comparative method is modeled on the statistical design. It is the 'method of testing hypothesized empirical relationships among variables on the basis of the same logic that guides the statistical method, but in which the cases are selected in such a way as to maximize the variance of the independent variables and to minimize the variance of the control variables' (Lijphart, 1975, p. 164). Likewise, David Collier *et al.* (2004, pp. 94–5) refer to small-N comparisons in terms of 'intuitive regression'. All this does not matter much in the end, since statistics is, in turn, modeled on the experiment. The point here is that the comparative method mirrors experimental (and statistical) methods: they all involve variable analysis, and they all try to establish general empirical relationships between (at least) two variables, by means of control.

Yet, there are differences, and they are important. First, comparative case studies allow the analyst to trace out the proposed causal mechanisms in their natural contexts: they are an important way of buttressing the correlational relationships found in experimental and statistical studies. Consider, for example, how a comparative analysis might supplement our hypothetical teaching experiment described in the preceding chapters. Once the statistician uncovers a correlation between class attendance and grades, she might then compare a handful of well-chosen cases in detail to see whether attendance (or perhaps some other variable) was in fact the cause of the variance in grades. This is the sort of thinking that underlies Lieberman's (2005) article on mixed-method strategies for comparative research.

A second important difference concerns case selection: the comparative method does not select its cases in random ways (as do experimental and statistical studies). Rather, comparative studies unabashedly select their cases on the dependent variable. For example, a student of revolutions would select France as an interesting case precisely because of the revolution that took place there; or a study of America's best-run companies would surely want to sample from among these (for example, Peters and Waterman, 2004). Alternatively, comparativists often search for 'negative' cases (for example, the absence of a war) in analyses that seek to explain positive outcomes of something that interests them (war, in this case) (Mahoney and Goertz, 2004; also Skocpol, 1979, pp. 99ff). As we shall discover, case selection is one of the great strengths of the comparative method – but it also introduces some problems. Prime among these is the problem of selection bias, which continually haunts comparative projects. This problem is compounded by another characteristic feature: a small number of cases. Whereas statistical studies regularly rely on hundreds – sometimes thousands – of cases, the comparative method rarely relies on more than three or four. Indeed, only exceptional cases – such as the much-admired work of Barrington Moore (1966) – tend to brave more than this.

The reason is quite simple. The number of possible comparisons increases rather substantially by the following formula:

$$([n(n - 1)]/2)$$

Thus, a comparativist wishing to compare nine cases must consider ([9(9 − 1)]/2) = 36 different combinations. This is quite a lot to consider and to juggle. For this reason, comparative studies are often referred to as 'small-N studies'. Because the number of cases is so small, problems of over-determination are a constant threat to comparative analyses.

In recent years, this numerical gap has been closed by an important methodological approach associated with Charles Ragin (1987). To fill the gap that separates small-N studies (working with three or four cases) and statistical studies (that begin with, say, sixty observations), Ragin introduced a Qualitative Comparative Analysis (QCA) approach for conducting comparative analyses based on Boolean logic. More recently, new tools have been developed to apply QCA to an even broader area: Multi-Value QCA (or MVQCA) allows analysts to pursue QCA logic while using richer (in other words, non-dichotomous) data (see Ragin (2000, 2004); Moses *et al.*, 2005, pp. 61ff; or visit the COMPASSS (COMParative methods for the Advance of Systematic cross-case analysis and Small-n Studies) website, at http://www.compasss.org/). These developments have made it more difficult to refer to a quantitative/qualitative divide in social studies.

Still, there is a significant amount of work done at the lower end of the N-scale, and this work tends to suffer from problems related to over-determination and selection bias. These shortcomings reduce the comparative method's ability to generalize about the nature of the Real World. It is for this reason that comparative analyses are often surrounded by methodological controversy, and that comparativists are often considered to be poor cousins to statisticians and experimenters. In the words of Arend Lijphart (1971, p. 685), 'the comparative method is not the equivalent of the experimental method but only a very imperfect substitute. A clear awareness of the limitations of the comparative method is necessary, but need not be disabling, because, as we shall see, these weaknesses can be minimized' (Lijphart, 1971, p. 685). So, what can a poor cousin do to correct for these shortcomings? This is the guiding question for the remainder of this chapter.

The Methods of John Stuart Mill

One of the most confusing aspects of the comparative method is the many names given to it. For example, in the literature we can find references to different systems/similar systems (Przeworski and Teune, 1970); comparable case strategies (Lijphart, 1975); focused comparison (Hague *et al.*, 1998); case-oriented comparisons (Ragin, 1987); the method of systematic comparative illustration (Smelser, 1973), and others. Because comparisons are used in all social scientific methods, it is easy to confuse their various subtypes. For this reason, we have decided to return to the beginning: to the early classic work of John Stuart Mill. Not only was his description the first systematic formulation of the modern comparative method, but he remains the conceptual instigator for much of the work done since.

John Stuart Mill (1806–73) had a remarkable education, not least because he was raised by a very determined father, James Mill, with the advice and assistance of the utilitarian philosopher, Jeremy Bentham. The result was an extraordinary boy. Little John Stuart began to learn Greek at the age of three, and by the age of eight had read famous Greek classics in their original language. He was introduced to Latin, Algebra and elementary Geometry at a very early age. By the time he reached twelve, he had studied differential calculus and written a history of Roman government. (In case there are any attentive or inspired parents among our readers, we hasten to point out that Mill suffered a severe nervous breakdown at the age of 21.) Although J. S. Mill's influence is rightfully recognized in several fields (among them philosophy, economics, logics and ethics, to name but a few), we shall focus our attention on his *A System of Logic* (2002 [1891]).

As we saw in the previous chapter, Mill begins by assuming that there is order and uniformity in nature. This assumption clearly reflects the ontological foundation of the naturalist's methodology. However, the very complexity of nature means that its uniformity is not always understood: it is not easy to see the complex ways in which the things in nature are related to one another. Empirical regularities may overlap and give the appearance of irregularity. However, because of the order and uniformity of nature, naturalists can be certain that there are stable connections and causal regularities lying beneath the apparently complex and confusing surface of things. These causal regularities may not be immediately obvious, but it is possible to discover them by using scientific methods – by experiment, or by systematic comparison.

Mill finds no need to distinguish sharply between experimental and comparative methods, because they both conform to the same logical design. Or, more precisely, to the same logical designs. Mill identifies four of them: the Method of Difference, the Method of Agreement, the Indirect Method of Difference and the Method of Concomitant Variation. We hasten to point out that Mill had an additional, fifth, Method of Residues. Following Durkheim (1964, p. 129), however, we do not think that this method has any special utility in the study of social phenomena. Social phenomena are too complex for us to eliminate the effects of all causes save one.

Before they are more properly introduced, it is worth noting that Mill was quite sceptical of applying these methods outside the natural sciences; to apply them to the political sciences was 'out of the question' (Mill, 2002, p. 297). Needless to say, this caveat is seldom heeded by students of social phenomena, who continue to use them undeterred.

The Method of Difference

The simplest methods are the Method of Difference and the Method of Agreement. The Method of Difference relies on the logical design of the experiment and is the more reliable method of the two. Mill describes it thus:

> *If an instance in which the phenomenon under investigation occurs, and an instance in which it does not occur, have every circumstance in common save one, that one occurring only in the former; the circumstance in which alone the two instances differ is the effect, or the cause, or an indispensable part of the cause, of the phenomenon.* (Mill, 2002, p. 256, emphasis in original)

The Method of Difference compares political/social systems that share a number of common features as a way of neutralizing some differences while highlighting others. In other words, case selection is used in a way to control for causal effect. By choosing cases that are largely similar at the outset, any observed difference between the cases cannot be explained by those similarities. In short, all cases share basic characteristics (effective control), but vary with respect to some key explanatory factor. The presence or absence of this factor can then be used to explain any variation in outcomes (as the other relevant explanatory variables are controlled for by case selection).

When the appropriate conditions are met, this method is closest to that of experiment, but Mill himself was quite sceptical about whether these conditions were met in the social sciences:

> If two nations can be found which are alike in all natural advantages and disadvantages; whose people resemble each other in every quality, physical and moral, spontaneous and acquired; whose habits, usages, opinions, laws and institutions are the same in all respects, except that one of them has a more protective tariff, or in other respects interferes more with the freedom of industry; if one of these nations is found to be rich and the other poor, or one richer than the other, this will be an *experimentum crucis* – a real proof by experience which of the two systems is most favourable to national riches. *But the supposition that two such instances can be met is manifestly absurd.* Nor is such a concurrence even abstractly possible. Two nations which agreed in

everything except their commercial policy would agree also in that. (Mill, 2002, p. 575, second emphasis added)

This scepticism has not stopped social scientists from employing the Method of Difference. Indeed, they tend to do so in four different ways: comparisons over time, within nations, over areas, and with counterfactuals. As we go down this list of applications, we begin to stray further and further from Mill's original intent. By the time we reach the fourth application we have distanced ourselves from Mill's inductivism, and find investigators engaging the method in more deductive frameworks.

The first applications of the method of difference are so-called *longitudinal* or *diachronic* comparisons. Mill's example is of a man shot through the heart. He argues that we can be certain that the gunshot killed the man 'for he was in the fullness of life immediately before, all circumstance being the same, except the wound' (2002, p. 256). Most circumstances were the same before and after the shot, except for two: (i) after tbe shot the man was stone dead; and (ii) he had a gaping wound in his chest. As these circumstances were the two most obvious, it is tempting to infer that the second was causally related to the first.

By a similar logic, we can compare the social conditions of a single country at two different points in time – before and after a major event – in order to establish the cause of the event. A useful example is Theda Skocpol's comparison of the abortive Russian revolution in 1905 with the revolutionary success in 1917. Russia was in all major respects the same country in 1917 as it was in 1905, save for two major differences: (i) by the end of 1917, Russia had gone through a social revolution; and (ii) Russia was weakened to the point of collapse by a major war. It is thus tempting to infer that the second is causally related to the first. In other words, the application of the Method of Difference can 'validate arguments about the crucial contribution to social-revolutionary success in Russia of war-related processes that lead to the breakdown of state repressive capacities' (Skocpol, 1979, p. 37).

The second application of this method compares intra-state differences. Examples include comparisons of policy variations within the fifty states of the USA, or the different provinces, counties or municipalities in a single state. Thus it is meaningful to assess the efficiency of hospital management by comparing how hospitals are financed and run in two or more Norwegian counties, say. Similarly, it can be meaningful to assess an educational reform by comparing its effects in two or more adjacent Swedish counties. These are all pairs of cases that are so similar that they will – to a major degree – fulfill the criterion of having, in Mill's terms, 'every circumstance in common save one'. These types of comparisons exploit the fact that a common national context provides enough similarity across subnational units to control (in effect) for the causal effect of shared influences.

In a third type of application, investigators control for a number of contextual variables by choosing states or polities that are relatively similar (for example, with respect to wealth, regime type, religion, culture and other key variables). Thus it would be meaningful to assess the efficiency of hospital management by comparing how hospitals are financed and run in Sweden and Norway; or it would be meaningful to assess the quality of education in Chile and Argentina; or the workings of democracy in Poland and Slovakia. These are all pairs of states that are similar enough to approach Mill's condition for using the Method of Difference. Indeed, the establishment of Area Studies in traditional Political Science – a field with a long and proud record – is predicated on this argument.

It is often assumed that countries situated in the same region (for example, in Latin America, the Middle East, East Africa) have so many significant variables in common that it is meaningful to compare them with respect to selected variables. The small Caribbean island of Hispaniola is divided in two: the Dominican Republic occupies the eastern half of the island, and Haiti has the western half. The first is a tourists' paradise, while the other is one of the most miserable and mismanaged spots in the Western hemisphere. The two countries are so similar in basic respects – they both have a colonial past, they share the same waters and are subject to the same climate and natural conditions – yet they are so different in levels of wealth and social order. What can account for this stark difference between these two neighbouring countries? To ask the question is to consider the two countries as a 'natural experiment' – that is, not a controlled experiment (in which the assigned treatment is determined by a controlling scientist), but a serendipitous, experiment-like situation where the treatment has been made 'by nature', as it were. This is the approach of Jared Diamond (2010), who treats the island of Hispaniola as a natural experiment in development studies. Area Studies often use geographic proximity as a means of controlling for many potential contextual explanations.

The fourth and final application is counterfactual. This approach takes Mill's caveat above seriously, and recognizes that it is not possible to find cases similar in all respects but one (the explanatory factor). However, even if this is not the case in practice, it is possible to *imagine* a case that is exactly similar – a theoretically pure instance of the phenomenon of interest (Fearon, 1991). In this application of the Method of Difference we can use counterfactual cases as a way of increasing the number of observations (even if one of them is fictitious). In addition, a counterfactual application allows the analyst to consider causal relationships in a way that is very similar to the role played by counterfactuals under experimental conditions (see Chapter 3). By this point, however, we've strayed some distance from Mill's inductive method. Still, the application of the Method of Difference follows exactly the same procedural design.

In theory, the Method of Difference is a powerful method, for when its (rather demanding) conditions are met, the Method of Difference is closest to that of the controlled experiment – and is, indeed, sometimes referred to as a 'natural experiment' (Robinson *et al.*, 2009; Diamond, 2010; see also Snow, 1855). In practice, however, analysts should realize that it is highly unlikely that these conditions will ever be met. The examples above tend to rely on rather heroic assumptions about similarities across time, and within states and regions.

The Method of Agreement

Because the Method of Agreement is not encumbered with the same sort of strict conditions as we saw in the Method of Difference, it lends itself more easily to social science. Also, its logical design is simple. Mill explains:

> *If two or more instances of the phenomenon under investigation have only one circumstance in common, the circumstance in which alone all the instances agree is the cause (or effect) of the given phenomenon.* (Mill, 2002, p. 255, emphasis in original)

Mill's variable analysis is clearly present in this quote. It is worth noting that Mill thinks in terms of co-variation between 'instances' of phenomena. As in the Method of Difference, he reasons in terms of dichotomies, in which phenomena are either absent or present. The Method of Agreement is simple in that the investigator merely collects cases of a particular phenomenon in an attempt to find common factors in these cases that are otherwise quite different.

Indeed, the Method of Agreement is by far the simplest and most straightforward of Mill's methods. It is, however, generally regarded as inferior. This is because it has a tendency to lead to faulty empirical generalizations. As with the Method of Difference, there is much resistance to applying the Method of Agreement to social science studies. In particular, Émile Durkheim was critical of applications of either the Method of Difference or the Method of Agreement, on the grounds that the social world was simply too complex. By relying slavishly on these methods, Durkheim felt that comparativists were jeopardizing the good reputation of sociologists:

> [T]he conclusions of sociologists have often been discredited because they have chosen the method of agreement or of difference – especially

the former – and have occupied themselves more with accumulating documents than with selecting and criticizing them. (Durkheim, 1964, p. 133)

As with the Method of Difference, the Method of Agreement controls for variation on the basis of case selection: the investigator merely begins to collect cases of a particular phenomenon in an attempt to find common explanatory factors in cases that are otherwise quite different. Each case is acknowledged to be inherently different, with the exception of a key explanatory factor. The phenomenon is then explained by the common presence of that factor.

The Method of Agreement can be used to tease out general tendencies in the data, or to track down causal factors. For example, in one of the great studies of revolution, Crane Brinton (1965 [1938]) compares revolutionary developments in four very different countries: The 'English Revolution' in the seventeenth century; the American and French revolutions in the eighteenth century; and the 1917 revolution in Russia. While Brinton cautions us not to expect revolutions to be identical (1965, p. 226), he employs the method of agreement to develop a general rule about the nature of revolutions. Four of Brinton's five revolutions (the American case being the outlier) followed a similar pattern: the revolution began moderately, became more radical over time, and passed through a reign of terror before ending up in a Thermidorian reaction.

Employing the same method for a different argument, Eric Wolf (1968) compares revolutionary movements that had significant peasant participation in Mexico, Russia, China, North Vietnam, Algeria and Cuba. Because these countries shared few common features, Wolf argues that the penetration of capitalist agriculture was the key explanatory factor (common to each account) for the appearance of revolutionary movements with broad peasant support. In short, the penetration of capitalist agricultural regimes appears as the only relevant factor common to all these disparate cases.

Note how both the Method of Agreement and the Method of Difference have been used to examine the different causes of revolutions. With the Method of Difference, similarities across contexts can be used to find the one (differing) variable that can account for the revolution – for example, by comparing the (unsuccessful) 1905 and the (successful) 1917 revolutions in Russia, we have something akin to a natural experiment, where most of the contextual variables are controlled for (it is the same country, with only twelve years separating the two cases). In studies employing the Method of Agreement, scholars use the many differences found across cases to isolate a common feature – the one variable that co-varies with the revolution across each of the otherwise disparate cases.

While not everybody agrees that Mill's comparative methods are useful when studying revolutions (see, for example, Burawoy, 1989), they have been employed frequently by some very influential scholars.

To appreciate the power of the Method of Agreement, consider a simple (and fictitious) example. Imagine four friends driving home from Pop's Food Barn. These friends are of different ages, sizes and weights – the only thing they seem to have in common is the fact that they are driving home from an extraordinary meeting of the Sons of Norway (called to take advantage of a special on Pop's famous seafood platter). Thus, they are all men, and all of Norwegian descent, but they don't seem to share any other relevant qualities. Suddenly, and without warning, Eddy begins to complain about queasiness. Soon his other companions – Doug, Sam and Bill – are also noticing growing unease. Eddy, who is driving, pulls on to the hard shoulder so that the four can jump out of the car before becoming seriously ill.

To understand what is going on, we apply Mill's Method of Agreement. If we assume that sex (male) and ethnicity (Norwegian-ness) are not generally associated with nausea and stomach cramps, then we can begin by recognizing that the only circumstance that these four unlucky fellows share is dinner at Pop's. All four victims had ordered the same \$6.99 seafood platter with hushpuppies, catfish, shrimp and oysters. (This was, after all, the point of the gathering.) But we can investigate even more closely, to see if there was something these unlucky chaps ate at the Food Barn that caused this common illness. Table 5.1 lays out the relevant variables.

In the language of the naturalist, we begin by defining the dependent variable – the phenomenon to be explained – and labelling it 'Fallen ill' or Y. We then define the four potential explanatory factors: Shrimp (X_1); Oysters (X_2); Hushpuppies (X_3); and Catfish fillets (X_4). The Method of Agreement allows us to examine cases of the phenomenon with an eye toward eliminating any of the four explanatory variables. We begin by

Table 5.1 ***The method of agreement and Pop's \$6.99 seafood platter***

Case	*Name*	*Outcome*	*Food eaten*			
		Fallen ill (Y)	*Shrimp* (X_1)	*Oysters* (X_2)	*Hushpuppies* (X_3)	*Catfish fillet* (X_4)
1	Eddy	Yes	Yes	Yes	Yes	Yes
2	Doug	Yes	No	Yes	No	Yes
3	Sam	Yes	–	Yes	–	No
4	Bill	Yes	–	Yes	–	–

creating a Table (5.1), where we examine one case after the other. Thus, in the first case (Eddy), we find all four explanatory variables present, so that we cannot be certain about which is the causal factor (any one of the platter items could have caused the illness). We then proceed to the next case (Doug). Here we see that Doug consumed neither shrimp (X_1) nor hushpuppies (X_3), so these two dishes can safely be dropped as explanatory factors. In the third case (Sam) we find that catfish fillets (X_4) cease to be a potential explanation. For this reason, we have not filled in the remaining scores in the matrix. At this point we can conclude that the falling ill (Y) was caused by the oysters (X_2).

Or can we? Mill believed that the main problem with this method is its inability to establish any necessary link between cause and effect. For example, the fact that all instances of illness occurred after eating oysters is no guarantee that oysters caused the illness. Both the oysters and the illness might be affected by some unidentified (underlying or lurking) third factor (in other words, Galton's Problem). For example, perhaps Pop's Food Barn was not a particularly hygienic eating establishment; it could be that bacteria near the oysters at the Food Barn caused the illness. Another serious shortcoming of this method is that it is completely incapacitated by the problem of equifinality, or multiple causation (Ragin, 1987 and Lieberson, 1991). If illness results from *either* hushpuppies *or* catfish fillets, then there may be instances where hushpuppies have caused people to fall ill and other instances when catfish fillets have caused people to fall ill. The Method of Agreement would lead to the incorrect conclusion that neither of these factors caused the illness.

These examples show that the Method of Agreement (like the Method of Difference) is really a method of *elimination*. The investigator begins by collecting examples of the event he is interested in: say, revolution or illness after Pop's Food Barn. He then begins to gather evidence of possible causes (for example, oysters, shrimp, hushpuppies and catfish). He compares all cases carefully for each of the proposed causes, eliminating one potential explanatory factor after another, until he is left with one factor that all cases have in common.

Finally, it is important to point out that Table 5.1 reveals an *over-determined* relationship. The analysis depends on too few cases relative to the number of explanatory variables. This is a very common and serious problem in small-N comparative studies, and one to which we shall return below.

The Indirect Method of Difference

The most reliable comparative method is the Indirect Method of Difference (or the Joint Method of Agreement, as Mill also called it).

This application is closer to the statistical method in that it involves cross-tabulations of causes and effects. It is not, however, the same as the statistical approach, for whereas the statistical method relies on probabilistic relationships, the comparative method establishes patterns of invariance (see Ragin, 1987, pp. 39–40).

By combining two (mirror) applications of the Method of Agreement, it allows the investigator to come closest to approximating experimental design with non-experimental data. In other words, the Indirect Method of Difference is also modeled on the experiment. It allows the investigator to draw on non-experimental data, yet approximate the logic of the experiment. This is evident in Mill's description of its procedural design:

> *If two or more instances in which the phenomenon occurs have only one circumstance in common, while two or more instances in which it does not occur have nothing in common save the absence of that circumstance, the circumstance in which alone the two sets of instances differ is the effect, or the cause, or an indispensable part of the cause, of the phenomenon.* (Mill, 2002, p. 259, emphasis in original)

The Method of Indirect Difference is not as complicated as it sounds. In effect, it relies on a double application of the Method of Agreement. This can be shown by extending the example of the four unfortunate friends to include 'negative' cases, and by comparing all cases systematically for agreement as well as for difference.

Imagine, now, a second car driving home from Pop's Food Barn, and carrying three other members of the Sons of Norway's local chapter: Robert, Jens and Tom. Noticing their friends curled up in a state of nausea, they pull over to offer some assistance. Robert, who was driving, interviewed each of the four prostrate victims. From that information he was able to assemble a mental matrix of his own – not unlike the one found in Table 5.1. But he could now extend that table to include 'negative cases'. Grabbing a stick, he quickly traced Table 5.2 in the sand at the side of the road. Note how the (shaded) top part of Table 5.2 reproduces his mental matrix (and copies Table 5.1 above).

Having established, by means of the Method of Agreement, that oysters were the likely cause of the illness, Robert sets to work employing the Indirect Method of Difference. He begins to collect cases where no illness had occurred (remember, Robert was one of the few students who had attended all ten methods lectures in Chapter 3). If it is true that oysters had caused his friends to fall ill, Robert expects to find that those who had *not* fallen ill had *not* eaten oysters. To search for this evidence, Robert didn't need to look any further than his own passengers, as neither himself, Jens nor Tom had eaten oysters that evening.

Table 5.2 ***The indirect method of difference***

Case	*Name*	*Outcome*	*Food eaten*			
		Fallen ill *(Y)*	*Shrimp* *(X_1)*	*Oysters* *(X_2)*	*Hushpuppies* *(X_3)*	*Catfish fillet* *(X_4)*
1	Eddy	Yes	Yes	Yes	Yes	Yes
2	Doug	Yes	No	Yes	No	Yes
3	Sam	Yes	–	Yes	–	No
4	Bill	Yes	–	Yes	–	–
5	Robert	No	No	No	No	Yes
6	Jens	No	Yes	No	No	No
7	Tom	No	No	No	Yes	Yes

By juxtaposing the positive and negative cases in Table 5.2 we can be more certain of the causal relationship at work. Not only did illness occur after every instance of oyster consumption, but the absence of illness was also associated with the absence of oyster consumption. Thus the major difference between the Indirect Method of Difference and the Method of Agreement is that the indirect method uses negative cases to reinforce conclusions drawn from positive ones.

Two elegant applications of the Method of Indirect Difference are mentioned in most introductions to the comparative method – and deservedly so. The first is Barrington Moore's *Social Origins of Dictatorship and Democracy* (1966). The other is Theda Skocpol's *States and Social Revolutions* (1979). Moore seeks to explain how different countries have developed from agrarian to industrial societies. He selects five important countries that have all gone through this modernization process – England, France, the USA, Japan and China. To explain the routes they took, he focuses on historical relationships among the main (economic) classes in these countries. In particular, he shows how different classes have cooperated and competed in different ways, and how different class alignments produce very different political results. In cases where the rising bourgeoisie allied with the aristocracy and the rural masses (and allowed agriculture to be commercialized), the result was liberal democracy – as in England and the USA. In cases where the rural masses allied with the traditional aristocracy against the bourgeoisie, the result was fascism (for example, Japan), and in cases where the rural masses took power, the result was communism (as in China). Thus Barrington Moore demonstrates that countries can travel three different routes toward modernization – liberal, fascist and communist, only the first of which leads to democracy. Toward the end of the book he introduces a final case, India, to examine more closely the complicated relationship between democracy and modernization (as India succeeded in securing the former before the latter).

Social Origins of Dictatorship and Democracy provides a unique understanding of 400 years of economic development and political history. It is a story ripe with insights and commentaries, and an argument that dispels traditional theories of development as it unfolds. It has fascinated two generations of social scientists, and is a monument to the power and fertility of the comparative method. From the naturalist's perspective, the problem with Moore's argument is that its comparative design is implicit; it takes a skilled methodologist to disentangle all the threads that the author weaves into his argument. We return to this important observation in Chapter 10.

Theda Skocpol, by contrast, has explicitly advertised her application of the Method of Indirect Difference. Her book, *States and Social Revolutions*, begins by discussing three social revolutions – the French (1789), the Russian (1917) and the Chinese (1947) – in order to identify probable causes of revolution (using the Method of Agreement). Skocpol then proceeds to study instances of social unrest that did *not* produce social revolution – with the Reform Movement in Hohenzollern Prussia (1807–14), the German upheavals (1848–50) and the Meiji Restoration in Tokugawa Japan (1868–73) being foremost among them. She then integrates these cases of non-revolution into her discussion as 'negative cases', 'contrasts' or 'counterpoints'. This integration of 'negative cases' lifts Skocpol's method up from the Method of Agreement to the Method of Indirect Difference – and produces 'the best book that has ever been written on revolutions' (Collins, 1980, p. 647).

Perhaps the reason for such acclaim can be found in the fact that the basic logic of her argument is very simple: the main cause of social revolution is a factor that is systematically present when revolution is present, but systematically absent in cases of turmoil when social revolution is absent. As she discusses her various revolutions in the light of one variable after another, Skocpol can home in on 'state collapse' as the most probable cause of revolution. On the one hand, all her positive cases of revolution – France, Russia and China – were preceded by the unravelling of state institutions, and on the other, all her negative cases involved rebellions that were struck down by the force of the state. The state apparatus, in other words, did not unravel in these negative cases. Rather, when a revolutionary movement gathers momentum, governments rely on state forces to block and stop the insurgent process.

One of the qualities that makes Skocpol's book so worthwhile is that she doesn't rest after she has identified her independent variable. Instead of proudly displaying state collapse as a cause of revolution, she pursues it even further: she asks what might have produced the collapse of the state. By beginning to unravel the chain of causality in this way, she eventually arrives at Great Power wars. State collapse is a key causal variable for social revolution; however, when she revisits her positive cases (all the weakened states that experienced revolution), she finds that each of them was weakened by a great war. Through a virtuoso application of the Indirect Method of Difference, Skocpol concludes that social revolutions are produced by the confluence of three developments: (i) an initial collapse or incapacitation of the central administrative and military apparatus of the state – occasioned, for example, by losing a major war; (ii) widespread peasant rebellions; and (iii) shifts of political allegiance among elite groups.

The Method of Concomitant Variation

We have arrived at the fourth, and last, of Mill's methods: the Method of Concomitant Variation. It is more sophisticated than the others because it is not limited to binary cases (as are the other applications): it observes and measures the quantitative variations of the operative variables. Consequently, the Method of Concomitant Variation can track variation in magnitude rather than in the simple presence or absence of a variable. As such, this method comes closest to the statistical method described in Chapter 4. Here, more than anywhere else, we see how closely related these (experimental, statistical and comparative) methods are to one another. Mill described this fourth method of comparison thus:

> *Whatever phenomenon varies in any manner whenever another phenomenon varies in some particular manner, is either a cause or an effect of that phenomenon, or is connected with it through some fact of causation.* (Mill, 2002, p. 263, emphasis in original)

While Durkheim was sceptical about applications of the Method of Agreement and the Method of Difference to social phenomena, his scepticism did not extend to Mill's Method of Concomitant Variation. He saw this method as the instrument par excellence of sociological research. For this reason, we might quote him at length on this subject:

> for [the method of concomitant variation] to be reliable, it is not necessary that all the variables differing from those which we are comparing shall have been strictly excluded. The mere parallelism of the series of values presented by the two phenomena, provided that it has been established in a sufficient number and variety of cases, is proof that a relationship exists between them. Its validity is due to the fact

> that the concomitant variations display the causal relationships not by coincidence, as the preceding ones do, but intrinsically. It does not simply show us two facts which accompany or exclude one another externally, so that there is no direct proof that they are united by an internal bond; on the contrary, it shows them as mutually influencing each other in a continuous manner, at least so far as their quality is concerned. (Durkheim, 1964, pp. 130–1)

Because this method not only examines the existence of correlations, but also gauges their relative strength, it is remarkably similar to the statistical approach described in the previous chapter. To underscore the difference that non-dichotomization can make, we can replace the dichotomous scores in Table 5.2 with non-dichotomous ones. In particular, we can use body temperature as a measure of illness, instead of falling ill (Yes/No). Similarly, we can note the number of helpings each Son of Norway took at the Food Barn, rather than using dichotomous scores for consumption (Yes/No). This is shown in Table 5.3.

If, for the sake of convenience, we focus our attention on the two relevant variables (number of oyster helpings and body temperature), we can note that the two variables not only co-vary, but they do so in a very systematic way. With closer observation we can note that a single increase in the number of oyster helpings corresponds to an increase in body temperature of one degree Fahrenheit (F). For example, Doug only had one oyster helping, and his temperature was only a little above average (99°F). Sam was not satisfied before he had two helpings of oysters, and his temperature was

Table 5.3 ***The method of concomitant variation***

Case	*Name*	*Outcome in °F (°C)*	*Food eaten: number of helpings*			
		Body temp. (Y)	*Shrimp (X_1)*	*Oysters (X_2)*	*Hushpuppies (X_3)*	*Catfish fillet (X_4)*
1	Eddy	102 (38.8)	1	4	1	1
2	Doug	99 (37.2)	0	1	0	4
3	Sam	100 (37.7)	–	2	–	0
4	Bill	101 (38.3)	–	3	–	–
5	Robert	98.6 (37.0)	0	0	0	5
6	Jens	98.6 (37.0)	5	0	0	0
7	Tom	98.6 (37.0)	0	0	3	1

a degree higher (100°F). This pattern continues all the way up to Big Eddy, who seems to have had an enormous appetite. His toll for consuming four helpings of oysters was a feverish temperature of 102°F (38.8°C).

While the Method of Concomitant Variation has significant analytical potential, it can easily be employed in a less ambitious, and more inductive, fashion, where testing causal relationships is downplayed in favour of understanding underlying commonalities. Indeed, this is the approach that underlies a classic work in comparative politics: Gabriel Almond and Sidney Verba's (1965 [1963]) *The Civic Culture*. In their contribution to a 'scientific theory of democracy' (1965, p. 10), Almond and Verba conducted 5,000 interviews, scattered across five different countries (Britain, Germany, Italy, Mexico and the USA), with an eye to identifying the political culture associated with democracy.

In particular, Almond and Verba compared levels of political participation and diverse citizens' attitudes toward government and politics in the five countries. Following J. S. Mill's lead, they began by providing clear definitions and measurements for the variables of interest. In this case, Almond and Verba had to operationalize a number of very slippery and amorphous concepts, such as 'pride'. To do this, they surveyed broad swaths of the population in each country and asked them similar questions, with the aim of providing compatible, cross-national data. On the basis of these responses, Almond and Verba were able to map systematic patterns across nations:

> Thus the Americans and the British with greatest frequency take pride in their political systems, social legislation, and international prestige. Italians in the overwhelming majority take no pride in their political system ... To the extent that they have national pride at all, it is in their history, the physical beauty of their country, or in the fact of being Italian. (Almond and Verba, 1965, p. 65)

On the basis of several such investigations and comparisons, Almond and Verba concluded that democracy relies on a participant culture – what they call a 'civic culture'. But they added that democracy is most stable in societies where participation is tempered by elements of subject and parochial attitudes. For example, they found that their measure of pride correlated with civic culture – noting that the citizens of the more democratic nations tended to be prouder of their polities.

Shortcomings

In the presentation above we used influential and real, as well as fictitious, examples to illustrate the breadth and appeal of the comparative method for social scientists. We shall conclude the chapter by returning to the caveats with which we began: that the comparative method often suffers from two significant shortcomings when viewed from the demands of the naturalist's methodology – over-determination and sampling bias. We close with a short discussion of each of these. It is important to point out that these problems are not restricted to small-N comparative projects – they only tend to be more common here.

Over-determination

Over-determination concerns our ability to generalize from the observations we have. To generalize we use inference, which is itself restricted by the amount of information we already have. As a general rule, we tend to assume that one piece of information cannot give independent information for more than one other fact. This rule translates into the concept of *degrees of freedom*, which you may recall from your introductory statistics course and Figure 4.2: degrees of freedom are the number of cases minus the number of explanatory variables, minus one.

Thus, when the analyst has only one case, and at least one explanatory variable, she is working with negative degrees of freedom: under these conditions, any claims about causation are worthless (see, for example, Campbell, 1975). The reason for this is clear: without more observations we can say nothing about the spread of the phenomenon. Without a grasp of the spread (or variation) of a given phenomenon, it is impossible to generalize with any degree of accuracy.

Consider a simple example: we begin with an observation of a poor state, whose GDP/capita (PPP) is $1,308. From this observation we have a measurement of the average wealth of a single country. However, we have no way of making comparisons to other countries and therefore no way of making assessments about the level or degree of poverty. We cannot know if this state finds itself at the high or the low end of the 'poor state' scale: we can say nothing about the representativeness of this observation with respect to poor states generally. However, if we were to gather more observations (for example, find that another poor country in the same year had a per capita income of $429, while a third had $2,484), then we could begin to develop a better understanding of what the universe of 'poor states' looks like. Thus, from a single observation, we can say nothing about other poor states. It is only when we have more than one observation that we can gather information about the spread of the population. These problems can be particularly troublesome in small-N comparative studies. Indeed, we noted it in relation to Table 5.1.

Following Lijphart (1971), we can divide the over-determination problem into its two main components: (i) too few observations; and

(ii) too many variables. To solve these problems, Lijphart proposes several solutions.

The first proposal is to 'increase N'. This echoes the mantra of naturalist science. By increasing the number of observations, we get a better sense of the spread of experience under investigation. We also improve our ability to control variations. Eventually, we may even be able to graduate to statistical analysis, climbing another rung up the methods' ladder. Increasing the number of observations also helps us to generalize, as we can assume that the sample becomes more representative as the N increases and we get a better mapping of potential spread.

The second proposal is to reduce the number of variables. One way to do this is to reduce the property space of the analysis by combining variables and/or categories. Researchers are encouraged to combine similar variables that encompass underlying characteristics. In this way, the number of explanatory variables will decrease relative to the number of observations – increasing the analyst's degrees of freedom. While this entails discarding costly information, the costs are generally seen to be affordable. In doing so, the analyst increases his or her analytical purchase and degrees of freedom.

Another way to reduce the number of variables is to use theories more vigorously to help in choosing only the most likely (important) variables. In other words, we can initially scan all the potential explanatory variables, but in the final analysis we need to economize in order to maximize the degrees of freedom. This proposal returns us to the choice of method, discussed by Mill. If we can choose our cases carefully (in other words, in the light of theory), we can effectively control for many of the potential operative variables in an analysis.

It is worth noting that the final solutions (reducing the number of variables and choosing the appropriate method) encourage the comparativist to engage theoretical issues, thus providing a greater role for theory. To improve on the potential shortcomings of the comparative approach, as developed for inductive studies, we need to introduce theories that will help us to define key variables, reduce the property space of variables, and focus on appropriate cases. Good comparative research exploits both deductive and inductive approaches to testing causal relationships.

Sampling Bias

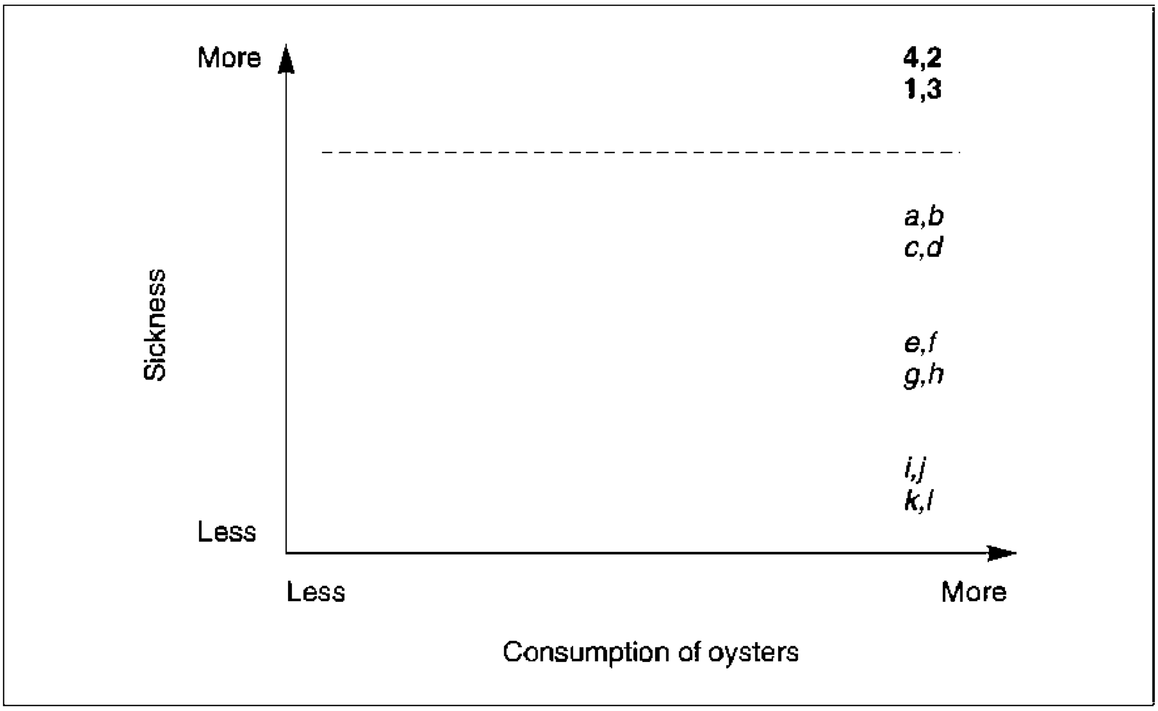
Whereas experiments and statistical designs are based on the principle of random selection, the essence of the comparative method is case selection. We choose our cases with an eye toward control. While this is one of the strengths of the small-N comparative method, it is also its bane: sampling bias can threaten the generalizability of any results we might produce. In its most blatant form, social scientists select only cases that support the theory in question, or draw only from certain types of sources. But it is not uncommon to find comparative studies where the cases are chosen by their score on the dependent variable. This can (but needn't always) raise some serious problems. As this last example is the least understood, we shall examine it more closely.

Most students learn in their introductory statistics courses that selecting on the dependent variable is forbidden. But few students remember why, or what the implications are of violating this taboo. The problem stems from the logic of explanation and we might understand it better by returning to our sick Sons of Norway lying at the side of the road. In the example depicted in Table 5.1 above (Method of Agreement), four cases were introduced on the basis of their scores on the dependent variable (all four men were sick). To elaborate on the problem of selection bias, we can consider what we might infer about the world on the basis of these four cases.

To do this we need to return to the scene of the four sick men – before their friends in the second car joined them. Imagine now that a state patrol car had pulled up instead: seeing a car pulled off to the side of the road, two state troopers arrived to investigate and provide support. Imagine also that inside the patrol car we find Officer Delaney and Officer Kaitlin; the first had just finished her graduate training in statistical criminology, while the second had majored in historical sociology. When the two troopers arrive, their (very different) mental processes shifted into high gear.

Delaney, the statistician, was the less worried of the two. She saw these four instances and realized immediately that there was no need to generalize on the basis of four individual cases. To illustrate the point, Delaney drew Figure 5.1 in the sand next to the four prostrate Sons of Norway. In this we can see the four observations from Table 5.1 nested in the upper-right-hand corner of the graph (and labeled by their case numbers: 1, 2, 3 and 4). By studying only these four cases, Delaney realizes that we can say nothing about the location of any other cases. Without that knowledge, Delaney simply assumes that the remaining cases (*a, b, c, d, e, f, g, h, i, j, k, l*) line up in a vertical fashion, as depicted in Figure 5.1 below the dotted line (in other words, that the illness was not related to the consumption of oysters). In this (as we have already discovered) she would have been wrong. But it is not an unreasonable interpretation, especially for someone trained in statistics. Delaney is rightfully wary of sampling on the dependent variable and generalizing on the basis of a very small number of observations. She can assume that this is an isolated incident and that there is no need for alarm.

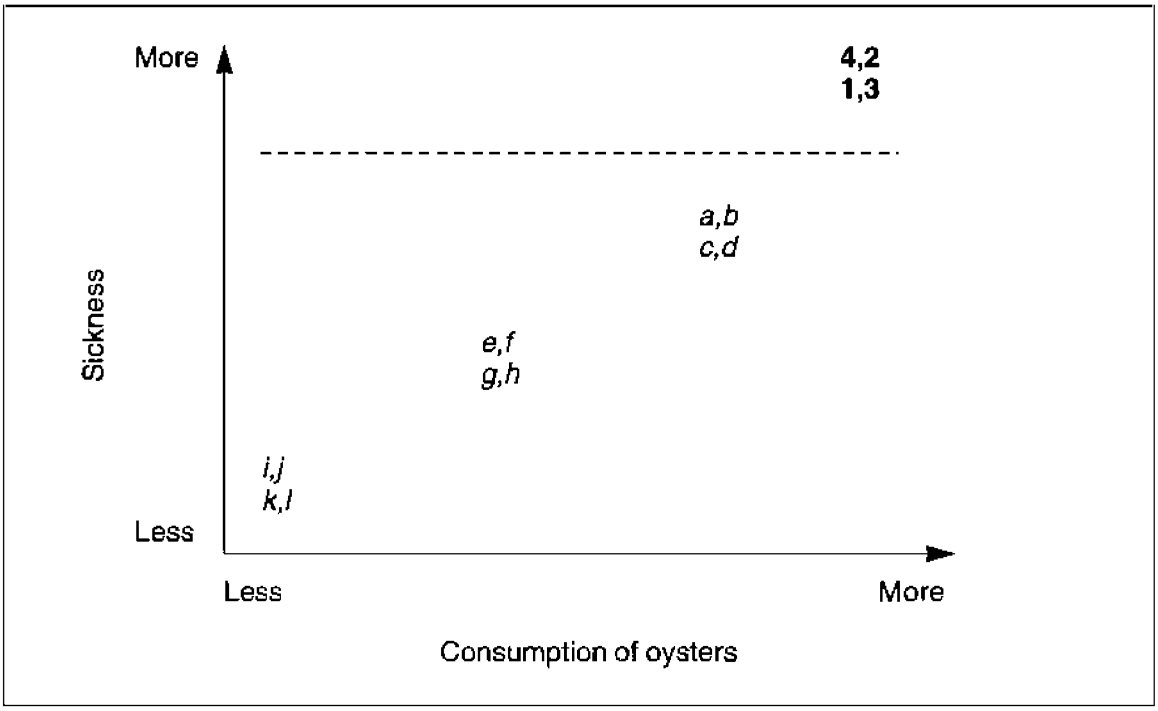
Figure 5.1 *The statistician's assumed relationship*



Given an opportunity, however, Officer Kaitlin would jump in to erase Delaney's drawing in the sand and replace it with her own. Given her training as a historical sociologist, she realizes that causal relationships can reveal themselves – even in areas characterized by a small number of observations, and where there is little variation in the dependent variable. From her methods training, Kaitlin would expect to find a different pattern in the remaining (unobserved) data – a pattern similar to the one depicted in Figure 5.2. If Kaitlin is correct, it is important to investigate further. Somebody needs to tell Pop that his oysters are foul, and other Sons of Norway should be contacted in order to map out the extent of the phenomenon.

This time Kaitlin was lucky, but it is important to note that Delaney's inferences were just as capable of being right (or wrong). Our point is not to show how the biases that drive statistical or comparative studies are better or worse than each other. Rather, our intention is to illustrate the problems associated with research projects that select a subject of study on the basis of the dependent variable, and where there is no variation in that variable (in other words, the analyst only chooses from one outcome on the dependent variable). The example above is inspired by Barbara Geddes (1990), in a piece that documents the seriousness of this problem in Area Studies, used to generalize about factors that can explain economic development. Because certain factors can be used to explain economic development in a given region of the world, it does

Figure 5.2 ***The comparativist's assumed relationship***



not mean that we can generalize from these findings to the universe of developing states.

By selecting cases on the basis of single scores on the dependent variable, we may jump to the wrong conclusion about the nature and location of the remainder of the (untested) population. In short, if we had selected a different set of cases, we might have derived a completely different conclusion about the nature of the relationship between these two variables.

Conclusion

We began this chapter with Francis Bacon's laboratory investigations into the optimal condition for the sprouting of seeds. We could equally have begun with other, more famous, social science examples – with Aristotle (1979 [c.350 BCE]), who compared a large number of constitutions in the ancient world in order to identify the best and most stable type, or with Machiavelli (1997 [1531]; 1961 [1532]) who compared the behaviour of many rulers to identify a few key maxims for efficient rule, or with Jomini (1971 [1838]) who compared hundreds of battles before drawing conclusions about the general nature of war. The examples are legion, because all science is, in one sense, comparison. We found, however, that Bacon's practical, hands-on laboratory tinkerings may, in fact, have provided the most illustrative example. For when the comparative method is stripped of all its bells and whistles, we are left with an attempt to mimic the scientific logic of experimentation but without the ability to fully control the assigned treatment. In comparative studies, control is conducted by way of case selection, and this introduces a number of characteristic problems that comparativists are always trying to correct.

We then described the main methods of comparison, as first introduced by J. S. Mill. These methods were originally designed as inductive tools for mapping the complex empirical patterns that exist in the Real World. To the extent that these methods are used in a purely inductive fashion, their shortcomings were clearly evident to Mill, Durkheim and many others. As a consequence, their application to the social sciences was strongly discouraged.

To correct for the main shortcomings of this method, social scientists are encouraged to increase their number of observations, in the hope of being able to apply (eventually) a statistical approach. Alternatively (and most commonly), social scientists can use theory as a way to overcome both of the major problems of this method: over-determination and sampling bias. Ultimately, good comparative studies combine deductive and inductive approaches to test hypotheses concerning causal arguments, even when the number of observations is relatively small.

Recommended Further Reading

As always, we recommend that you turn to the original source. In the study of comparative methods, this means John Stuart Mill's *A System of Logic* (2002 [1891]). Still, there are a number of other good and influential introductions to the comparative method, including Przeworski and Teune's *The Logic of Comparative Social Inquiry* (1970) and Todd Landman's *Issues and Methods in Comparative Politics* (2000). Our favourite is Charles Ragin's *The Comparative Method* (1987).

Chapter 6

History, Interviews and Case Studies

In this chapter we discuss the role of history, broadly understood, in the naturalist's toolbox of methods. Perhaps the easiest way to begin is to return to Robert Fogel and his study of cliometrics, introduced in the closing pages of Chapter 4. Here we find quantitative methods and behavioural models from the social sciences applied to the study of history. This relatively new 'scientific' approach to history has proved to be both popular and fruitful – Mr Fogel, after all, received a Nobel Prize in Economics as an acknowledgement of his work. Yet cliometrics itself does not provide us with a method that is distinct from those already covered in previous chapters; cliometricians such as Robert Fogel have simply borrowed statistical methods and theories from naturalist social science and applied them to historical queries.

There are other ways to think about historical methods. Many journalistic accounts rely on the historical approach. Social scientists, in turn, often depend on such accounts, and on the interviews that sustain them, to gain first-hand information about particular events. They use such information as inputs in studies that employ other (comparative or statistical) methods. In a similar fashion, case studies offer a way of gaining knowledge about Real World events. Here, the social scientist uses historical methods (or the work of historians) to collect data on a particular aspect of a larger phenomenon. In short, historical accounts, interviews and case studies are commonly used as tools in scientific comparisons and analyses.

To say that the historical approach is used as part of a case study or interview strategy in naturalist social science is to say very little about what the procedural design actually entails. To clarify this procedure, the first part of this chapter describes what might be termed the technical aspects of traditional historiography. This description builds on two particular aspects of the historical approach: the criteria used to establish reputable sources, and the way in which these sources are tapped for information and used (in other words, the overall objectives of historical analysis). Both aspects have a long and established lineage in contemporary historiography.

These two features do not make historical studies scientific – but they do provide a strong empirical foundation for subsequent social science. It is for this reason that we dwell for a little on the historical approach: it provides the groundwork for so much subsequent social scientific analysis. Once this groundwork is in place we can see how the historian's critical approach to primary sources is evident in social science interviews and case studies.

But there are important differences separating historical and social scientific approaches. The relations between the two are marked by long-standing tensions. The rivalry is largely methodological in nature and is most evident in the contentious role that case studies play in naturalist social sciences. Good case studies employ the approach of historians, but it is not used for the sake of a good narrative alone; it is harnessed to a purpose beyond the immediate narration. A 'case' is a 'case of something'. A case is always accompanied by theory, and the case under study is meant to be just one observation in a larger comparative study. As such, historically-informed case studies tend to occupy an important, if relatively low, rung in the naturalist methods hierarchy. As Lundberg (1926, p. 61) disparagingly noted, they very often become 'a helpless tail to the statistical kite'.

The Historical Method

Before we can understand the utility of historical narratives and case study methods in naturalist research projects, we need to grapple with the tenuous relationship between history and science. To do this we can try to anchor the historian's method in the naturalist's methodology.

This, as it turns out, is no easy task. At one level, the historical approach is as straight as an arrow: historians write stories backed by evidence. The core of the historical method is to probe the evidence to ascertain whether it is solid. At this level of generality it is easy to see the utility of historical approaches for the social scientist.

Beyond that, however, it is hard to identify any particular properties of the historical method. After all, there is no clear demarcation principle separating history from fiction. Indeed, the community of historians does not even possess a technical vocabulary that is distinctive to its members. David Hackett Fischer (1970, p. xii) reminds us that historians, when asked about the nature of history, might 'respond as Fats Waller (or maybe Louis Armstrong) did, when asked to explain the nature of jazz. "Man," he said, "if you don't *know* what it is, don't mess with it."'

To be honest, we find this attitude to be one of the most refreshing qualities of historical research: a historian presents his case in everyday language. Another endearing quality is history's variety of approaches and ideological perspectives. Historians do not limit themselves to specific hypothetico-deductive techniques or experimental controls to determine the veracity of their claims. Among historians, Isaiah Berlin (1954, p. 5) once observed, 'there plainly exists a far greater variety of methods and procedures than is usually provided for in textbooks on logic or scientific methods'.

While the historian's approach is remarkably varied, there is a pragmatic, down-to-earth simplicity to it that might be summarized as the practical application of common sense. Indeed, no less an authority than Lord Acton (1834–1902) noted how 'common sense' lay at the core of the historical method. In his [1895] inaugural lecture at Cambridge on the study of history, Acton noted that common sense should complement the more 'technical' aspects of the historical method. The main thing to learn, he insisted,

> is not the art of accumulating material, but the sublimer art of investigating it, of discerning truth from falsehood and certainty from doubt. It is by solidity of criticism more than by the plentitude of erudition, that the study of history strengthens, and straightens, and extends the mind. And the accession of the critic in the place of the indefatigable compiler, of the artist in coloured narrative, the skilled limner of character, the persuasive advocate of good, or other, causes, amounts to a transfer of government, to a change of dynasty, in the historic realm. For the critic is one who, when he lights on an interesting statement, begins by suspecting it. He remains in suspense until he has subjected his authority to three operations. First, he asks whether he has read the passage as the author wrote it ... Next is the question where the writer got his information ... third ... is their dogma of impartiality. (Acton, 1906a, pp. 15–16)

These technical and commonsensical aspects of historical methods are aimed at generating dependable, verifiable information about past events as they actually happened.

At the core of the historical method lies a kind of systematic doubt trained on the historians' sources. If we are to believe Lord Acton, historians do not have much more than this by way of scientific procedures – because the historian's work is distinguished neither by the use of particular equipment nor by special processes. However, historians know what they have and they make the most of it. Indeed, historians are so adept at what they do, that social scientists might do well to observe their procedures and learn from them. For the historical method of systematic doubt – often referred to as 'source criticism' – is really the core component in all social science methods.

Leopold von Ranke

The purpose of historiography is to generate dependable knowledge about the past, as it really happened – *wie es eigentlich gewesen war*. This was the maxim of Leopold von Ranke (1790–1886), the name most closely associated with the modern historiographic method. With him we can begin to trace the unique traits of the modern scholarly approach to historical study. Ranke's impact on that approach is enormous, as it builds on his three important legacies: (i) he helped to establish history as a separate discipline, based on describing history 'as it really happened'; (ii) he established that discipline with a reputation for impartiality; and (iii) he developed an explicit outline of historical methods based on source criticism. While the third point alone focuses on the technical aspect of good history writing, all these contributions helped to secure a foundational role for historical work in the naturalist hierarchy of methods. For this reason we want to spend a little time examining von Ranke's contribution.

Ranke's quest for objective historiography was prompted by his concern about the nature of contemporary public education in Europe (especially Germany). In the wake of the French and Industrial Revolutions, public education was gradually introduced to Europe – and history played a central role in this education. But Ranke feared that the history being taught was litrle more than the inculcation of patriotic myths in the young by the old.

Quellenkritik

The effect of this approach to the study of history as a scholarly discipline was of concern to von Ranke. His original training was in philology, where he learned about methods recently developed in the study of ancient and medieval literature. These methods were used to determine whether a given text was true (or corrupted by later interpolations); whether it was written by the author to whom it was usually attributed; and which of the available versions was the most reliable.

After turning to the study of history in the 1820s, Ranke established a seminar at the University of Berlin where he instructed advanced students in his new approach to historical research. His instruction focused on the critical study of sources – *Quellenkritik* – which he had largely imported from his training in philology. In particular, Ranke established a hierarchy of sources, ranked according to their reliability. History, he taught, should be written from sources that were located as close as possible to the events in question. Most preferably, history should be based on eyewitness reports and what Ranke called the 'purest, most immediate documents' (Ranke, 1956, p. 54).

Soon Ranke became Europe's premier teacher of historiography. Students came from across the Continent to learn about his new, scientific approach to history. When they left Berlin these students had learned that history should avoid edifying and moral-raising projects: the task of the historian was to recreate the past truthfully and objectively. By unfolding events carefully, the historian could show how they produced a specific condition, event or event sequence. To do this properly, the historian would have to find the correct sources and use them in a self-conscious and critical way. In short, behind all serious historical research lies a systematic quest for original source material. At the core of any good historical narrative lies a systematic assessment of the nature and the quality of every identified document.

Ranke recognized two kinds of sources: primary and secondary. Historical research, he argued, should rely on *primary sources* to the greatest degree possible. These are the direct outcomes of historical events or experiences. They include eyewitness accounts (written in letters, noted in diaries or recalled in interviews) and original documents (such as diplomatic reports, original assessments and papers given to decision-makers, papers and minutes from committee meetings, and so on).

But the historical researcher also has access to *secondary sources*: those that are once removed from original events. For example, the historian might find information in the form of a narrative that is (itself) based on primary sources; a newspaper report that is based on eyewitness accounts; or even a summary of important statistics. Secondary sources can help the historian to establish a chronological chain of events and a theme for his work. They can aid in mapping out the field of research, to find out what has been recorded. They can be useful for finding out which issues have been broached and which have not, to identify which questions have been raised and how they have been answered. However, the distance of secondary sources from the actual historical event makes them less trustworthy.

For Ranke, the job of the historian was to root out forgeries and falsifications from the historical record. (Think now of Descartes and his barrel of apples.) To do so, the historian had to stick to primary sources, and to establish internal and external consistencies. This should not appear as surprising to the modern reader, and it is surely an exaggeration to claim that Ranke was the first to employ these techniques. After all, Islamic tradition holds that the early Caliphs (from the first half of the seventh century) authorized Zaid bin Thabit to supervise a team that would collect and transcribe the Qur'anic revelation. As the Qur'an is held to record the voice of Allah himself, it was absolutely essential that Thabit's team made no mistakes in its task. To ensure an authentic version of the voice of Allah, each verse of the Qur'an is said to have been verified by at least two witnesses who had heard them spoken by the Prophet Muhammad himself.

Even if Ranke wasn't the first to employ careful source criticism, his mark is planted firmly on modern European historiography:

> Whatever the means they use, historians still have to engage in the basic Rankean spadework of investigating the provenance of documents, of enquiring about the motives of those who wrote them, the circumstances in which they were written, and the ways in which they relate to other documents on the same subject. The perils which await them should they fail to do this are only too obvious. (Evans, 1997, p. 19)

The Aim of History

The uncovering of indubitable facts through basic spadework among primary sources is the first, and fundamental, component of historical research. The second such component is loftier: it concerns the goal of history. Ranke saw history as a corpus of ascertained facts. These facts constitute a series of witness statements, available to the historian in document form. The historian reads these documents, systematizes their content and creates a narrative of 'what really happened'.

It may be useful to compare Ranke's maxim with that of Sherlock Holmes. As we have already seen in Chapter 2, Holmes explained the investigative part of his method to Dr Watson in terms of a two-step procedure: first, collect all relevant evidence; then sort through it, because 'when you have eliminated the impossible, whatever remains, however improbable, must be the truth' (Doyle, 1930, ch. 6). The comparison is useful, as both Ranke and Holmes were products of the same historical epoch and the empiricist spirit that marked it. They were birds of a feather in an age characterized by progress and scientific innocence where 'the new historians walked in a Garden of Eden, without a scrap of philosophy to cover them, naked and unashamed before the god of history' (Carr, 1987, p. 20).

Ranke was affected by the nineteenth-century philosophy of science. Yet it is too simple to depict him as a naïve empiricist. His famous phrase, '*wie es eigentlich gewesen war*', translates literally as 'what actually happened', but it may be better to interpret the phrase to mean 'how it essentially was' (Evans, 1997, p. 17). This is because Ranke's goal was not just to collect facts but also to understand the essence, or the inner being, of the past. A deeply religious and conservative man, Ranke did not believe that God would prioritize different historical epochs: each had to be similar in His eyes. For this reason, the past could not (should not) be judged by the standards of the present. It had to be understood on its own terms. Thus, the objective of history was to come to understand these universal truths about each historical epoch. It was this view that separated Ranke from the Prussian school of (deeply nationalistic) German historians. All states (not just Prussia) were examples of God's will, in Ranke's mind. No state's history could be prioritized. It is this position that underlies Ranke's reputation for impartiality.

This approach to impartial history is not itself an artifact of history; rather, it is a property that marks the several generations of historians that succeeded Ranke. Most famously, Lord Acton encouraged the ideal of objectivity in his letter of instruction to the authors of the first book in the multi-volume work *The Cambridge Modern History*: a historian's account of the battle of Waterloo must be painstakingly impartial, he insisted. It must be a Waterloo 'that satisfies French and English, German and Dutch alike; that nobody can tell, without examining the list of authors, where the Bishop of Oxford laid down the pen, and whether Fairbarn or Gasquet, Libermann or Harrison took it up' (Acton, 1906b, p. 318).

The Cambridge Modern History, then, was to offer the modern reader:

> a unique opportunity of recording, in the way most useful to the greatest number, the fullness of the knowledge which the nineteenth century is about to bequeath ... By the judicious division of labour we should be able to do it, and to bring home to every man the last document, and the ripest conclusions of international research.
>
> Ultimate history we cannot have in this generation; but we can dispose of conventional history, and show the point we have reached on the road from one to the other, now that all information is within reach, and every problem has become capable of solution. (Acton, 1907, pp. 10–12)

Thus it would appear that history could not only be impartial, it could also be definitive. By combining the technical expertise and commonsensical aspects of historical research, the modern scholar could contribute to human progress and understanding.

It is here that we can clearly see a naturalist's affinity in this approach:

> Out there, in the documents, lay the facts, waiting to be discovered by historians, just as the stars shone out there in the heavens, waiting to be discovered by astronomers; all the historian had to do was apply the proper scientific method, eliminate his own personality from the investigation, and the facts would come to light' (Evans, 1997, pp. 20–1).

Barbara Tuchman

In modern form, the Rankean tradition is reflected in the works of a number of contemporary historians. The most readable of them is surely Barbara Tuchman, the prize-winning author of *The Guns of August*, *The Zimmermann Telegram* and several other deservedly popular books. Tuchman explains that historiography is, first and foremost, narrative history. She sees herself as a storyteller – 'a narrator who deals in true stories not fiction' (Tuchman, 1981, p. 18). She agrees explicitly with Leopold von Ranke, who saw it as his purpose to reveal 'how it essentially was'.

The Phase of Research

In the spring of 1963 Barbara Tuchman was invited to Radcliffe College to present a lecture on her research methods. This lecture was eventually published as a chapter in her 1981 book, *Practicing History*. As soon as she spoke, it became evident that she subscribed to Sherlock Holmes' maxim of dividing the research process into two distinct phases: the research phase, and the processing phase.

In the research phase, Tuchman explained, she would collect all relevant evidence. In practice, she would begin by reading books by other historians. However, she warned against doing too much of this introductory reading; it may be a hazardous thing to read such secondary sources too carefully. It is best to use them as guides at the outset of a project 'to find out the general scheme of what happened', and then to jump quickly into the primary sources (Tuchman, 1981, p. 19). The primary sources would include memoirs, letters, diaries, minutes, generals' campaign reports and so on. Such sources are systematized in national archives. Serious research into international events must include visits to such archives, and will therefore involve much travel. A research project on the outbreak of the First World War would most certainly include visits to archives in London, Paris and Berlin – and often also to national libraries and special collections. In addition, it is useful to visit the places where the action occurred to get a sense of the geography, the landscape and the climate in which the events occurred.

Tuchman knew perfectly well that historians seek to explain. But explanations need not take the same form as those we find in the sciences. Adhering to Ranke's dictum, Tuchman argued that the historian should not even think about causality in the natural science sense of the term. For the historian who worked carefully with his sources, the causal chain of events would emerge naturally. 'As to the mechanics of research', she explained,

> I take notes on four-by-six index cards, reminding myself about once an hour of a rule I read long ago in a research manual, 'Never write on the back of anything.' Since copying is a chore and a bore, use of the cards, the smaller the better, forces one to extract the strictly relevant, to distill from the very beginning, to pass the material through the grinder of one's own mind, so to speak. Eventually, as the cards fall into groups according to subject or person or chronological sequences, the pattern of my story will emerge. (Tuchman, 1981, p. 20)

The main problem with this phase of research has nothing to do with knowing how to explain; rather, it is knowing when to stop. Her advice to young historians is this: 'One must stop *before* one has finished; otherwise, one will never stop and never finish' (ibid., emphasis in original).

The Phase of Processing

Knowing when to stop is difficult in the research phase as this is heady, fun and 'endlessly seductive' (Tuchman, 1981, p. 21). By contrast, the processing phase is hard and difficult work. It involves much thinking. Most of all, it involves writing. 'One has to sit down on that chair and think and transform thought into readable, conservative, interesting sentences that both make sense and make the reader turn the page' (ibid.). It is laborious work that involves writing, revising, rearranging, adding, cutting and rewriting.

This work can be gruelling. First, the writing process itself is slow, often painfully so. Sometimes the writing is agonizing; for example, when last week's text is found to have strayed from its object and has to be rejected *in toto*. If that wasn't hard enough, the historian must keep tabs on the many references and sources that trail along with any serious history text, because the historian must back every claim with sources through a painstaking process of reference and bibliography.

Different scholars use different rules and conventions here. Social science authors tend to refer to their sources simply by putting the name of the author and the publication year of his/her text in parentheses and inserting this into the text – like this (Ranke, 1956). They follow this up by supplying full details in a bibliography at the end of the book. Historians do this as well. However, some historians prefer more elaborate systems.

The most common alternative is to use notes – either footnotes (which are printed at the bottom of the text page) or endnotes (which are collected in a special section at the end of the chapter or the end of the book), which refer not only to sources, but also to specific documents in carefully ordered archives. In other words, historians may use a wider diversity of sources than social scientists, and put different demands on their reference system. Some authors (or editors) do not want to interrupt the narrative flow by any visible reference. They may prefer to publish a list of sources as a special section of the book; here the book's narrative is substantiated page by page, and the sources used are accounted for in the order they are used. These different referencing and source systems are outlined in Figure 6.1.

The point, of course, is a dual one. First, any scholarly text must display its sources clearly and obviously. Second, the display must help the reader find his way through the sources that have been used in making the analysis. It is worth repeating this second point, because an astonishingly large number of students suffer through years of education without paying attention to references – though they encounter them every day in their readings – and without noticing the strict logic to which they are subjected. In short, students can study, study and study, and still waste much of their time by overlooking some of the most basic of scholarly points: (i) scientific research is a public act; (ii) science depends on testing; and (iii) scholarly references provide the key to both of them.

A highly influential methods book established these basic points in its introductory pages: 'Scientific research uses explicit, codified, and public methods to generate and analyze data whose reliability can therefore be assessed' (King *et al.*, 1994, p. 8). That's it! That's the core of science and scholarship – historical or otherwise. Any author who wants to write a scholarly text must *publish* his work in some way. In doing so, he must expose his sources, thus laying his argument open for any reader to test. A scholarly author must afford *everyone* the opportunity to check and double-check his scholarly claims. If he does not do this, his text is not scholarly.

As if writing well was not difficult enough, good historiography also involves writing objectively. Tuchman (1981, p. 22) explains that this is best ensured if she tries to write '*as of the time*, without using the benefit of hindsight' (emphasis in original). The critical reader may question whether this type of objectivity is even possible. After all, since the historian tries to recreate the past, she *does* know the outcome of the story. Worse, it is this very knowledge that establishes an event as interesting or important in the first place. There can be little doubt that this knowledge will affect the historian's approach to the material: it will necessarily influence the way she reads the documents, selects material for her database, and converts her data into a coherent, flowing narrative. Such knowledge must influence the way the historian selects, emphasizes and adds causal connections to make the narrative flow.

Figure 6.1 *Denotation of sources – references and bibliographies*

References

There are two common systems of reference: author–date and footnote.

1. The *author–date system* has been the standard among social scientists for many years. Here, the source is the name of a person – an author whose work has been relied upon in crafting an argument. The author's name is set in parentheses, together with the publication year of the work (and the relevant page number), thus: (Rampolla, 2002, p. 67). This author–date system is particularly convenient when relying on secondary sources. It needs to be complemented by a bibliography, which usually appears after the main text.
2. The *footnote (or endnote) system* is still used by many historians. One reason that historians still use this rather than the author–date system, is that footnotes are more convenient when using primary sources. It does not have to be complemented by a bibliography – but a bibliography is always a very helpful addition, especially if the work is long and the sources are many.

   Typical footnotes will include references to a book,[1] an article[2] or an archival document.[3] This method tends to rely on Latin abbreviations to help the reader locate the first (and full) bibliographic reference. The most common of these are:

   - *ibid.*: short for *ibidem*, which means 'in the same place';
   - *idem* (*or id.*): means 'the same';
   - *op. cit.*: short for *opera citato*; means 'in the work cited';
   - f. (pl. ff.): means 'and following'.

Bibliography

A bibliography is a listing of books on a particular topic, usually arranged alphabetically according to the authors' surnames. It is bound by very strict rules, but these rules tend to vary with the publication venue. Two of the most common are:

- Titles of books are referred to in italics; publication information (year and publisher) denoted thus:

  Tuchman, Barbara (1962) *The Guns of August* (New York: Macmillan).
- Journal articles are referred to in inverted commas; the journal's name is written in italics; the volume and issue in which the article appeared must be clearly denoted, together with the page numbers it spans. For example:

  Holland, Paul (1986) 'Statistics and Causal Inference', *Journal of the American Statistical Association*, 81(4): 945–60.

Notes

1. For example: Mary Lynn Rampolla (2002) *A Pocket Guide to Writing History* (New York: St. Martin's Press).
2. For example: Paul Holland (1986) 'Statistics and Causal Inference', *Journal of the American Statistical Association*, vol. 81, no. 4, pp. 945–60.
3. For example: Carnegie Endowment for International Peace (1916) *Diplomatic Documents Relating to the Outbreak of the European War*, 2 vols. Ed. James Brown Scott (New York: Oxford University Press).

But Tuchman insists that it *is* possible to be objective. Indeed, to do this she warns that the historian must not be concerned with causation. She writes:

> To find out what happens in history is enough at the outset without trying too soon to make sure of the 'why'. I believe it is safer to leave the 'why' alone until after one has not only gathered the facts but arranged them in sequence; to be exact, in sentences, paragraphs, and chapters. The very process of transforming a collection of personalities, dates, gun calibers, letters and speeches into a narrative eventually forces the 'why' to the surface. It will emerge of itself one fine day from the story of what happened. It will suddenly appear and tap one on the shoulder, but not if one chases after it first, before one knows what happened. Then it will elude one forever. (Ibid., p. 23)

Like good historians, we do not wish to exaggerate. Tuchman is an artist as much as a scientist, and she reveals this in a short note on references that closes her *Guns of August*. Here, Tuchman is willing to relax the Rankean constraint:

> Through this forest of special pleading the historian gropes his way, trying to recapture the truth of past events and find out 'what really happened.' He discovers that truth is subjective and separate, made up of little bits seen, experienced, and recorded by different people. It is like a design seen through a kaleidoscope; when the cylinder is shaken the countless colored fragments form a new picture. Yet they are the same fragments that made a different picture a moment earlier. This is the problem inherent in the records left by actors in past events. That famous goal, '*wie es wirklich war*,' is never wholly within our grasp. (1962, pp. 441–2)

If Tuchman pulls back from definitive objective history, there are still others who are willing to carry on. In particular, there remains an influential strand of neo-Rankean history represented, for example, by the works of Elton (1967) and Goldstein (1976). This is a tradition that Ian Lustick (1996, p. 12) despairingly refers to as the Forrest Gump theory of history, where 'History is as historians do'. In Elton's *The Practice of History*, history is seen as the search for an objective truth about the past. Like Lord Acton's preface to the *Cambridge Modern History*, Elton is suggesting that it is possible to write a definitive history of something, so definitive that it would never need to be written again.

Criticism

Critics of Ranke have noted that his approach produces a very slanted view of history. His demand for primary sources – for official documents, letters and diaries – tends to favour those historical agents who leave traces behind in the form of such documents; this, in turn, marginalizes those actors who do not. As G. R. Elton notes (1967, pp. 20–1):

> Lively minds of little knowledge like to charge historians with asking the wrong questions or with treating uninteresting problems. The history of princes and politics, of war and diplomacy, is often called dull and insufficient; why do we not hear more about 'ordinary people', the lives of the poor, the whole of 'society'?

The problem, Elton notes, is that we do not have direct evidence of this history: 'The past is over and done with: it cannot be relived' (ibid.). For this reason, traditional history was largely political history, as official state papers were the most carefully preserved and easily accessible.

While this is a popular criticism of Ranke's method, it is hardly a devastating one. The individual historian begins a research project by formulating a research question. He then casts about to determine which sources are available and which are most appropriate. It is the responsibility of the historian to choose appropriate sources; and these, in turn, are a reflection of the questions asked – not the other way around. A historian who allows the content of a well-known archive to determine her topic is akin to the drunk who arrives home late one night and loses his house key. Though he has dropped the key in the dark grass by his front door, the drunk chooses to begin his search under the lamppost further down the road (where the light is better).

There is another kind of criticism that is more to the point: Ranke, and the tradition that shadows him, seems to have an unadulterated faith in objectivity. In a multicultural age of many perspectives, this faith may appear anachronistic, if not naïve. However, to those scholars fatigued by postmodern study, this quest for objectivity makes the historical method all the more appealing.

Surveys, Polls and Interviews

This chapter began with a long introduction to the historian's approach: perhaps too long for a book aimed at introducing social science methods and methodologies. We risked such a long introduction because basic Rankean spadework lies at the bottom of all social scientific work, whatever its methodological point of departure. Also, social scientists can only benefit by paying close attention to the historian's high standards of referencing. For reasons such as these, we need to know how historians approach their subject. In particular, we have used the above review to show how the historian provides naturalists with the nuts and bolts of everyday social science. Aided by the historical method, the naturalist can accumulate solid facts, with which subsequent scientific arguments can be crafted.

One of the most common ways of accumulating these sorts of solid facts is by way of the interview. After all, the interview is the journalist's method of choice: it provides a quick and convenient means of getting the news, straight from the horse's mouth. Social scientists rely on interviews – and their related kin, such as surveys and polling – for the same reasons. For the social scientist, interviews tend to be conducted in small groups (so-called focus groups) or one-on-one. The former are simply small groups of individual respondents. The latter can take many forms: they can be conducted face to face, by means of a written or online questionnaire, over the telephone and so on. Whatever their form, naturalists conduct interviews with an eye to securing reliable information that can usefully be plugged into comparative contexts, in order to infer general patterns. Consequently, the techniques, promise and challenges of the naturalist interviewer are quite similar to those faced by the historian: they both develop strategies to deal directly with the threats of sampling bias and source criticism; that is, they need to overcome both sampling error and measurement error.

The first concern is to ensure that the information gleaned by the interview, survey or poll is representative of the larger population from which the interview subjects are drawn. This is the problem of *sampling error*, though it is not always a problem. For example, if we wanted to gauge students' impressions about the effectiveness of our new approach to teaching methods (introduced in Chapter 3), we could ask every single student (as the total number of students was small). But if our ambition is to map opinions in the Minnesota chapter of the Sons of Norway, we might imagine that the universe of potential interview subjects is very large indeed. In this and other large-N situations, we need to ensure that our sample of subjects is representative of the larger universe in terms of the theoretically-relevant characteristics of the sample.

In doing this, the strategy for sample selection is the same as we saw when choosing theoretically-relevant cases in quasi-experimental settings. For example, if we think that sex is a relevant variable for explaining the outcome in which we are interested, then it is important to ensure that the interview sample has equal numbers of men and women. At the same time, we need to develop an explicit strategy for dealing with those subjects whom we had planned to include, but who did not respond (for whatever reason), as this too can affect the representativeness of the remaining sample. Thus, if we are interested in the relationship between attendance and methods' learning, and we conduct our interviews during class time, then the non-respondents are those students who did not attend the lecture – in other words, these are the very students whom we expect to be learning less from our new methods' teaching approach. Consequently, class attendees would be over-represented in the remaining sample.

A good example can be found in the early history of political opinion polling, when journalists began to ask readers how they would vote in order to predict election outcomes. Among the US media that conducted these sorts of polls was the *Literary Digest*, who used its subscription list to mail out over 10 million postal ballots in the run-up to the 1936 US presidential election. When two million of these were completed and returned, the *Literary Digest* was able to (incorrectly) predict that Alf Landon would become president by a significant majority. The problem is the *Literary Digest*'s readership was more highbrow than the rest of the population, so the magazine's sample was unable to capture the substantial number of unemployed and lower-income workers who voted Franklin D. Roosevelt into office.

The second challenge to interview-based researchers is the need to overcome problems of *measurement error*. This error is akin to the historian's *Quellenkritik* – in that the resulting data need to be both valid and reliable. Validity refers to whether our questions actually measure the underlying phenomena we are trying to capture. In other words, we need to develop questions that are able to accurately describe the world as it really is (to borrow from Ranke). We do this by framing the questions in a way that can ensure the questions will not be misunderstood, that the questions themselves are not loaded or leaning, and that the interview subject is responding honestly and in good faith. In much of this, the interviewer – like the historian – needs to employ common sense to secure dependable and verifiable information. As with the experimental designs we saw in Chapter 3, interviewers need to limit the potential for interviewer effects. Finally, the interviewer needs to ensure that the data gathered are reliable; in other words, that the questions we pose will produce identical answers under different conditions (and at different times). In the same way that the historian is looking to triangulate independent sources, interviewers aim to generate reliable responses from several different attempts at a particular question.

These problems of reliability and validity are especially evident in the experiment conducted by Sullivan *et al.* (1978), introduced in Chapter 3. Remember that Sullivan and his colleagues doubted whether respondents to the US National Election Survey had become more ideologically sophisticated after the 1964 election. To test whether this was the case, the three colleagues used an experimental design to compare responses across two different sets of questions. The motivation for conducting this experiment was a suspicion that the evident change in responses reflected a change in the question formats (and the meanings of some items) in the questionnaires used before and after 1964. Consequently, the questions posed were actually measuring different things over time. By conducting the experiment, Sullivan *et al.* were able to show that observed variance in the survey output was the result of faulty questioning over time, and not the ideological maturation of the American voter.

In the light of the previous section, it should now be easy to see how interviewing or surveying techniques mimic the approach used by historians: each tries to minimize sampling and measurement errors in an attempt to secure the sort of careful, objective data that naturalists need when employing their statistical or comparative projects. Like the historian, an interviewer in the naturalist tradition is seeking to uncover the world as it really is (à la Ranke), in a way that can be thoroughly documented and replicated by subsequent researchers. As we shall see later, constructivists also use interviews, but in this situation they use them to secure insights about motivations, processes, even empathy, so that the focus is less on representativeness and more on the relevance of the actual interview subject.

The Case-Study Method

The approach of historians is also used by social scientists of the naturalist persuasion when they generate case studies. Case studies, as noted above, are histories with a point. They are 'cases of something'. The case under study is interesting, relevant or 'in focus' because of that 'something'; because of a larger theoretical concern or a specific research project. While case studies often draw on the techniques of historical scholarship, history itself is usually employed as a database for the construction and testing of theories. It is for this reason that the naturalist tradition is mistrustful of case studies. It believes that studying a single case can yield only limited results. King *et al.* (1994, p. 211) don't mince their words on this account: 'the single observation is not a useful technique for testing hypotheses or theories'.

Still, there is a growing appreciation among naturalists of the knowledge generated by case studies. One reason for this is that case studies have delivered much fruitful work in recent social science. Case study approaches have proved to be particular useful when combined with other, more reputable, approaches of a statistical or comparative nature (see, for example, Bates *et al.*, 1998; Bennett, 2002; Laitin, 2003; and Fearon and Laitin, 2008). Many naturalists recognize that the application of methods' triangulation (Jick, 1979); multitrait/multimethods (Campbell and Fiske, 1959); or nesting (Lieberman, 2005) strategies can produce more robust understanding. When done well, 'multimethod research combines the strength of large-N designs for identifying empirical regularities and patterns, and the strength of case studies for revealing the causal mechanism that give rise to political outcomes of interest' (Fearon and Laitin, 2008, p. 758). In this light, case studies take on a supporting role to approaches that are better endowed to identify empirical patterns.

Another reason behind the growing appreciation of case studies in recent years is its close connection to the historical method. The business of historiography is to show how events 'really happened'. In practice, this is done by presenting a series of interconnected events. The business of the naturalist case study is to isolate particular connections in the expectation that they might turn out to be causal. In this way, the case study may home-in on causal processes as they actually existed in the Real World, untainted by control techniques. The social scientist engaged in this kind of process tracing 'often looks at a finer level of detail or a lower level of analysis than those of the proposed theoretical explanations. The goal is to document whether the sequences of events or processes within the case fit those predicted by alternative explanations of the case' (Bennett, 2008, p. 705). In other words, case studies in this tradition are usually employed to confirm the presumed causal processes that lie beneath larger-N studies. The case-study researcher's focus is trained on explaining a single outcome. Her aim is to unearth evidence of a hypothesized causal mechanism buried in the experience of a particular case.

The assumption here is that patterns exist in the social world and are part and parcel of a larger mechanism that is inherent in the nature of things, and that these patterns can be captured, as J. S. Mill averred, by a succession of simple variable analyses. 'The mechanism linking an independent and dependent variable can be conceptualized as a "machine", where each hypothesized part of the mechanism is seen as a toothed wheel that transmits the "dynamic causal energy" of the mechanism to the next toothed wheel that ultimately results in a given outcome Y' (Beach and Pedersen, 2010, p. 8). When these causal mechanisms are embedded in time, they display an ontological assumption that is consistent with a naturalist perspective. Thus, Jeffrey Checkel (2006, p. 363) notes that the case-study specialists uncover linear causal processes embedded in time, where 'A causes B, B then causes C, C then causes D and so on'.

Types of Case Studies

In spite of suffering from a relatively low status among naturalist social scientists, the case study remains one of the most frequently employed approaches in social-science research. Case studies are used in all the social sciences and are employed in a remarkably large number of different ways. Indeed, at least one book (Ragin and Becker, 1992) has been dedicated to the definitions of, selections of, and criteria for evaluating cases in social scientific enquiry.

There are two very different approaches to the case study. On the one hand is a practical or didactical approach, while the other is an analytical, theory-anchored social-science approach. This chapter will discuss the second approach. However, it may be useful to begin with a few words about the first approach.

The First Kind of Case Study: The Didactical Case Study

The first, didactical approach to the case study is most commonly found in disciplines with a practical cast to them, such as Law, Business and Military studies. Here the case study invites students to investigate particular events in depth in order to learn from them. The legal case brief is an example.

In the case study approach to Law, the student is presented with a detailed description of a conflict or a social issue and the way in which a legal body – a court of law or a legislature – seeks to work out a just and orderly solution. One such case study discusses whether a toxic waste incinerator should be built four miles outside Kettleman City, a small farm-workers community in California's Central Valley. Another case takes as its vantage point the rapid depletion of fish stocks in the waters around the Channel Islands between England and France. The students are asked how the decline in traditional stocks can best be halted. They are encouraged to observe how similar cases have been addressed in other states or countries and to find a suitable solution for the Channel Islands, such as establishing fishery protection zones or marine reserves, or introducing various techniques of maritime management – all of them political solutions that would require the writing and passing of laws.

In business schools or military academies, students are often presented with a thick description of an actual case which called for a decision on the part of a chief executive or a commanding officer. Students are then invited to investigate decisions that were actually made in the past and assess them in the light of for-and-against tallys and decision-making techniques. Similarly, students of diplomacy may be asked to delve into thick descriptions of the 1962 Cuban missile crisis, for example, in order to assess the way in which President John F. Kennedy intervened in the dangerous chain of events and gained control over a situation that threatened to bring the rival superpowers into nuclear war. In all cases, the students are invited to cut their teeth on real situations and learn from them.

These kinds of case studies serve to socialize students into the main moves and characteristic reasonings of a professional field. They involve students in case-study exercises to train them in decision-making. They encourage them to draw lessons from past cases and to distinguish between good and bad decisions. These are the closest some students will get to experiencing an apprenticeship.

The Second Kind of Case Study: The Generalizing Case Study

The second approach to case studies is more typical of the analytical social sciences. It springs from a theoretical interest and has a generalizing purpose. Whereas the first case-study approach is interested in the *how* of the case, the second is interested in the *why*.

This second approach is commonly used in all the social sciences. Indeed, it is one of the dominant methods in the social sciences today. Consequently, there is a large literature that discusses the definitions and applications of case studies, and provides case study typologies. From this literature we choose the influential typology of Arend Lijphart (1971) to show the variety of different roles case studies can play when lined up along an imaginary continuum stretching from descriptive to theoretical.

Lijphart (1971, p. 691) distinguishes between six types of case study:

1 atheoretical;
2 interpretive;
3 hypothesis-generating;
4 theory-confirming;
5 theory-infirming;
6 deviant.

The first two types are of litrle interest for the naturalist, as cases are examined because of an interest in the case per se. In *atheoretical* and *interpretive* case studies there need not be a generalizing dimension to the cases. Consequently, they fit uncomfortably with the theorizing and analytical ambitions of the naturalists (though they would be embraced gleefully by historians). The last three types (*theory-confirming*, *theory-infirming* and *deviant*) are case studies that aim to test an existing hypothesis or assess a theory. It is these types of case study that fit most easily under the naturalist's rubric. To economize somewhat, we combine the theory-infirming and the deviant cases into a single category, called 'mis-fitting' in the discussion below.

This leaves us with the third (*hypothesis-generating*) type of case studies. It is a bit different than the others in that its aim is to use a case to help formulate definite hypotheses or theories (for further testing subsequently). While naturalists loathe to generate theories (or to generalize broadly) on the basis of a single case, they can recognize the heuristic value of case studies.

Thus we find it convenient to distinguish between three types of case studies: fitting, mis-fitting and generalizing. The typical features of each type come from the ways in which they connect with general propositions or theory.

Fitting or Theory-confirming Case Studies. 'Fitting' or 'theory-confirming' case studies investigate the degree to which a given case fits a general proposition. These types of case studies tend to describe a single event and then compare it to an existing conceptual scheme. In short, they serve to demonstrate the explanatory power of a particular theory. In a 'fitting' exercise, a case is chosen as an empirical venue for applying a particular theory. As such, this type of case study tends to be less ambitious than its more critical brethren (the mis-fitting case). In short, it is illustrative. It resembles an attempt to verify a given theory, in a way not unlike the 'verification' principle introduced by the Vienna Circle (see Chapter 2).

As Karl Popper pointed out (whenever he could), early Marxist historiography is rife with examples of this type of 'fitting'. Committed socialists would regularly study historical events and then demonstrate how they conformed to – and confirmed – the Marxist theory of historical materialism. Some of the more obvious examples were written by party intellectuals and published by party presses. Others are far more subtle and possess high scholarly qualities. One case in point is the work by the French historian, Albert Soboul. He occupied the prestigious chair of the French Revolution at the Sorbonne for many years, and studied the revolutionary events in France from a Marxist perspective. His doctoral dissertation from 1958 on the Parisian sans-culottes, is a study of the 'revolution from below' – over a thousand pages of deep and detailed analysis of popular revolutionary movements in Paris during one year of the phase of Terror. Conservative critics have insisted that his work strayed far and wide from the Rankean ideal; that Soboul's (1958, 1962) influential books were not objective history at all but rather an application of Marxist social theory to the causes and courses of the French Revolution. A similar criticism has been levied against the immensely popular books of Eric Hobsbawm (most notably his famous trilogy: *The Age of Revolution*, *The Age of Capital* and *The Age of Empire*).

One of the most common applications of a fitting case strategy is what Harry Eckstein referred to as a *plausibility probe*. In a world with limited resources, Eckstein suggests that researchers might choose to run a sort of trial test of a given theory on a particular case (before investing too much money, time and energy in a full-blown test): 'In essence, plausibility probes involve attempts to determine whether potential validity may reasonably be considered great enough to warrant the pains and costs of testing, which are almost always considerable, but especially so if broad, painstaking, comparative studies are undertaken' (Eckstein, 1975, p. 108).

As an example of a plausibility probe, we can use Eckstein's own effort to explain how democracies work and why they are so stable. One day, a simple argument struck him: democracy involves open and accessible processes of political decision-making, and such processes are more likely to exist in societies that have deeply-rooted egalitarian values, and less likely to evolve in societies that are marked by deep divisions and rigid hierarchies of authority. Could it be that simple, Eckstein wondered? Could democracy and democratic stability simply be a question of egalitarian culture? He decided 'to find out whether or not the idea would sink if properly evaluated' (Eckstein, 1980, p. 14), and began to look around for a stable democracy to study. His choice fell on Norway, for three reasons: (i) he had relatives there whom he had never visited; (ii) Norwegians reputedly had a reasonable command of English so that he could conduct first-hand interviews; and (iii) he knew nothing else about the country. The last of these reasons was very important to him: it meant that the country played no part in the original formulation of the idea.

After a couple of weeks of 'ad hoc anthropological research' (which involved filling out piles of 4 x 6 index cards), Eckstein concluded that here was a very stable democracy marked by high degrees of equality and mutual trust, and by exceedingly flat structures of authority in most aspects of life. It was, comments Eckstein, 'an uncanny match' for what the initial congruence idea would lead one to expect (ibid., p.18).

Two things are worth noting about Eckstein's plausibility probe. First, he travelled to Norway with an idea or a theory in mind, and his intention was to find out 'whether or not the idea would sink'. This intention reverberates with Popperian premises – there are echoes of Popper's falsificationism as well as Hempel's covering law – which serve to pull Eckstein's plausibility probe in the direction of the theory-infirming case study, discussed below. Second, Eckstein did not travel home to the USA triumphantly claiming that his idea was verified or his theory strengthened. His conclusion was more modest than that: he concluded that his simple hunch about a congruency between egalitarian values and stable democracy was not entirely improbable. His brief visit to Norway had encouraged him to pursue the argument further and perhaps develop it into a full-fledged theory. This is what plausibility probes aim to accomplish. In Eckstein's case, it eventually led to several books and articles, each of which helped to refine the celebrated 'Eckstein's congruency theory' of democratic politics.

Mis-fitting, Theory-infirming or Deviant Case Studies. Whereas 'fitting' case studies seek to demonstrate how a case fits a general proposition, the 'mis-fitting' case study seeks to show how a case does not easily fit a general or a universal claim. In Lijphart's (1971, p. 691) typology, mis-fitting cases correspond to his theory-infirming (case studies that weaken a theory marginally) and deviant cases (case studies where cases are known to deviate from established generalizations). In this way, the mis-fitting case employs a logic that mimics that of the falsification principle associated with Popper: a well-chosen case can provide strong support for, or falsify, a given theory. The point is to choose a case which is, in theory, falsifiable and tests a central theoretical claim.

A good example of a theory-infirming study is Mark Peceny's (1997) discussion of the Spanish-American War. Peceny's theoretical vantage point is the popular 'democratic peace' theory – a voluminous literature that links democratic governance with peaceful interstate relations. This posited relationship between the two variables, 'democracy' and 'peace', is so strong that it encouraged Levy (1989, p. 270) to refer to it as the nearest 'we have to an empirical law in international relations'. In the context of such strong theoretical expectations, Peceny chose to study a case where the co-relationship does *not* hold; a case which seems to challenge the democratic peace contention. Democratic peace theory claims that democracies do not go to war against other democracies. By the standards of the late nineteenth century, both the USA and Spain are considered to be democracies – yet war broke out between them in 1898. Peceny examined this case with an eye to testing the validity of different strands of the democratic peace literature.

Peceny finds that only one particular version of the democratic peace literature can explain the outbreak of the Spanish–American War – a version he calls the 'constructivist' theory. This theory invokes the power of global norms and shared international identities to account for the peaceful relationship between democracies. Peceny shows how Spain in 1898 did not share these global norms, nor any form of common identity with the USA; indeed, the USA did not really consider Spain to be a liberal democracy. Hence none of the solidarity-building mechanisms that tend to maintain openness, dialogue and a will to compromise among democratic governments existed in the Spanish–American case. Thus, when conflict increased and the threat of war presented itself, there were no mechanisms to prevent it from breaking out. The outcome of Peceny's argument is twofold. First, he can explain the outbreak of the Spanish–American War – and do so in light of a general theory. Second, he singles out one particular version of the democratic peace theory and shows how its explanatory power outperforms other versions of the same theory. As a consequence, Peceny can use the Spanish–American case to refine and deepen the general proposition of democratic peace theory.

Heuristic Case Studies. The first two types of case studies – fitting and mis-fitting – lean heavily on the deductive side of the inductive–deductive model introduced in Chapter 2 (more precisely, in Figure 2.4). It is also possible to use case studies in ways that lend themselves to the inductive side of the model. It is, of course, impossible to induce reliable knowledge from a single case; however, the study of single cases may spark more general questions or hypotheses. (For example, the French revolution ended with the rise of Napoleon; do other revolutions result in a military regime?) Case studies can even – on more ambitious occasions – provide room for theory building. These are what Lijphart (1971) referred to as 'hypothesis-generating case studies', or what Eckstein (1975, pp. 104ff.) called 'heuristic case studies'. These studies exploit the author's familiarity with a given case to help generate new hypotheses or theories, which can subsequently be tested with a more rigorous design.

Given the complex nature of the relationship between an analyst's familiarity with the empirical terrain and his capacity for theory building, Eckstein (1975, p. 104) – following Becker (1968) – suggests we should think about these types of cases in terms of 'building blocks'. The analyst studies a given case to generate a preliminary theoretical construct. Because this construct is based on a single case, it can do little more than hint at a more valid general model. This model, is then confronted by another case – which, in turn, might suggest ways of amending and improving the construct. These cases can then be assembled, like building blocks, into a stronger theoretical edifice.

On Case Selection

This simple typology of case-studies can be used as a tool for thinking about a case-selection strategy in the naturalist tradition: we choose our cases to gain insight about the nature of causal processes in those contexts where we expect the relationship to hold (fitting); cases where we expect them not to hold (mis-fitting); or in areas which promise the possibility of generating new hypotheses and theories for subsequent testing (generalizing). These strategies have been the most common for selecting cases for more detailed study.

But the question of selecting cases has been a hot topic of discussion among social scientists, and it would be misleading to suggest that these decisions are straightforward. As we saw in the previous chapter, Barbara Geddes (1990) has cautioned against the dangers of trying to infer general conclusion on the bases of cases selected on the dependent variable. King *et al.* (1994, pp. 142–6) note the need to base cases on the independent variable, with no knowledge of the scores on the dependent variable. As Evan Lieberman (2005, p. 444) points out, this opportunity seldom presents itself to small-N scholars: they usually know the outcome to be explained. Instead, Lieberman suggests an intricate 'on/off line' strategy for choosing cases, depending on the goal (for example, model-testing and/or model-building) of the small-N analysis.

Cases might even be chosen randomly, as demonstrated by Fearon and Laitin (2008), to avoid problems of investigator bias. While noting that Algeria was a likely candidate for civil war, on the basis of a larger statistical analysis, their random selection strategy forced them to look more closely at this case. In doing so they discovered a different mechanism at work (per capita income and civil war), than the one posited in the general model. This discovery allowed Fearon and Laitin (2008, p. 773) to conclude that 'random case selection of cases for narrative development is a principled and productive criterion in studies that mix statistical and case-study methods, using the former for identifying regularities, and the latter to assess (or to develop) new explanations of these.'

In short, naturalists can choose from a variety of case types and selection strategies. What all these types of case studies share is an attention to mapping causal mechanisms in contexts that assume a patterned relationship inherent to the social world being studied. In the process, the value of case studies lies in the way that can support (or undermine) more general arguments that have been (or can be) tested in more comparative and experimental frameworks.

The Utility of Case Studies

Naturalist social science is divided on the utility of case study research. On the one hand are the sceptics who, often invoking the principle of induction, claim that case studies can make no more than modest contributions to the social sciences. They hold that induction cannot produce reliable knowledge (as Hume famously averred); attempts to generalize on the basis of a single case are thus seen to be risky endeavours indeed.

Having said this, it is useful to note a simple truth. While the likes of Popper do not trust induction to produce general truths in any case, naturalists do not really care where hypotheses come from, as long they can be tested for veracity. For this reason, induction should not be rejected out of hand; we just need to lower our analytical sights. There is nothing to prevent a scholar from inducing a general proposition on the basis of deep familiarity with a single case, and run the risk of his statement being proved false. Indeed, in practice, general social science statements are often induced from single cases. In so doing, however, scholars do not – of course – present these inductions as true statements; they present them as hypotheses. And hypotheses, as we know from previous chapters, are tentative statements; they are created for the explicit purpose of being tested.

On the other hand, we find more pragmatic scholars who claim that case studies have a proven record. Their number has been rising rapidly in recent years, thanks to the development of new, history-based, research methods. One of these is the method of 'process tracing', touched on earlier in this chapter. It is a form of case study method that allows scholars to trace out underlying causal mechanisms which are nested in the complex patterns and mechanisms of the social world. More recently, a number of interesting (and potentially radical) contributions have examined the nature of 'within-case explanations' to question and de-prioritize the naturalist's traditional reliance on correlation-based understandings of causation. In doing this, naturalists have found it necessary to maintain the integrity of the surrounding context of investigation. Should this understanding of causation spread, case studies are destined to play a much more important role in contemporary naturalist social science.

When this happens, case study research would move closer to a constructivist's understanding of the nature of social reality. Indeed, we can understand these new tools – process tracing and within-case methods – as useful bridges for spanning the methodological divide that has long separated naturalist and constructivist approaches. These approaches can be employed by scholars in both traditions, and many of their objectives seem to be shared. It is for this reason that we've postponed a more detailed discussion of process tracing and within-case methods until later in the book – in Chapter 9.

Conclusion

The historian's method is clearly evident in many of the approaches used by naturalist social scientists. In this chapter we have traced the historian's careful, critical and public techniques in the work of social scientists who employ interviews, polling and case studies. The information gathered by these techniques constitutes solid building blocks or data inputs for subsequent, more scientific, analysis. They become the untainted apples in Descartes' metaphorical barrel.

At this level of understanding there is no problem of incorporating historical research techniques into the naturalist's hierarchy of methods. Good social science and good history both rely on *Quellenkritik*. But there is an important difference that separates history from social science, and this difference reveals itself most noticeably in the complex role that case studies play in the naturalist's tool box of methods.

After all, a case looks beyond the object immediately at hand. As a case of something, it bows before theory and seeks to move from a purely empirical level of exposition to a level of general statements. In practice, case studies force the analyst to jump right into the middle of the methodological muddle. The analyst's nearness to the empirical detail and her heavy reliance on theory mean that she is constantly forced to address the sundry ways in which theoretical claims and empirical evidence often collide. As a result, case study researchers need to be extremely careful about their research design, objectives and case selection.

This concern and focus is – itself – evident in our desire to emphasize the various ways that case studies use different theories, and are used in different research designs. Because of their nearness to the empirical detail, practitioners using case studies are often forced to be much more conscious and explicit about the ways in which they engage their theories, design their research programmes and choose their cases. Case study researchers tend to be more aware of the practical limitations of dividing scientific work into deductive and inductive projects. As a result, case studies tend to involve, in complex ways, a combination of scientific objectives: including both theory development and theory testing.

While case studies provide the researcher with a more direct experience of the interplay between theory and data, and a credibility that is itself derived from the researcher's familiarity with context, these qualities are a real handicap that limits the case study's appeal to the broader community of naturalist social scientists. In particular, the focus on single cases makes it difficult to test hypotheses in systematic and complex ways against empirical evidence beyond the specific case in question. It is for this reason that the case study method remains at the bottom of the naturalist's hierarchy of methods.

Recommended Further Reading

As Carl Hempel's 'The Function of General Laws in History' (1942) ties history to the naturalist science project, it is a good place to start. There are a number of good and well-written introductions to historiography; we recommend G. R. Elton's *The Practice of History* (1967) and Richard Evans's *In Defence of History* (1997). For an introduction to *Method and Meaning in Polls and Surveys*, see Howard Schuman's (2008) book with that title. Finally, there are several very good introductions to case studies in social science. Among them, we recommend Ragin and Becker's *What Is a Case?* (1992) and Robert Yin's *Case Study Research: Designs and Methods* (1994).

Chapter 7

Sowing Doubts About Naturalist Methodology

Up to now we have presented the naturalist tradition. We have tried to make a fair and straightforward presentation that is familiar to social scientists in a wide range of subject areas. We have tried to avoid creating a straw man for what is easily the hegemonic methodological tradition in social science. We have attempted to identify some of the key philosophical assumptions of the naturalist approach, but have exposed these assumptions to little critical thought. In this chapter we change gear.

Here we begin to assess the naturalist approach more critically. We intend to raise some questions about the naturalist tradition, and we want to raise some doubts about its application. Such questions and doubts will serve to open up a space for alternative approaches. They will, in other words, pave the way for our presentation of the constructivist methodology that follows in Chapter 8. This constructivist alternative will, for the remainder of the book, prod us to explore a menu of alternative social science methods.

In a sense, this chapter functions as the book's fulcrum, upon which the naturalist and constructivist approaches teeter. In particular, we aim to show how sensible people can hold different opinions about the nature of social reality, and that different ontological positions may lend themselves to new epistemologies that are less beholden to the naturalist tradition.

This chapter is divided into three main sections, which discuss, respectively, ontological, epistemological and methodological aspects of naturalism. Each section sows seeds of doubt in its particular aspect of the naturalist approach. We hope to bring these seeds to fruition in the chapters that follow.

We are aware of the danger of throwing the baby out with the bathwater. However, recognizing the important shortcomings to the naturalists' approach does not mean that we must jettison the naturalist project *in toto*. In fact, pointing out naturalist shortcomings and raising doubts about the methodology's ontological preconceptions, can make it possible to adapt the naturalist approach in ways that will strengthen its analytical powers or identify more accurately the areas in which ir works best.

Ontological Doubts

Since Chapter 2 we have accepted several important naturalist assumptions. Most important of these are: (i) that a Real World exists out there; (ii) that this World exists independently of our interrogation of it; and (iii) that it is ordered. We have trusted the arguments of John Stuart Mill and others, and accepted their claim that the world is characterized by patterns and regularities. This trust has not been blind or frivolous. After all, there can be no doubt that these ontological assumptions have yielded great rewards. Elaborate theories, grounded in these assumptions, have taught us much about our world and allowed us to master many aspects of our universe. It is difficult to imagine sending a rocket to the moon, building an artificial heart, or connecting the world through a dense network of computers, without theories that rest on these important ontological assumptions.

Nevertheless, doubts about these assumptions have a long and influential pedigree. Most famously, perhaps, Plato argued forcefully for the ephemeral and unreliable nature of the material world – and of the knowledge that is derived from it. Indeed, it is worth recalling that many of the naturalist approach's founding fathers did not take the material world for granted. David Hume struggled with the nature of reality and René Descartes with its relationship to a benevolent God.

To consider the meaning of such doubts with respect to social science research, we can organize them under two headings: doubts about the reality of the natural world, and similar doubts about the reality of the social world. For obvious reasons, we want to spend most of our time addressing the latter type of doubt.

The Natural World

Assumptions about inherent patterns in the world are most common (and more reasonable) in the natural sciences. It is not controversial to suggest that hydrogen's relationship to oxygen is relatively fixed in a given context; it is more difficult to claim that the relationship between democracy and Protestantism, or despotism and Islam, is of the same invariant nature. It was during the course of investigations into the natural world that the naturalist ontology was born, and it is in this context that it thrives. Still, even here, it is possible to raise doubts about whether the basic ontological assumptions of naturalism hold at all levels. These doubts can be raised on two fronts: one metaphysical, and the other physical.

At a metaphysical level, it is easy to question assumptions about the existence of an ordered nature – in other words, that the Real World consists of regularities, patterns and recurrences. As we have noted repeatedly, this assumption is crucial to the naturalist's endeavour: it allows scientists to formulate universal laws, and to employ inductive methods in their search to uncover them.

This assumption was easier to accept at a time when the scientific community believed in the existence of an all-powerful God, who could be held responsible for the order that scientists sought to uncover. In the era after which Nietzsche (among others) proclaimed 'God is dead', it may be less convenient to assume that the world is characterized by a divinely sustained order. For Friedrich Nietzsche (1967, p. 113), 'our attitude toward God as some alleged spider of purpose and morality behind the great captious web of causality, is *hubris*' (Nietzsche's emphasis). Without the convenient resort to a Great Designer, it has become more difficult to assume that the world is characterized by an underlying order; at the very least, it is now necessary to invoke other explanatory principles.

Ontological doubts, however, needn't always spring up from metaphysical terrain. There are many other reasons for doubting the existence of universal laws and patterns in nature. Some of these are derived directly from experience. Consider the experience of Franz Boas (1858–1942), a Prussian merchant's son who studied Physics and Geography at the universities of Bonn, Heidelberg and Kiel in the 1870s. While working on a dissertation in Physics, he ran into empirical inconsistencies when he tried to discuss the properties of seawater. In particular, he relied on observers who disagreed about the colour of the water: some claimed the water was blue; others that it was green; still others described the colour as something in-between. In short, it was unclear whether the colour patterns he was trying to document were an artefact of the water itself, or its observer.

In the end, Boas accounted for this difficulty by invoking Immanuel Kant, who acknowledged that there are differences in human perception (as we shall soon see). Nevertheless, the experience plagued his research and affected his scholarly development. Boas recognized that even the most systematic observations might be distorted by subjective elements. Eventually, this led him to develop a 'psychophysical' theory, which sought to account for problems in empirical research by reference to psychological variables.

Like Boas, other natural scientists have come to acknowledge that their world of study might not be characterized by the universal laws and patterns that have traditionally anchored their ontological point of departure – at least not at all levels of inquiry. Over time, the religious context of science has changed in ways that make it less completing to assume a patterned logic to nature. Finally, there is an increased realization that the world is a very complicated and complex place.

The Social World and Its Paradoxes

Since the beginning of social science, concerns have been voiced about whether approaches to studying the natural world are applicable to studies of the social world – concerns that have only grown with the realization that some of the basic ontological assumptions don't seem to hold, even in the natural world. For many observers, the natural and social worlds are inherently different, and this difference is obvious: people, unlike particles, *think*. The subjects of social studies are self-aware, reflexive, creative and intentional: they rationalize their actions; they are motivated by purpose; and they enjoy a certain freedom of action. All these inherently human capacities make it possible to doubt whether mechanistic assumptions about natural patterns in a Real World make sense when studying the social world.

This concern is clearly evident in the career path of Professor Boas, who migrated from Physics and Geography into Anthropology. Indeed, Boas's intellectual development offers a window from which we can see broader developments in the philosophy of social science. In 1883, he took part in a geographical expedition to map the Baffin Islands in the North American Arctic, where the generosity and kindness of the native Inuits made a lasting and significant impression on the young graduate. Coincidentally, this was the same year that the polymath German philosopher, William Dilthey (1833–1911) published *Einleitung in die Geisteswissenschaften*, a learned critique of the attempts to apply natural science approaches to 'the sciences of man, society and the state'.

After a year in North America, Boas returned to Germany in 1884. That year also saw the publication of Wilhelm Windelband's (1848–1915) essay comparing *Geisteswissenschaften* with *Naturwissenschaften*. Windelband (1911 [1884]) invoked Immanuel Kant to explain the difference, arguing that there exist two kinds of scientific reason: one (*nomothetic*) is typical of the natural sciences and seeks to generalize and derive laws that explain objective phenomena; while the other (*idiographic*) characterizes the human sciences and seeks to specify an effort to understand the meaning of contingent, unique and often subjective phenomena.

Boas came to embrace these arguments, and contributed to a doubt that has always plagued the social sciences – one shared by Kant, Dilthey, Windelband, and many others since: whether approaches to studying the natural world are in fact applicable to studies of the social world. Once we distinguish between the natural world and the human world – and we introduce two corresponding scientific logics – we also begin to see how patterns in the social world might appear to be fleeting, subjective and even unreal to the careful observer. Sceptics among us begin to wonder if the patterns we observe are not of our own making.

The next section introduces three important ways in which the social world is significantly different from the natural world. These differences concern the importance of: (i) ruptures; (ii) agency; and (iii) perspective.

Unpredictability: The Trouble with Swans

In the naturalist approach, we assume that the Real World is patterned, that we can observe and learn about these patterns and exploit their potential. But how suitable is this approach for handling patterns in human behaviour? If we, as social actors, have made these patterns ourselves, couldn't we also *un*-make them? There are plenty of examples where established social patterns have been undone – either phased out by slow and steady evolution (for example, driven by technological evolution) or disrupted by sudden events (such as revolution or war). Should social science, then, focus on the patterns and overlook the ruptures? Or should it study the changes? Should our attention be drawn to the stable equilibrium points, or the asymmetric shocks that catapult us from one set of patterned understandings to another?

To consider this problem, think back to Popper's example of white swans, and the intellectual commotion that was created by the 'discovery' of black swans 'down under'. This is what Nassim Taleb (2007) does. Modern social scientists, Taleb reminds us, do a remarkably good job at identifying patterns in the world: we seem to have a good grasp of the white swans, and society is well aware of their existence. Indeed, in exploiting this knowledge we provide order and predictability in our lives. But herein lies the problem: as social science discoveries (what we might call the documentation of white swans) have a disciplining effect on society. As a result, when black swans do appear, they have an inordinate impact on the social world (and our understanding of it).

Taleb defines a Black Swan (with capital letters) as an event with three attributes: (i) it is rare; (ii) it has a big impact; and (iii) it comes as a surprise. Black Swan events are surprising. They lie outside of the realm of induction and cannot be predicted. The length and severity of the First World War was such an event. The Great Depression was another. The rise of Nazi Germany, the outbreak of the Second World War, the collapse of the Soviet Union, the spread of the internet, the 9/11 terrorist attack on the USA, the rise of Islamic fundamentalism ... these were all Black Swans: low-probability but high-impact events.

Once a Black Swan event occurs, social scientists react with surprise: they wonder what they had previously missed and scramble to make up explanations *after* the fact. When we stop to think about it, writes Taleb (2007, p. xviii):

> A small number of Black Swans explain almost everything in our world, from the success of ideas and religions, to the dynamics of historical events, to elements of our own personal lives. Ever since we left the Pleistocene, some ten millennia ago, the effects of these Black Swans has been increasing. It started accelerating during the industrial revolution, as the world started getting more complicated, while ordinary events, the ones we study and discuss and try to predict from reading the newspapers, have become increasingly inconsequential.

Agency

It is important to be attuned to the existence of Black Swans. They lurk beyond the horizon of our inductive powers, from where they may come unpredicted and unannounced – sometimes with shocking effect, and always as a reminder of the limited nature of human reason. The arrival of Black Swans should remind us that we are self-aware, reflexive and creative actors; that we think and reason, and interact self-consciously with our environment; that we develop norms, rules and regularities to order our society. We are active agents, motivated by purpose. We rationalize our actions and enjoy a certain freedom of action. We are, in short, very different from the largely passive subjects studied by natural science.

This fundamental truth problematizes our reliance on scientific approaches that assume the existence of rather mechanical and autonomous patterns in the natural world. This observation, in turn, raises the important question as to whether the patterns we see in the social world are actually inherent to the world – or whether they result from human agency in that world (and hence change with human circumstance).

To consider these difficult questions, imagine yourself as a seventeenth-century diplomat. Your profession has long understood (and argued) that sovereign states find themselves in constant conflict. But now, in the early seventeenth-century, you and your colleagues begin to recognize the existence of a 'balance of power': a force that seems to provide some semblance of order among sovereign states in Europe. On recognizing this force, you employ it relentlessly: wars are now explained as a 'breakdown of the balance of power'; interventions are justified as means to redress the lack of balance among states; peace treaties are signed in order to maintain a stable equilibrium. In short, balance-of-power theory has become balance-of-power practice.

Now fast forward to the recent past. Throughout most of the post-Second World War period, international politics was dominated by a superpower conflict between the USA and the USSR. After the end of the Cold War, when the nature of the post-communist world was still unclear, Samuel Huntington (1993, 1996) developed a controversial 'Clash of Civilizations' argument. He argued that a new international cleavage was developing across civilizations. When he introduced his argument in the early 1990s, few observers were willing to recognize the existence (or importance) of such civilizational cleavages. Some twenty years later, many social science students find in Huntington an accurate depiction of the nature of today's international society.

These two examples make us wonder whether the existence of a balance of power and/or a civilizational divide are inherent qualities of the social world, or whether these evident patterns are the results of our own making. Was Huntington's observation the result of a remarkable prediction, or was his observation in fact an important factor in bringing this pattern to life?

If these patterns are the result of our actions, as social actors or observers, we open up a whole new series of questions that need to be asked. Not only do we need to document the existence of these patterns, we also need to know where they come from, and why they came when they did. After all, why didn't diplomats discover the balance-of-power principle earlier (or, did a balance of power exist earlier, but was somehow hidden or less evident)? Where did the clash of civilizations come from? Has it always been with us, lying dormant under the Cold War? Or is it something new? Given Huntington's stature and influence, could his argument have become a self-fulfilling prophesy? Perhaps the world *is* as we see it, because a respected authority such as Huntington told us to see the world in this way?

Perspectivism

The previous two points – the problem with predictability and agency – lead us to our third, which was also Boas's basic point: that the objects we observe may change in appearance when placed in different contexts and viewed from new perspectives. Recognizing the constructed nature of social reality is the starting point for many postmodern approaches, which aim to rid social inquiry of rigid assumptions about fixed identities. This is a form of ontological pluralism that can be traced back to Friedrich Nietzsche (1844–1900). From his point of view, there are no patterns in the Real World; in fact, there *is* no Real World. There is no intelligible world to be known. To the extent that we find the world intelligible, it is a result of the observer imposing his or her conceptual framework on to the subject. What science gives us, argued Nietzsche, is not a description of the world as it is in itself, but a practical and useful way of organizing our experiences.

Michel Foucault (1984, p. 127) popularized this Nietzschean position: 'We must not imagine that the world turns towards us a legible face which we would only have to decipher.' The world, he continues, 'is not the accomplice of our knowledge; there is no prediscursive providence which disposes the world in our favor'. Quite the opposite, in fact: we impose our discourse on the world and make it intelligible.

Each of us has these 'illegible' faces. You and I, our football club, our political party and our nation (to name just a few examples) have multiple identities. You might consider yourself to be a student, a sailor, a drinker, a footballer, a mother, a blues guitarist, a denizen of the world and any number of other things. And you can be all of these people, at different times, in different places, to different audiences.

Larry Preston made this point in one of the most readable pieces ever published in the *American Political Science Review* (Preston, 1995). He is mainly concerned with how the voices of marginalized people are appropriated and perverted by scholars who allegedly represent them. His personal anecdote of a return to the hospital in which he worked as a younger man shows how identity can be affected by representations (here by clothing). As a young man, working as a janitor in the local hospital, Preston was consciously aware of how 'invisible' he was to the hospital staff. As a janitor he was unimportant. Later, as a professor, he happened to return to the hospital – this time armed with the professional's body armour of suit and tie. The staff's reaction to him was now one of respect and acknowledgement. He was a different person now, an important person.

If we accept that signals and interpretations can vary from time to time, or from context to context, then it becomes increasingly difficult to be certain about the reality, the concreteness, the singularity of the objects/actions we are surveying as social scientists. Recognizing this, however, does not leave the analyst stranded helplessly on the sidelines. These very 'weaknesses' (in the eyes of the naturalists) can be turned, judo-like, to the analyst's advantage. Meaning, understanding, empathy and purpose become keys to understanding when simple observation escapes us.

Conclusion

To conclude this section, it is possible to raise doubts about three central ontological assumptions associated with the naturalist approach. First, it is clear that some law-like patterns exist in nature, and that natural scientists can identify them and exploit them to great advantage. But it is not at all clear whether it is reasonable to assume that the social world can (or should be) treated in the same manner.

Second, given the role of agency and meaning in human activity, there may be good reasons to doubt whether the social world exists independently of its interrogator. Social science is not the same as natural science. The two realms do not only obey different forms of logic (as Kant argued); they perform different functions in society. In the words of the Nobel-prize-winning economist, Thomas Schelling (1978, p. 19):

> Social scientists are more like forest rangers than like naturalists. The naturalist can be interested in what causes a species to be extinct, without caring whether or not it does become extinct. (If it has been extinct for a million years his curiosity is truly without concern.) The ranger will be concerned with whether or not the buffalo do disappear, and how to keep them in a healthy balance with their environment.

Finally, there are sufficient reasons to doubt that the social world exists as a single entity accessible equally to any observer with the proper instruments and attitude. The social world – or better the social worlds (in the plural) – seem less certain, more contingent, and capable of presenting themselves in many different forms.

Social scientists study the world with the aim of improving it. Most of us think that knowledge is power and we hope that the patterns we discover, and the insights we gain, have some use. It is for this reason that we seek to identify and appreciate these patterns (as well as when and why they lapse), and employ appropriate epistemological techniques to understand them. It is to these techniques we now turn.

Epistemological Doubts

Once we relax our naturalist assumptions and consider the possibility that some of the social world's apparent patterns might be neither universal, natural nor independent of our observations, we are quickly made aware of the limitations of an empiricist epistemology. In a world that reveals itself in so many complex ways, can observation alone be sufficient to understand it? The limits of the naturalist epistemological approach can be grouped under three headings, concerning the roles of: presuppositions; meaning; and scientific authority. These limitations, in turn, provide support for alternative epistemological traditions less anchored in the empiricist tradition.

Presuppositions

The first epistemological doubt arises from the role of presuppositions in framing our empirical investigations. Today, this position is associated with Robin G. Collingwood (though earlier authors, in particular Immanuel Kant (1929 [1787]), play an important part in getting the ball rolling). In his *An Essay on Metaphysics*, Collingwood (1962 [1940], pp. 144ff) argues against the (naïve) view that it is possible simply to observe facts via the senses, and to classify them by means of logical thought. Facts are not just 'out there'. For Collingwood, facts are social and historical phenomena. Furthermore, they are made by humans.

Collingwood's view is evident in the very etymology of the word itself: 'fact' is derived from the Latin *facere*, which means 'to make'. This logic is also evident in other languages whose word for 'fact' is not derived so directly from the Latin root. In French, for example, a fact is *une faite*, from the verb *faire* ('to make'). In Spanish, a fact is *un hecho*, from the verb *hacer* (again, 'to make'). In Italian: *un fatto* (from the verb *fare*, 'to make'). In German, a fact is *ein Faktum* or *eine Tatsache* (*Sache* = 'matter'; *Tat* = deed, from the verb *zu tun*: i.e. 'to make' – literally: a thing that is made).

In which sense can social facts be made by human beings? An influential epistemological answer is that observations of them (and the classifications that follow) depend critically on what Collingwood called *presuppositions*. The notion of presuppositions is really very simple – and this simplicity is the main reason we use Collingwood to illustrate this important epistemological point. He wrestled with this point while on the open sea, on a voyage undertaken to improve his failing health. The first chapter of his *An Essay on Metaphysics* was written aboard the MV *Alcinous* and refers to a seemingly trivial event:

> I write these words sitting on the deck of a ship. I lift my eyes and see a piece of string – a line, I must call it at sea – stretched more or less horizontally above me. I find myself thinking 'that is a clothes-line', meaning that it was put there to hang washing on. When I decide that it was put there for that purpose I am presupposing that it was put there for some purpose. Only if that presupposition is made does the question arise, what purpose? If that presupposition were not made, if for example I had thought the line came there by accident, that question would not have arisen, and the situation in which I think 'that is a clothes-line' would not have occurred. (Collingwood, 1962, p. 21)

In order to observe anything, Collingwood concludes, we must observe it in relation to something else – to some pre-existing criterion or condition. In other words, we must first have some idea of what we are supposed to see before we see it. Otherwise, the 'facts' under our noses make no sense to us.

Karl Popper (1989, p. 61) made a similar point. He recalls how he once began a lecture with the following instructions to his students: 'Take pencil and paper; carefully observe, and write down what you have observed!' The students, of course, were puzzled: what was it that Professor Popper wanted them to observe? Clearly the instruction, 'Observe!' was absurd on its own. This was, of course, Popper's point: his aim was to demonstrate three things. First, that observation always requires specific directions; that it 'needs a chosen object, a definite task, an interest, a point of view, a problem'. Second, that description presupposes a descriptive language, with real words and a system of classification, which in turn 'presuppose interests, points of view, and problems'. Finally, Popper wanted to show how presuppositions and language are formed by the needs and interests of the observer. Thus a hungry animal would divide its environment into edible and inedible things, and an animal in flight would perceive the world in terms of roads to escape and hiding places. 'Generally speaking ...' Popper observes:

> objects can be classified, and can become similar or dissimilar, only in this way – by being related to needs and interests. This rule applies not only to animals but also to scientists. For the animal a point of view is provided by its needs, the task of the moment, and its expectations; for the scientist by his theoretical interests, the special problem under investigation, his conjunctures and anticipations, and the theories which he accepts as a kind of background: his frame of reference, his 'horizon of expectations'. (Popper, 1989, pp. 61–2)

Presuppositions are related to needs and interest and they give rise to different frames of reference for understanding the world. They raise doubts about the ability of sensory perception to guarantee objectivity – perceptions can be framed by presuppositions to help us see one of many potential faces of reality. It is in this light that Dick Sklar once noted, 'theories are conceived in ideological sin rather than scientific virtue' (Sklar, cited in Geddes, 2003, p. 21).

Hanson's (1958) book, *Patterns of Discovery*, is filled with amusing examples of how a picture can be interpreted in a variety of ways. At some time in our life, each of us has probably seen one of a series of fun illusions that depict a pretty young maiden and an old hag (concomitantly). In Figure 7.1, we have reproduced the famous wife/mother-in-law illusion. As often appears to be the case (though neither one of us speaks from personal experience!), the wife of one's dreams can turn instantly into the mother-in-law from hell. Both creatures, it seems, coexist in the fragile frame at the altar.

As love would have it, we are – at first – drawn to the pretty girl, while the unsightly mother-in-law initially escapes our detection. It is only after we are told that the mother-in-law actually exists (perhaps by our best mate), that we begin to see a different picture. Under this new investigatory light the other identity emerges.

Figure 7.1 ***Wife and mother-in-law***

Source: The original source of this picture (*left*) is said to be an anonymous German postcard from 1888. The picture was used as part of an advertising campaign for the Anchor Buggy Company from 1890 (*right*).

Being told to look for a mother-in-law is akin to having a theory that tells you to search for something in the empirical data before you (just as Sherlock Holmes knew what to look for in the mud outside Silver Blaze's stable). Indeed, as Martin Hollis (1994, p. 79) has it: 'Observation has become so bound up with interpretation and hence with theory that, in deciding what the facts of observation are, we may be deciding between rival theories.'

A pertinent example of this dilemma is found in the way that scientists have attempted to test psychic 'talents'. Consider the highly publicized feats of Uri Geller. He was investigated and endorsed by several prominent scientists – though none of them actually witnessed his spoon-bending powers under controlled conditions. Remarkably, Geller convinced the investigating scientists that many of the control arrangements being suggested were 'aesthetically unappealing' and the scientific observers succumbed to this argument:

> To comprehend how such prominent scientists can paint themselves into such a corner we must view the situation from their perspective. When they wrote their article, they had already become convinced of

> Geller's paranormal powers. They realized that no such powers had ever yet survived scrutiny by scientific methods. From their perspective, then, the major task was to find a way to keep the powers they credited from fading under investigation. If they could find conditions that enabled the 'psychic' to produce his phenomena reliably in the laboratory, then they could later bring in the skeptics and use more traditional scientific methods. (Hyman, 1989, p. 148)

Meaning

Empathy is frequently employed to explain historical and social events. This is because the social world is saturated with meaning, and meaning can be used to help the analyst understand an actor's motivations.

It is generally assumed that this extensive web of meaning is one of the most important differences that separate the natural and the social world. Richard Rorty, however, thinks this is a mistake: the natural world too is caught up in its own webs of significance and meaning:

> when it is said that 'interpretation begins from the postulate that the web of meaning constitutes human existence,' this suggests that fossils (for example) might get constituted *without* a web of meanings ... To say that human beings wouldn't be human, would be animal, unless they talked a lot is true enough. If you can't figure out the relation between a person, the noises he makes, and other persons, then you won't know much about him. But one could equally well say that fossils wouldn't be fossils, would be merely rocks, if we couldn't grasp their relations to lots of other fossils. Fossils are constituted *as* fossils by a web of relationships to other fossils and to the speech of the palaeontologists who describe such relationships. (Rorty 1982, p. 199, his emphasis)

Rorty took an argument that had been developed by Dilthey, Windelband, Boas and others, and applied it to the natural sciences. In doing so, he stirred up a good deal of controversy. Natural scientists did not take well to the idea that facts are not things that can simply be observed.

Most of us are familiar with the important part that meaning plays in interpreting everyday events. A classic example of this was made famous by Clifford Geertz in his introduction to *The Interpretation of Cultures*. Geertz refers to Ryle's discussion of 'thick description', where we are asked to consider:

> two boys rapidly contracting the eyelids of their right eyes. In one, this is an involuntary twitch; in the other, a conspiratorial signal to a friend. The two movements are, as movements, identical; from an I-am-a-camera, 'phenomenalistic' observation of them alone, one

> could not tell which was twitch and which was wink, or indeed whether both or either was twitch or wink. Yet the difference, however unphotographable, between a twitch and a wink is vast; as anyone unfortunate enough to have had the first taken for the second knows. (Geertz, 1993 [1973], p. 6)

To distinguish one meaning from another, the observer has to *interpret* the phenomenon in the constitutive context to which it is anchored. To the extent that naturalists embrace an 'I-am-a-camera' perspective (and we think this is a pretty good description of their empiricist epistemology), they will have trouble distinguishing between similar phenomena of this type. As a consequence, preserving and enhancing constitutive contexts must be a central objective for those who hope to employ meaning to explain social phenomena.

A slap in the face may be the only significant consequence of misinterpreting a blink for a wink. But in the social world, interpretations and misinterpretations of simple images may have significant consequences. Kevin Dunn (2006, p. 371) reminds us of this when he comments on two photographs that circulated in the media in the aftermath of the flooding in New Orleans in the wake of Hurricane Katrina:

> The first showed a couple chest-high in water with bags full of groceries. The caption stated that this couple had 'found' food. The second photo was of a similar scene, a woman chest-high in water with a bag full of groceries, but she was identified as a 'looter'. This disparity generated much attention because the 'finders' were Caucasian, while the 'looter' was African American. But beyond the racial elements at work here, these representations enabled and justified certain actions. Police, for instance, would be expected to assist the couple and arrest or even shoot the single woman.

Scientific Authority

This brings us to our final epistemological challenge: the naturalist's reliance on scientific authority. As we noted in Chapter 2, the naturalist approach leans heavily on an empiricist epistemology, mixed with a healthy dose of rationalism. So far we have mainly discussed difficulties concerning observation, and questioned the empiricist basis of scientific authority. In this section we shall suggest that naturalism's reliance on reason is not without problems. In fact, much of the power of science comes not from its reliance on reason or sense perception, but on rhetoric and on science's own image as an important source of authority in the modern world.

We begin with the power of reason. While academics are often loath to acknowledge it, privileging reason introduces and sustains a number of biases into the nature of our study. Reason can make us ignore and devalue important parts of the human experience. This approach leads us to:

> favor the head over the heart; the mechanical over the spiritual or the natural ... the inertly impersonal over the richly personal ... the banal collective over the uniquely individual, the dissociated anomic individual over the organic collective; the dead tradition over the living experiment; the positivist experiment over the living tradition; the static product over the dynamic process; the monotony of linear time over the timeless recurrence of myth; dull, sterile order over dynamic disorder; chaotic, entropic disorder over primordial order; the forces of death over the forces of life. (Graff, 1979, p. 25)

Worse, once we recognize the fleeting and subjective nature of social activity, we might begin to doubt the utility of prioritizing 'scientific' insights, derived from sterile and structured empirical proofs mixed with reason. In this new ontological setting we might wonder whether the Harvard-trained statistician is really a better student of contemporary human behaviour than the popular rap or country music artist (whose exposure to the real world may be more authentic).

Post-structuralists, such as Michael Shapiro, are adamant in their critique of the social scientists' over-reliance on scientific authority:

> Part of what must be rejected is that aspect of the terrain predicated on a radical distinction between what is thought of as fictional and scientific genres of writing. In the history of thought the distinction has been supported by the notion that the fictional text, e.g., the story, play or novel, manufactures its own objects and events in acts of imagination, while the epistemologically respectable genres, such as the scientific text, have 'real' objects and events, which provide a warrant for the knowledge-value of the text's statements purporting to be about the objects and events. (Shapiro, 1988, p. 7)

Shapiro's book, *Reading the Postmodern Polity* (1992), is a masterful example of how the voices of novels and myths have a legitimate and convincing voice in social scientific discourse. His comparison of DeLillo's *Libra* (1988) and Bellah *et al.*'s *Habits of the Heart* (1986) shows how a fictional biography might outperform a large scientific project in capturing America's cultural diversity. If novels are legitimate authorities for social understanding, why not graffiti? Beavis and Butthead? Prisons? The body itself? Indeed, analysts have explored all these venues (and more!) in search of insights into the social condition.

The economist Donald McCloskey makes a similar – if more explicit – point in his *The Rhetoric of Economics* (1986). While mainstream economists tend to market themselves as top-shelf methodologists, adorned in sophisticated formal and econometrical labels, the bite of their argument (if and when it holds) usually rests on masterful rhetoric: reference to a popular truth, a myth, an established authority and so on.

The power of myth among contemporary economists was clearly evident in debates over adopting the euro. In the run-up to European monetary union, a consensus developed for fixed exchange rates that was frequently argued over and defended in terms borrowed explicitly from Homer's *Odyssey* (see, for example, Giavazzi and Pagano, 1988). Like the Sirens, the beauty of whose singing bewitches sailors far from home, inflation and devaluation were said to have seduced the vote-lonely politician. It is best, this argument holds, that the hands of public officials be tied to a rigid (fixed) mast:

> Therefore pass these Sirens by, and stop your men's ears with wax that none of them may hear; but if you like you can listen yourself, for you may get the men to bind you as you stand upright on a cross piece half way up the mast, and they must lash the rope's end to the mast itself, that you may have the pleasure of listening. If you beg and pray the men to unloose you, then they must bind you faster. (Homer, 1999, p. 105)

The Homeric myth was a very effective rhetorical device in debates among economists over the utility of fixed rates of (currency) exchange. Presumably, the modern economist is familiar enough with the *Odyssey* to understand the relevance of the 'binding to the mast' parable. (But perhaps not familiar enough to remember Circe's second caveat: to impair the hearing of the crew – presumably the *demos* – by filling their ears with beeswax.)

We are not suggesting that economists cannot wield good empirical and rational arguments for why (and when) a country should adopt a fixed exchange rate regime. Our point is simply that we need to be more aware of the role that rhetoric (and in this case, the role of myth) plays in convincing us of this option.

An Example

To consider how some of this chapter's ontological and epistemological doubts apply to social scientific study, we propose to take a closer look at an influential textbook in comparative methods for social scientists.

Przeworski and Teune's *Logic of Comparative Social Inquiry* (1970) is a classic example of the naturalist approach to social science, where the authors introduce students to the explanatory and predictive goals of science with reference to the voting behaviour of an imaginary Monsieur Jacques Rouget. In particular, readers are asked to explain why it is that Monsieur Rouget votes communist. To do this, Przeworski and Teune sketch a two-staged research activity, not unlike the one depicted in Figure 2.4 (see page 46). First, the social scientist is encouraged to collect a number of relevant observations about M. Rouget: he is a male, aged 24, with blond hair and brown eyes, and he works in a large factory. (As we shall see, not all of these observations are relevant; but too much information is always better than too little.)

The social scientist is then encouraged to draw on generally probabilistic statements that are relevant for explaining voting behaviour. (In other words, the second step of this research design finds us at the apex of the research triangle depicted in Figure 2.4.) These statements have already been induced from previous empirical studies, so that we can be confident of their applicability. In particular, we know that:

> One out of every two workers votes Communist; and employees of large organizations vote Communist more often than employees of small organizations; and young people vote Communist more often than older people. (Przeworski and Teune, 1970, p. 19)

From the empirical observations about M. Rouget, and the probabilistic statements listed above, the social scientist can generate a hypothesis about M. Rouget's voter behaviour: it is likely that he will vote Communist. This hypothesis can then be tested empirically by observing his future vote.

The example of Monsieur Rouget is a concise illustration of the power of modern naturalist explanations. The power of this explanation rests on its strong inductive foundation and the implicit recognition that there are law-like patterns in social behaviour. The patterns allow us to predict the probability of a young male worker in a larger factory voting communist. On this foundation, empirical observations are combined with generalized statements (themselves based on previous induction) to formulate hypotbeses that can be verified empirically. This careful procedure provides the social scientist with secure knowledge that can better help us to interpret future voter behaviour. While Przeworski and Teune explicitly recognize that this explanation is incomplete – several other factors may be relevant for predicting M. Rouget's behaviour – this particular explanation enjoys a relatively high level of probability. It is, after all, for these reasons that the naturalist's approach to social phenomena today is hegemonic.

But this approacb is not the only way to predict Monsieur Rouget's voting behaviour. Just as M. Rouget was a hypothetical construction of Przeworski and Teune to illustrate the power of naturalist social science methods, it is possible to construct a hypothetical context around M. Rouget, imbued with patterns and meaning. For example, we can consider an entirely different epistemological vantage point, one provided by M. Rouget's wife, Kikki.

Kikki has lived with Jacques Rouget for the past six years in a small flat in a middle-class suburb just north of Paris. Jacques drives a BMW that he cannot afford and appreciates the finer things in life. As a result, Kikki and Jacques are always short of money – which Jacques unfailingly blames on the French state's passion for taxing his small factory salary. From Kikki we learn that her husband's main passion in life is football (soccer). This is, we learn, the main reason he joined the factory union: it was a prerequisite for playing on the team. When he is not following market developments on his computer at home, he is watching, playing or dreaming about football. Jacques manages the factory's football team, having held (unchallenged) the position of centre forward for the past five years. As team manager, he travels a great deal, and socializes increasingly with the factory's management (who also follow the team with great interest). In addition, we learn that Jacques has become gradually more conservative in his view of the world, especially his political view, since his father died three years before. If we were to ask Kikki, she could tell us with complete certainty that Jacques will vote Gaullist (RPR) in the next election.

We have now presented two very different means of explaining M. Rouget's future voting behaviour: Kikki's understanding of M. Rouget's behaviour is quite different from that of Przeworski and Teune's, but both provide important insights that allow us to predict and understand Jacques's voting behaviour.

At first glance, the most significant difference between the two examples may concern questions of cost or efficiency. Can we really expect to have detailed, familiar knowledge about every voter in France? While recognizing that this is an important consideration for the investigator in the field, it is not one that we feel is significant in itself, for two reasons. First, money will flow to legitimate projects: the initial struggle is about legitimization. Second, there are several political issues where resources are not an important part of the analysis: constructivist studies of nations, parties or government decisions, for example, needn't be more expensive or time-consuming than 'naturalist' ones.

Rather, we would like to focus on the more significant differences distinguishing these two approaches. In particular, Kikki's explanation is different in that it:

- recognizes Jacques Rouget as a conscious political being, one that can formulate his political perspective independently of the structural determinants that are said to inform political behaviour;

- understands that Jacques's voting behaviour depends critically on a thorough or complete interpretation of Jacques as a complex creature in a given context saturated with meaning; and
- relies on a broader scope of authority. Our confidence in Kikki's interpretation depends on her authority (as Jacques's wife), and her ability to describe how his political vision is a product of several larger developments in his life over the past decade or so.

In short, the ontological doubts we considered in the first section of this chapter have made alternative epistemological approaches more attractive. No longer does the scholar need to confine himself to empirical or rational proofs, or authorities who rely on these 'ways of knowing'. Myths, revelation and other authorities (such as novelists, film characters, wives and so on) become potentially relevant interpretive authorities.

It is on the basis of these ontological and epistemological doubts that we can understand why it is that Kikki Rouget's explanation of her husband's voting behaviour might be more convincing. Her familiarity with Jacques's life and experiences provides her with an interpretive perspective that is more legitimate than that provided by inductively derived generalizations of voter behaviour. At the same time, these ontological and epistemological doubts provide us with a critical vantage point from which we might question the way in which mainstream (naturalist) approaches use reason and sensory perception as part of their rhetorical tool kit.

Look again at Przeworski and Teune's explanation of M. Rouget's voting behaviour; but pay particular attention to its *style*. For Przeworski and Teune (1970, p. 19), the explanation took the following form:

> One out of every two workers votes Communist; and employees of large organizations vote Communist more often than employees of small organizations; and young people vote Communist more often than older people.
>
> Therefore, it is likely that
>
> M. Rouget votes Communist.

There are three particularly relevant observations about the form of their explanation (we can assume that the 'content' is correct). First, the explanation is framed in the form of a covering law (indeed, Hempel is referred to earlier on the same page). Second, the language is authoritative/ scientific. Consider the following (immediately preceding) passage, which oozes scientific authority:

> The second premise consists of a conjunction of general statements describing with a high likelihood the behavior of skilled workers,

employees of large factories, and young persons. (No interaction is assumed.). (Ibid., p. 19)

Finally, the very style, or form, of exposition is meant to mimic a mathematical theorem: note the nature of the indentations and the structured format! The last sentence is broken in two, with 'M. Rouget votes Communist' whisked off to a new line, as if placed on a pedestal for all to see. QED. What other role can this style of presentation play if it is not to parrot scientific authority?

This is McCloskey's (1986) 'rhetoric' of social science, as introduced above. To the extent that the reader is convinced by Przeworski and Teune's argument, it could be that the conviction is grounded in the authors' use of authoritative reference, voice and form (as much as rational and empirical support). The empirical content of the covering law is not supported at all (of course, this is a fictitious example), nor is there any explicit attempt to explain why these factors (and not, say, the man's hair or eye colour) are relevant.

Methodological Doubts

This chapter has introduced a number of doubts about the natural approach to social science research. Its purpose has been to challenge the social scientist to consider alternative ontological and epistemological outlets. No longer are we limited to the sorts of reasoning, facts and authority that have permeated scientific discourse for so long. The methodological consequences of this revolution are wide-ranging – they stretch across a continuum that includes subscribers to a weak methodological hierarchy, to those who might be called methodological anarchists.

Beyond this continuum lies the ideal of methodological holism, or the idea that a single methodology should suffice for the study of both social and natural phenomena. Indeed, there is a long, and fairly varied, tradition – one that includes such disparate authors as Comte, Mill and even Karl Marx – that strives for methodological unity. But this tradition is itself divided.

On the one hand, we find the hard-core traditionalists, exemplified by the logical positivists of the Vienna Circle, who argue that all sciences should be modeled as closely as possible on Physics. Today, this tradition is represented by Edward O. Wilson's (2003) campaign for *Consilience*.

On the other hand is Ilya Prigogine. He believed, like E. O. Wilson, that there is no difference between the natural and the social world. However, Prigogine also believed, unlike Wilson, that the social sciences ought to provide the methodological norm, not the natural sciences. The natural world has been poorly described; it is really more akin to descriptions of the human world, he averred. Prigogine drew this conclusion after studying thermodynamics – and having been awarded the 1977 Nobel Prize in Chemistry for his work on processes of self-organization of non-equilibrium systems. Living organisms do not have a monopoly on either communication or organization; inanimate matter also communicates and self-organizes, argues Prigogine (1997).

This chapter has provided a number of illustrations that should encourage people who doubt the unity of science and question the campaigns of Wilson and Prigogine for methodological monism. Michel Foucault, for example, one of the most influential questioners of the last quarter of the twentieth century, advocated methodological pluralism. For Foucault (1970, p. xiv) science is not one thing; it is many things and we should 'approach it at different levels, and with different methods'.

Those who criticize methodological monism tend to subscribe to one of two possible positions. On the one hand are those who want to argue for methodological pluralism. These analysts are willing to accept that some methodologies are more appropriate than others for studying certain types of phenomena. The problem, however, is agreeing on the measure of 'appropriateness'. Some remnant of a demarcation principle (or principles), no matter how diluted, remains.

At the other end of the spectrum, many postmodernists find methodological assumptions to be both alien and violent. They tend to speak about strategies, not methodologies, and they are especially doubtful of any attempt to impose a demarcation barrier. For McCloskey, the imposition of any strict methodological criterion as a demarcation barrier constitutes a conversation stopper: 'In practice, methodology serves chiefly to demarcate Us from Them, demarcating science from nonscience' (1986, p. 26). For many who are unfamiliar with (or unsympathetic to, or both) this approach, this sort of methodological agnosticism seems like cheating: if there is no methodological standard by which to evaluate scientific contributions, then arguments about authenticity appear little more than shouting matches about who has better access to the authentic.

To illustrate this problem we can refer to a real-life classroom example. Several years ago, one of us invited a guest lecturer on postmodernism to his introductory political theory class. This guest ended his entertaining discussion about the postmodern subject with a short (and equally entertaining) analysis of why young, middle-class, white youths buy rap music. His argument was that these kids bought rap music because it reinforced their stereotypes of violent, sex-driven, black youth. As the lecture was presented to a bunch of young, middle-class, primarily white kids, its objective was surely to provoke argument – which it did. When this interpretation was challenged by a young African male student in the front row (who wanted to explain the inherent qualities of the music, and its deep roots in African tradition and culture), the two ended up in a shouting match. Without any methodological criteria for reference, each needed to convince the audience of his authenticity and experience. The student claimed authority with reference to his ethnic background; and the guest lecturer with reference to his academic qualifications. In a situation like this, how can we decide which argument is better?

Perhaps the best answer is that we should suspend our decision. Perhaps we should be open to the possibility that discussions like this have left the realm of scholarly pursuit and entered the sphere of social contest? Reason is a powerful tool and it can, as with all such tools, be used for various ends: philosophers explain how reason can be used in situations of scholarly dialogue as a means of increasing knowledge and insight in a common quest for truth (see, for example, Plato, 1987). Evolutionary psychologists argue that reason is also used in social contests as a means of persuading, intimidating and defeating a competitor in a struggle for authority and power (Mercier and Sperber, 2011).

This problem of authority is difficult to shake off. Even those of us trained in the naturalist tradition can be (and often are) influenced by alternative (non-scientific) authorities (by long-dead economists, ideologues, prophets or just good storytellers). It was noted above how fiction may sometimes provide a better understanding of historical events than academic treatises. For example, America's decision to enter the Second World War is told beautifully in Gore Vidal's (2000) *The Golden Age*, and the assassination of John F. Kennedy in Don DeLillo's (1988) *Libra*. Novels set in foreign countries may sometimes spark empathy and provide an understanding of that country in ways that social scientific analyses cannot. Orhan Pamuk (1994; 2004), Khaled Hosseini (2003; 2007) and Kader Abdolah (2010) may have enlightened tens – if not hundreds – of thousands of Western readers about the life and ways of Turkey, Afghanistan and Iran, respectively. Each of these authors is able to convey authentic and plausible depictions of historical events in fictionalized form. It is *because* these authors do not pretend to be authentic or universal that their fictional accounts carry so much explanatory punch.

There may also be other reasons for shunning methodological standards. Stanley Fish (1987), the well-known American lawyer and literature scholar, argues that a preoccupation with methods belongs only to those logocentric systems that claim to be externally valid, seeking transcendental truths. Worse, as McCloskey was hinting at above, methodological criteria often serve as a means of narrowing discussion – keeping out the voices from the margins, and narrowing the rhetorical discourse.

Paul Feyerabend (1924–94) straddles these two methodological positions. While his methodological position is probably closer to the first ideal type (methodological pluralism) than it is to the latter (methodological anarchy), the solution he proposes is suitable for both camps.

Feyerabend's work is grounded in actual examples of scientific change. This sort of grounding encourages a proliferation of new and incompatible theories, competition and notions of scientific progress. For Feyerabend, scientific progress is derived from theoretical and methodological pluralism.

Indeed, in his most famous work, *Against Method*, Feyerabend (1975) argues that science has no special features that render it intrinsically superior to other kinds of knowledge, such as ancient myths or voodoo:

> [S]cience is much closer to myth than a scientific philosophy is prepared to admit. It is one of the many forms of thought that have been developed by man, and not necessarily the best. It is conspicuous, noisy and impudent, but it is inherently superior only for those who have already decided in favour of a certain ideology, or who have accepted it without ever having examined its advantages and its limits. (Feyerabend, 1975, p. 295)

In short, Feyerabend wished to downgrade the importance of empirical arguments by suggesting that aesthetic criteria, personal whims and social factors play a more decisive role in the history of science than rationalist or empiricist epistemologies would indicate. Feyerabend's argument about methodological pluralism (like that of many postmoderns) is an argument about emancipation: individuals should be free to choose between science and other forms of knowledge. Feyerabend sees our dependence on scientific authority today as a parallel to the dominance of the Catholic Church at the time of Galileo: our high regard for science is a dangerous dogma, and a direct threat to democracy. To solve this problem, Feyerabend argued that free, democratic societies needed to ensure that 'all traditions have equal rights and equal access to the centers of power' (Feyerabend, 1978, p. 9). He argues that, to defend society from scientific experts, science should be placed under democratic control: experts should be consulted, and controlled democratically by juries of laypeople.

Recommended Further Reading

For a description of how Galileo's telescope changed the nature of truth and altered our understanding of the world, see James Burke's *The Day the Universe Changed* (1985). Fritjof Capra's *The Turning Point* (1982) also provides a very accessible introduction to a new way of understanding the world. For a very broad introduction to the philosophy of social science issues, see Martin Hollis's *The Philosophy of Social Science* (1994). R. G. Collingwood's *Essay on Metaphysics* (1962) and his *The Idea of History* (1956) provide central contributions to an alternative to the naturalist methodology, while Paul Feyerabend's *Against Method* (1975) provides additional philosophical support.

Chapter 8

A Constructivist Philosophy of Science

Behind us, in Chapters 1–6, we have left the empirical quest for certain knowledge; ahead of us lie doubt, difference and dissent. Chapter 7 planted the seeds of doubt, and here we seek to identify some of the wild methodological vines that have grown from those seeds. Our intention is to harvest a constructivist alternative to the naturalist philosophy of science described in Chapter 2.

In Chapter 2 we began by introducing David Hume and hailing his *An Inquiry Concerning Understanding* (1983 [1748]) as a major contribution to Western philosophy of knowledge. In this chapter we introduce a rival, constructivist view. This chapter too begins with Hume. However, it does not discuss the naturalist legacy that emanated from his *Inquiry*; instead, it focuses on the reactions it provoked. First, we turn the spotlight on Immanuel Kant. He read Hume's argument with disbelief and made it his life's vocation to dispel it. In our view, it is in Kant's sustained reaction that we find the ontological taproot for the constructivist approach to the social sciences.

The naturalist and constructivist traditions both recognize the need to map and explain patterns in the world. However, they differ sharply over the source of these patterns – as is reflected in their respective titles: naturalists understand patterns and regularities to be an essential part of nature; constructivists trace these patterns back to the mind that observes them. For the constructivist, then, the world we observe is, in a sense, a world of our own making. Consequently, naturalists and constructivists tend to have different attitudes toward, and approaches to, uncovering the truth; constructivists often wonder whether there is in fact a singular truth out there at all.

To gain access to Kant, we invoke an almost forgotten Kantian scholar from the nineteenth century: William Whewell. He will help us to consider the different ways in which we are ourselves responsible for the patterns we observe in the social world. With Whewell it is easier to see how knowledge is dependent on context – how history, society, ideas and language influence the patterns we observe and the concepts we use to explain and understand them. Consequently, Whewell's approach is less beholden to empiricism, and encourages us to embrace a much larger range of epistemological outlets.

From the vantage point provided by Whewell, we can then survey the broad field of contemporary constructivist approaches and elaborate on the core components of constructivist social science. With these methodological components as a vantage point, we can help students to compare a constructivist philosophy of science with its naturalist counterpart, as depicted in Chapter 2. In addition, these common methodological elements can help us to better understand how constructivism is applied in the particular methods' chapters that follow.

On Natural and Other Worlds

Constructivists begin by recognizing that there is a big gap separating the natural and the social worlds. As we saw in Chapter 7, constructivists share this position with a much larger group of social analysts. As a result, we find events being explained in different ways when they occur in either the natural or the social world.

To see these differences, let us return to John Stuart Mill, who once remarked that '[a] bird or a stone, a man or a wise man, means simply an object having such and such attributes' (Mill, 2002 [1891], p. 59). Clearly, all three objects are material; and as such they share common characteristics (for example, they have mass and extension), and are subject to the same natural laws.

Imagine Galileo climbing the stairs of Pisa's Leaning Tower carrying a stone in one hand, followed by a wise man carrying a cage with a bird inside. After dropping the stone and the bird cage from the top of the tower, and taking careful notes, we might expect Galileo to conclude that the stone and the bird drop in accordance with their relative weight. After all, each of them acts as a material object. Provided the bird was still in its cage. Or dead.

Alive, of course, the objects would behave differently. If Galileo dropped a stone from the top of the tower, it would fall straight down to the ground below. Should he take the bird out of its cage, its behaviour would deviate radically from that of the stone: it would fly away. And if Galileo revealed his intentions to throw the wise man over the parapet, he would probably put up a lively struggle. (Once tossed, however, we would expect the wise man to drop like the stone, albeit with more animation.)

If we twist this example one more turn, we might think about how a puzzled observer on the ground would respond after witnessing the entire procedure. When interviewed by a local journalist about these odd circumstances we can imagine her revealing answers to a string of questions:

Journalist:	Why do you think he dropped the stone?
Witness:	I guess it was to see how quickly it dropped. Galileo is known in the neighbourhood for doing these sorts of things.
Journalist:	Why did he drop the bird?
Witness:	I suppose he wanted to see if it could fly. Why else would you drop a bird from the top of a tower?
Journalist:	Why, then, do you think he dropped the man off the top of the tower?
Witness:	How the hell would I know? I didn't see any sort of struggle. Perhaps the guy was a rival scientist? This is all very unsettling ....

In short, when we begin to look beyond an object's material qualities, and come to recognize the real differences that distinguish stones, birds and men, then we begin to discover that different principles of explanation might apply to each of them. There is nothing particularly odd about dropping a stone, so the observer focuses on the natural factors pulling the stone: we want to know how it works. A bird's actions are more varied, so we begin to look for explanations in the bird ('it can fly') or in factors external to the bird (for example, in the density of the air and references to the laws of aerodynamics). With the most complex object, a man, we begin to search for more complex reasons: we search for a meaning. The sundry attributes of diverse objects encourage us to think in terms of different explanations for their behaviour.

This is the sort of puzzle that David Hume worked on when speculating about the nature of causation. But Hume's laboratory of choice was not a leaning tower, but a billiards hall. Hume wanted to know why a particular billiard ball moved. He reasoned that we must search for a *cause* that is external to the ball – for example, that it was hit by another ball. Likewise, if we want to know the *reason* why that second ball moved, we may find that it was set in motion by a pool player – again, an example of an external cause. But if we want to *explain* why the pool player set his ball in motion, the search for an external cause becomes more complicated. In one sense, we can find an external cause in the rules of the game of billiards. But game rules are hardly an external cause in the material sense of the term. The rules of the game are a social construct; they are something that pool players have invented; they are a convention. Herein lies a dilemma, then, as the cause can also be seen to be internal, because the rules of the game *are* the game of billiards.

The rules *constitute* the game. As such, they also give meaning to the pool player's action (that is, setting the ball in motion).

To summarize from Hume's example: we can immediately distinguish between three clear reasons (or causes) for why a man sets a billiard ball in motion: (i) a physical cause (on which Hume focused); (ii) an intentional cause (the man wanted to play snooker); and (iii) an institutional cause (the rules of billiards informed the man what he could do). We may add more; we may, for example, add a functional cause (the man knew what would happen if he used the pool cue in the usual way).

For naturalists, it is important to delineate a common underlying structure for scientific explanations, even while recognizing that they could rely on different (deductive and inductive) types. In Chapter 2 we used Hempel's covering law to introduce this structure. Constructivists, by contrast, are less interested in the common structure of explanation as they are in mapping the different forms of explanations, and the origins of this variance.

The examples above illustrate some of this potential variance, and Table 8.1 presents a typology of several kinds of explanations. We hasten to point out that this is a very simple typology for thinking about the different principles of explanation and their relationship to their objects of study (and their requisite scientific discipline). We do not mean to suggest that we are limited to these types of explanations; that some types of explanation are better than others; or that students of human behaviour should not use causal or functional arguments (for example).

In the left-hand column of the table we distinguish between inanimate and animate objects (the latter being further divided into plants, animals and humans). The middle two columns describe the properties and scientific disciplines usually associated with these types of objects – Botany

Table 8.1 ***Objects, sciences and their principles of explanation***

Object	*Properties*	*Science*	*Principle of explanation*
Inanimate	Mass and extension	Physics	Causality
Auimate	Mass and extension		
Plants	+ vital force	Botauy	Adaptation
Animals	+ vital force	Zoology	Function
Humans	+ vital force	Social	Volition, interest
	+ will and reason	Sciences	Meaning, rules, institutions, praxis

Source: Inspired by Elster (1979).

studies plants, Zoology studies animals and so on. While the scientific disciplines are fairly straightforward, we might explain the content of the second column in a little more detail: here we see that inanimate and animate objects share material qualities (mass and extension), but animate objects are different from inanimate objects in that they are alive (they are characterized by what Whewell calls a 'vital force'). Among animate objects, humans distinguish themselves further by having recourse to will and reason (in addition to having both mass and extension, and the vital force).

In the column entitled 'Principle of explanation' we indicate the several ways in which the various objects are commonly explained within their proper discipline. Inanimate objects lend themselves to *causal* explanations – this is the traditional explanatory principle in Physics. Animate objects, however, may be accounted for in different ways. The behaviour of plants and animals can also be explained in terms of causality; but more often they are accounted for in terms of *adaptation* or *function*. Human behaviour can be explained in all these terms. However, because human beings are endowed with reason, language and free will, human actions can also be explained by other principles (for example, *volition*, *interest* or *meaning*).

There are two points worth emphasizing in this table. First, it is possible to detect a pattern: the simplest objects are associated with the simplest explanations, while the more complex objects come with correspondingly complex explanations. Second, we note that the typology is inclusive: all objects (both inanimate and animate) have mass and extension. For this reason, all these objects can be measured, weighed and counted – and their behaviour can be explained in terms of external causality. But when we begin to note the more individual attributes of an object, we see that other principles of explanation can also apply: because of the vital force inherent to them, the behaviour of plants and animals (including humans) can be explained in terms of adaptation and function (in addition to causality). Finally, humans can be further distinguished by their use of reason, will and meaningful speech. These capacities give rise to an even wider variety of potential explanations.

These examples are used to describe the complicated nature of the relationship between the natural and social worlds. In many important respects, the two worlds are quite alike, and these similarities mean that explanatory principles developed for studying the natural world can often be applied (with great effect) also to social phenomena. On the other hand, the examples also suggest that the nature of human interaction is quite different from the way in which inanimate objects interact. Consequently, it is possible to explain human interaction by recourse to a much larger set of explanatory principles.

Beneath all of this complexity lies a view of the world that recognizes the subjectivity and illusiveness of social patterns. The next section will introduce the ontological foundations of such a view.

The Awakening

In Chapter 2 we learned that David Hume was an empiricist. Like other empiricists before him, Hume believed that we have access to the Real World through our senses. We look out of the window and see trees and bushes, rocks on the grounds, buses on the roads, and birds in the air. From these observations we gather systematic knowledge about the world, and if we are scientists, we seek to induce general statements from our observations.

But Hume was also a sceptic. In spite of his empiricist sympathies, he warned us of induction's potential pitfalls. After all, we cannot trust inductive reasoning to produce general statements that are true; because induction is based on observed events, and observed events can never embrace *all* possible objects/events of the world. Our experience with past regularities is no guarantee that the future will bring similar regularities. Karl Popper illustrated this point with reference to the colour of swans. Bertrand Russell illustrates this point with another bird: the 'inductive turkey'. On the first morning a turkey arrives at a farm he notices that feeding time is five a.m. Each day the turkey experiences the same thing: food comes at five. With the passing of time, and with the turkey having noted the regularity of his feeding time, the turkey eventually infers that he is always fed at 5 a.m. Unfortunately (for the turkey), this inference proved to be faulty. At 5 a.m. on 25 December, the unlucky turkey was not fed, but slaughtered for Christmas dinner.

In a similar way, Hume argued that we cannot infer beyond our own limited experience. This is a big step for any empiricist. To make this step easier, Hume retreated from the most radical destination to which it led; he took refuge in a pragmatic argument that rested on the principle of human habit. In short, Hume came to accept that there are natural limitations to what we can know about causality.

On Pure Concepts and Natural Ideas

Hume's argument was earth-rattling stuff for scientists in his day. Causation was (and is) a central object of scientific discovery, and to suggest that it rested on such flimsy ground had the effect of shaking the very foundations of science and metaphysics. The effect was strong enough to wake Immanuel Kant from what he later described as his 'dogmatic slumber' (1969 [1783], p. 302). Kant understood the serious implications of Hume's argument, and he was not willing to leave causality resting on such shaky foundations.

If Hume was correct, the whole of science was in danger. Worse (for Kant, who was a philosopher by profession), if causality proved to be beyond the grasp of our understanding, it is possible that other metaphysical concepts might prove to be just as elusive. Kant immediately set out to construct a sturdier basis for understanding causation. As he sought to improve on Hume – who understood causation as a habitual expression (mechanically produced by the association of ideas) – the scope of Kant's enquiry expanded. Causation was not habit, Kant averred; it was part of a bigger and more general property of the nature of the human condition.

On the surface of things, it appears as though Kant ended up in the philosophical vicinity of Hume: both developed a philosophy of knowledge that directed attention away from the Real World and turned it on the nature of the human mind. But surface appearances are often misleading. The two philosophers developed very different ways of understanding human knowledge, and ultimately informed very different philosophies of science.

To understand the differences that separate these two great thinkers, we need to recall Hume's understanding of causation (from Chapter 2) and how it rested on his theory of sense perception: namely, that the human mind absorbs impressions through the senses. Kant was willing, in part, to accept this theory of sense perception. He agreed that the senses presented perceptions to the mind. However, he could not agree with the notion that the human mind is an empty vessel, into which sense impressions fall passively. For Kant, the senses merely brought perceptions to the doorstep of the mind. It was then up to the mind to organize these perceptions, categorize them, and store them for later use. To perform this task, the human mind comes already equipped with basic preconditioning concepts – which it then uses to harness the flux of sense perceptions delivered to its doorstep. Thus Kant concluded that the mind is an agent in its own right. It acts as an interpreter of the impressions that come to it from the external world.

But if each human mind is an active interpreter of sense impressions, how is it possible for different people to agree on what the world looks like? How is it possible to agree on anything at all? The answers to these important questions are not as daunting as they first appear. Kant argued that we all share certain basic preconditioning or organizing ideas. Indeed, possessing these basic ideas is part of what it means to be human. In other words, all human beings share a set of basic categories and concepts that organize the perceptions that our senses deliver to the mind from the outside world.

In the end, Kant identified 12 such pure concepts (or forms of understanding), through which all human perceptions must pass on their way to objective knowledge. These are listed in Table 8.2, where we can see that Kant organized these basic ideas into four sets: (i) quantity of objects; (ii) quality of objects; (iii) their relation to each other; and (iv) their mode of existence (or modality). After these 12 pure concepts had done their work – after their sorting work was done – the processed sensations were conveyed to the conscious mind.

Everything we perceive is channelled through these categories of our mind. Without them we could not perceive or know anything. Arguing in this way, Kant was able to save modern science from Hume's excessive scepticism. Newtonian physics and the universal laws of nature (for example) were saved from the horrible uncertainty to which Hume had exposed them. With Kant, scientists could continue to assume that the laws of nature would apply indefinitely. But Kant's rescue came at a very high cost. In providing the necessary groundwork for assuming the universality of nature's laws, Kant shifted the ontological terrain from nature to the human mind. In other words, Kant shows us how Newton's ordered universe (for example) was not anchored in nature; it was anchored, instead, in universal and necessary concepts of the human mind.

This is an important argument. We should point out that Kant is *not* making a distinction between the social and natural worlds, as we did in the introduction to this chapter. Instead, he is distinguishing between a Real World and the way it is perceived by us. In other words, Kant is telling us that the laws of nature may not belong to the Real World. Worse (for naturalists, at least), Kant is claiming that those Real World patterns (that we observe so clearly) belong to the human mind; that the human mind imposes its own patterns on nature and the world. The implication is, of course, that we can never observe or know the Real World – 'objectively' as it were. We can never say anything about how the Real World is 'in itself'. This was precisely what Kant taught Boas, after struggling with ways to define variations in blue water: that the

Table 8.2 *Kant's pure concepts of understanding*

Quantity	*Quality*	*Relation*	*Modality*
Unity	Affirmation	Substance–accidents	Possibility
Plurality	Negation	Cause–effect	Actuality
Totality	Limitation	Causal reciprocity	Necessity

Source: Based on Kant (1929 [1787], p. 113).

only thing we can really observe are our perceptions of the world: how the world appears to us.

The World of Our Making

This discussion is leading us down a very difficult and winding path, and at its end is the unanswerable question about whether a Real World actually exists, independent of our existence. For Kant it was important to emphasize that he was not denying the existence of a Real World. He was simply saying that we have no way of knowing anything about that Real World (the *noumena*). All we know is that our perceptions (*phenoumena*) of the Real World are somehow related to it. But the nature of that relationship remains complex and ambiguous: they seem to coexist simultaneously. (As Kant's pure concepts include causation, it is problematic to say that the *noumena* cause us to have perceptions of *phenoumena*.) Nor was Kant advocating more metaphysical speculation; he was committed to pursuing philosophy within the narrow 'limits of pure reason', and to recognizing that most positive knowledge could only come about through sense perception.

Kant introduces a rather serious problem for social scientists interested in understanding the world. He forces us to recognize that our human faculties are limited: our sense perceptions and our reason pertain only to the world of *phenoumena*, not to the *noumena*. In effect, Kant makes us realize the limits of both reason and sensory perception as tools that can help us to understand the Real World.

The Unwieldy World of William Whewell

In Immanuel Kant we have found a philosophical sponsor for the constructivist approach. Kant introduced an important ontological twist: the realization that the world we live in is a world as it appears to us – a world of *phenoumena*. Again, this is not to say that the Real World doesn't exist; only that it is beyond our capacity to observe and understand it directly. Under these very different ontological conditions, we need to rethink the role of our senses and reason in providing neutral or objective knowledge. Before we can do this, however, we need to think about how these pure concepts might generate patterns of relevance for social scientists. For this, we turn to William Whewell.

From today's vantage point, William Whewell (1794–1866) appears as a rather obscure British philosopher of science. In his own context, however, Whewell was well known. He was also controversial, because he explicitly challenged the naturalist ontology and engaged in debate with John Stuart Mill – the very embodiment of the naturalist tradition in mid-nineteenth-century Britain.

Whewell seems to have been joined at the hip to Trinity College, Cambridge: he studied there, became a fellow, then a tutor, and finally served as its Master from 1841 until his death. His academic output was exceptional, in both abundance and diversity. He taught and published on subjects as wide-ranging as astronomy, the tides, technology and moral philosophy. However, his principal work – in length and by the central position it occupied in his thought – was in the field of scientific methodology, as collected in two major studies: his *History of the Inductive Sciences* (1967 [1837]) and his *Philosophy of the Inductive Sciences* (1996 [1840]). The former is a general history of the natural sciences with a strong critique of empiricism, while the latter provides a systematic summary of the lessons Whewell drew from his historical investigations.

Whewell's critique of naturalism took aim at one of its originators: John Locke. Though Locke had argued that induction lies at the heart of modern science, his own approach was remarkably theory-driven. As Whewell showed, all indications suggest that Locke subscribed to his theory of sense perception long before he had found the facts needed to support its presuppositions. Whewell, by contrast, did what Locke and other empiricists should have done: he looked carefully at how science had actually evolved, and how its method was revealed in history. The result was his impressive, three-volume work, *History of the Inductive Sciences*.

The cumulative results of Whewell's work were three strong attacks on the naturalist tradition. First, he argued that the naturalist's *methodology* is completely wrong: naturalists (such as Locke and his followers) had misunderstood Bacon and his concept of induction. Scientists do not begin with particular observations and infer general theories from them. Scientists begin with a question. They then imagine many possible answers. Finally, they test various answers against the available facts in a process of active tinkering and systematic experiment.

Whewell singles out the breakthrough case of Johannes Kepler to illustrate the praxis of science. Kepler had many observations of the night sky at his disposal – he knew where many heavenly bodies had been on thousands of different dates. He struggled to find a pattern into which all of these could fit, and worked for years to make the heavenly bodies fit into a simple, general conception. Whewell wrote:

> [We] know from his own narrative how hard he [Kepler] struggled and laboured to find the right conception; how many conceptions he tried and rejected; what corrections and adjustments of his first guesses he afterwards introduced. In his case we see in the most conspicuous

> manner the philosopher impressing his own ideal conception upon the facts; the facts being exactly fitted to this conception, although no one before had detected such a fitness. And in like manner, in all other cases, the discovery of a truth by induction consists in finding a conception or combination of conceptions which agrees with, connects, and arranges the facts.
>
> Such ideal conceptions or combinations of conceptions, superinduced upon the facts, and reducing them to rule and order, are *theories* . . . [A theory, then,] . . . is a truth collected from facts by induction; that is, by superinducing upon the facts ideal conceptions such as they truly agree with. (Whewell, 1996 [1840], p. 42f)

Whewell's approach seems to be very close to what the nineteenth-century American philosopher, Charles S. Peirce (1992 [1898]), referred to as 'retroduction'. Its essence involves the forming and accepting (on probation) of a hypothesis to explain surprising facts. Peirce argued that retroductive reasoning was similar to induction in that it involved a movement from individual observations to a connective proposition; but it was different from induction in that it ended in a self-consciously conjectural act – in a hunch or a proposition which could, in turn, be tested.

Whewell's second broadside was aimed at the naturalist's reliance on empiricist *epistemology*, which he held was sadly incomplete and half-right at best. The naturalists correctly assume that sense perception is vitally important to the acquisition of scientific knowledge; but Whewell argued that sense perception is only half the story: science also depends on the appropriate processing of perceptions and on this count the naturalists fall woefully short. In this argument, Whewell draws heavily on Kant. Indeed, he freely admits that he 'adopted Kant's reasoning respecting the nature of Space and Time,' though he distanced himself from the metaphysical system of Kant and his followers (Whewell, 1996, p. x). Whewell was not the person to push this argument and probe its deeper implications; he did not direct his scholarly attention toward speculations on the inner workings on the human mind. Instead, Whewell focused his attention on the empirical world (which scientists investigate), and on society (in which scientists live).

Finally, Whewell charged the naturalists with being *ontologically* arrogant. Here, too, he borrowed arguments from Kant, but sharpened them to a polemical point. Naturalists, he claimed, are full of themselves: they are convinced that there is a Real World out there, but they have few if any metaphysical arguments to show that this is the case.

In short, Whewell argued that naturalists are methodologically wrong, epistemologically incomplete and ontologically shallow. We can now understand better why he drew so much critical attention. Whewell showed how naturalists claim to have accumulated a good deal of knowledge about the world. But they can't show that it is *true* knowledge. Indeed, they can't even show that their knowledge (even if it were true) is knowledge about the *Real* World.

Disparate Pieces to a New Philosophy of Science

It is not enough to recognize that the mind uses pure concepts (or 'fundamental ideas' as Whewell calls them). We need to know how these concepts can create patterns – patterns that attract the interest of the social scientist. Whewell recognized that we acquire knowledge through our senses, but not through the senses alone. Clearly, more factors are involved, but what can they be?

Whewell's work on the history and nature of science is encyclopaedic. The modern reader can easily follow its rich seams and extract from them arguments about how we create and grasp the patterns central to our understanding of the world. Here we want to focus on four such seams: the roles of *history*, *society*, *ideas* and *communication* (or language). Though Whewell himself did not produce this exact list of factors, it is not difficult to trace them in his writings. In doing so, we hope to show the breadth and power of constructivist approaches, as represented in the work of more recent authors. In other words, we follow Whewell's initial insights with several influential and more contemporary examples. By dividing the literature in this way, it is important to emphasize that our list is not meant to be exhaustive. We provide one possible path through a vast and varied terrain.

The Role of History

On the basis of his vast study of the history of ideas and of scientific discoveries, Whewell concluded that history displays no steady accumulation of singular insights. There is no clear and obvious pattern of cumulative growth in the history of human knowledge. Instead, it displays periods of rapid progress, interspersed with periods of stagnation. If the history of science had a pattern, argued Whewell, it was not steady progress, but a dialectical movement in which inductive periods alternate with periods of synthesis and generalization.

Instead of entertaining a simple, historical teleology of human knowledge, Whewell cast knowledge in sociological terms. He argued that societies share a pool of common knowledge, and envisioned these pools as being dynamic and ever-changing. Knowledge changes over time – often in fits and starts. For example, in the past, people were not commonly aware that the planets orhited the sun; even learned Renaissance astronomers claimed that the planets travelled in perfect circles around the Earth. When Copernicus, Kepler and Galileo argued that this was an erroneous view, they ignited a scientific revolution, in which the old idea of a geocentric universe was replaced by a new, heliocentric one.

With examples such as this, Whewell argued that science – indeed, human knowledge in general – is historical in nature. More recently, this basic notion has been popularized by one of the most influential philosophers of science in the twentieth century: the American physicist and historian, Thomas Kuhn (1922–96).

Brother, Can You Paradigm?

Kuhn's first book, *The Copernican Revolution* (1957) was a case study of the episode that Whewell used to illustrate his view of scientific change: the story of how the old Aristotelian approach to the physical sciences broke down when confronted with the observation-based arguments of Copernicus and Galileo. Kuhn concluded that this change involved something more than a simple victory of 'reason' over prejudice; it involved a more basic change in perspective and world view.

In his second book, *The Structure of Scientific Revolutions* (1970 [1962]), Kuhn cultivated this conclusion and argued that scientists are not as open-minded as is commonly assumed. Rather, scientists are committed to established truths – 'conceptual, theoretical, instrumental and methodological' (Kuhn, 1970, p. 42). Indeed, the Church scholars who defended Aristotle against Galileo and the New Sciences were representative of the way in which scientists generally behave: they seek to defend established theories and reject the arguments of their critics.

Most scientists conduct problem-solving tasks within an orthodox, commonly-accepted, theoretical framework. Kuhn calls this framework a 'disciplinary matrix' or a *paradigm*, which he defines as 'the entire constellation of beliefs, values, techniques and so on shared by the members of a given community' (Kuhn, 1970, p. 173). He then calls the puzzle-solving routine activities that take place within these paradigms *normal science*.

The practitioners of normal science form a collegial group: they are tied together by commonality and a commitment to the kinds of questions asked; they follow similar procedures to answer those questions; and they agree about the form that those answers should take. The questions asked, procedures followed and answers inferred are then assessed by colleagues. This peer review process draws on the most relevant experts to evaluate the research being produced. In doing so, the process reproduces normal science as a self-sustaining, puzzle-solving process within the framework of a dominant paradigm.

A revolution occurs when one of these dominant paradigms breaks down. This might result from some observant scientist discovering an inconvenient fact that does not fit easily within established theories – as when Copernicus observed that the planets did not travel in perfect circles around the earth, or when Galileo noted that there were mountains on the moon. Efforts to explain new and anomalous observations complicate existing theories and introduce inconsistencies. Normal science no longer performs in the expected manner, as it cannot provide satisfactory answers. It fails or goes astray:

> And when it does – when, that is, the profession can no longer evade anomalies that subvert the existing tradition of scientific practice – then begin the extraordinary investigations that lead the profession at last to a new set of commitments, a new basis for the practice of science. The extraordinary episodes in which that shift of professional commitments occurs are the ones known in this essay as scientific revolutions. They are the tradition-shattering complements to the tradition-bound activity of normal science. (Kuhn, 1970, p. 6)

The basic point of Kuhn's argument is that scientists typically go around for years believing one thing – despite mounting evidence to the contrary – happily practicing the established routines of normal science. All of a sudden they notice a mass of conflicting evidence, change their minds, and wonder how they could have ever believed otherwise.

Naturalists may accept this basic idea, admitting that scientific knowledge is not merely a product of slow and steady accumulation; however, they do so reluctantly. Some naturalist social scientists embrace Kuhn's description of the structure of scientific revolutions by arguing that the social sciences are pre-paradigmatic; that the social sciences are younger than the natural sciences, and that they have not been able to draw on a similar amount of resources as the natural sciences. The argument holds that when social science matures and is properly funded, we can expect to see it reach the same paradigmatic stage as the natural sciences: becoming cumulative, stable and predictive.

Constructivists, by contrast, embrace enthusiastically the idea that human knowledge has evolved, not through accumulation but through sudden shifts and bounds. In fact, most constructivists would probably embrace Whewell's hazy original more readily than Kuhn's souped-up argument that science goes through revolutionary periods driven by the discovery of new sensual evidence. This is because constructivists like to point out that old paradigms in the social sciences may be replaced, but they seldom fade entirely away. Constructivists choose to situate such changes in a larger, social context and point to the way in which social scientific fashion swings in tandem with various constellations of power. This brings us to our second source of patterns: society.

The Impact of Society

Whewell recognized that science relied on specialized knowledge, produced by specialized scholars. Scientists – a word that Whewell seems to have invented – are knowledgeable people. Yet knowledge alone does not make scholarship; and knowledgeable people do not always become scholars and scientists. A scientist is not a scientist simply by virtue of the many facts he knows. For Whewell, knowledge is affected both by individuals (as 'carriers' of knowledge) and by the societies they compose (as 'pools' of knowledge).

Individuals as Carriers of Knowledge

How is an economist different from other people who talk about money? How is a political scientist different from other people who talk about politics? One important difference concerns the *nature* (not the amount) of their knowledge. Scholars are self-conscious about the methods and theories that they have at their disposal; 'other people' may be interested in money and politics, but they do not master the methods and theories of the professional economists or political scientists (and may not even have a desire to do so).

Another difference concerns the *context* of the knowledge. Scholars command facts, methods and theories; but these are always subjects of controversy and objects of discussion. Facts and arguments presented by one scholar are immediately seized on by others and subjected to scrutiny, checking and criticism. Scholars are both aware of and familiar with these sorts of professional debates. As professionals they know the history of their discipline – including its history of controversies.

Finally, there is the *social* or *communal* aspect of scientific knowledge. Scholars are tied together in distinct scholarly communities by a common knowledge of debates and arguments – in the past, as well as in the present. These communities institutionalize themselves as professional societies and associations. In the earliest times, this was done on an informal basis, in terms of acquaintance networks. More recently, however, scholars have organized themselves into scientific societies, with formal memberships, annual conferences and membership journals.

These societies of scholars facilitate the circulation of arguments and encourage scientific discussions. In particular, they help to ensure that new arguments are subjected to scrutiny, control and criticism by fellow scientists. The result is the development of distinct disciplinary heritages, myths and academic traditions, and a web of interrelationships and acquaintanceships among scholarly colleagues that strengthen professional solidarity. These professional societies are, in other, words community- and identity-building mechanisms that tie distinct communities of scholars together with a common knowledge of debates and arguments.

Societies as Pools of Knowledge

Whewell considered Locke's philosophy of science to rest on a simplistic and dubious claim: that sense perception is the basis for all knowledge. If this were true, knowledge would depend on the individual and on the his or her perceptions, and as a consequence, all knowledge would be contingent. But knowledge is *not* contingent. Furthermore, it is clearly more than the sum of individual perceptions. Whewell argued that facts, ideas and arguments do not always originate with individuals; they are sustained and maintained by social relationships and thus have an impersonal quality to them.

In theory, knowledge is based on sense perception. In practice, however, people do not obtain knowledge by observing the world; they obtain it by interacting with other people. Two consequences flow from this view of science as a social activity. First, people get most of their knowledge by learning from others – through watching, listening and by reading texts written by others. In short, people obtain knowledge by consulting a pool of available and common knowledge produced and maintained – or carried by – members of the society that exist around them. Second, knowledge is social and impersonal – or, better, transpersonal or interpersonal. Knowledge is part and parcel of the social community in which people live. This community shapes people's knowledge and affects the way they perceive the world.

This argument has evolved into what we now refer to as 'sociology of knowledge' (*Wissenssoziologie*), a term coined by Max Scheler in Germany in the 1920s. Scheler drew on Marx, Nietzsche and others to show how human ideas, knowledge and consciousness in general are conditioned by social conditions, but not determined by them. His writings triggered a debate in Germany, which was quickly carried into the English-speaking world – to a large extent by Jewish refugees from Hitler's Nazi regime. It was introduced to Britain by Karl Mannheim (1936), who held a more radical view than Scheler – arguing that the social context determined not only the appearance but also the content of human knowledge. It was taken to the USA by authors such as Alfred Schütz and members of the *Institut für Sozialforschung* in Frankfurt am Main.

This so-called 'Frankfurt School' had a political agenda. Its members included, among others, Herbert Marcuse, Max Horkheimer, Theodore Adorno, Erich Fromm, Leo Lowenthal and Jürgen Habermas; they aimed to develop a new, interdisciplinary and critical theory of contemporary society, by drawing on the works of Hegel, Marx, Nietzsche, Freud and Weber (see Jay, 1973, and Wiggenhaus, 1995, for overviews). The Frankfurt School reflected on the limits of claims made for certain kinds of knowledge. They used their analyses to question the foundations of knowledge and science, as practiced in modern society. In particular, they pointed out that contemporary society was filled with repressive and inhuman mechanisms that distorted or alienated people. For these critical theorists, political liberalism can be decadent, and science the instrument of political oppression. In short, critical theorists believed it was important to use their knowledge to criticize the status quo and promote radical change.

Members of the Frankfurt School were engaged in a project that sought to specify the ways in which the community we belong to influences the way we perceive and understand the world. Individual members of the School disagreed about how, and through which mechanisms, society influences its members in practice. They also quarrelled about whether individuals, in turn, affect the nature of society. Some held that individuals constantly (re)created society through their patterned behaviour; while others held that changes occurred from the self-conscious and wilful acts of reform, rebellion or revolution. But they all embraced the basic notion of individuals as carriers, and societies as pools of knowledge.

Though students are sometimes loath to admit it, social scientists are people too. They are members of society and are, like everybody else, influenced by the society in which they live and work.

The Role of Ideas

Our discussion brings us to the third framing device found in Whewell: the role of ideas. Whewell was well aware of the complex ways that facts and ideas could relate to one another, and he summarized his main argument as an aphorism on one of the very first pages in the first volume of *The Philosophy of the Inductive Sciences*:

> *Fact* and *Theory* correspond to Sense on the one hand and to Ideas on the other, so far as we are *conscious* of our ideas: but all Facts involve Ideas *unconsciously*; and thus the distinction of Facts and Theories, is not tenable, as that of Sense and Ideas is. (Whewell, 1996 [1840], p. xvii, emphasis in original)

A few pages later, he reiterates the point: 'Facts are the materials of science, but all Facts involve Ideas' (1996, p. xxxvii). In other words, human knowledge comes from sense perception, yet scientific knowledge hinges on more than perception alone. Perception is conditioned by ideas. Without ideas we cannot make sense of the things our senses bring to us. Ideas perform a crucially important role in guiding the flux of sensory impressions as they enter the mind. Consequently, our knowledge of the world depends on the way in which ideas affect our perceptions – how they are evaluated, discussed and strung together. Perception is not the result of lenses alone: 'People, not their eyes, see. Cameras and eyeballs are blind,' Norwood Hanson (1958, p. 6) reminds us.

Science is more than the collection of reams of facts. It also involves the creative organization, interpretation and assessments of those facts. Whewell claimed that the naturalist tradition undervalued these other aspects of science: routinely overlooking the role played by individual inspiration and scholarly imagination, and ignores the important role that ideas play in creating scientific knowledge.

For Whewell, the decisive act of scientific discovery involves the 'colligation' of facts. Good science relies on both facts and ideas. But Whewell draws this argument out even further by arguing that a good idea eventually becomes incorporated into experience. When an idea is convincing enough, it becomes so tightly integrated into experience that we come to think of it as a fact. By Whewell's account, yesterday's theories become the facts of today. The facts of today (for example, that the Earth revolves around the sun), began as yesterday's ideas. Our susceptibility to facts is framed by ideas, readily available in the pool of common knowledge.

This claim is intimately related to the concept of foreknowledge – a concept that flies in the face of the inductivist position of the naturalist methodology, described in Chapter 2. Foreknowledge, it must be noted, is not bias. For the constructivist, foreknowledge is both necessary and integral to any research project. Thus, right from the start, the hermeneutic approach assumes that we form an expectation about the unknown from what we already know. Diesing (1992) suggests that foreknowledge must be made explicit and formulated as an initial hypothesis:

> The initial hypothesis guides the search for and interpretation of details, which in turn revise the hypothesis, which leads to reinterpretation and further search, and so on. In case of conflict, the circle tends to widen farther and farther into the contexts on the one side and our foreknowledge on the other side. (Diesing, 1992, p. 109)

This circular or dialectical aspect of constructivist science is one of its characteristic features. It is also its main point of criticism. This dialectical approach tries to explain something (x) in terms of something else (y), before turning around and explaining y in terms of x. In short, there is no clear verification principle on which we can fall back: we can only continue to offer competing interpretations. Aware of this problem, proponents of this approach argue that it is the most honest. Our understanding of the world is not based on a secure ontological starting point: it is circular in nature. Indeed, Otto Neurath (1959, p. 201) once likened it to the problem to repairing a faulty boat at sea: 'We are like sailors who must rebuild their ship on the open sea, never able to dismantle it in dry-dock and to reconstruct it there out of the best materials.'

Teutonic Treatments: Verstehen and Hermeneutik

It is easy to see how Whewell's argument lends itself to the concept of *verstehen* – a concept associated with an important branch of modern social research. The concept of *verstehen* is a shoot from the Kantian root, tended and groomed by German gardeners such as Wilhelm Dilthey, Heinrich Rikert, Georg Simmel and Max Weber.

At the very start, Dilthey (1833–1911) maintained that understanding is an outcome of *empathy* – that in order to understand an action or an argument, it is necessary to put oneself in the agent's (or author's) shoes, relive her experiences and image oneself in her social location, as it were. Our attempt to tap into Kikki Rouget's empathetic knowledge of her husband (in Chapter 7) is an example of this sort of understanding.

Eventually, Dilthey distanced himself from this approach because he saw that it might easily lead down the path to subjectivism, at the end of which loomed the threatening ghost of relativism. Because, if all our perceptions are phenomenal, and all knowledge is personal, then there is no guarantee that different observers have a common knowledge of the world. It becomes hard to assess whether you and I (and the woman next door) understand the same thing when we refer to trust, marriage, power, deceit and so on.

Dilthey needed to find a way to show that some understandings are truer than others; and that some propositions are good and others are bad. To do this, he invoked the ancient technique of *hermeneutic understanding* – an old and recognized procedure of the interpretation of texts, particularly biblical texts, whereby any understanding must be shown to fit a distinct context. The first hermeneuticians were theologians, and for them the privileged position was granted an omniscient God: Hermes carried God's messages, and the art of reading those messages was thus labelled 'hermeneutics'. God has since retreated from the sciences – as we noted in the previous chapter. Yet the notion of a privileged position remains.

Hermeneutic understanding offered Dilthey a way to do two things. First, it could separate the natural from the human sciences – the *Naturwissenschaften* from the *Geisteswissenschaften*. Natural science hinges on *erklären*: it seeks to explain natural phenomena in terms of cause and effect. The human sciences (and the budding social sciences) involve *verstehen*: they seek to understand social phenomena in terms of relationships.

Second, hermeneutics offered Dilthey an independent perspective from which the human and social sciences could privilege knowledge – in other words, to sort good understanding from bad. This independent perspective can be obtained by interpreting particular passages by reference to the larger whole. As we learn from Outhwaite (1975, p. 34), Dilthey argued: 'The totality of a work must be understood through its individual propositions and their relations, and yet the full understanding of an individual component presupposes an understanding of the whole.' This constant movement between the whole and its parts is the famous 'hermeneutic circle', which Dilthey calls 'the central difficulty of the art of interpretation'.

By this move, Dilthey made hermeneutics philosophical. Suddenly it was no longer a didactic aid for other disciplines. The old question, 'How to read?' was pushed aside by the much broader question: 'How do we communicate at all?' This question invited a philosophical discussion about understanding symbolic communication as such, and several social scientists responded. Dilthey's distinction between explanation and understanding was elaborated by sociologists such as Max Weber. His hermeneutic approach was pursued by sociologists and social philosophers – most famously by his student, Martin Heidegger, and Heidegger's student, Hans-Georg Gadamer (1900–2002).

For Gadamer, knowledge is not about providing universal truths, but about expanding our own horizons and understanding. We do this by examining life as a product embedded in culture, and reflecting practical activity. Understanding is based on a feeling for the individuality and uniqueness of people; it is a way to understand the inwardness of the other (Gadamer, 1984, p. 57). Thus understanding a text does not involve recovering the author's original intention; rather, it is a matter of encountering a text from one's current position in time:

> every age has to understand a transmitted text in its own way, for the text is part of the whole tradition in which the age takes an objective interest and in which it seeks to understand itself. The real meaning of a text, as it speaks to an interpreter, does not depend on the contingency of the author and whom he originally wrote for. (Gadamer, quoted in Gunnell, 1982, p. 317)

In short, the meaning of each particular item comes from its place in the whole. For example, if we want to know the meaning of a particular word or phrase in a sentence, we often use the context of the sentence (or paragraph, or section, or piece) to understand what is meant. To understand the meaning of a piece, we can also place it in its larger context. As Gadamer (2002, p. 291) put it (with reference to the work of Friedrich Schleiermacher, a German theologian and philosopher), 'as the single word belongs in the total context of the sentence, so the single text belongs in the total context of the writer's work'.

The same sort of interactive method can be used to interpret social phenomena. In one interpretation of this method, the researcher starts with an initial proposition and projects it on to a particular context. He probes it for suitability and then returns to the proposition with an assessment of goodness-of-fit and notions of how to reformulate the original proposition (which in turn leads to another reinterpretation and a further search, and so on). The common hermeneutic strategy of 'tacking' back and forth between the particular and the general allows the researcher to develop a more flexible relationship with her subject.

Anglo-American Formulations: Structures and Institutions

For the British sociologist, Anthony Giddens (1982), this sort of tacking is similar to the naturalist notion of hypothesis testing. For him, however, such testing is not enough. Like many constructivists, Giddens calls for yet another level of hermeneutic understanding, one which he referred to as the 'double hermeneutic'.

At the first hermeneutic level, 'history matters'. Karl Marx (1852) hinted at this first-level understanding in a famous observation that 'human beings make their own history, but not in circumstances of their own choosing'. Giddens (1984) explores the full importance of Marx' aphorism in his theory of 'structuration', explaining that all human action is carried out within the context of a preexisting social structure governed by a set of norms and rules that are distinct from those of other social structures. Therefore, all human action is to some degree predetermined by the contextual rules under which it occurs. However, the structure and rules are not permanent. True, they are sustained by human action; but they are – at the same time – constantly modified by human action in complex processes of feedback. At the core of Giddens' concept lies the notion that social actors create and recreate the social structures they inhabit.

This understanding of the relationship between humans and society creates difficulties for social scientists, for at least two reasons. First, social scientists (unlike scientists who study the natural world) are members of the society that they study, therefore they can't observe the world from an external point of view. Second, they observe a social world that is already being interpreted by other actors who also inhabit it, and on whose observations the scientific observers are forced to rely.

As social actors we have the capacity to understand and respond to our analyses; thus our knowledge of the social world can actually affect that world. Indeed, it affects it in two ways. This is where the second hermeneutic level comes in: as a description of the two-tiered, interpretive and dialectical relationship between social scientific knowledge and human practice, where social analysts are part of the social world that they analyze.

This second-level understanding has been given a famous description in C. Wright Mills' notion of the 'cultural apparatus'. For Mills, our knowledge is greater than the simple sum of our observations: 'No man stands alone directly confronting a world of solid fact. No such world is available' (Mills, 1970 [1959], p. 405). Echoing the Kantian themes of Whewell, Mills notes that our knowledge of the world is provided by observers we have never met – and will never meet. Indeed, most of what we think of as solid fact is provided to us by others. Hence, all our knowledge is secondary. In fact, we all live in 'secondary worlds'.

What does this mean, exactly? Does it mean that human beings form the world in which they live? Or does it mean that consciousness in humans is formed by the world around them? For Mills, the answer is neither.

> The consciousness of men does not determine their material existence; nor does their material existence determine their consciousness. Between consciousness and existence stand meanings and designs and communications which other men have passed on – first in human speech itself and, later, by the management of symbols ... They provide the clues to what men see, to how they respond to it, to how they feel about it, and to how they respond to these feelings. Symbols focus experience; meanings organize knowledge, guiding the surface perceptions of an instant no less than the aspirations of a lifetime.
>
> ... For most of what he calls solid fact, sound interpretation, suitable presentations, every man is increasingly dependent upon the observation posts, the interpretation centers, the presentation depots, which in contemporary society are established by means of what I am going to call the cultural apparatus. (Mills, 1970, p. 406)

A vast, 'cultural apparatus', then, stands between individual humans and the world. It is the lens through which we (think we) see the world.

Gallic Contributions: structures quotidien and habitus humaine

French historians also probed this kind of reasoning during the early 1930s. Their main venue was the journal *Annales d'histoire économique et sociale*. Its imaginative editors and authors – foremost among whom were Marc Bloch and Lucien Febvre – enriched their understanding of past events by combining history with geography, sociology, collective psychology and other social sciences. In the process they produced a distinctive approach to the past that was often referred to as 'the *Annales* School'. These historians were less interested in topics such as war and high politics than in social groups and cultural history, and in collective attitudes and widespread world views of the past. Bloch (1973 [1924]), Febvre (1983 [1942]) and others referred to these studies as 'history of mentalities' (*histoire des mentalités*).

One of the most influential expressions of this basic idea comes from the *Annaliste* historian, Fernand Braudel, in the first volume of his magisterial study on the evolution of early capitalism – in a volume entitled, *Les structures du quotidien* [The Structures of Everyday Life]. Here Braudel showed how the lives of most sixteenth-century people consisted of routine behaviour. Over time, this routinized behaviour came to have diverse effects on people: imprisoning some, while giving meaning to the lives of others. Braudel argues that this still applies. With a nod to Hume and his concept of 'habit', Braudel writes:

> I think mankind is more than waist-deep in daily routine. Countless inherited acts, accumulated pell-mell and repeated time after time to this very day become habits that help us live, imprison us, and make decisions for us throughout our lives. These acts are incentives, compulsions, ways of acting and reacting that sometimes – more frequently than we might suspect – go back to the beginnings of mankind's history. Ancient, yet still alive, this multicenturied past flows into the present like the Amazon River pouring into the Atlantic Ocean of the vast flood of its cloudy waters. (Braudel, 1977, p. 7)

The basic notion of the *Annalistes* – and in particular Braudel's idea of the 'structures' of everyday life – has been developed in many ways by many authors. Some of them follow Braudel and investigate the formative impact of material routines of daily work – Michel de Certeau (1980), for example, who relies on the concept of 'practice'. Others direct their attention toward patterns of social relationships – for example, Pierre Bourdieu (1977) who, with a nod to Hume, has coined the concept of 'habitus'. Still others explore the more abstract exchange acts or patterns of thought and speech – such as Michel Foucault (1972), who reintroduced the concept of 'discourse'.

'Practice', 'habitus' and 'discourse' are not synonymous concepts. For Braudel and de Certeau, 'practice' involves countless inherited acts that are repeated in everyday routines and accumulated over time – they become habits that both give order to our lives and imprison us. For

Bourdieu (1977, p. 72), 'habitus' denotes a form for intersubjectivity or socialized subjectivity or 'the internalization of externality and the externalization of internality'. For Foucault, human beings do not recreate society through their behaviour as much as through their 'discourse' – that is, through the routine use of everyday language that constantly re-presents society, thereby maintaining it. For Foucault (for example, 1978, p. 12), then, 'discourse' maintains 'systems of thought' composed of terms, concepts, ideas, beliefs and practices that systematically (re)construct the subjects and the worlds of which they speak. Foucault's approach is consistent with Kant – indeed, he relies on Kant for some of his concepts. But Foucault also pushes the argument by gauging its social and political implications. Discourses, Foucault argues, serve to preserve society and legitimate power relations.

Discourses, then, connect language to knowledge and power, and through power to politics. Statesmen and nation-builders use discursive mechanisms to shape and mould their citizens. They use schools, hospitals, prisons, military camps and other institutions to socialize and discipline their citizens, to mould systematically the mentalities of the nation. The stated goal of the government is to maintain a well-ordered and happy population. Foucault argues that the actual effect is to produce citizens who are suited to fulfilling the government's policies. This practice is a widespread 'art of government' in modern societies – especially in liberal democracies or neo-liberal societies. Foucault (1991) coined the term 'governmentality' (*gouvernementalité*) to label this mode of governing.

Constructivists – be they French, Anglo-American or German – emphasize the part that the surrounding community plays on the way we perceive and understand the world around us. However, they disagree markedly about the nature of this influence. Some (such as Marx) portray the relationship between human agents and social structures as a simple dialectic; while others depict it by using the more complex arguments of a double dialectic (Dilthey or Mills, for example). Some (such as Giddens and Braudel) seek to capture this relationship through the concept of structure, while others (Gadamer and Bourdieu, for example) doubt the notion of lasting but latent structures and prefer to see this influence in terms of strategic or (re)constitutive acts. These authors are often influenced by theories of communication and language, our fourth framing device.

On Communication and Language

As we have already noted, scientists live in society and must relate to all kinds of people, among them, their fellow-scientists. In doing so, scientists read and review one another's writings; they discuss procedures and results; and they exchange facts and ideas. In short, they communicate, and their communication is mediated by language. Whewell was aware of the importance of language in science, and began his *The Philosophy of the Inductive Sciences* with a discussion 'concerning the language of science'.

Later, Thomas Kuhn elaborated on Whewell's claims about language and wove them into a more systematic discussion. In doing so, Kuhn took Whewell's arguments a long step further. For example, Kuhn did not just recognize that the distinction between fact and theory was unclear; he also argued that facts are theory-dependent – they are only meaningful in relation to some theory. In addition, Kuhn introduced a new and troublesome twist: he argued that facts are language-dependent. This threw an enormous wrench in the naturalist machinery. If facts are language-dependent, then so too is the world (as the world is composed of facts).

Following Kuhn, we find ourselves in a reality that cannot exist independently of language. Of course, Kuhn was not the first to make this connection. Members of the Vienna Circle also discussed the role of language – indeed, Alfred Ayer's (1952 [1936]) influential introduction to Logical Positivism was entitled ***Language**, Truth and Logic* (emphasis ours). The positivists, however, did not probe such questions deeply; this would have drawn them too far away from their focus on truth and logic. For the naturalists, language is partly a tool through which observations and knowledge are expressed, and partly a transparent medium that preserves the vast body of human knowledge.

For the constructivists, by contrast, language is much more. We have already noted how Kant influenced Whewell. We should add that Kant also influenced German idealists such as Johann G. Herder and Wilhelm von Humboldt, who argued that language is more than a transparent medium of communication; language affects the way we look at the world. Anthropologists have, in turn, relied on Herder and Humboldt to explain how vocabulary and grammar shape thought. One of the most celebrated of these explanations is formulated by one of Franz Boas's star students: Edmund Sapir (1906). Sapir claimed that language not only affects thought, but it also affects perception and cognition. One of Sapir's students, Benjamin Lee Whorf, went on to become a fire insurance investigator and relied on this claim when he wrote 'Blazing Icicles' – an unpublished yet legendary report which demonstrated how semantic misunderstandings led to a number of easily preventable fires.

For Sapir and Whorf, human thought and action were shaped by language and society (Whorf 1956 [1940]). Their claims – that speakers of different tongues think and observe the world differently – were greeted with much excitement in the 1930s and 1940s. Enthusiasm wore off, however, when no evidence was found to support the basic claims. By the

1970s, social scientists had become disenchanted with the Sapir–Whorf hypothesis. It was all but abandoned when two novel approaches emerged on the scene. First, there were new postmodernist elaborations – such as the claim that language is encased in conventions which are products of discursive practices that systematically (re)construct the subjects and the worlds of which they speak (Foucault, 1970, 1972; Shapiro, 1984; White, 1987; but see also the caustic essays by Pullum, 1991). Second, there was new evidence from cognitive psychology – for example, that people who speak languages that rely on absolute directions develop an uncanny sense of direction, and that people who think differently about space also tend to think differently about time (Boroditsky and Gaby, 2010).

The Linguistic Turn

For the constructivist, language does not merely concern the relationship between the observer and what is being observed; it involves the whole of society. Indeed, for some constructivists, language makes possible those acts of communication that constitute human society. But what kind of relationship is this? What does communication entail? How does it happen? And how does it relate to society? The major contributors to the naturalist tradition – Locke, Hume, Mill and the members of the Vienna Circle – are curiously silent on these questions. Naturalism simply assumes that observations are written down and disseminated to others in a neutral, or instrumental, fashion. But over the years there have been many rebel forces launching linguistic offensives against this aspect of the naturalist camp.

There has been no single, unified philosophical movement or a particular linguistic impulse behind these offensives; what we find instead is a plethora of guerrilla snipers. Thus it is hard to get a proper handle on the nature of this linguistic turn. However, to simplify the discussion, we can distinguish between two kinds of influences: a formalist approach to linguistics that originated in Eastern Europe toward the end of the nineteenth century; and a structuralist social philosophy that emerged in France.

The formalist approach can be traced to two ideas of the Swiss philologist, Ferdinand de Saussure (1857–1913). The first idea is that there is not necessarily a relationship between words and things; and the second is that language is made up of much more than just words.

Saussure's first idea comes from Immanuel Kant. If we point to a tree and say 'There is a tree', most people would make an immediate connection between the word and the thing in the world we call a 'tree'. However, Saussure did not; he argued that to assume that words point to things is to assume that the objects in the world present themselves to us pre-digested, as it were. Kant had explained that this was not the case. He had argued that when we observe things in the world, the human mind takes in the sense impressions and then begins to work actively with them and to fashion the impressions into recognizable objects. According to Saussure, Kant's theory suggests that the human mind performs two functions: it forms a sense impression into an image and determines that the image thus constructed is separable from all the other shapes and colours around it. These two mental functions are the key points for Saussure's analysis.

Saussure's second idea was entirely his own: that words are the elemental units of language, but a language is much more than a selection of words cobbled together. This idea, that a language is more than the sum of its individual components (words), implies that there is an underlying principle determining interrelationships among words. This principle affects the form that individual words assume (for example, whether they are conjugated or declined according to tense, case, number or gender).

Saussure drew a sharp distinction between words (*paroles*) and language (*langue*). A language, he argued, contains two different things: words and the principles that direct their use. The first component, the word, has no natural relationship to any object in the world. The second component – the principles which specify the usage of the word – Saussure called 'the structure' of a given language, and it is this structure that gives a word its meaning. The implications of Saussure's idea fired imaginations far beyond his own discipline.

In the wake of the First World War, this claim revolutionized the study of language everywhere. In America, linguists such as Leonard Bloomfield embraced Saussure's notion of 'structure' to develop a new science of 'structural linguistics'. In Europe, similar developments were nursed by Louis Hjelmslev in Denmark and Antoine Meillet in France. Most significantly, Saussure made an enormous impression on Russian and Eastern European linguists. In Russia, Saussure stimulated a distinct school of linguistic formalism which influenced thinkers such as Mikhail Bakhtin. In Prague, Roman Jakobson and Nikolai Trubetzkoy pursued Saussure's notion that the meaning of a word is determined not by its content but by its placement – 'not by what it contains but by what exists outside of it' (Saussure, 1986 [1916], p. 114). This so-called Prague School developed a now standard theory in linguistics, where the inventory of sounds in a particular language could be analyzed in terms of a series of contrasts or opposites. The Prague School also contributed to the electrifying effect that Saussure's imagery had on scholars in other fields.

Around the time of the Second World War, the notion of structure began to animate the social sciences. In France, the anthropologist

Claude Lévi-Strauss applied Saussure's discussion about *langue* and *parole* in his ambitious, Kant-like search for the basic structures of the human mind. A Jew, Lévi-Strauss fled France during the war, spending most of the war years among a community of intellectual émigrés in New York City. Here he met Franz Boas, Roman Jakobson and others who inspired him to search for the formal codes and universal mental structures that he believed lay beneath all myth and kinship relations. Lévi-Strauss was particularly interested in patterns associated with parenthood and family relations (*The Elementary Structures of Kinship*, (1969 [1949]); in totem mentalities (in *La pensée sauvage*, 1962) and primitive myths (first in *Mythologiques*, 1964–71, and later in particular myths associated with different eating habits, for example, *The Raw and the Cooked* (1979 [1964]), and *From Honey to Ashes* (1973 [1967]). In these studies, Lévi-Strauss examined social relationships with an eye to uncovering the underlying structure of societies.

(Before turning to explain the title of this subsection, we want to draw attention to the fact that the New York encounter between Franz Boas and Lévi-Strauss was full of tragic symbolism. Apparently, while meeting Lévi-Strauss for lunch at Columbia University's Faculty Club on 21 or 22 December 1942, the 84-year-old Boas collapsed and fell from his chair. Lévi-Strauss tried to revive the fallen Boas, but he died of a heart attack in the Frenchman's arms. The details of this tragic lunch are both fuzzy and contested (see, for example, Lowie (1947).) We learned of this story in an internet post from Dan Everett (2009), who provides us with a fitting epitaph to this section. When Boas collapsed that day in the arms of the young founder of French anthropology 'Lévi-Strauss assumed from his fallen colleague the symbolic mantle of leadership, becoming the most important living anthropologist of the twentieth century, a distinction he maintained for another 67 years').

Now back to our story. The title of this section, 'The Linguistic Turn', is a reference to an influential book from 1967 with the same title, edited by Richard Rorty. In the decades that followed, work in the humanities and social sciences increasingly recognized the importance of language in framing the way we see and interpret patterns in the world. This linguistic turn paralleled other developments in a broader structuralist movement, which searched for underlying patterns and regularities upon which meanings rested. Though individual members were reticent about being associated with it, the structuralist movement often attracted individuals of a radical persuasion, especially in France, where it was associated with radical Marxists such as Louis Althusser and Nicos Poulantzas.

While structuralism allowed its followers to distance themselves from the normative framing that accompanied Western academia, it did so at the cost of local knowledge. This is a tremendous liability for most constructivists. Indeed, the structuralist's willingness to distance herself from historical and contextual reference points produced a backlash in the form of post-structuralism (as associated with people such as Julia Kristeva and Jacques Derrida). Post-structuralists reintroduced the importance of culture and context in understanding a text or social situation. Typically, post-structuralists hold that the meaning of any work is itself a cultural phenomenon.

Recapitulation: A Constructivist Way of Knowing

In this chapter we have tried to portray an alternative approach to social study, a competitor to naturalism. The portrait we have painted is sketchy, and made with broad strokes. Nevertheless, we hope to have captured some of constructivism's most distinctive features. In doing so, we have granted Immanuel Kant a central role in the constructivist tradition. As a consequence of the ambiguous and contentious nature of Kant's arguments, they continue to influence the nature of contemporary debates about what constitutes science.

We have swept quickly through a wide swath of the Western world's academic history – from historical authorities such as Kant and Whewell, to the many interwar intellectuals who fled the rise of fascism in Europe, to even more recent authorities on discussions about context and meaning. At first glance, it appears difficult and daunting to unify this disparate and varied group of thinkers under any single methodological claim. We realize that the diversity of these thinkers makes it difficult to find among them any single ontological claim, any uniform epistemological vision, or any particular methodological stance. Indeed, we worry that many constructivists will balk at the idea of trying to unify such diverse thinkers as Kant, Kristeva and Kuhn. But we take some comfort in the fact that the same thing could be said of scholars from the naturalist camp. After all, both traditions are diverse; the difference between them is more a matter of degree than of nature.

At a pinch, we are prepared to argue that the naturalist camp is the less diverse of the two. The vast majority of naturalist scientists are willing to share a small handful of philosophical assumptions – for example, they agree that there is Real World out there, and that scientists have access to it through their senses. In contrast, it is more difficult to reach a consensus among constructivists on any given ontological or epistemological position. While many constructivists would accept that social scientists do have access to a Real World by way of their senses, many others question the existence of that World. Still others would argue that there is a Real World, but that neither perceptions nor human reason allow us guaranteed access to it, as it is buried under so many layers of conceptual and contextual meaning (many, many turtles down). In short, the constructivist camp covers much territory, and as a consequence it may house a more heterogeneous group of fellow travellers than the naturalist camp.

If we are to discuss the constructivist camp at all, however, it is necessary to provide it with some unifying properties – if only to help us juxtapose this tradition with that of naturalism described in the first part of the book. Such unifying properties do exist; the problem is that they are distributed unevenly among members of the constructivist camp. To understand and depict these unifying characteristics we might think of them in terms of Wittgenstein's (1999, §§ 66–71) reference to 'family resemblances': a set of features that are recognized as being similar, but which have no single thing in common.

The Constructivist Other

Family photographs depict a group of individuals who share noticeable traits. That is not to say that every member of the family shares one or two dominant features; rather, they resemble each other in that they, together, on closer scrutiny, share a set of features distributed unequally among them. A few of the men may have the same big ears, some of the women may have the same thick neck, some (both men and women) may have the same kind of blunt nose, others may share the same mass of black, straight hair, and so on. But, compared to the physical characteristics shared by other families, it is possible to distinguish a family resemblance. It is in these ways that we can think of the family of constructivist social scientists: we recognize that no single methodological feature is shared by every member of the constructivist troop, but some features are shared by some of the members in a way that distinguishes them from other methodological families.

One of the most commonly held family features in the constructivist camp is a deep scepticism of the naturalist approach to social science. This takes aim at the core ontological, epistemological and methodological claims of the naturalist tradition. As this scepticism is broadly shared, residents of the constructivist camp might be construed as a collective Self by virtue of their common opposition to a naturalist Other.

At the end of Chapter 2 we identified three broad joists that sustain the naturalist tradition – the notion that the Real World exists; that this world is a realm of independent particulars that relate to each other in regular and patterned ways; and that humans have access to this world through systematic observation. In Figure 8.1 we identify three basic

Figure 8.1 *The three basic joists of constructivist social science*

- An *ontology* based on the precepts that women and men are malleable, and that each of us participates in the construction of our own world.
- An *epistemology* which, in addition to sense perceptions and human reason, relies on a much broader repertoire of epistemological devices (such as empathy).
- A *methodology* which seeks to identify socially constructed patterns and regularities.

joists in the constructivist tradition. It is important to note that none of these joists were hewn from the trunk of the natural sciences. In fact, all three were developed in self-conscious opposition to naturalism. It is this opposition to the naturalist tradition that is perhaps the most important single feature that can unify the disparate constructivist camp.

The first joist is *ontological.* Constructivists convey a basic uncertainty about the nature of the world. For them, the world does not exist independently of our senses; it is a world of appearances. More to the point, the world we study is one that appears to people who find themselves situated in different contexts. Consequently, the world appears differently to different people; its appearance varies with the contextual setting (temporal, geographical, engendered, ideological, cultural and so on) of the observers.

This constructivist ontology is at odds with the one shared by empiricist philosophers such as John Locke and David Hume in at least two important ways. First, constructivists do not eagerly embrace the naturalist notion of a Real World. Rather, they tend to argue that the world is a human construction. Second, constructivists harbour a deep suspicion toward Locke and others who endow humans with fixed and permanent attributes. Constructivists are not fond of invoking human nature; they tend to portray human beings as adaptable and malleable creatures.

In short, the common point of departure for most constructivists is an agreement that the naturalist tradition provides an unsatisfactory basis for social science. On this point, constructivists tend to distance themselves from scientific realists, as we explained in Chapter 1.

Constructivists also agree that it is important to discuss and consider the nature of the relationship between the mind and its world. For as long as this relationship remains unsettled, constructivists and naturalists cannot agree about the source of the patterns that both traditions agree exist (and which cry out for explanation). Naturalists are familiar with Kantian arguments – they tend to sample them, feign polite interest in their basic tenets, and then move on quickly to more practical tasks. Constructivists, in contrast, tend to linger on these Kantian arguments. While many constructivists would agree that the physical world is material, concrete and given by nature, they are loath to accept the same description of the social world. For them, there is no clearly delineated single social world: there are many. None of these worlds are naturally given; all of them are socially constructed. Each world is created by human beings – not in the sense that humans consciously set about building their world from some original blueprint, but in the sense that this world has evolved as a result of human interaction in society, through history, with ideas, using language. Having said this, we should point out that constructivists disagree about how much of the naturalist philosophy we can and should keep. Also, they differ markedly on the distance they want to travel to find a more credible alternative.

This has significant consequences for the constructivist attitude toward truth. Given the ontological certainty of the naturalist approach, it is common to find naturalists who are firmly committed to uncovering real and unyielding truths about the world. While this commitment to singular truths can be found among some constructivist scholars, they generally tend to be more agnostic on issues of truth. To paraphrase Rorty (1979, p. 377), the point for many constructivists is to keep the conversation going rather than to find objective truth.

This brings us to the *epistemological* joist of constructivist science. Given the more open-ended ontological position shared by constructivists, we should not be surprised to find their epistemological joist to be of sizeable dimensions. Constructivists refuse to be limited to sensual perception and reason as the only means of accessing knowledge. Instead, they tend to embrace a much broader selection of epistemological devices, prioritizing those that protect, enhance and exploit contextual meanings.

In short, constructivists tend to be epistemological pluralists. They are willing to employ different tools to understand the unique nature of the social world. This willingness flows from two related claims. The first is ontological: that the natural and social worlds are different. The second is epistemological: that in order to obtain knowledge about the social world, it is necessary to break away from the mechanical notion than the whole is a simple aggregation of its parts: we need to understand how the parts relate to one another in the context of the whole. For the social sciences, knowledge is carried by individuals but anchored in collectives.

For the constructivist tradition, then, knowledge is not a subjective thing threaded through and through with relativism (as some of its critics charge). Knowledge is intersubjective. The world is real. It is an object – a *phenoumenon*, a thing-for-us – and we can obtain knowledge about it. But how do we do that? The short constructivist answer to this important question is: very carefully!

The reason for being so careful is related to the constructed nature of the social world. The truth isn't just 'out there'. Knowledge about the social world is always knowledge-in-context; it is socially situated and has social consequences. As a result, knowledge is always *somebody's* knowledge. It is, in Robert Cox's (1996, p. 87) famous formulation, '*for* someone'; it serves somebody's purpose. To 'know' is to be in a position to dominate or enslave.

Because knowledge and power are so closely associated, constructivists hold that it is necessary to approach knowledge with both scepticism and great self-awareness. We need to be attuned to the context in which knowledge is engendered, by whom and for what purpose. This suggests a more strategic relationship to epistemology (than we find among naturalists). We also have to approach knowledge with the proper attitude. For example, we need to consider knowledge in political solidarity with the more marginalized members of society or with the proper respect for (and empathy) with the object at hand. In short, constructivists approach the world and its knowledge *critically*.

But besides being careful and critical, how do constructivists approach the social world when they search for knowledge about it? Constructivists differ on this point. Some are pragmatic and argue that the question, the purpose and the sources at hand must determine the method: for example, sometimes statistical analyses and hypothesis testing is the way to go; and sometimes an interpretive narrative approach is the more natural choice. Others shun any procedural design that smacks of naturalism. Some constructivists have found in hermeneutics a basic method that dovetails nicely with the ontological and epistemological tendencies of constructivism. Our point is that constructivists often rely on the same basic methods as do naturalists, but they do so in different ways and toward different ends. This important lesson is elaborated on in the chapters that follow.

From these ontological and epistemological commitments we find a confirmation of the constructivists' *methodology*. Constructivists realize that the world is filled with repetitions and regularities, but they insist that these patterns are socially constructed, even as the world appears to us as objective fact. For this reason, constructivists approach their study with tools and approaches that can identify these socially-constructed patterns in the world, and understand them in the light of the contexts that give them meaning. Thus the focus of their inquiry is just as often the inquirer (and her context) as it is the particular object of inquiry – because it is here that the roots of these patterns lie buried.

Conclusion

In this chapter we have traced the constructivist approach back to David Hume, who jarred Immanuel Kant from his dogmatic slumber. As a consequence of this rude awakening, Kant produced a contentious, ambiguous and important argument that has kept entire philosophy departments busy for well over two centuries. Kant's argument about the human (in)ability to understand (directly) the Real World still lies at the heart of constructivist approaches today. Given Kant's reputation for opaqueness and obscurity, we have relied on William Whewell to shine a light on the key precepts of constructivist thought. Among these is the insightful recognition that our knowledge is framed by history, society, ideas and language.

Whewell's ideas took on a new urgency in the closing decades of the twentieth century. The result has been a varied and multifaceted approach to social science that shares certain ontological beliefs, but little else (except, perhaps, a common antagonism to the naturalist approach). This constructivist approach to social science is sceptical of the naturalist quest for truth and order; it is willing to embrace new epistemological outlets; and it is wary of rigid demarcation principles. As a consequence – and as we shall see in the chapters that follow – constructivists use social scientific methods in ways (and toward ends) that differ substantially from the naturalists'.

The constructivist's priority is to protect (historical, social, ideational and language-based) contexts, as these provide insight and meaning. While naturalists employ their hierarchy of methods to map the Real World's inherent patterns, constructivists use similar methods to map and explain the variance in patterns observed, and to zero-in on the nature of the explanations that link the observed patterns. In the chapters that follow we shall see familiar methods employed in new ways toward these constructivist ends.

We shall also see that it is more difficult to rank the methods employed by constructivists into any sort of hierarchy. While most constructivists have a soft spot for narrative approaches (as these provide scholars with a proximity to the data and context that is necessary to gain insight), constructivists also employ comparisons, statistics and even experiments. But they employ these methods in ways that are designed to protect, nurture and reveal the contexts and meanings that constructivists cherish, and/or to map and explain the different ways in which we come to see and understand our social world(s).

Recommended Further Reading

To begin at the beginning, read William Whewell's *History of the Inductive Sciences* (1967 [1837]) and his *The Philosophy of the Inductive Sciences* (1996 [1840]). Thomas Kuhn's *The Structure of Scientific Revolutions* (1970 [1962]), Peter Winch's *The Idea of a Social Science and Its Relation to Philosophy* (1958) and Ludwig Wittgenstein's *Philosophical Investigations* (1999 [1953]) are classic references. For more accessible introductions to the philosophy of constructivist science, see Berger and Luckmann's *The Social Construction of Reality* (1966) and John Searle's *The Construction of Social Reality* (1995).

Chapter 9

From Story Telling to Telling Histories

The social scientific project, regardless of its methodological point of departure, seeks to find and explain patterns. For naturalists, the project is a fairly straightforward process of observing (or experiencing) and noting the patterns found naturally in the world. For constructivists, however, the patterns of interest are illusive and complex; they cannot be taken at face value; their nature and origins need to be probed and pondered. In short, while naturalists believe that facts speak for themselves, and that knowledge will grow by their relentless accumulation, constructivists tend to doubt the innocence of facts and question whether facts come to the social analyst 'like fish on the fishmonger's slab' (Carr, 1987, p. 9).

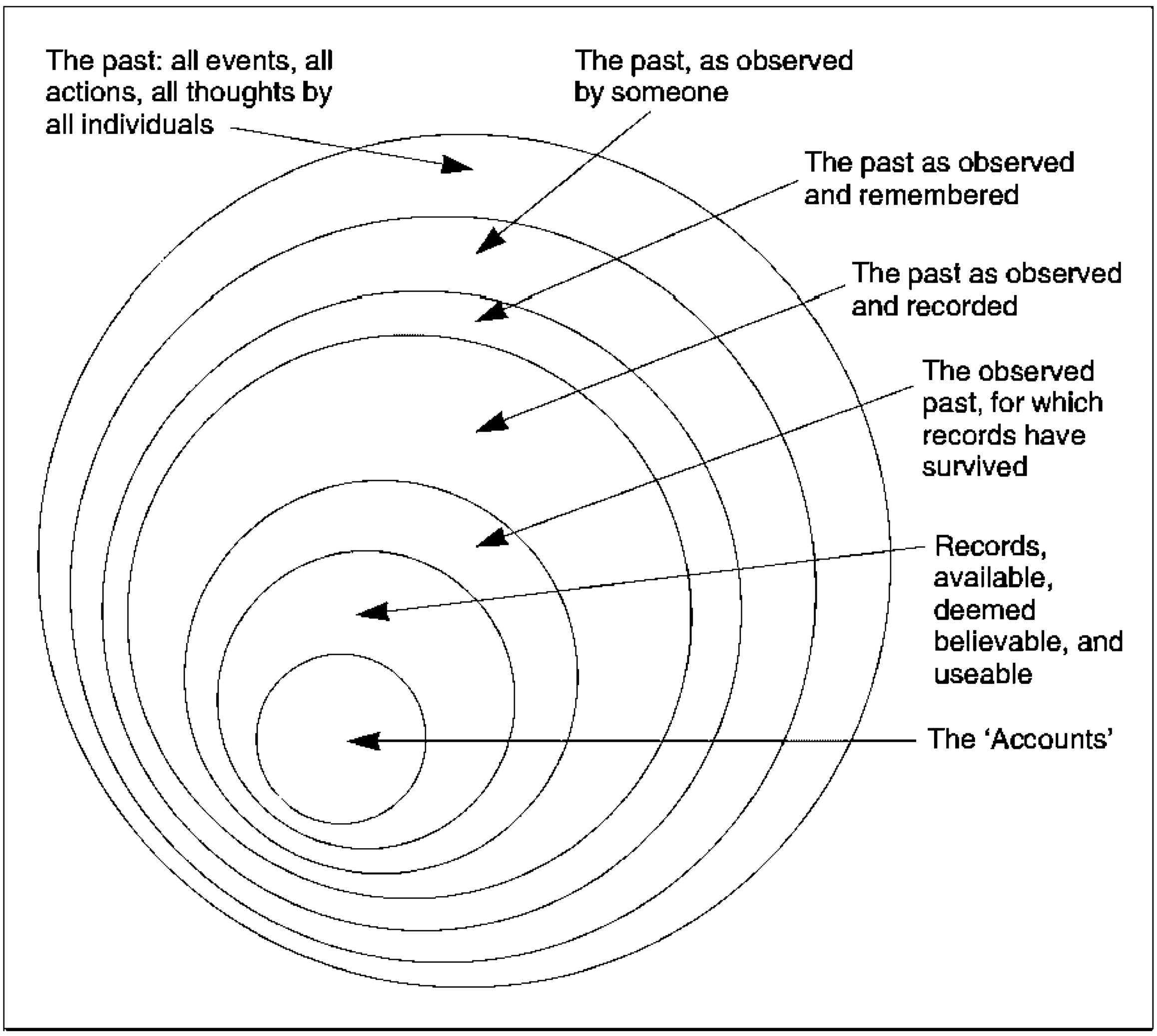
The main reason for this difference lies in the naturalist's assumption that the subject can he separated from its object of study; that the student of social phenomena can be separated from his facts. When facts are understood to be (wo)man-made, the relationship between the analyst and his facts becomes more complicated. This difficulty can be divided into two parts: the nature of social data; and the analyst's relationship with that data.

The nature of social data is a two-edged problem for students of social phenomena: facts are both too plentiful and too sparse. On the one hand, we can find ourselves overwhelmed by the sheer mass of facts. As E. H. Carr (1987, p. 14) notes with some envy, few facts survive from the very distant past, and scholars of ancient history appear to be competent 'mainly because they are so ignorant of their subject'. Modern social scientists enjoys none of the advantages of this built-in ignorance. They must cultivate the necessary ignorance for themselves. In other words, students of social phenomena often find themselves suffocating under the massive weight of potentially relevant information. Method, for both naturalist and constructivist approaches, is the traditional means of lightening this load.

On the other hand, students of social phenomena often find themselves at the mercy of too little data. This problem is especially acute for those interested in historical phenomena. As we have already noted (in Chapter 6), one problem is the historian's reliance on primary sources. As a result of this, the historical analyst is often confined to a narrow field of research: kings, generals and Members of Parliament are more likely to leave primary accounts than are housewives, gravediggers, fishermen, bakers and mimes. The problem is exacerbated by the fact that sources (even primary sources) tend to shrivel up over time. By the time a phenomenon finds an interested sponsor, its remnants may be vastly incomplete. Worse, there are no guarantees that the surviving historical record is – in any way – representative. The nature of this problem is illustrated in Figure 9.1, where the past – and our access to it – can be understood in terms of concentric circles.

If we let the outermost circle in the diagram in Figure 9.1 represent the entire past, we can see how a number of largely random factors can severely restrict our access to it. For this past to enter into the analyst's

Figure 9.1 ***Representing the past***



account (represented here by the innermost circle), it has to be observed, remembered and recorded – and the records must have survived. Each of these steps is represented by circles of decreasing size within the largest circle (representing the entire past). These more-or-less random factors (observation, remembrance, recording, survival) determine the representativeness of the account that remains. For example, if we assume that the outermost circle in Figure 9.1 captures the entire past in some sort of Cartesian space, and we note that the final account ended up in the lower left-hand 'corner' of that space, then we have an account that is not 'centred' or representative. Without additional information to explain or interpret this bias, the analyst would probably assume (in error) that history is centred on the analyst's account.

It is because of these difficulties that constructivists tend to be critical of the three basic assumptions of naturalist historians: (i) that there is 'a past' that can be captured by scholars; (ii) that data are available to scholars that are in some way objective or representative of that past; and (iii) that these data are simply there for the taking. Thus Edward Carr speaks disparagingly of the naturalist who considers his facts as if they were fish on the fishmonger's slab. In practice, of course, the social analyst interacts with his data:

> The facts are really not at all like fish on the fishmonger's slab. They are like fish swimming about in a vast and sometimes inaccessible ocean, and what the historian [or social analyst] catches will depend, partly on chance, but mainly on what part of the ocean he chooses to fish in and what tackle he chooses to use – these two facts being, of course, determined by what kind of fish he wants to catch. By and large, the historian [or social analyst] will get the kinds of facts he wants. (Carr, 1987, p. 23)

While traditional historians are aware of this problem, it would seem that they prefer to ignore it (or disarm it with humour) rather than tackle it head-on. Thus, in 1867, the English Rankean, James Anthony Froude (1963, p. 21), noted: 'It often seems to me as if history is like a child's box of letters, with which we can spell any word we please. We have only to pick out such letters as we want, arrange them as we like, and say nothing about those which do not suit our pnrpose.' Or, following the fictional Catherine Morland, the heroine of Jane Austen's *Northanger Abbey* (quoted in the dedication to Carr's book): 'I often think it odd that it should be so dull, for a great deal of it must be invention.' In contrast, constructivists have come to a sober realization about the importance of their relationship to their data.

Constructivists also appreciate a good story, but they differ on the way such stories are told and on the role they can (or should) play in the social scientific project. This means, first, that constructivists favour thick descriptions, where the analyst can climb into an intricate (hi)story and get to know it from the inside out. But it also means that the constructivist is attracted to many unique histories and the storytellers behind them. For this reason, much of this chapter focuses on the storytellers themselves, and their contexts.

To illustrate the sundry roles that historical depictions can play in constructivist accounts, this chapter lays out a menu of different framing mechanisms. In doing so, we borrow the same basic framework from Chapter 8: we consider how patterns can be framed by social, ideational, communicative and historical references. Through doing this, we encounter some of the most radical, but also some of the most innovative and popular, approaches in contemporary social science.

By telling their stories in these different ways, constructivists problematize the way that naturalists employ historical accounts as unbiased 'facts' in larger social scientific analyses. Constructivists are not committed to a single historical account (and tend to see attempts at depicting history in this way as misleading and dishonest). But constructivists do not employ historical approaches only to criticize naturalist accounts. Constructivists celebrate the diversity of perspectives, while producing better interpretations of the phenomena being studied. For them,

> '[b]etter' interpretations do not aim at arriving at the final, objective (in the positivist [naturalist] sense) truth of the matter but rather are those that are at one and the same time aware of their own conditionality and are open to the distortions occasioned by conditions of radical inequality ... 'Better' interpretations are readings in which the subject might recognize himself or herself, his or her meanings, his or her actions, and might even agree. And 'better' interpretations are those that are simultaneously attentive to participants' self-understanding and the way power functions in language. (Euben, 1999, p. 45)

The remainder of this chapter is divided into five parts. We begin with a quick return to Sherlock Holmes' approach to detection, and note how an alternative, more constructivist, mode of sleuthing is offered by Miss Jane Marple. We then investigate the different ways that constructivists use social context, ideas, communication devices and historical settings to investigate and explain the sundry social patterns under study. As the reader will soon discover, some of these approaches share close affinities with naturalist approaches, while others do not.

The Mysterious Ways of Miss Marple

Naturalist scholars rely on disciplined and repeatable procedures, transparent designs, logical arguments and publicly verifiable sources to reveal and understand the patterns they seek. As we have seen, their method of choice is the experiment and their mascot is the professional London detective, Sherlock Holmes. Holmes exemplifies the naturalist historian with his unflinching faith in empiricism and his insistence that there is, in fact, a truth to be found.

Given the constructivist's scepticism toward a singular truth, and the role of empiricism in uncovering it, analysts in this tradition seek another detective mascot – one more sympathetic to their own methodological priorities. One likely candidate is Agatha Christie's marvellous Miss Jane Marple, from the small English village of St Mary Mead. Spinster, busybody and a shrewd observer of human nature, Miss Marple also exposes thieves and murderers. However, she relies on a radically different approach than does her male counterpart.

Sherlock Holmes subscribed to the traditional correspondence theory of truth. For him, a statement was true if it corresponded to the facts of the case. Miss Marple is more circumspect. On the one hand, it would be unfair to suggest that Miss Marple denies a claim to be true when it corresponds to the facts. On the other hand, however, she seems to have a very different notion of what constitutes a fact. Also, she seems more reluctant to confirm the existence of a single, unchangeable world; she may see many worlds – and consequently, she may also see many truths. Finally, Miss Marple is not bound to induction as the only way of gaining knowledge. She also draws on insights into human nature, including keen within-village insights into comparable cases, and she trusts her imagination to provide explanatory principles and associations.

On Eccentricities and Serendipities

Miss Marple's approach is well demonstrated in one of her most famous cases, *A Pocket Full of Rye* (Christie, 2000 [1953]). When a wealthy financier, Rex Fortescue, is found murdered at his desk, his discontented wife is at first suspected of the crime. But then his wife too is murdered – poisoned with cyanide at teatime. Soon after, their maid is murdered while hanging up newly washed clothes to dry – strangled with a stocking in the garden, and found with a clothes-peg on her nose. Not surprisingly, the police are at a loss to explain the string of murders. When it turns out that all three victims were found with grains of rye in their pockets, their confusion is complete. Miss Marple, however, is able to discover a pattern and prods Inspector Neele to investigate the involvement of blackbirds.

The inspector brushes aside the batty old spinster, who – for good measure – begins to recite a traditional nursery rhyme:

> The king was in his counting-house, counting out his money,
> The queen was in the parlour eating bread and honey.
> The maid was in the garden, hanging out the clothes,
> When down came a blackbird and pecked off her nose.

Suddenly insight shines on the inspector as well: Rex is the king, of course; he was murdered while working at his desk, counting out his money (as it were). His wife represents the queen: she was murdered in the parlour while having tea. The maid was murdered in the garden, hanging out the clothes. In short, Miss Marple had seen that the murders all seemed to be connected to the old nursery rhyme 'Sing a Song of Sixpence'. It dawns on the inspector that the old bat may be on to something. But what? And where do the blackbirds come into it?

Miss Marple is well positioned to answer these questions. First, she is as well-informed about the wealthy Fortescue family as she is about every other family in the village. Her approach to solving crimes depends on her intimate knowledge of the people and the context in which the crimes are committed. For example, she knows that the murdered financier had two sons from a previous marriage, both of whom had spent some time abroad, exploring possible investments in mines and minerals. Further investigations would reveal that father and sons quarrelled over the ownership of an apparently worthless venture in the USA: the Blackbird Mine.

This brief example illustrates why Miss Marple constitutes a reasonable representative of the constructivist approach. Like the naturalist, Miss Marple begins her investigation by looking for patterns or regularities. To do this, however, she does not rely on induction – at least, not on induction alone. Miss Marple suggests that there are many different ways of making sense of events – and one of them goes by way of Mother Goose's rhymes. As the perpetrators of the crimes are familiar with Mother Goose, their actions were influenced by the nursery rhyme – either consciously or subconsciously. The point is that, once the pattern is recognized, Miss Marple can use it to piece together the missing bits of the puzzle and come to a better understanding of what had happened. In a similar way to Sherlock Holmes, Miss Marple can use her approach to solve baffling mysteries.

Miss Marple's method, though clearly unorthodox, delivers the criminals. As with many constructivists, she does not seem to be too concerned about formal or explicit rules or methods. It is for this reason, perhaps, that naturalists might refer disparagingly to the constructivist method as 'serendipitous' or an approach of 'accidental discovery'. This method, the naturalist will argue, does not involve systematic reasoning. Rather, it relies on gut feelings and dumb luck – and neither deserves a place among the methods of the social sciences.

Despite its poor reputation among naturalists, this serendipitous method enjoys a wide following. Journalists use it all the time, with an expectation of observing some event first-hand and producing newsworthy eyewitness accounts. Anthropologists also use it; their fieldwork method exploits the virtues of 'thick description' based on insight, which only participant observation can yield. Historians often work in the same way: they delve fully into some past society with the hope of gaining a deep insight about its people and culture. While the method is often belittled by naturalists for its simplicity, there can be no denying that it remains an important and influential way for analysts to explain and understand social behaviour. The insight and meaning generated by this serendipitous method can have an enormous impact on the way we come to see the world.

Social Framing

The attractiveness of constructivist approaches is fueled, in part, by the exaggerations of practicing historians (and the social scientists who rely on them). The classic elaboration of this point was formulated by E. H. Carr (1987 [1961]) in his book *What Is History?*

There are several reasons why Carr's book enjoys classic status. First, it is extremely well written: it is peppered with seductively formulated eruditions and pithy claims. Second, Carr did a marvellous job of capturing the mood of his day – the book was written in the early 1960s, and found many adherents among rebellious students of the time. Finally, *What Is History?* is a learned book, written by an extremely well-read man who himself was a practising historian as well as an analyst. Indeed, Carr was one of his age's most important and influential analysts of current international events. While there is much depth to this little, readable book, we shall focus on just one of its more influential currents: its discussion of social context.

Carr (1987) argues that history constitutes a dialectic between general statements and facts. In particular, Carr sees history as a dialectic between the historian and his facts (in Carr's first chapter) and between society and the individual (in Carr's second chapter). As we have already introduced the problem of the historian and his facts, we shall focus now on the second dialectic.

Carr places the historian, as a social creature, centre stage. While this has always been acceptable for historians with respect to the objects of their study (for example, that a historian of Thomas Hobbes should recognize that Hobbes's thought is a product of his time), Carr insists that we must do the same for the subject – that is, for the historian – as well. After all, historians are also individuals, born of a specific time and context.

Though naturalist social scientists may be aware of how context affects the actions of those they aim to study, they are less eager to apply the principle to themselves. Instead, they tend to hide behind the naturalist ideal of scientific objectivity. Constructivists, in contrast, emphasize that the social analyst always acts from within a context, and under the influence of a distinct society (though they may disagree about the significance we should attribute to this context). The analyst is a product of, and a spokesperson for, the society to which she belongs.

Scholars and Society

This argument is not new, of course, and it is relatively easy to locate. It is evident in Carr's short description of how leading historians reflected the social context of their disparate times. For example, we can see it in his discussion of the social context supporting George Grote's (2002 [1846–56]) famous *History of Greece*. Grote was a British banker – and hence a member of the upper bourgeoisie – in the 1840s; he invested the aspirations of the rising and politically progressive British middle class in an idealized picture of Athenian democracy. The same concern is apparent in Carr's discussion of Theodore Mommsen's (1898 [1854–56]) celebrated *History of Rome*. Mommsen, a German liberal, was disillusioned by the German revolution of 1848–9 and was imbued with a sense that Germany needed to be saved by a strong leader. This sentiment was reflected clearly in Mommsen's admiration for decisive Roman emperors. Grote's book tells us much about Greece, of course, but it also provides a good deal of (indirect) information about the English society of Grote's day. Likewise, Mommsen's history of Rome provides us with a great deal of indirect information about the Germany of his time. Carr's basic point is that, 'you cannot fully appreciate the work of a historian unless you have first grasped the standpoint from which he himself approached it; secondly, that that standpoint is itself rooted in a social and historical background' (1987, p. 38f).

The point is easily illustrated if we follow the evolution of a particular historical project over time – preferably a project where sources are scarce, so that there is some room for the historian to compensate for missing facts with flights of fiction-like fancy. This may give us a chance to monitor the waves of fashion that wash across history and the social sciences with depressing regularity.

Consider the historiography on the lost city of Troy. Homer's *Iliad* is the only source of the decline and fall of this fabled city. Consequently, Troy's fate has tickled the curiosity of scholars for nearly 3,000 years. Indeed, Troy's fall is one of the great conundrums of Western historiography: several generations of learned scholars have sought to determine the causes of its decline and fall. Despite this, Troy eluded scholars for centuries. Its very location remained a mystery until the final quarter of the nineteenth century, when a German expedition, led by the controversial archaeologist, Heinrich Schliemann, claimed to have found its ruins in Hisarlik at Mycenae in north-western Turkey. Schliemann's findings triggered a ferocious debate that preoccupied the world of ancient historians for several decades.

Given the paucity of data, historians and archaeologists had a tendency to see their own social conditions in the remnants of Troy. These interpretations suggest that the patterns that develop in the data are not just the outcome of chance (or deposited by History). Archaeologists, historians and social analysts are not isolated individuals, acting in a vacuum. They analyze and interpret their data in – and under the impulse of – specific social contexts. Different archaeologists did not just devise different explanations; they devised explanations that reflected the major preoccupations of their own time.

In the first round, before the First World War, the German archaeologists Heinrich Schliemann and Wilhelm Dörpfeld devised explanations that reflected the preoccupations of a young, insecure German nation. Victory in a war against France in 1871 helped to unify the German state, but the state always existed nervously, constantly fearing France's revenge. Schliemann's and Dörpfeld's interpretations of the fall of Troy focused on a savage war that swept through the area between 1193 BC and 1184 BC (Wood, 1986, p. 68).

Subsequent interpretations during the interwar period by the American archaeologist, Carl Blegen, and others, focused on the role of economic crises in the fall of Troy – reflecting the economic uncertainties of their own age. Blegen's account of the crisis that took place in Troy before its fall employs an imagery that Blegen himself must have witnessed in the USA during the years of the Great Depression – indeed, there are instances where Blegen actually interprets his finding at Troy with reference to 'soup kitchens'. Later, when Blegen revisited some of his findings in the 1950s, American politics was preoccupied with the implications of the 'fall of China', the Korean War, and a tense and uncertain phase in the Cold War. This context was marked by a powerful fear by Americans of a surprise Soviet (nuclear) attack, and these contextual preoccupations are reflected in Blegen's new interpretation, where Troy was seen to have been sacked in a vicious attack that engulfed the city in flames.

More recent archaeologists and historians have rejected Blegen's account of the fall of Troy. During the 1980s and 1990s, Schliemann's and Blegen's war-based theories fell from fashion and new explanations became more popular (and were supported by new evidence, collected by new expeditions). These more recent arguments have stressed hitherto neglected factors pertaining to the social texture and the environmental context of ancient Troy. We might expect that our own generation of explanations will tend to reflect our own, post-Cold War, context: mass migrations, multiethnic problems, biological deterioration, overpopulation, overdevelopment, epidemics among the population, and ecological imbalance.

It is important to recognize that the influence of social context need not be seen as a handicap, as the above illustration of Troy seems to suggest. 'Great history,' writes Carr, 'is written precisely when the historian's vision of the past is illuminated by insights into the problems of the present' (1987, p. 37). It was to prove this point that Carr had introduced the historical works of Grote and Mommsen. They help Carr to illustrate 'two important truths': (i) 'you cannot fully understand or appreciate the work of the historian unless you have first grasped the standpoint from which he himself approached it'; and (ii) 'that the standpoint is itself rooted in a social and historical background' (Carr, 1987, p. 39).

An insightful example that recognizes the role of contextualization is provided by Norman F. Cantor (1991) in his *Inventing the Middle Ages*. Cantor shows that there is no single historical record for the medieval period, but rather many historical records, each of which is a function of a given expert's personal context, the more general political/ideological context and/or the expert's choice of method. These individual biases are not the result of poor scholarship – indeed, Cantor traces these autobiographical tendencies in the 'Great Medievalists' of the twentieth century (a less than motley crew that includes some of the biggest names in historiography, such as Bloch, Curtius, Gilson, Halphen, Haskins, Huizinga, Kantorowicz, Knowles, C. S. Lewis, Mommsen, Maitland, Panofsky, Postan, Power, Schram, Strayer, Southern and Tolkien). Cantor hammers Carr's point home: to understand history we must first understand the historians who write it.

Ethnomethodology: Explaining Context

As we have seen, the influence of a social context on a researcher is fascinating, but it is only one side of a necessarily two-sided coin. After all, researchers themselves – indeed all of us – help to construct that social context. The study of how we do so – how people produce the social world (and their understanding of that world) – is called *ethnomethodology*. This is an approach associated with the American sociologist, Harold Garfinkel's *Studies in Ethnomethodology* (1984 [1967]) – but its roots can be traced back to Weber's concept of *verstehen* and Alfred Schütz's notion of phenomenological reconstruction. As ethnomethodology refers to the study of ways in which people make sense of their social world, it exemplifies the constructivist approach. But it is also a somewhat extreme version of that approach, in that ethnomethodologists begin by assuming that social order is illusory.

Ethnomethodologists hold that the order we see in social life is constructed in the minds of social actors. In particular, society confronts us as a series of sense impressions and experiences that we must organize in some way into a coherent pattern. For Garfinkel, this organization process (which he called 'the documentary method') was individual and psychological in nature. When faced with a given context, human beings tend to select certain facts and use these to establish a pattern that is used subsequently to make sense of the remaining facts (in terms of that pattern).

To illustrate how this documentary method works, Garfinkel invited a number of students to take part in an experiment. The students were told they could talk to an 'adviser' about their personal problems, but they could only pose questions requiring a yes/no answer. The students could not see the adviser, and were forced to communicate with him/her through an intercom. What they *weren't* told was that the adviser was not actually listening to the questions being asked, but responded instead with a list of predetermined and random sequences of yes/no answers.

As we might expect, the advisers were not able to give consistent or (apparently) meaningful answers. Despite this, the students managed to make sense of these answers by placing them in a patterned context that allowed them to balance and weigh contradictory evidence. For example, one student asked whether he should drop out of school and was surprised to hear the adviser respond in the affirmative. Confused, he asked again: 'You really think I should drop out of school?' This time, the adviser responded in the negative. Rather than doubt the sincerity of the adviser, or dismiss the advice as nonsensical, the student struggled to find its meaning. Indeed, most students seem to have found the advice given was both reasonable and helpful!

This documentary method is used by ethnomethodologists to show how we use cultural competence and contextual (indexical) knowledge to make sense of commonplace events. This reflexive characteristic is what makes our actions (and interpretations) mutually intelligible. With this knowledge in hand, ethnomethodologists employ research strategies that force subjects to 'break' with commonplace routines in order to reveal the way in which cultural competence is always framing our understanding (for example, by examining how family members react when we pretend to be a stranger, or when we blatantly cheat at a game.) In studying these sorts of examples, ethnomethodologists can demonstrate the creativity with which we (as members of society) are able to interpret and maintain social order. In short, they show us how we construct a social reality to make sense of our often senseless interactions. By using the documentary method we bring order to what is, in fact, a chaotic situation.

Ideas Matter

We have already noted the importance of ideas in the work of earlier philosophers of science: we have discussed Whewell, who stressed how ideas affect our notion of facts; we have referred to Collingwood, who emphasized the role of presuppositions in our observations of the world; we have discussed Boas, whose early projects were initially hampered by differences of perception among individual observers. More recent authors have elaborated on the role of ideas, preconceptions and observational differences, and this section is devoted to a more detailed discussion of some of them.

The modern roots to this ideational approach can be traced back to Émile Durkheim and a school of French historians influenced by him. Durkheim set out to investigate the basic principles that maintained order in society. He introduced the concept of solidarity, and conceived of it as a set of generally-accepted norms, rules and perceptions embraced by all members of society. This way of thinking had a great influence on French social sciences in the twentieth century, most noticeably on the *Annales* School. The next section uses this school as a springboard to elaborate on how an author's ideational context might affect how he frames and comes to see and understand the patterns he studies.

From Mentalities to Discourse

The *Annales* School takes its name from the journal founded by Lucien Febvre and Marc Bloch in 1929, entitled *Annales d'histoire économique et sociale*. The founders were not preoccupied with traditional political history, but with social and economic history, and in particular with the history of norms and rules and human knowledge. They developed an approach that studied the history of knowledge in terms of calm pools of collective knowledge (*longues durées*), with sudden rushes of revolutionary change (*ruptures*).

The *Annales* became the hub of a historical school that conceived of human knowledge in terms of 'mentalities' and 'epistemologies': 'mentalities' constitute wider world views of past social or cultural groups, while 'epistemologies' capture the mindsets or common assumptions that characterize the inhabitants of entire regions in certain epochs. Different epistemologies evolve in different historical eras, cultures and socio-political contexts.

For example, Marc Bloch's (1973 [1924]) *The Royal Touch* was a serious historical investigation into the early kings' ability to cure certain diseases by touching ailing people. Other historians had brushed aside this purported royal ability as superstitious nonsense. But clearly, Bloch argued, people had faith in their monarch and his magical properties. This faith cannot just be discarded – indeed, it is a bad historian who appoints herself as a judge over the people she investigates. According to Bloch, this faith in royal properties was intimately connected to the business of government and it is interesting for at least two reasons. First, such faith in royal powers was part and parcel of royal authority. It was an element of the political mentality of medieval and early modern society that legitimized the authority of the regime. As such, it should be of great interest to any historian who investigates political issues. Second, it is interesting because it alerts us to the difference between our own, late-modern notions of power and the notions held by earlier societies.

The concept of collective mentality is equally evident in Lucien Febvre's (1983 [1942]) study of irreligion in sixteenth-century France, *Le problème de incroyance au XVIe siècle: la Religion de Rabelais.* As the title suggests, this is not merely a study of religious faith and free thought; it is a study on the limits of thought. Febvre argued that it was practically impossible not to believe in God in early modern France. Consequently, it is possible to delineate clearly between what was possible to think and what was impossible to think (and say) in sixteenth-century French society.

Similar studies were subsequently made by other *Annales* School historians – among them Emmanuel Le Roy Ladurie (1966) and Carlo Ginzburg (1980). Each of them studied peasant societies against a common backdrop: the importance of 'mentalities'. Le Roy Ladurie and Ginzburg sought to understand the behaviour and arguments of the villagers; they discussed past mentalities and sought to identify the characteristic properties of past thought – to define the people's major mental preoccupations, the structure of their thought and the limits to what they could think.

This is another way of claiming that truth varies. Bloch, Febvre, Le Roy Ladurie and Ginzburg all demonstrate that social truths vary from one era to the next. This might be understood as the central insight of the French *annalistes*: that truth varies. This is also the guiding insight behind the historical works of the French philosopher, Michel Foucault, whose doctoral thesis examined the ways in which the French medical profession in the past had treated the mad and the insane. One of his guiding arguments is that the definition of madness has varied over time (in other words, what was considered mad in the seventeenth century was not the same thing as what was considered mad in the eighteenth or nineteenth centuries). Another of Foucault's arguments is that such definitions are part and parcel of the collective mentalities of the medical experts of a distinct epoch. A third argument is that such mentalities help form and reform society: they dictate the criteria for what kind of behaviour can be considered an innocent deviation, and what kind requires public intervention or incarceration in a mental institution. Mental (pre)suppositions, in other words, affect people's freedom. They order society. They affect the exercise of political power.

In 1966, Foucault published *Les Mots et les Choses* (literally: 'The Words and the Things', but often translated as *The Order of Things* (Foucault, 1970), thus missing all connotations to the Saussurean point noted in Chapter 8). Here, Foucault broadened his focus: not only did he discuss the disciplinary exercise of bio-political power; he also turned his attention toward more subtle mechanisms of power – to the power of discourse. To do so, he drew on Febvre and the *annaliste* notion of a social mentality – on the notion that there are discursive limits to what can be said (and generally understood) in a given society.

Archaeology

To uncover the complex mechanisms and relationships that create and maintain this notion of social mentality, Foucault developed a method he called 'the archaeology of knowledge'.

Foucault borrowed this term from Immanuel Kant – who had coined it to designate 'the history of that which makes a certain form of thought necessary' (Kant, 1942, p. 341). According to this view, the past can be treated as being akin to an archaeological site; it can be 'excavated' using a special set of analytical tools, layer by layer as it were (see Foucault, 1972). This archaeological method enabled Foucault to 'rope off' sections of the past in order to excavate them.

Through his excavations, Foucault hoped to uncover various layers of collective presuppositions. He conceived of these as historical systems of thought, and called them *epistemes*. The word '*episteme*' is borrowed from Aristotle and used to denote those structures of thought that make argument and reasoning possible. For Aristotle, the term refers to human knowledge. For Foucault, however, an episteme does not refer to knowledge itself so much as to the *preconditions* for knowledge – it refers to those structures of thought that condition our thinking and discipline our thoughts. Epistemes form the preconditions for thought and define the limits of what can be thought or said.

Discourse Analysis

While the archaeology of knowledge could uncover epistemes, Foucault realized that the formation of these collective presuppositions was derived from language, and how the world was (re)presented through language. When a series of such representations appear together in a lasting way, they produce a *discourse*. A discourse is also a system of meaning, in the light of which meaningful claims can be presented (and *re*-presented).

Discourse analysis, then, becomes another important means by which we can uncover these different understandings. Foucault uses this approach in his *The Order of Things* (1970) to capture and analyze the basic framework of scientific knowledge over the past few centuries. He argues – as did Kant, Whewell and many others – that this type of discursive framework affects the scientific process. It affects the questions raised, the ways in which the questions are pursued, and the way that research results are formulated. In addition – and this is one of Foucault's major points – the system of thought also rules out certain questions, methods and concluding formulations. Discourses, in other words, not only determine what can meaningfully be said; they also define the limits of what can be said.

Foucault may have been inspired by Marx, Febvre, Lévi-Strauss and many other previous thinkers. But his argument differs from theirs on a couple of points. First, he does not reduce 'discourse' to 'ideology' as many Marxists do; 'discourse' is not merely a reflection of the material interests of society. Second, Foucault is searching systematically for more precise mechanisms of influence, authority and reproduction than are Febvre and Bloch. The historians of *mentalités* broke new ground by opening up the study of culture for historians. However, they did not create a coherent method of their own (Darnton, 1980). Instead, they borrowed from other traditions – especially from cultural anthropology.

It is because of the limitations of these earlier studies of mentalities, myths and ideologies that Foucault turned to the concept of 'discourse' and his method of 'discourse analysis'. 'Discourse' refers to specific patterns in the use of language – broadly conceived, it refers to regularities in a linguistic system – regularities that can tell us something about the speakers and their contexts. Discourse analysis is a widely-employed approach for examining all kinds of dialogue (written, spoken, or any semiotic event) in an attempt to identify the rules and reasons behind them.

The easiest way to grasp the utility of discourse analysis is to see it in practice. One of our favourite examples of such is James Ferguson's (1994) *The Anti-Politics Machine*. Ferguson wanted to show how 'development institutions such as the World Bank, generate their own form of discourse, and this discourse simultaneously constructs [poor countries such as] Lesotho as a particular kind of object of knowledge, and creates a structure of knowledge around that object' (1994, p. xiv). It is important to note that Ferguson is not concerned with seeing what policies actually generate development. Rather, he studies how ideas and discourses about development have very real social consequences: 'For the question is not "how closely do these ideas approximate truth," but "what effects do these ideas (which may or may not happen to be true) bring about? How are they connected with and implicated in larger social processes?"' (Ferguson, 1994, p. xv).

Ferguson is concerned with the discourse of development agencies working in Lesotho in the mid- to late 1970s, and he recognizes that the development discourse does not deal simply with the 'facts per se, but with a constructed version of the object under study' (ibid., p. 29). He finds the development discourse about Lesotho is transforming the country into something it is not (and which is hardly recognizable): it is becoming a generic Less Developed Country. In reconstructing Lesotho as a country 'with all the right deficiencies' (ibid., p. 70), the development institutions (which are responsible for the discourse) become perfectly positioned to lend their (paid) assistance.

> For an analysis to meet the needs of 'development' institutions ... it must make Lesotho out to be an enormously promising candidate for the only sort of intervention a 'development' agency is capable of launching: the apolitical, technical, 'development' intervention ... [A development discourse that can move] the money ... presents Lesotho as a likely target for the standard 'development' intervention, and serves as a charter to justify and legitimate the sort of programs [*sic*] that the bureaucratic establishment is there to execute. (Ferguson, 1994, pp. 69–70)

> First technical problems such as isolation, lack of markets, lack of credit, unfamiliarity with a cash economy, lack of education, lack of fertilizer, lack of tractors, lack of purebred livestock, lack of farmers' associations and cooperatives, and lack of appropriate energy technology are exaggerated or invented to take the place of things like unemployment, low wages, influx control, political subjugation by South Africa, and entrenched bureaucratic elites; then an institutional apparatus is unleashed to combat these largely illusory technical problems. (Ibid., pp. 87–8)

The point of *The Anti-Politics Machine* is to show how the 'development discourse' itself creates a picture of Lesotho that is not necessarily accurate, but conforms to the needs of those directing the discourse. Ferguson does not see this influence in some sort of direct, intentional terms – there are many unintended outcomes that result when planned interventions are implemented. In his book, Ferguson flaunts his constructivist credentials by referring to his method in terms of vivisection (p. xv), and with reference to Foucault's conceptual apparatus and the need to approach the subject like a good physiologist (p. xvi), or in terms of a genealogy (with an implicit nod to Nietzsche) (p. xvi). At the same time, he provides a refreshingly honest appraisal of the shortcomings of a constructivist approach – including an epilogue where he notes how this sort of approach can lead to scepticism and political passivity.

Patterns of Communication

In Foucault's notion of representation we saw the importance of language in framing our understanding of social reality. This section examines several different ways in which the patterns that interest us are linked to authorship: in communicating with our audience, we impose structures and patterns on our arguments – often unknowingly. Our stories are shaped by the way we choose to *present* them.

Once we acknowledge that the social analyst is telling a story (and not just reporting facts from the research frontier), the next step must be to investigate the narrative form taken by these stories. Indeed, to the extent that constructivists are willing to focus on the story at the expense of explicit methodological criteria, then the role of narrative structure in framing the story becomes all the more important. This section will show how even the most inductivist and dispassionate social analyst often organizes narratives in standard forms that are similar to those used by writers of fiction.

Barbara Tuchman's explanation of the historian's writing process (as described in Chapter 6) is a very good one. When Tuchman examines her sources, the key information in each source is, ideally, distilled into a few sentences and written on 4 × 6 inch index cards. When the research is completed, the cards can be arranged chronologically and, as Tuchman (1981, p. 20) explains, 'as the cards fall into groups according to subject or person or chronological sequence, the pattern of my story will emerge'.

Tuchman's explanation may sound like magic. However, as most historians know, this is a method that works. It is a good method; perhaps the best. It is tested, true and drawn from the experience of a professional historian with a string of successful books to her name. Yet even this technique ignores the key element in writing history; it is silent about the very act of historiographical creation. Because, if the scholar simply arranges the notes on her sources in chronological order and writes them up, she has written nothing more than a catalogue of events!

Events, on their own, do not produce a story. The sources are absolutely central elements in the story, but the scholar needs to assemble them in particular ways to produce a legible and convincing story. Hayden White (1987, p. 92) formalizes the process in the following way. He begins by assuming that the social scientist has collected a box full of event notes, and that he has arranged them in chronological order. In the depiction below, each letter represents a social fact; and chronology is depicted by alphabetical order:

$$a, b, c, d, e, \ldots, n$$

For these ordered facts to result in a story, they have to be combined in a narrative. In providing this narrative, the social analyst takes many (mostly hidden) steps. First, the analyst has to add descriptive elements that tie together various facts into a coherent whole. In addition, he needs to add an active or binding component that provides meaning. Finally, the analyst needs to interpret the different sources – emphasizing or de-emphasizing particular facts according to their role or function in the larger story.

Of course, a list of facts can be assembled in an almost infinite number of ways. Four of these potential assemblies are suggested below, where we have maintained chronological order for the sake of simplicity. In this matrix of facts, the capital (large) letters represent events or facts that are in some way privileged over the others. For example, in the second assembly of facts, event *b* is emphasized by the analyst as a determining event *B*, and given a key role in the analyst's story:

$$A, b, c, d, e, \ldots, n \qquad (1)$$
$$a, B, c, d, e, \ldots, n \qquad (2)$$
$$a, b, C, d, e, \ldots, n \qquad (3)$$
$$a, b, c, D, e, \ldots, n \qquad (4)$$
$$etc.$$

White's argument is not particularly novel. It is possible to find traces of it in the work of a number of different historians, ranging from the Hegelian-inspired R. G. Collingwood to the proto-Rankean Barbara Tuchman. But White's discussion is richer than the others in that he is searching to explain how the historian (or social analyst) moves from data to argument; how he transforms a collection of facts into a plausible story; how he structures his facts through the operation of 'emplotment' (White, 1973, p. 7).

Imprisoned by Plots

White argues that to create a story out of a collection of facts, the social analyst needs to use literary techniques. Drawing on Northrop Frye (1957), White distinguishes four main types of emplotment:

- *Romance* celebrates the triumph of the good after a series of trials and tribulations. Romantic stories are filled with progress and happy endings. The evolution (or progress) is propelled by deep, conflicting forces that ultimately produce a state of harmony or bliss.
- *Tragedies* are stories of potential progress that fails; they stress the irreconcilable element of human affairs, and lament the loss of good that is inevitable when values collide. The tragic struggle is seen as heroic (as it is in romantic stories), but it is a struggle that ends in failure (and this failure is usually rooted in some notion of human flaw).
- *Satire* is a reaction to romance, but a critical and mordant reaction: a reaction with a normative agenda of its own. In satires, human affairs are not depicted in terms of success or failure; satirical stories see only meaningless change in human life. Indeed, a satire doesn't just seek to present an alternative story, it wants to show that the romantic story is naïve and simple-minded.
- Finally, a *comedy* celebrates the conservation of human values against the threat of disruption. Like the other plot structures, comedies are also a reaction to romance (but comedy breaks less with it than do the others). In short, the basic structure of a comedy is ultimately a story of progress toward a happy ending, but where the progress is neither clear nor linear.

To give the reader a taste of how these plot structures can be read into different types of social analyses, we turn to four influential attempts to summarize the twentieth century as it neared its close: Eric Hobsbawm's immensely popular *The Age of Extremes* (1994); Mark Mazower's *Dark Continent* (1998); François Furet's *Passing of an Illusion* (1999 [1995]); and Bruce Russett and John Oneal's *Triangulating Peace* (2001). These historians saw the transition to a new millennium as a convenient occasion to summarize our age, systematize its key themes, and identify its most conspicuous patterns (see Knutsen (2002) for an elaboration).

The books by Hobsbawm, Furet and Mazower share many features. They agree that the twentieth century was the bloodiest in history: more people were known to have been killed in conflict and war than in any other century. They also tend to agree about what constitute the key events of the century. With respect to the first half of the century, they all emphasize the First World War, the postwar recession, the revolution in Russia, the Nazi takeover in Germany and the impact of the Second World War. In the second half of the century, all of them emphasize the nuclear rivalry between East and West, and the dangerous decades of the Cold War.

Despite these important similarities, each of these authors tells a radically different story about the twentieth century. Each book can be discussed in terms of the narrative tissue that connects the (largely known and largely common) historical facts – with concepts drawn from Frye (1957) and along lines developed by White (1973). For example, Hobsbawm and Furet have both written stories in which Marxism and Marxist movements play leading roles. However, whereas Hobsbawm has cast Marxism in the role of the hero, Furet has given it the role of the villain. Both authors have concluded that Marxism dies in the last act. But for Hobsbawm this death is a tragic event; it means that the forces of light and promise have lost out to the forces of darkness. For Furet, in contrast, the death of Marxism means the ultimate retreat of the story's seductive scoundrel, and the restoration of a natural order. In terms of literary form, Hobsbawm has written a tragedy, while Furet has written a comedy.

Mazower, in contrast, has written a satire. *The Dark Continent* paints a deeply disturbing portrait of the twentieth century. While he recognizes that the twentieth century is marked by democratic progress, he warns that democracy is far more fragile than most people assume. Instead of rejoicing in democracy's victory after the Cold War, Mazower questions the very notion of victory. Instead of seeing communism and fascism as horrible aberrations from Europe's past, he sees them (along with democracy) as natural products of the twentieth century.

When searching for plot structures in these summaries of the twentieth century, we are struck by the distinct absence of a romantic story line. To find a romantic depiction of the twentieth century we must leave the historians' den and search among social scientists. Indeed, a good number of the romantic histories of the recent past have been written by social scientists: by economists who challenge Hobsbawm's tragic tale with upbeat stories about the steady evolution of wealth and liberty; and by political scientists and sociologists who see a global development of democracy and political stability. One case in point is the Hegelian-inspired argument that history has come to an end (Fukuyama, 1992). Another is the Whiggish claim that democracies do not to go to war against each other (Russett, 1993).

It is in this, romantic, way that our fourth book, by Russett and Oneal, can be read. The main claim of *Triangulating Peace* is that the twentieth century has been marked by the hopeful evolution of a zone of peace among the world's democracies. This has been orthodoxy among peace researchers for many years, but Russett and Oneal provide new and more convincing explanations for the phenomenon. In particular, they invoke Immanuel Kant's 'Perpetual Peace' (1991 [1795]) essay and see Kant as a visionary advocate for a 'triangular peace' – that is, for peaceful relations among states, conducted within the civilizing frameworks of three interacting sets of institutions: republican constitutions, 'cosmopolitan law' (which guarantees free commercial transactions), and multilateral treaties and international organizations.

These four accounts of the twentieth century tell the same (basic) story, but in different ways. They share many common concerns, and draw on many of the same social data and events. In other words, they look at the real world, and see different things. This is the essence of the constructivist approach. Each interpretation encourages us to see that world through the eyes of the respective author. Indeed, it is significant and useful to recognize and stress the differences that separate these interpretations – there is no need or desire to prioritize one over the other. What utility can there be in claiming, for example, that Hobsbawm's history is better than Furet's? Rather, constructivists embrace the remarkable differences that separate these authors' individual interpretations.

Patterns in Time

This last framing device considers the way that patterns can be ordered by history, making it difficult to explain those patterns in terms of general laws. Process tracing, path dependency and within-case studies are the catchwords of this particular framing device – these include some of the fastest growing and most interesting developments in contemporary social science methods. As we shall see, many of these methods bridge the gap that once separated constructivists from naturalists, as both methodologies embrace these methods, but often with very different objectives.

Process Tracing

At the end of Chapter 6, we noted how many naturalists had discovered the promise of process tracing. Indeed, as originally formulated by

Alexander George (1979, p. 46), process tracing lies comfortably in the naturalist tradition: it can be understood as a useful strategy for assessing the causal proclamations that are generated in larger correlational studies.

Similarly, Andrew Bennett (2008, p. 704) notes that 'process tracing seeks a historical explanation of an individual case, and this explanation may or may not provide a theoretical explanation relevant to the wider phenomenon of which the case is an instance'. In doing so, he suggests that process tracing can cut both ways. As with our critical examination of Przeworski and Teune's (1970) imagined voter, researchers may find that the best explanation for M. Rouget's voting behaviour lies in the particulars of his life history. Just as plausibly, process tracing might be used to confirm the sort of inductively formed hypothesis that Przeworski and Teune were hoping to encourage.

To the extent that process tracing is meant to uncover hidden causal patterns, or to document the nature of the causal links posited in larger general studies, then the approach lends itself to naturalists. But when process tracing is used in a more inductive way – for example, to understand the uniqueness of particular events from different perspectives or as a part of different discourses, say – then the method also appeals to constructivists. In this way, process tracing can be used to shift the investigator's focus from *what* happened, to *how* and *why* it happened: 'it provides a way to learn and to evaluate empirically the preference and perceptions of actors, their purposes, their goals, their values and their specification of the situations that face them' (Vennesson, 2008, p. 233). In doing so, constructivists can demonstrate how conceptions of truth are related to positions of power.

Perhaps the best recent example of this kind of argument can be found in Bent Flyvbjerg's (1998) celebrated *Rationality and Power*. Through a detailed case study of the Aalborg Project in Denmark (a project that integrates environmental and social concerns into Aalborg city politics and planning), Flyvbjerg shows his readers how rationality is not something that is fixed – neither a standard by which to evaluate policies, nor an outcome derived from open discourse. Instead, he shows rationality to be a function of power and that it is context dependent. In doing this, Flyvbjerg (1998, p. 228) recognizes how his work can give credence to that of ethnomethodologists, such as Harold Garfinkel, who argue that the rationality of a given activity is produced 'in action' by participants via that activity.

In a myriad of detailed descriptions, Flyvbjerg outlines the complex ways in which knowledge, power, truth and rationality intertwine and become inseparable.

> the Aalborg study shows that the relationship between knowledge and power is commutative: not only is knowledge power, but, more important, power is knowledge. Power determines what counts as knowledge, what kind of interpretation attains authority as the dominant authority. Power procures the knowledge which supports its purposes, while it ignores or suppresses that knowledge which does not serve it. (Flyvbjerg 1998, p. 226)

The Problem of Generations

When we employ process tracing, we push history on to centre stage. In doing so, we shine a light on another popular, and related, approach to researching the patterns inherent to history: the notion of *path dependency*. With path dependency, social patterns flow into unique historical channels, from which it is more difficult to generalize.

To understand the concept of path dependency, consider the now classic example of how our computer keyboard has been locked into an inefficient layout. In a wonderfully readable story, Paul David (1985) tells us how we came to key in this manuscript on an obsolescent keyboard. In telling his story, he employs Cicero to remind us that the 'logic' of the social world is sometimes serendipitous:

> Cicero demands of historians, first, that we tell true stories. I intend fully to perform my duty on this occasion, by giving you a homely piece of narrative economic history in which 'one damn thing follows another.' The main point of the story will become plain enough: it is sometimes not possible to uncover the logic (or illogic) of the world around us except by understanding how it got that way. (David, 1985, p. 332)

David's story is one of path-dependency. He shows us how the QWERTY keyboard layout (named after the first six letters on the topmost row) was originally designed to impede quick typing, in order to reduce the frequency of the type-bars jamming in early typewriter models. The technology of the time could not keep pace with a rapid typist, so the keyboard was designed to slow him up (that, and to include all the letters in the word TYPEWRITER on the top row, to help the sales staff impress their customers!).

While type is no longer set by long metal arms that swing up, arc-like, to strike the paper and carriage in front of us, we remain beholden to a keyboard layout that everybody recognizes to be less efficient than newer alternatives (for example, the Dvorak Simplified Keyboard, or DSK). To illustrate this rather uncomfortable fact, David reminds us that the Apple

IIC computer had a built-in switch that converted the keyboard from QWERTY to a virtual DSK! How can this be?

> The agents engaged in production and purchase decisions in today's keyboard market are not the prisoners of custom, conspiracy, or state control. But while they are, as we now say, perfectly 'free to choose,' their behavior, nevertheless, is held fast in the grip of events long forgotten and shaped by circumstances in which neither they nor their interests figured. Like the great men of whom Tolstoy wrote in *War and Peace*, '(e)very action of theirs, that seems to them an act of their own free will, is in an historical sense not free at all, but in bondage to the whole course of previous history' (Bk. IX, ch.1). (David, 1985, p. 333)

The notion of path dependency, or historical bondage, can be found in a number of social and political accounts that employ process tracing, where politics – as Kathleen Thelen (1999, p. 385) reminds us – 'involves some elements of chance (agency, choice), but once a path is taken, then it can become "locked in", as all the relevant actors adjust their strategies to accommodate the prevailing pattern.'

If we set aside the new terminology, we might recognize this as familiar terrain. Being 'locked into' a particular context is akin to what the *Annales* School understood as the *mentalités collectives*, or to what Karl Mannheim referred to as the problem of generations:

> It is of considerable importance for the formation of consciousness which experiences happen to make those all-important 'first impressions' ... Early impressions tend to coalesce into a natural view of the world. All later experiences then tend to receive their meaning from this original set, whether they appear as that set's verification and fulfilment or as its negation and antithesis. (Mannheim, 1952, p. 298)

Mannheim anchors his generations in socio-historical contexts, as part of the larger sociological theory of knowledge, with which he is commonly associated (see Chapter 8). In the late 1970s, Lynne Zucker employed an ethnomethodological approach to map the particular patterns of politics that arrive from this sort of social path dependency. We examine the particulars of this argument in Chapter 12, as Zucker employs an experimental approach, but the more general point can be summarized with reference to Berger (1968): 'Each actor fundamentally perceives and describes social reality by enacting it, and in this way transmitting it to the other actors in the social system' (Zucker, 1977, p. 728).

Here path dependency (or cultural persistence) is explained by the shared understanding (and its transfer across generations) of social reality. Accordingly, variance across cultures can be explained by different experiences and understandings. For constructivists, the focus of study should be on explaining the nature of the variation (across culture, over time) rather than documenting a fixed pattern in the world of study.

This sort of project is more common among students of International Relations, where '[i]dentities and interests are not only learned in interaction, but *sustained* by it' (Wendt, 1999, p. 331, emphasis in original). For Wendt, 'social systems can get "locked into" certain patterns by the logic of shared knowledge, adding a source of social inertia or glue that would not exist in a system without culture' (ibid., p. 188).

Similar, Peter Katzenstein's analysis of the evolution of Japanese security policy reveals how the definition of appropriate conduct, as well as the shape of actor identities and interests, was influenced by collectively-held norms. These norms 'inform how political actors define what they want to accomplish' (Katzenstein, 1996a, p. ix). For Katzenstein (1996b, p. 2), 'State interests do not exist to be "discovered" by self-interested, rational actors. Interests are constructed through a process of social interaction.' *The Culture of National Security* seeks to define diverse security interests, and it does so in terms of 'actors who respond to cultural factors' (ibid.). In short, by looking closely at specific cases (such as Japan), Katzenstein shows us how the concept of national identity can be understood as a society's collective interpretation of itself – as a community. It is not some fixed component of the country; but it is constructed by a complex, historical, process. As a consequence, scholarship does not aim to define the one best (for example, rational) standard by which all performance is measured. Rather, it should 'make intelligible the political logic inherent in different kinds of substantive rationalities' (Katzenstein, 1996c, p. 511).

As with process tracing, path dependency arguments lend themselves to both naturalist and constructivist approaches, depending on how they are defined and employed. But even when they are employed in naturalist frameworks, path dependency approaches depend on maintaining and leveraging the unique context under study.

Conclusion

In this chapter we have aimed to show how historical approaches are employed by scholars who subscribe to a constructivist methodology.

By realizing that the patterns they aim to explain are creations of their own making – rather than some permanent artifact of the social world – constructivists refuse to recognize a single form for history. They employ historical approaches to secure insight and understanding – leveraging the human motives that often underlie social patterns. This provides for a remarkable diversity of approaches, some of which appear to be serendipitous or casual to the untrained eye. In the hands of a well-trained constructivist, however, this serendipitous approach can generate significant understanding and insight.

That is not to say that everyone who uses this approach does so equally well. As in the naturalist tradition, there are those on the margins of constructivist scholarship who employ these techniques poorly. We have aimed to limit our focus to some of the best examples, as we recognize that each methodological tradition includes examples that can undermine the legitimacy of their respective approaches. This is, perhaps, most evident in the constructivist camp, as much ink has been spilled over the threat to social science scholarship represented by 'postmodern' approaches. This chapter aims to show how many of the approaches associated with postmodernism can make (and have made) important contributions to the social scientific project.

Indeed, we think it is too easy (and too common) to emphasize the differences that separate the historical method (as outlined in Chapter 6), and the approaches described here. Both methodological traditions share a healthy scepticism toward naïve inductivism, and both embrace the importance of mastering accurate empirical detail. In historical scholarship of either methodological persuasion, there is no substitute for an analyst's familiarity with a data set or set of sources. Historians still spend their lives immersed in archives; and constructivists are no exception. For example, few researchers have dug as painstakingly and systematically into national archives as Michel Foucault.

Where the approaches differ is in ontological and epistemological terms: about the source of the patterns they seek to describe, and the epistemological approaches that can uncover and understand these patterns. Constructivists accept multiple stories; indeed, they hold that multiple stories are better (and more honest) than those that hold firmly to a master narrative. For the constructivist it is important to celebrate this difference in perspective: granting a given perspective a privileged position is more of an exercise in power than a question of truth. As a result, the focus of this chapter has been on describing the different ways that analysts come to see and understand the patterns that they study. This – more than inserting historical studies into larger social scientific projects – is what constructivism is all about.

Recommended Further Reading

E. H. Carr's (1987 [1961]) *What Is History?* and Edward W. Said's (1978) *Orientalism* provide two masterful (and very different!) examples of the constructivist approach. To learn more about the role of emplotment, read Hayden White's *Tropics of Discourse* (1978) and his *The Content of the Form* (1987). Likewise, Michel Foucault's *The Order of Things* (1970) and his *The Archaeology of Knowledge and the Discourse on Language* (1972) provide much of the philosophical weight to an archaeological approach to social science. Finally, Phillip Hammond's *Sociologists at Work* (1964), which, as the title indicates, provides interesting examples of sociologists at work.

Chapter 10

Comparing Contexts

In Chapter 9 we saw how constructivists are committed to understanding the uniqueness of social phenomena. To do this, they favour narrative techniques that provide insight into social contexts or reveal the contextual settings of the social scientist observer. With the realization that the patterns they study are largely of our own making, constructivists prefer to climb into a particular problem and examine it from the inside out. Like Miss Marple, they exploit their familiarity with contexts to arrive at understandings that are consistent with those of the subjects they are studying.

The constructivist's preference for particular stories and narratives does not imply that she avoids comparisons; indeed, comparison plays a central role in the constructivist project. However, constructivists and naturalists use comparisons in different ways, and these differences stem from their disparate ontological and epistemological positions. When constructivists employ comparisons, their primary concern is not so much variable correlations as how to preserve and exploit the qualities associated with thickly descriptive narratives.

Like the storytelling approaches described in Chapter 9, many constructivists use comparisons to depreciate the naturalist project. For this reason, we begin with a critical discussion of comparisons in social science, based largely on the work of Alasdair MacIntyre. MacIntyre doubts the entire project of a comparative political science – in other words, he questions whether it is possible to have a political *science* that formulates 'cross-cultural, law-like causal generalizations which may in turn be explained by theories' (MacIntyre, 1972, p. 9).

In the second section we examine some of the fundamental background issues associated with the constructivist approach. In particular, we introduce the different ways in which constructivists address their data, choose their cases and relate to generalization. This section helps us to distinguish between naturalist and constructivist approaches, but it doesn't give us a very clear idea of the promise of constructivist comparisons (in their own right). To show the potential of comparing contexts, the closing part of this chapter introduces four different types of constructivist comparisons, where comparisons are used to question existing generalizations, find hidden opportunities, develop new types of associations and interrogate our biases.

We conclude by arguing that comparisons are almost as central to the constructivist project as they are to the naturalist project. Once we are aware of how comparisons are used in different ways – to question the mechanistic way that naturalists interpret the world, to protect and draw out contextual features, to celebrate diversity and uniqueness, and so on – then it is relatively easy to extend this reasoning to the other comparison-based methods (such as statistics and experiments). Consequently, this chapter functions as an antechamber to Chapters 11 and 12 by introducing the important (but different) role that comparisons play in constructivist scholarship.

Apples and Oranges

Constructivists do not use comparisons to uncover law-like generalities in the social world. Particularity and context are the banners under which constructivists gather: they march toward meaning rather than laws, and they search for meaning by examining individual cases closely (and the contexts within which that meaning is situated).

While most social scientists believe in the utility of comparisons, there are sceptics in both the naturalist and constructivist camps. Some naturalists are acutely aware of the ontological problems associated with a comparison-based social science. J. S. Mill, as noted earlier, was particularly sceptical of attempts to assume enough likeness in the social world to exploit his comparative methods. But most social scientists conveniently ignore Mill's caveats, and proceed with social scientific comparisons. Most, but not all, throw caution to the wind.

Arguably the most provocative argument against the use of comparisons in social science is Alasdair MacIntyre's (1972) influential piece, 'Is a Science of Comparative Politics Possible?' MacIntyre takes Mill's criticism very seriously, and shows us how many of the apparent similarities in comparative social science are superficial and misleading. To illustrate this point we can visit two of his more entertaining examples.

MacIntyre's first example builds on a critique of Almond and Verba's (1965 [1963]) influential book *The Civic Culture*. As we saw in Chapter 5, *The Civic Culture* compares concepts of 'pride', to argue that some cultures identify less with their government than do others. MacIntyre doubts that the notion of pride means the same thing in different countries. He then shows how Almond and Verba simply assume that the notion of pride is constant, using it to gauge levels of identity across cultures. In contrast, MacIntyre argues that pride has different meanings – and plays different roles – in various cultures. 'Pride' in England is not the same as 'pride' in Italy:

> The notion of taking pride in Italian culture is still inexorably linked ... to the notion of honour. What one takes pride in is what touches on one's honour. If asked to list the subjects which touched their honour, many Italians would spontaneously place the chastity of their immediate female relatives high on the list – a connection that it would occur to very few Englishmen to make. (MacIntyre, 1972, pp. 10–11)

If pride means different things in different cultures, it becomes difficult to use it as a standard for cross-national comparisons.

It is an inherent temptation in comparative scholarship to assume that things called by the same names are intrinsically similar. The dangers of this assumption may seem fairly straightforward when discussing something as amorphous as human 'pride', but are these dangers any less real when we compare more concrete institutions that share the same name? For example, is it meaningful to compare political parties across countries or cultures, or over time? Is the Swedish Social Democratic Party (SAP) the same thing as the Social Democratic Party in the Philippines (PDSP)? For that matter, is the Swedish Social Democratic Party in 2011 really comparable to the SAP of 1935?

MacIntyre doubts the utility of such comparisons and points to the example of Ruth Schachter's description of political parties in some African nations. African party members 'were interested in everything from the cradle to the grave – in birth, initiation, religion, marriage, divorce, dancing, song, plays, feuds, debts, land, migration, death, public order – and not only electoral success' (MacIntyre, 1972, p. 14). He then wonders why Western political scientists think of these social formations as political parties rather than, say, churches. Their likeness to European or American political parties is clearly questionable. Comparing North American and African political parties is hardly as straightforward as comparing the boiling point of water on each continent: 'Where the environment and where culture is radically different the phenomenon is viewed so differently by those who participate in it that it is an entirely different phenomenon' (MacIntyre, 1972, p. 14).

At one level, MacIntyre is simply repeating the obvious (and, for that matter, Mill). As social analysts we have to be very careful in describing the relative similarity (or not) of the phenomena we wish to compare across cultures and over time. Here, clearly, definitions matter (Sartori, 1970), but it is quite possible that, in criticizing all attempts at scientific comparison, MacIntyre throws his baby out with the bathwater.

For us, it is important to emphasize that comparisons *are* possible; they are instructive and important, even in the absence of similarities. Indeed, similarities should not be the sole focus of comparisons:

> let us beware of a misunderstanding from which the comparative method has only too frequently suffered. Too often people have believed or affected to believe that its only aim is to search for similarities ... On the contrary, the comparative method, rightly conceived, should involve specially lively interest in the perception of the differences, whether original or resulting for the different developments from the same starting point. (Bloch, 1967, p. 58)

MacIntyre himself provides the proof for this pudding. In the act of criticizing the use of rigid comparisons across different contexts, MacIntyre relies on comparisons (albeit implicitly). As reasonable as MacIntyre's argument might be, we simply cannot know that English and Italian conceptions of pride are different without actually comparing them. In criticizing the way that others use comparisons, MacIntyre actually provides us with a useful glimpse into the way that comparisons are used by constructivists: often implicitly, and with little explicit methodological reflection.

Perhaps it is easier to think about this other type of comparison if we return to Wittgenstein's notions of family resemblances. In trying to find resemblances in a family photograph, we need to look closely at patterns that might not reveal themselves in every individual – we jump back and forth between the individual and the group to try to find deeper, underlying, similarities. The resulting process of comparing and contrasting is difficult to formalize or explicate, but all of us have some experience of it. More important, this type of comparison does not lend itself to the sort of tests/controls that naturalists employ (for example, under conditions such as these, falsification does not provide a very satisfying standard of proof). For these reasons, constructivists tend to have a rather relaxed or commonsense attitude toward comparisons.

Identifying Constructivist Comparisons

This chapter examines the way in which comparisons are used in constructivist efforts, with the aim being to understand (rather than to generalize). This is no easy task, as constructivists often use their comparisons implicitly (as the MacIntyre examples suggest). Our job is to flush these comparisons out, and we do this in two stages. First, we introduce three fundamental concerns that constructivists often use to distance themselves from the naturalist project. As we mentioned in Chapter 8, a common discomfort with the methodological rigours of naturalism is one of the things that unite constructivists in a common methodological tradition. In the section that follows, we then examine the ways in which constructivists actively employ comparisons to better understand their subject of study.

Because many constructivists steer away from explicit references to method and methodological issues, it is often necessary to look for signs of deviation from the hegemonic (naturalist) methodological approach. To identify the methodological perspective of a given author, we have found it useful to look at three fundamental points of departure: (i) the author's commitment to generalization; (ii) the author's approach to case selection; and (iii) the nature of the data employed by the author. Each of these points can be used to help position a given comparativist methodologically.

On Laws and Patterns

In David Lodge's whimsical novel *Changing Places*, we learn that Persse McGarrigle intends to write his Ph.D. thesis on T. S. Eliot's influence on Shakespeare. One reason for choosing this topic is that it serves as an excellent conversation starter and pick-up line in academic pubs, for whenever Persse tells a stranger the topic of his dissertation, they invariably seek to correct him:

> 'You mean to say that you are studying Shakespeare's influence on T. S. Eliot.'
>
> 'But my thesis isn't about that,' said Persse. 'It's about the influence of T. S. Eliot on Shakespeare.'
>
> 'That sounds rather Irish, if I may say so,' said Dempsey, with a loud guffaw. His little eyes looked anxiously around for support.
>
> 'Well, what I try to show,' says Persse, 'is that we can't avoid reading Shakespeare through the lens of T. S. Eliot's poetry. I mean, who can read *Hamlet* today without thinking of "Prufrock"? Who can hear the speeches of Ferdinand in *The Tempest* without being reminded of "The Fire Sermon" section of *The Waste Land*?' (Lodge, 1993, p. 280)

Lodge's example illustrates one irreverent way that constructivists employ comparisons. In a similar fashion, we could note Kenneth Waltz's influence on Jean-Jacques Rousseau – not on Rousseau himself, of course, but on the way we now read Rousseau's analysis of war and peace. Many of today's students of International Relations see Rousseau as a realist, in the light of Waltz's (1959) reading. But Rousseau might just as easily be depicted as an early, and extremely influential, radical (for example, see Knutsen, 1997). In a similar fashion, we think that Theda Skocpol has had an enormous influence on Barrington Moore. This claim addresses the methods of comparison directly, and requires a closer examination.

Barrington Moore's (1966) *Social Origins of Dictatorship and Democracy* is perhaps the most influential comparative piece of historical sociology of the twentieth century. The work is commonly associated with the naturalist approach, and is often used as a model for social scientific comparison. This rubric, however, has been placed on him by others; it is not of his own doing. In particular, his students and disciples are the ones responsible for squeezing Moore into the naturalist mould. Thus it is Skocpol and Somers (1994, pp. 79–80) who refer to his application of Mill's Methods of Agreement and Difference.

Indeed, a methodologically innocent reading of his text reveals a remarkably casual and implicit methodology: Moore is extremely careful about how he frames his question, how he approaches his data, and the role he allots to human understanding and agency. It is tempting to conclude, after a second or third reading of this influential book, that Moore effectively straddles our two methodological approaches. He seems to want to have his methodological cake and eat it too. This, and his strong (explicit) moral commitments, may offer a far better explanation (rather than his Millian brilliance and his methodological orthodoxy) for Moore's enduring importance and influence.

Like many constructivists, Moore was reluctant to formalize his approach. In particular, he did not explicate his theoretical and comparative framework: there is no concrete methodological depiction of his theory, his choice of cases, or the nature of his data. Instead of an explicit research design we find a constant emphasis on the importance of the particular at the expense of the general, and an implicit recognition of agency in social history. This relaxed attitude to methodological conformism is already evident in the opening paragraph of the book. Moore advertises *Social Origins* as 'an attempt to discover the *range of historical conditions* under which peasants and landed lords have become important forces behind the emergence of the modern Western world – both the parliamentary versions of democracy as well as dictatorships of the right and left, that is, fascists and communist regimes' (Moore, 1966, p. xi, our emphasis).

The history of the twentieth century is often cast as a triangular contest between three modern regime types: liberal democracy, fascism and communism. Barrington Moore's ambition is to explore the advent of modernity and the historical preconditions for its three major regime types. His method hardly conforms to the principles laid out by John Stuart Mill, though; it is more akin to that of William Whewell. On closer inspection, *Social Origins* is not an attempt to use comparisons to capture underlying (and fixed) patterns of social reality. Rather, Moore points to a range of concrete historical circumstances that can be understood as so many preconditions for understanding the advent of modernity itself (in its three variations). Moore's discussion stresses variation and range of possibilities; it lacks the claim to sufficiency (and predictability) that is the hallmark of the naturalist approach.

Indeed, the closest Moore comes to a law-like generalization (and it is the one most often used to summarize his work) is the slogan-like claim: 'No bourgeois, no democracy' (1966, p. 418). But on closer examination, this too is used to examine the range of historical conditions. Moore is clearly not forwarding a law of social action. Worse, his reference to the bourgeoisie is almost always taken out of context; his point is to emphasize the role of other agents in democracy (in particular, the agrarian sector). The bourgeoisie is seen to be the principal actor, but not the *only* actor. The rest of the paragraph reads as follows:

> No bourgeois, no democracy. The principal actor would not appear on the stage if we confined our attention strictly to the agrarian sector. Still the actors in the countryside have played a sufficiently important part to deserve careful inquiry. And if one wishes to write history with heroes and villains, a position the present writer repudiates, the totalitarian villain sometimes has lived in the country, and the democratic hero of the towns has had important allies there. (Moore, 1966, p. 418)

So why is Moore so often presented as a strong candidate for sainthood in the naturalist church? We think it is largely because influential reviewers of his work have represented him as such. First among these is Theda Skocpol, who re-presented Moore's argument in a naturalist light: she demonstrated the complex interconnections of Moore's variables and exposed the logical design of his comparative argument – all in the light of Mill's naturalist design. This demonstration has had a significant influence on subsequent interpretations, even if Skocpol herself was careful to note that '*Social Origins of Dictatorship and Democracy* is not organized or written in the style of a scientist trying to elaborate clearly and minutely justify a falsifiable *theory* of comparative modernization. It is, rather, like a giant mural painted in words' (Skocpol, 1994, p. 26, emphasis in original). Despite Skocpol's care and caveats, subsequent scholarship has used her interpretation to squeeze Moore into the naturalist box.

Once we are aware of Skocpol's influence on Moore, however, we can free ourselves from its naturalist representations and find in Moore a number of references to the particular, at the expense of the grand. Indeed, in his very readable introduction, Moore warns that 'too strong a devotion to theory always carries the danger that one may overemphasize facts that fit a theory beyond their importance in the history of individual countries'. He elaborates:

> In the effort to understand the history of a specific country a comparative perspective can lead to asking very useful and sometimes new questions. There are further advantages. Comparison can serve as a rough negative check on accepted historical explanations. And a comparative approach may lead to new historical generalizations. In practice, these features constitute a single intellectual process and make such a study more than a disparate collection of interesting cases ... *That comparative analysis is no substitute for detailed investigation of specific cases is obvious.* (Moore, 1966, pp. xiii–xiv, our emphasis)

In short, Moore suggests that comparisons can be used to ask new questions, to check/test existing hypotheses, and to produce new historical generalizations. He is not saying that his comparisons should be used to construct firm, law-like generalizations about human behaviour. Rather, he compares in order to problematize the nature of theory in social science; comparisons are used to rejoice in the particular. Moore wants to discover the *range* of historical conditions, not to elaborate on the causal variables that lead to specific outcomes. His argument, as always, is nuanced:

> The thesis that neutrality is impossible is a powerful one, convincing at any rate to me. But I do not think that it leads to a denial that objective social and historical analysis is possible. Different perspectives on the same set of events should lead to complementary and congruent interpretation, not to contradictory ones ... Gradations of Truth with a capital T, rightly in my estimation, arouse angry suspicion. But this does not mean that objectivity and truth with a small t lead to comfortable complacency. (Moore, 1966, p. 522)

In the work of Michel Foucault we find an even more explicit dedication to understanding the diversity of human action and the importance of the particular. For Foucault, Paul Rabinow writes,

> there is no external position of certainty, no universal understanding that is beyond history and society. His strategy is to proceed as far as

> possible in his analyses without recourse to universals. His main tactic is to historicize such supposedly universal categories as human nature each time he encounters them. Foucault's aim is to understand the plurality of roles that reason, for example, has taken as a social practice in our civilization not to use it as a yardstick against which these practices can be measured. This position does not entail any preconceived reduction of knowledge to social conditions. Rather, there is a consistent imperative, played out with varying emphases, which runs through Foucault's historical studies: to discover the relations of specific disciplines and particular social practices. (Rabinow, 1984, pp. 4–5)

This attempt to discover the relations of specific disciplines and particular social practices is expressed most famously in Foucault's *The Order of Things* (1970), introduced in Chapter 9. Since this book has had such an enormous influence – and since it is a comparative study – it merits a second glance.

The book's first chapter, entitled 'Las Meninas', is unusual for a comparative social science study: it offers a long and difficult analysis of Velázquez' famous painting of the same name from 1656, a picture depicting himself at work. In the picture, we don't see what he is painting; we see only the back of his vast canvas. The canvas dominates the left edge of the picture and partly obscures the artist himself; he is leaning out to see his subject, brush and palette in hand. But what is his subject? We don't know and we can only guess. However, a small mirror hangs on the wall behind the painter (and to his left); it reflects two faces – these could be the painter's models, in which case the painter is painting a double portrait of the two. The mirror could, of course, also reflect his spectators; it could reflect an audience of two people who are looking at Velásquez as he paints. Could the audience be the subjects being painted by Velázquez?

Despite all appearances (and the energy and attention exerted), Foucault's intent is not to analyze Velázquez' painting. Rather, Foucault is hinting at the utility and playfulness of multiple understandings and plural perspectives. This, in itself, is a central quality of much constructivist scholarship.

If *The Order of Things* is a comparative study, what is Foucault comparing? First, he compares three objects of study: nature, language and wealth. From a synchronic comparison of the fields of Natural History, Grammar and Economics, Foucault wants to demonstrate that each academic field obeys the same basic discourse of science. Before 1620, for example, the three fields coexisted within the larger framework of the Renaissance system of thought; they all observed the world and established meaning in their observations on the basis of the principle of similitude. After 1620, they coexisted within the larger framework of the classical system of thought; they established meaning in their observations, in light of mechanical principles of order. Foucault argued that scholars who studied languages around 1610 thought very differently from those who studied languages some 30 or 40 years later. This is because the two sets of scholars were affected by very different systems of thought. Likewise, Foucault argued that scholars who studied languages around 1610 thought in very similar ways to their contemporaries who studied natural history or wealth. Though they studied different subjects, they did so within the same system of thought.

This section has suggested that an author's attitude toward generalization can be one of the most obvious clues to that author's methodological vantage point. Constructivists tend to shun a strong or explicit devotion to general explanations. Instead, they celebrate the particulars of an investigation. They tend to emphasize the differences and variations of the world, rather than the similarities, and employ comparisons as a way of thinking differently about a given subject. After all, for constructivists, it is these different perspectives, as much as the object being viewed, that call for explanation.

On Case Selection

As we saw in Chapter 5, case selection is an important way by which naturalist comparativists control for explanatory purposes. Unable to exploit experimental or statistical controls, the comparativist tries to choose cases with an eye toward exploring variation on the dependent variable. Case selection is also intricately linked to the naturalist's admiration of statistical techniques: cases must be chosen to avoid selection and/or sampling bias. These concerns – most of which are borrowed from a statistician's world view – are largely irrelevant for the constructivist. Consequently, attention to case selection is an important means for distinguishing the methodological priors of a given comparativist.

Barrington Moore's choice of cases is perhaps the main reason that he is so often seen as a contributor to the naturalist tradition. While he doesn't explain the reasons behind his choice of cases (this, in itself, is noteworthy), and he does not pay equal attention to all his cases (again, worthy of note) it is difficult to argue that his choices are accidental or whimsical. He seems to be choosing cases by sampling on the dependent variable – Britain, the USA and France are offered as cases of the democratic route to modernization; and Japan and China are the main cases of the fascist and communist routes, respectively. More significantly, the discussion of the Indian case (the most careful case study in the book), is used to show how India differed from the other cases of democratic transition.

By contrast, constructivists tend to be more casual in their choice of cases. For example, Reinhardt Bendix, in his *Kings or People* (1978), unlike in his earlier *Nation Building and Citizenship* (1964), provides a clear, if very brief, justification for his choice of cases. If the reader wonders why Bendix relies on (mainly) the same cases in both works (England, France, Germany, Russia and Japan), the reason is explained in terms of personal interest: 'The countries included in this book are those which I have studied for a number of years' (1978, p. 14). In his brief discussion of his cases, Bendix recognizes that these countries are among the most industrialized and that they have experienced some of the world's great revolutions. More important, Bendix recognizes that his choice of cases is not exclusive – and that there are important omissions. However, in a book that has 692 pages, Bendix focuses the limitations of his study on a discussion of other potential cases in a paragraph that straddles pages 14 and 15. This might be contrasted with the chapter-length methodological discussion in books that fit more comfortably in the naturalist approach. Bendix is simply not interested in justifying his choice of cases in terms of proving (or disproving) a theory.

Foucault, once again, can be used as an example in this regard. When discussing his choice of cases in *The Order of Things*, Foucault explicitly rejects the privileging argument that usually underlies case selection. He asks himself, rhetorically, why he has chosen to compare Natural History, Grammar and Economics, and responds that he had not sought to privilege any academic field or discipline:

> if, in fact, one took General Grammar, and tried to define its relations with the historical disciplines and textual criticism, one would certainly see the emergence of a quite different system of relations; and a description would reveal an interdiscursive network that was not identical with the first, but which would overlap at certain points. Similarity, the taxonomy of the naturalists might be compared not with grammar and economics, but with physiology and pathology: there, too, new interpositivities would emerge (one only has to compare the taxonomy/grammar/economics relations analysed in *The Order of Things* with the taxonomy/pathology relations studied in *Naissance de la clinique*). The number of such networks is not, therefore, defined in advance; only the test of analysis can show whether they exist, and which of them exist (that is, which can be described). Moreover, every discursive formation does not belong (necessarily, at least) to only one of these systems, but enters simultaneously into several fields of relations, in which it does not occupy the same place, or exercise the same function (the taxonomy/pathology

> relations are not isomorphic with the taxonomy/grammar relations; the grammar/Analysis of Wealth relations are not isomorphic with the grammar/exegesis relations). (Foucault, 1972, p. 159)

From this perspective, cases are not selected to try to uncover the hidden and universal patterns of the social world. Indeed, the constructivist's selection of cases is not made in the light of larger theoretical or methodological designs, nor are the chosen juxtapositions privileged against others. Here too, as in the previous section, we find a celebration of the particular at the expense of the general. In the case of Foucault's analysis, the problem of case selection is really non-existent. Because, if his argument is correct and the discourse of the age pervades academic discourse in general, it does not matter which disciplines he chooses as cases. Theology, Geography, Political Philosophy, Alchemy or Military Science ... the discourse of the age would have made its mark on all of them. Foucault can choose a small number of disciplines to investigate (lest his entire project should grow far too big to manage), because it simply doesn't matter which cases he selects.

We might add, in closing, that constructivists are equally nonchalant about the 'problem' of selection and/or sampling bias. These concerns come from the naturalist affinity for statistical inference. As a consequence, they tend to hold little sway for constructivists. As with the tendency to generalize, an author's level of attention to questions of case selection and sampling does not need to signal poor scholarship or methodological ineptitude. It is quite possible that an author's lack of attention to these concerns reflects his underlying methodological position. For most constructivists, issues of sampling and case selection are simply not methodologically relevant or interesting.

On Data Selection

This brings us to our final fundamental point of contrast: data selection. Scholars in the naturalist tradition aim to provide public, firm and reproducible accounts of the universal patterns they aim to uncover. For this reason, great emphasis is placed on quantification, source authority and replication. For the constructivist, however, these aspects of scholarship may not be very useful for understanding the way in which meaning is embodied in agency. As a result, a broader spectrum of data and evidence is required; the constructivist draws freely from less orthodox sources and on data generally frowned on by scholars in the naturalist tradition. She might, for example, use private insights (intuition), subjective information (empathy) or even imagined examples, events or characters (for example, from novels or plays).

The problem with data is perhaps most glaring when we think of how someone might capture the sorts of constitutive meanings that are the focus of many constructivist accounts. Anthropologists have always struggled with this problem. In the classic instance of anthropological fieldwork, a highly educated Westerner travels to a remote society – in the geographical as well as the cultural sense – in order to observe, understand and communicate their understanding in texts and pictures.

J. Donald Moon captures the dilemma of anthropological fieldwork in his description of Edward Banfield's (1958) *The Moral Basis of a Backward Society*. Moon notes how Banfield's study is an obvious interpretation of the society in which the author lived; it is an interpretation of what members of the Montegranesi society say and do. Then he adds:

> We cannot simply *ask* members of a society to explain the basic assumptions and orientations underlying their actions, since it is in terms of these constitutive meanings that people understand themselves and their own actions. Even if our informant understood the question, his answer would not be privileged, since we are concerned, for example, not with what would be the proper thing to do in some context but with understanding the concepts and the presuppositions in terms of which something can be said to be what is 'done' or 'appropriate'. Understanding actions, in this respect, is analogous to understanding a language; a native speaker's intuitions may be decisive when it comes to determining whether a given statement is properly formed, but he may be totally ignorant of the rules according to which proper utterances can be formed or of the logical and other presuppositions of a given utterance. (Moon, 1975, p. 170)

The same can be said of Åsne Seierstad's celebrated account of war-torn Afghanistan. Seierstad was living briefly with an Afghan family in Kabul when Western powers invaded in November 2001. She witnessed how the Taliban regime was toppled, and experienced the bubbly optimism of Kabul's citizens during the spring of 2002. She converted her unique experiences into an insightful bestseller, *The Bookseller of Kabul* (Seierstad, 2002), which showcases the life of a particular Afghan family caught up in these dramatic world events. Seierstad appears to have done what Moon says we should not: she simply asked Afghans to explain the basic assumptions and orientations underlying their actions, without grasping the constitutive meanings within which Afghans understand themselves and their actions.

One needn't travel as far as Banfield and Seierstad did to experience novel cultural insights, however. Michael Shapiro (1992) compares radically different strategies for describing the complexity of American culture. On the one hand, he notes the utility of investigations that rely on in-depth interviews of the kind that are familiar to naturalist social scientists (as exemplified by Robert Bellah *et al.*'s (1986) highly acclaimed *Habits of the Heart*). Against this scientific description of American culture he juxtaposes Don DeLillo's (1988) *Libra* – a 'true life novel' about Lee Harvey Oswald and the others who may (or may not) have been involved in the murder of the US president, John F. Kennedy. For Shapiro, both types of 'data' are legitimate and insightful. More important, by juxtaposing the novel and interview accounts, Shapiro is able to reveal the normative undercurrent in the latter. For Shapiro, *Habits of the Heart* reveals a 'mythic plot', despite the fact that it is an investigation that purports to be controlled by its 'non-fictional' dimensions – for example, systematic interviews and objective definitions, concepts and data (Shapiro, 1992, pp. 68–9).

By contrasting radically different types of data, Shapiro shows us the methodological limitations of both the data and the approach of traditional naturalist approaches to social phenomena. In doing so, he reminds us of something that Sigmund Freud (1907, p. 8) recognized: that writers are 'valuable allies ... [who in] their knowledge of the mind ... are far in advance of us everyday people, for they draw upon sources which have not yet opened up for science'. In short, constructivists tend to realize that art, literature and narrative often help us to comprehend the world in which we live.

As Shapiro hints, popular culture can provide a key to understanding society: analyzing popular culture can help us to say something about the society in which the culture in question is prevalent. This sort of analysis begins by assuming that the fads and fashions revealed in books, films or popular music reflect more basic concerns – the norms and values, but also the uncertainties and fears – of the society that sustains them. Following this logic, an analysis of the runaway international success of the Harry Potter books can serve as a gateway into the main strands of international youth culture and globalization (see, for example, Nexon and Neumann, 2006). Similarly, Edward Said's influential study of Western representations of 'the Orient' is not based on sources concerning military might or economic prowess; its empirical basis is, ultimately, a selection of British novels (Said, 1978).

In this section we have shown how naturalist and constructivist scholars differ in their approaches to three fundamental issues: their views on generalization; case selection; and choice of data. These differences can be traced to the disparate ontological and epistemological beliefs associated with each tradition. Our objective has been to provide a few simple indicators or signifiers of an author's methodological commitment.

Constructivist Comparisons

If comparisons aren't used as a control measure to test arguments about the patterns and regularities of the social world, what role do they play in constructivist investigations? In this section we introduce four different ways in which comparisons are used by constructivists. As we saw in Chapter 9, and with the examples provided by Alasdair MacIntyre in the introduction to this chapter, much of constructivism aims to dispel the mechanical and generalizing tendencies of naturalist scholarship. For this reason, we start by looking at how comparisons can be used in less formal ways to challenge existing explanations and to explore possibilities. The first couple of examples challenge established truths. The first illustration draws on critiques in the French *annaliste* tradition, crafted by Bloch (1953) and Foucault (1970). The second example, by Sandbrook *et al.* (2007), shows how constructivists can compare different path-dependent cases to show how possibilities and opportunities exist, even in what appear to be binding contexts.

The last two examples show how constructivists can also use comparisons to establish associations. Traditionally, these associations have taken two related forms. In our third example, comparisons are used in a hermeneutical fashion to uncover meanings by juxtaposing the particular against the general. Finally, comparisons are used to investigate the way in which our particular biases often alienate us from the object of our study. To illustrate this fourth type of comparison, we introduce Roxanne Euben's (1999) *Enemy in the Mirror*. Constructivists use these sundry types of comparisons to emphasize the uniqueness, particularity and complexity of social and political phenomena.

Challenging the Old and Constructing the New

Unstructured comparisons can be used as a way of challenging existing hypotheses (derived from more rigid social theories), and generating new frameworks for historical or social study. By using comparisons in this way, the constructivist does not aim to replace one explanatory variable with another (in hopes, for example, of increasing an argument's R^2); rather, he uses comparisons to challenge the notion of rigid explanatory structures. This is best done with a firm empirical grasp on the details of particular stories.

Marc Bloch, for example, used a rather superficial comparison when he discussed the role of gold in the European economy in the Middle Ages. His aim was to challenge the dominant historical argument about the reason why medieval Florence and Genoa were the first to issue gold-based coins. The traditional argument held that the vast wealth and rapid economic growth of these two cities could explain their issuance of gold coins. Bloch pointed out that Venice was as wealthy as the other provinces, but – in contrast to them – relied on silver-based coinage. This enabled him to question traditional analyses and open up the possibility of an alternative explanation. In particular, Bloch turned to examine the nature of each city's wealth and found that Florence and Genoa grew rich on Asian trade (paid for in gold), whereas Venice grew rich on more traditional trade with the Levant (paid for in silver). Because Venice's wealth was accumulated in silver, it was neither interested in, nor able to issue, gold coins.

Thus, Bloch used comparisons to demonstrate the insufficiency of an existing theory. At the same time, his comparisons provided a clue as to where new explanations might be uncovered. In 'Toward a Comparative History of European Societies', Bloch (1953) emphasized this 'discovery' aspect of comparisons to explain how rough contrasts led him to discover the enclosure movements in southern France of the fifteenth to seventeenth centuries. Given his familiarity with research on contemporary English enclosure movements, Bloch wondered if something similar might have happened in France. The implicit comparison produced a new research question – indeed, it opened up an entirely new field of research for French economic historians (Sewell, 1976, p. 209).

Again, we would be remiss if we didn't refer to the way in which Foucault uses comparisons to challenge our presuppositions about historical patterns and generalizations. In *Discipline and Punish*, Foucault (1977) uses comparisons in a masterful, albeit implicit, way. The opening pages of the book begin with a presentation of Damiens, who had murdered a member of the royal family, and a morbid description of the French disciplinary regime in the mid-eighteenth century:

> On 2 March 1757 Damiens, the regicide, was condemned 'to make the *amende honorable* before the main door of the Church of Paris', where he was to be 'taken and conveyed in a cart, wearing nothing but a shirt, holding a torch of burning wax weighing two pounds'; then, 'in the said cart, to the Place de Grève, where, on a scaffold that will be erected there, the flesh will be torn from his breasts, arms, thighs and calves with red-hot pincers, his right hand, holding the knife with which he committed the said parricide, burnt with sulphur, and, on those places where the flesh will be torn away, poured molten lead, boiling oil, burning resin, wax and sulphur melted together and then his body drawn and quartered by four horses and his limbs and body consumed by fire, reduced to ashes and his ashes thrown to the winds'. (Foucault, 1977, p. 3)

The description continues in this gruesome detail for another three pages, when the reader is thrown, unexpectedly, into a new (but subsequent) punishment regime 80 years later. Foucault then introduces Léon Faucher, drawing up rules 'for the House of young prisoners in Paris':

> Art. 17. The prisoners' day will begin at six in the morning in winter and five in summer. They will work for nine hours a day throughout the year. Two hours a day will be devoted to instruction. Work and the day will end at nine o'clock in winter and at eight in summer.
>
> Art. 18. *Rising*. At the first drum-roll, the prisoners must rise and dress in silence, as the supervisor opens the cell doors. At the second drum-roll, they must be dressed and make their beds. At the third, they must line up and proceed to the chapel for morning prayer. There is a five-minute interval between each drum-roll.
>
> Art. 19 ... (Foucault, 1977, p. 6)

Foucault is toying with our expectations. He does not coach us to compare, or tell us how to interpret the contrast. He is confident that the reader will compare these two regimes herself and draw conclusions about the changes that have taken place since March 1757, when Damiens was condemned to be the focus of a bestial spectacle in front of the Church of Paris. Foucault knew that by doing this we, his readers, will think of the first regime in terms of medieval barbarianism and the second as modern civility. In short, Foucault is forcing us to see how we are, in effect, conditioned to think in comparative, historically progressive terms. He then uses the rest of the book to challenge and criticize this notion of linear progress. In doing so, he offers a critique of the Enlightenment project as an unambiguously progressive era in the history of the social sciences.

More than anybody else, Foucault exploits comparisons to illustrate complexity. In the 'Foreword' to the English edition of his *Order of Things* (1970), Foucault is quite explicit about the comparative nature of his project. Yet his understanding of the nature and purpose of comparisons is a far cry from the naturalists' 'method of testing hypothesized empirical relationships among variables on the basis of the same logic that guides the statistical method' (Lijphart, 1975, p. 164). Foucault wants to compare in order to illustrate the wondrous ways in which things can be related to one another. His aim is not to produce some universal pattern of social action; rather, he compares in order to produce

> results that are often strikingly different from those to be found in single-discipline studies. (So the reader must not expect to find here a history of biology juxtaposed with a history of linguistics, a history of political economy, and a history of philosophy.) There are shifts of emphasis: the calendar of saints and heroes is somewhat altered ... Frontiers are redrawn and things usually far apart are brought closer, and vice versa: instead of relating the biological taxonomies to other knowledge of the living being ... I have compared them with what might have been said at the same time about linguistic signs, the formation of general ideas, the language of action, the hierarchy of needs, and the exchange of goods. (Foucault, 1970, p. x)

Comparisons allow Foucault to shine his spotlight on what he calls a *positive unconscious* of knowledge: namely, a kind of innate knowledge that eludes the consciousness of the scientist who possesses it and yet is part of scientific discourse. He explains:

> What was common to the natural history, the economics, and the grammar of the Classical period was certainly not present to the consciousness of the scientist; or that part of it that was conscious was superficial, limited, and almost fanciful ... but, unknown to themselves, the naturalists, economists, and grammarians employed the same rules to define the objects proper to their own study, to form their concepts, to build their theories. It is these rules of formation, which were never formulated in their own right, but are to be found only in widely differing theories, concepts, and objects of study, that I have tried to reveal, by isolation, as their specific locus, a level that I have called, somewhat arbitrarily perhaps, archaeological. (Foucault, 1970, p. xi)

In these ways, comparisons can be used to emphasize the superficiality of existing causal arguments, while proposing new arguments that emphasize the complexity of history and the possibilities of agency. Rather than using comparisons to test general theories, constructivists tend to use comparisons to prise open our imagination – to consider the possibilities and to encourage new readings and understandings of the empirical literature.

Leveraging Opportunities

In the same way that naturalists build comparative studies by aligning detailed case studies, constructivists may compare cases that employ process tracing, path dependency and/or within-case approaches to reflect on the nature of relevant social patterns. As with the examples given in Chapter 9, constructivists' comparisons prioritize the importance of the context in generating lessons or understanding: they tend to lower their theoretical sights (or are more modest, theoretically); they tend to be less explicitly concerned with methodological issues; and they often convey a strong normative component.

In this respect, Richard Sandbrook *et al.*'s (2007) *Social Democracy in the Global Periphery* offers a first-rate example of well-executed comparisons in the constructivist tradition. Tellingly, they begin their study by noting Albert Hirschman's (1971, p. 28) enjoinder for social scientists to embrace 'a passion for the possible'. Indeed, each of the four commendations on the paperback version of the book (by Peter Evans, Atul Kohli, Dietrich Rueschemeyer and Michael Walzer) seem to applaud the hopeful and inspiring message conveyed in this careful comparison.

Of course, naturalist comparisons can also inspire hope, but they do not usually wear it so boldly on their lapel. Sandbrook *et al.* chose their cases (Kerala, Costa Rica, Mauritius and Chile) by using their score on the dependent variable ('on the grounds of both their celebrated status as social-democratic pioneers and the diversity of their approaches') (2007, p. 9). The naturalist may be struck by the odd combination of cases; they are scattered across the globe, capture different levels of government (Kerala is a state in the Indian federation, not an independent nation-state), and represent very different forms of social democracy.

Each case is then described in wonderful detail, elaborating four complex, unique and path-dependent trajectories to social democracies in the global periphery. Thus, in Kerala, we learn of '[t]he critical juncture of 1957 that locked in a social-democratic trajectory was itself the product of the historical convergence of social, political and institutional factors' (ibid., p. 74). For Costa Rica: 'The 1940s in Costa Rica constituted a critical juncture in which the outcome of political struggles, key leadership choices, and institutional changes set the country firmly on a path toward consolidating a social-democratic developmental state' (ibid., p. 98). In Mauritius, 'conjunctural factors such as astute policy, good leadership and preferential trade arrangements contributed to [the country's] success, historical/structural ones created the foundations for a democratic developmental state able to take advantage of opportunities' (ibid., p. 131). Finally, in Chile, we learn that the 'legacies of military rule have constrained Chile's post-1990 brand of social democracy, a case of path dependency' (ibid., p. 164).

In short, the cases are not ordered in a structured way, à la Mill, to coax out a hidden underlying pattern. Rather, they are variations of a common theme or 'exceptionalisms of a general type' (ibid., p. 177): the comparisons are used to illustrate contingency, particularity, historical junctures and path dependencies. The patterns eventually revealed (in a part of the book entitled 'Patterns and Prospects') are curiously fragile: 'Even when all the above domestic factors are favorably aligned for social democracy, they can be trumped by external interference' (ibid., p. 187). Even though certain historical conditions seem to underlie these social democratic cases, the 'comparative analysis suggests that ... there are multiple paths to social democracy in the periphery' (ibid., p. 211).

The result is four detailed analyses of complex causal patterns, one in each country. These patterns reveal how it is possible for small states to secure a small corner in the global economy, in which they can nurture a social-democratic state. This brings hope to the authors (and to the supporting readers surveyed on the cover of the book). But this hope needs to be tempered by the extremely contingent nature of these documented successes. The focus here is not on trying to uncover a specific constellation of variables that can explain social democratic success; rather, the objective is to understand how unique and special is each case. As the authors note with reference to the Mauritius case (Sandbrook *et al.*, 2007, p. 145): 'Yet this attractive model has emerged from an unusual history and is thus unlikely to be widely emulated.'

Hermeneutic Understanding

One of the ways in which constructivists encourage new interpretations is by employing hermeneutic approaches – approaches that are inherently comparative. Though we introduced the hermeneutic approach in Chapter 8, we can use this section to show how it works in practice. As the attentive reader will recall, hermeneutic understanding is produced by juxtaposing the particular with the general, the local with the distant. In hermeneutic studies, the comparisons are often implicit, but the contrast between particular events and general norms helps us to understand the event as something more than just particular, or local.

Clifford Geertz provides several examples of this type of comparison. His book *Islam Observed* (1971) compares two very different Muslim societies. Yet his aim is not to generalize about religious life; rather, it is to investigate local cases to become more specific and more concrete. He hopes 'to find in the little what eludes us in the large; to stumble upon general truths while sorting through special cases' (Geertz, 1971, p. 4). At the same time, he wants to show us how different these two societies are in order to shake the commonplace notion that Muslim societies are all alike.

Geertz' (1972) article on Balinese cockfights provides another example of this attitude. The article begins by introducing the author and describing his (and his wife's) first encounter with Bali and a Balinese cockfight. What at first seems like a remarkable local event (a large cockfight held in the public square to raise money for a new school) is casually compared and contrasted with larger social symbols, institutions and practices in

Bali. Again, he finds 'in the little what eludes us in the large'. The study helps Geertz (and his reader) to develop an eventual understanding of the cockfight as something more than a local or particular event: 'In the cockfight, then, the Balinese forms and discovers his temperament and his society's temper at the same time. Or, more exactly, he forms and discovers a particular face of them' (1972, p. 28). This purpose is reflected in the main title of the piece: 'Deep Play: Notes on the Balinese Cockfight'.

In another piece, Geertz (1975) applies thick descriptions to Java, Bali and Morocco. After presenting three parallel interpretations of the way in which the Javanese, the Balinese and Moroccans view their sense of self, Geertz explains his method thus:

> notice the characteristic intellectual movement, the inward conceptual rhythm, in each of these analyses, and indeed in all similar analyses ... a continuous dialectical tacking between the most local of local detail and the most global of global structure in such a way as to bring both into view simultaneously. In seeking to uncover the Javanese, Balinese, or Moroccan sense of self, one oscillates restlessly between the sort of exotic minutiae (lexical antitheses, categorical schemes, morphophonemic transformations) that make even the best ethnographies a trial to read and the sort of sweeping cauterizations ('quietism,' 'dramatism,' 'contextualism') that makes all but the most pedestrian of them somewhat implausible. Hopping back and forth between the whole conceived through the parts which actualize it and the parts conceived through the whole which motivates them, we seek to turn them, by a sort of intellectual perpetual motion, into explications of one another. (Geertz, 1975, pp. 52–3)

In this moment of explicit methodological reflection, Geertz shows us the central role played by comparison in his interpretation. But even here, at his most explicit, Geertz avoids the word 'compare': the analyst *moves*, *tacks*, *oscillates* and *hops* to interpret.

Geertz is using comparisons to appreciate the local significance of knowledge. Rather than using comparisons to produce larger generalizations about the nature of the social world, constructivists use comparisons to interpret particular events with frequent contrasts to larger contextual settings. It is these contexts that provide the constitutive meaning to the particular events.

In practice, hermeneutic studies often require two levels of comparison. The first level juxtaposes particular events with general forms/norms. Here the cockfight is positioned against a more general Balinese culture. The second level of comparison is necessary to determine the nature of these general norms. At this level, comparisons are made across general forms in order to distinguish the unique characteristics of each form. Thus Geertz studies cockfights to further a hermeneutic dialogue in other cases – Java, Bali and Morocco – cited in his 1975 article. Here the three cases are clearly juxtaposed against one another in parallel and enclosed depictions: 'Making the self "smooth"' (Java); 'A theater of status' (Bali); and 'A public context for a private life' (Morocco). While Geertz does not point our attention toward it, the 'global structure' inherent in each (unique) case is provided by the implicit comparison of the three cases. Not only are we 'hopping back and forth' between the whole and the parts; but we are doing so between the three cases. In other words, to know what is 'Balinese', we need to know what is 'not Balinese'.

Contrasting Us with Them

As hinted at in the previous section, comparisons are often used by constructivists to show how our approach to an object of study can actually hinder our access to it. In today's political climate, this problem is perhaps most evident in Western attitudes toward Islam, and 'fundamentalist Islam' in particular. For this reason, we use this closing section to describe an attempt at understanding 'Islamic Fundamentalism and the Limits of Modern Rationalism' (Euben, 1999). Roxanne Euben holds that the methods and categories employed in Western social scientific explanations actively distort fundamentalist ideas, making it difficult for us to understand how these ideas could be so appealing to so many.

Euben compares Islamic fundamentalism with various Western critiques of rationalism to illustrate unexpected similarities shared by the two theoretical traditions. She begins by noting how political Islam is commonly depicted as a threat to modern, legitimate politics, dividing the world into two antagonistic blocs (the Islamic World versus the West). We in the West have come to see Islamic fundamentalism as the irrational Other to our intelligible Self – a negative mirror reflecting back Western life (Euben, 1999, pp. 43, 44). A gulf separates these two blocs, if only because 'social scientific explanations portray the Islamic fundamentalist as the paradigmatic irrational rational actor; that is, the actor apparently rational enough to gravitate toward an ideology that is an effective and therefore appealing vehicle for essentially pathological reactionary sentiment' (ibid., p. 24). Under these conditions, and for the sake of understanding what fundamentalism is about, Euben says we must 'strive against our own moral impulses and intellectual reflexes, to hear voices critical of our own deeply held convictions about the way the world does, or should work' (ibid., p. 16).

For those who aren't old enough to remember the Cold War, the same thing could be said about mindsets on both sides of the Iron

Curtain. The Western image of the Soviet state was so saturated with negative images – indeed, straight inversions of the West's own political values – that it was simply impossible for many denizens of the West to understand why Soviet communism could have been embraced by so many, and for so long. (Of course, the same thing could be said of Soviet depictions of the West.) Today, the Red Menace has been replaced by a Green Menace (green being the colour of Islam), but the threat to our cherished political values is seen to be just as ominous.

Euben (1999) employs comparisons, at two levels, to break down these barriers to understanding. On the one hand, she wishes to help us to understand Islamic fundamentalist thought on its own terms: 'to provide a window into fundamentalists' own understandings of the movement's meaning and purpose' (ibid., p. 8). To do this, she examines the work of one representative and influential thinker in that tradition, Sayyid Qutb (1906–66), to show how it is a complex reaction to a cacophony of sources (for example, Western imperialism and colonialism, corrupt regimes in the Middle East, Arab secularist power, modern forms of power and sovereignty, and the Western rationality that justifies them).

She then compares Qutb's writing with those of important predecessors in the same tradition: namely Jamal al-Din al-Afghani [al-Asadabadi] (1839–97) and Muhammad 'Abduh (1849–1905). This is done to place Qutb's argument in its own context (to better understand its message, on its own terms), but also to challenge commonplace arguments that portray fundamentalism as the inevitable return of an Islamic 'essence', or as some sort of 'natural' reaction of archaism against modernity (ibid., p. 117).

Against this home-grown depiction of Islamic fundamentalism, Euben then compares Qutb's writings with a handful of more recent Western critics of modernity to show how Qutb's argument is neither pathological nor unfamiliar. By examining Hannah Arendt's analysis of modern authority; Alasdair MacIntyre's, Charles Taylor's and Richard John Neuhaus's discussion of modern moral discourse; and Robert Bellah's and Daniel Bell's arguments regarding the decline of modern community, Euben shows us how Qutb's basic argument (where modernity is depicted as a crisis defined by a degeneration of common meanings) is not antimodern – but rather another perspective on, and an attempt to redefine, what it means to live in the modern world (ibid., p. 87).

In short, Euben does not use comparisons to develop universal truths. Rather, she uses them to emphasize dilemmas and questions that straddle both cultures and time. In so doing, she creates room for the radical notion that there is humanly significant knowledge that lies outside the confines of Western political thought. Euben's comparisons allow her to depict Islamic fundamentalism as something understandable – something that would be recognizable to Islamic fundamentalists themselves. Her intention is not to proselytize, or paint a sympathetic picture of fundamentalists; Euben is far too sceptical of universal truths to take that role. Rather, she recognizes that *understanding* Islamic fundamentalism means that we must bring its followers in from beyond the pale – allow them access to the realm of rational discourse. In other words, Euben's use of comparisons allows us to understand what naturalism hides: naturalist science defines discursive practices in ways that tend to obscure the very theoretical and transcultural aspects of fundamentalist thought that are central to the meaning of Islamic fundamentalism (ibid., p. 156). A constructivist approach employs comparisons that allow us access to those important theoretical and transcultural aspects.

Conclusion

This chapter has aimed to generalize about those who avoid generalization. Consequently, our objectives have been rather modest. We have aimed to help the inquisitive reader to recognize constructivist scholars by the disparate ways in which they employ comparisons. In particular, we have shown how constructivists tend to distance themselves from naturalist norms when it comes to embracing generalization; how they choose their cases; and the nature of the data or evidence they employ. It is in opposition to the naturalist Other that constructivists have come to define and understand themselves.

We hasten to note, however, that the constructivist's use of comparison entails more than just a critique of naturalism. Comparisons, as associations, are a central means for constructivists to understand complex social phenomena and to illustrate the particularity and contingency of social/political life. In the process, constructivists employ process tracing, storytelling and creative comparisons to shine a light on their subject matter (and its analysts). Constructivists turn to hermeneutic or dialectical approaches to secure similar ends. Thus constructivists tend to compare in a way that is consistent with their method of choice – as an extension of the narrative approaches described in the previous chapter.

By comparing thick, in-depth and informed stories, constructivists are able to see things that are easily obscured by the naturalist approach. As they are not limited to using reason or observation, constructivists draw from a much broader set of experiences. They compare contexts, judgments, practices, trials and errors, experiences, intuitions and bodily sensations to learn and understand. By saturating their comparisons in a local context, they are able to better understand the promise and limitations facing their subject matter. The result is a fuller understanding of particular phenomena – an understanding where comparison plays a central, if often hidden, role.

Recommended Further Reading

As is noted in the text, we think it is useful to begin with Alasdair MacIntyre's 'Is a Science of Comparative Politics Possible?' (1972), as it provides a critique of naturalist comparisons. We also recommend Piotr Sztompka's 'Conceptual Frameworks in Comparative Inquiry' (1988). For detailed applications of constructivist comparisons, we recommend Michel Foucault's remarkable *The Order of Things* (1970) and Sandbrook *et al.*'s (2007) captivating *Social Democracy in the Global Periphery*. Michael Shapiro's collection *Reading the Postmodern Polity* (1992) provides another vantage point for the constructivist approach.

Chapter 11

Contextualizing Statistics

It is not easy to find constructivist authors of statistical studies. This, in itself, should not surprise us. After all, the traditional objective of the statistical method is to remove the subject matter from its constitutive context in order to probe its nature in terms of correlational patterns. For the constructivist, where meaning and context are prioritized above all else, this method can do more harm than good; it contributes to a twofold distancing: between the data and their context, and between these and the analyst.

This does not mean that statistics can't play an important role in constructivist analyses. Indeed, some of the most exciting new developments in statistics lend themselves to constructivism. Not only are traditional regression and measuring techniques being harnessed to study the constructed nature of the patterns that interest us, but also new developments in graphic displays, Bayesian logic, and discourse analysis have made statistical approaches more relevant and attractive to constructivist scholars. As a consequence of these developments, constructivists are able to find patterns, associations and meanings that are not entirely obvious in or easily captured by more traditional, narrative-based, approaches.

This chapter is divided into two parts. The first uses history to suggest a reason for why constructivists have traditionally held such a critical view of statistical approaches, and the second introduces a number of different statistical approaches that are consistent with constructivist beliefs about our world and how we come to understand it. These statistical approaches can be embraced by constructivists as they aim to capture and maintain the contextual integrity of the things they study, or because they facilitate contextual analyses at higher levels of aggregation.

The Dark Side of Statistics

As we saw in Chapter 4, statistics emerged as a tool for social analysis rather late in the game: it was only with the coming of the nineteenth century that statistics was embraced (and, at first, hesitantly) by the scientific community. One reason for this late debut was the rise of an entirely new approach to society and social issues.

During the age of absolutism, society was ordered in terms of a steep and fixed hierarchy of natural endowments. Under such an order, it made little sense to record the key characteristics of all citizens – to lump them together, and analyse them all in the same way – as if they were all equal units. Enormous gulfs separated the king, the aristocrats and the common peasants. This difference made it obvious that the king could not be discussed in the same way as the common butcher or baker (or candlestick maker); he was different, better and above the others. In an autocratic and many-layered society such as that of the *ancien régime*, it was inconceivable to convert all members of society into numbers of equal value and to treat them all in the same standard fashion. In other words, it was as practically impossible to be a modern statistician in sixteenth-century France as it was to be a freethinker (Febvre, 1983). This contextual bias is reflected in the ways that statistics were first gathered and applied: people were first counted as soldiers and as taxpayers.

The Enlightenment changed all of this. Knowledge is power, wrote Bacon (1854, p. 80), and in knowledge 'the sovereignty of man lieth hid'. Through systematic knowledge about the world, human beings could take control of their own destiny, and fashion a world that was good, orderly and peaceful (Adorno and Horkheimer, 1979, p. 42). Treating citizens as units of equal worth – whether as statistical units or as voters whose ballots count equally at the polls – began to make sense once thinkers came to embrace the essential equality of humankind. The rise of statistics, then, is intricately associated with the rise of a new perspective on humanity – it coincides, in fact, with the advent of what Foucault calls 'the modern system of thought'. For this reason, it makes good sense to discuss its rise in the light of Foucault's concepts of governmentality and bio-power.

The initial establishment of the Statistical Society of London (in 1834) reflected the renewed growth of interest in statistical approaches among governing groups. Indeed, the Society was founded after a presentation by Adolphe Quetelet. The Belgian scientist was invited to the British Association for the Advancement of Science (BAAS) to present a paper on the relationship between the statistics of crime and age in France and Belgium. As the hosting BAAS did not have a formal statistics section, Quetelet was asked to present his ideas privately to a smaller audience, which included Thomas Malthus and Charles Babbage. Apparently, Babbage was so intrigued by the talk that he suggested a new section be formed to deal expressly with statistics. As we saw in Chapter 4, the general assembly of the BAAS was not entirely happy about this, and required that the new statistical group, Section F, should deal in facts and stay away from opinion and interpretation. Many of the subjects to which statistical techniques were being applied (such as crime, social conditions and medicine) had broad political and social implications, and the last thing the association wanted was to become mired in politics.

Over time it would prove remarkably difficult to separate politics and statistics. Indeed, statistical techniques came to play a very important role in developing and assessing the new ambitions of government, such as combating pauperism, vagrancy, unemployment and crime (to name just a few examples).

A Second Look at Sir Francis Galton

As we have already noted, few individuals played a more important role than Sir Francis Galton (1822–1911) in the renaissance that followed. Galton and his students sharpened the techniques of Graunt and honed them into powerful tools in his many scientific projects. But in our haste to introduce Galton's accomplishments (and wishing to remain loyal to the naturalist modus operandi), we skipped quickly over any details of his life and surrounding context.

This is a shame, if only because Galton was a remarkably multitalented man: among other things, he was known as a geographer, tropical explorer, anthropologist, meteorologist, criminologist, contrarian, mathematician and best-selling author. In short, Galton was a polymath, and a fascinating one at that. His intellectual legacy lives on by way of a long autobiography, *Memories of My Life* (1908) (in which he boasts about it all), and an impressive website dedicated to spreading knowledge of his work (www.galton.org).

While Chapter 4 describes the regression techniques that Galton developed, we touched only briefly on how these techniques were actually applied. In addition to studying peas and human height, Galton applied his new-found techniques to a study of the distribution of intelligence in society. On the basis of several such studies, Galton claimed he had discovered a social law: though various properties are unevenly distributed among humans, such differences tend to even-out over the longer haul, because with each new generation these properties will tend to regress toward the mean of the race. These techniques were then employed as tools for a number of Darwinistic social projects. This is the seamier side of modern statistics – a historical and political context that statisticians tend to forget, or ignore.

Francis Galton was a man of his times and a half-cousin of Charles Darwin. Galton eagerly collected material to garner evidence for his cousin's arguments and to apply them to human society. While Galton made important contributions in several fields, he concentrated his efforts on sociobiological questions, and was a central force behind the effort to establish the science that he named: *eugenics* – the study of ways to improve the human race by means of genetic manipulation. When he died, Galton bequeathed University College, London with enough money to endow a Chair of Eugenics.

As we saw in Chapter 4, Galton's initial work (on peas and human height) showed that tall parents tend to yield tall offspring. From there it was a short step to arguments concerning other human qualities, such as reasoning: Galton argued that intelligence, like height, is distributed unevenly, and that the intelligence of children regresses toward the mean. Consequently, very intelligent parents tend to have children who are also intelligent – but less intelligent than themselves (yet more intelligent than the average). Similarly, unintelligent parents tend to have children who are unintelligent – but, on the average, more intelligent than themselves (yet less intelligent than the average). For Galton, this realization delivered clear policy consequences that could be exploited for the greater good: since intelligent parents would have intelligent children (and stupid parents would have stupid children), then intelligent people should be encouraged to procreate. On the other hand, stupid people should not be encouraged to procreate; and the truly stupid people should, in fact, be barred from doing so: for if stupid people procreated at a greater rate than intelligent people, then the entire race would suffer as a result (Dean, 1999, pp. 136ff).

Galton didn't stop there. In his book *Hereditary Genius* (1869) he argued that a system of arranged marriages between men of distinction and women of wealth would eventually produce a gifted race. Later, in a lengthy letter to the editor of *The Times*, Galton (1873) advocates the transfer of property in Africa from its traditional residents ('negroes possess too little intellect, self-reliance, and self-control to make it possible for them to sustain the burden of any respectable form of civilization without a large measure of external guidance and support') to the more industrious and numerous Chinese (who 'possess an extraordinary instinct for political and social organization' and 'are good-tempered, frugal, industrious, saving, commercially inclined, and extraordinarily prolific'). In both cases, Galton thinks like an engineer, for whom no problem is too large: 'No very serious obstacle seems to stand in the way' of eradicating an entire race and moving another from one continent to another (ibid.).

As a statistician, Galton conceives of the world in terms of independent variables: he has no difficulty in removing his subject matter from its original constitutive context and tossing it around in different combinations. As a eugenicist, he was concerned with improving the quality of the race – partly by checking the birth rate of the unfit, and partly by furthering the productivity of the fit (Blacker, 1952, p. 111). He was also concerned with recruiting talents who could contribute to his eugenicist research – men such as the multi-talented Karl Pearson, who continued the eugenic research of his mentor and developed several new statistical techniques (such as the 'chi-squared test' and the 'standard deviation').

Of course, Galton was a product of his times, and it is perhaps understandable that he held these views in 1873 (though a rejoinder to Galton's letter by Gilbert Malcolm Sproat (1873) suggests otherwise). Eugenics only became controversial in the twentieth century, after the Second World War, in the wake of Nazi Germany's race experiments. At that point, Galton's eugenics projects were allowed to fade quietly into infamy. Students of statistics today seldom hear of their topic's eugenic roots.

Statistics Lack a Sense of Context

Galton himself is an example of the main shortcomings of his statistical method. His approach and arguments are flawless and impressive feats of logic. But it is beneath these strengths that we find the method's weaknesses buried. Our first caveat, then, is that the statistical approach is foreign to both the human and the humane. It is in itself insensitive to ethics, morality and politics. In the same way that students of statistics are sheltered from Galton's eugenic past, the statistical method shields its analyst from her own human context.

Our second caveat is related to the first: namely, that the statistical approach requires the analyst to distance herself from the context of the study. This distancing makes it difficult for the scholar to be immersed in the constitutive meanings of the data. Indeed, this problem is clearly evident in the apparent difficulty of modern social statisticians to distinguish between actual and statistical significance (see, for example, McCloskey and Ziliack, 1996, 2004).

This distance, itself, is a product of two factors. First, quantification necessitates abbreviation. The very process of quantification requires that much meaning is lost as descriptive characteristics become indexed on to five-point scales (for example). Thus, the first casualties of quantification are interpretation and context. Interpretation is jettisoned because it is assumed to conflict with the scientist's need for dispassionate objectivity; and context is shunned because it problematizes the analysis, as is clear from the definition of 'data' in the *Dictionary of Statistics and Methodology*:

> Data are often thought of as statistical or quantitative, but they may take many other forms as well – such as transcripts of interviews or videotapes of social interactions. Non-quantitative data such as transcripts

> or videotapes are often coded or translated into the numbers *to make them easier to analyze*. (Vogt, 1993, p. 59, our emphasis)

Another reason for this distancing of researcher and data results from the way in which larger statistical projects often depend on the compilation of figures by numerous observers – each with his own background, presuppositions and conceptual schemes. Barry Hindess makes this point eloquently:

> [The sociologist or some other interpreter] cannot assume a uniform interpretation of these categories in terms of observable objects and events. Such interpretation would be legitimate only if it could be demonstrated that the initial observers and the compilers of the statistics in questions used the same rules of categorization, and that these rules were sufficient to eliminate classification by fiat in every case. (Hindess, 1973, p. 21)

Anybody who has ever worked on a large statistics-gathering project (and we speak from experience) eventually has an attitude not unlike that of a butcher's toward sausages (or Bismarck's toward laws): once you are made aware of their ingredients, you are likely to lose your appetite for them.

In today's large data-mining projects, graduate students are often employed to 'code' descriptions of a given social phenomenon into numerical indexes (which themselves are the product of a researcher's presuppositions of the sort of variation she expects to find). Quite often these indexes are modelled after 'classic cases' of the phenomenon in question. Imagine, for example, a project aimed at collecting data and coding the 'degree of corporatism' for a large, cross-national, cross-temporal database. To gather this enormous amount of data, a lead researcher would begin by employing a number of graduate students with enough proficiency (language and otherwise) to collect the relevant data over a number of disparate countries. This data would be collected by way of a code book, which encourages the students to go through the case study literature in search of specific, standardized responses for the country in question. We can expect that most of the student researchers would not be able to answer all the relevant questions in a large code book, but would do their best to provide reasonable answers, in the light of what they had read. In other words, there will be much interpretive give-and-take as the students try to fit the histories of various labour movements, for example, into distinct and inflexible numerical depictions.

But the interpretive distancing doesn't stop there. As the data collection part of the project approaches its end, the head researcher will need in some way to bridge the interpretive divide that exists between the different nations and cultures in the study: to try and make the data comparable across states. For the constructivist, there is simply too much room for interpretive slippage in the gap that exists between (i) the historical and conceptual context; and (ii) the end data collected in matrix form.

The Statistical Worldview

The third major drawback to statistics from a constructivist point of view concerns the exportability of what McKeown (1999) calls the 'Statistical Worldview'. In response to King *et al.*'s (1994) influential methods primer (which draws on a statistical approach to the world), McKeown argues that the logic of statistical study is not hegemonic to social phenomena, and that trying to apply it to different types of studies (in particular, case studies), is both misleading and problematic. By referring to the lack of statistical inference in most of scientific history, and by showing how alternative logics are quite successful in finding solutions, McKeown shows how the statistical world view is different from other world views. Indeed, the statistician's view of the world, as well as the statistician's view of his own role in it, is often unselfconscious and vague – to the point of being inaccurate and misleading:

> [R]esearchers almost never begin from the starting point envisioned by Descartes or Hume – their thought experiments involving radical doubt radically misstate the situation facing the researcher. Typically, the research task is not how to move from a position of ignorance to one of certainty regarding the truth of a single proposition. Rather, it is how to learn something new about a world that one already knows to some degree. Framed in this fashion, the basic tasks of research are then (1) to devise ways of leveraging existing understanding in order to extend our knowledge, and (2) to decide what are sensible revisions of prior understanding in light of the knowledge just acquired. (McKeown, 1999, p. 187)

As we shall see below, Bayesian statistics is one way to address these issues.

This problem is particularly pronounced in the case of regression analyses. The reader will recall from Chapter 4 that regression analysis is used to manipulate, conceptually, partial correlations in a design that holds other variables constant. In doing this, statistical projects are about constructing a new version of reality, one that is insensitive to the ways in which the social world has meaning. The statistician is explicitly reconstructing the world to better investigate it. Her method is not so much about discovering facts of social life, but rather about constructing a different (new) version of that life, through statistical manipulation. The lessons we learn from this new, constructed reality can only be transferred to the social world by making a series of very demanding and controversial assumptions.

On the Bright Side

So far we have tried to show why constructivists tend to be sceptical of statistical approaches. We are somewhat reluctant to do this, as there is already a tendency to link constructivist approaches with 'qualitative' approaches and the fear of hard numbers. Our depiction is also somewhat misleading, as some of the most sophisticated, advanced and technical of today's statistical approaches are those aimed at maintaining and protecting the very sorts of context that constructivists embrace. In short, there is no clear or necessary relationship between constructivism, technophobia and/or the fear of large numbers.

In this second part of the chapter we would like to paint a more nuanced picture. Here we introduce four different ways in which statistical approaches are employed by constructivists: (i) to develop sophisticated graphics that can reveal new descriptions of complex social phenomena; (ii) to recognize and leverage the power of our presuppositions and initial knowledge in Bayesian approaches; (iii) to document the important role that social context plays in generating the patterns that interest us as social scientists; and (iv) to map, quantitatively, and in a systematic and comparative way, the nature of political and social discourse.

The first two examples receive more attention, as they are about developing new approaches to overcome the many shortcomings of traditional regression techniques (from a constructivist perspective). The latter two examples are introduced briefly, as they tend to downplay those shortcomings and employ traditional statistical approaches to the types of questions that are more relevant to constructivists.

Descriptive Statistics and Quantitative Graphics

Done properly, statistics can present a large amount of information in various patterns that allow us to better understand the role of individual pieces of information. To the extent that statistics-based graphics allow us to see the role of the particular in larger patterns, their use is consistent with a constructivist approach: in effect, quantitative graphics can facilitate a hermeneutical tacking between the general and the particular.

To understand this potential, we can begin by considering the role of an 'outlier' in statistical analyses. An outlier is defined in terms of its relation to the normal distribution of a population (recall the role and influence of Thandeka (T) in Figure 4.4). To capture and understand an outlier, the researcher must first define the 'inliers'. With just a little reflection we can see how this process is not much different from the way in which constructivists (recall Geertz) use implicit comparisons with other cases to define the uniqueness of a case (from which we can proceed to understand the role of the particulars, given that case). This process is clearly demonstrated with Anscombe's (1973) celebrated contrast of data in tabular and bivariate scatter plots.

As illustrated in Table 11.1, Anscombe begins by providing four columns of (apparently) similar data: A, B, C and D. We are then told that a single linear description fits all four rows of data: $Y = 3 - 0.5X$ (before the residuals are examined). So far, the quantification has not provided any greater understanding of the phenomenon (whatever it is), as the reader is lost in the particulars, unable to see any larger pattern. It is only when the numbers are presented in two-dimensional Cartesian space (as is done in Figure 11.1) that we begin to understand how radically different each row of data actually is, and the patterns (or unique logic) that is associated with each. At this point, and only at this point, is it possible to reveal the outlier(s) – or even whether there are outliers – and to come to see the unique differences that separate the columns (A, B, C and D) of data.

This example should appeal to (and please) the constructivist, because constructivists tend to shun the standard, the simple and the average. In other words, constructivists tend to be holists. As such, they are likely to embrace the methodological assumption that social phenomena must be grasped as complex units – as 'systems' and/or 'structures'. On the basis of this assumption, the properties of a social system cannot be determined or explained simply by the sum of its component parts.

It is often assumed that statistics can only capture simple aggregations of parts, but if this were true, statistics could not play a very useful role in holistic analyses. This assumption is not true, however: Edward Tufte, a statistician who also champions holism, has worked hard to show how descriptive statistics can be a very useful tool for conveying the whole picture, or the larger story. In particular, Tufte's work on visual displays (1983, 1997) provides us with several illustrations of how graphical depictions can help us to better understand the nature of a situation by placing particular pieces of information in a useful, interpretive context or pattern. But this work began long before Tufte.

We can begin by considering a classic example of descriptive statistics: Dr John Snow's 1855 plotting of the location of cholera deaths in central

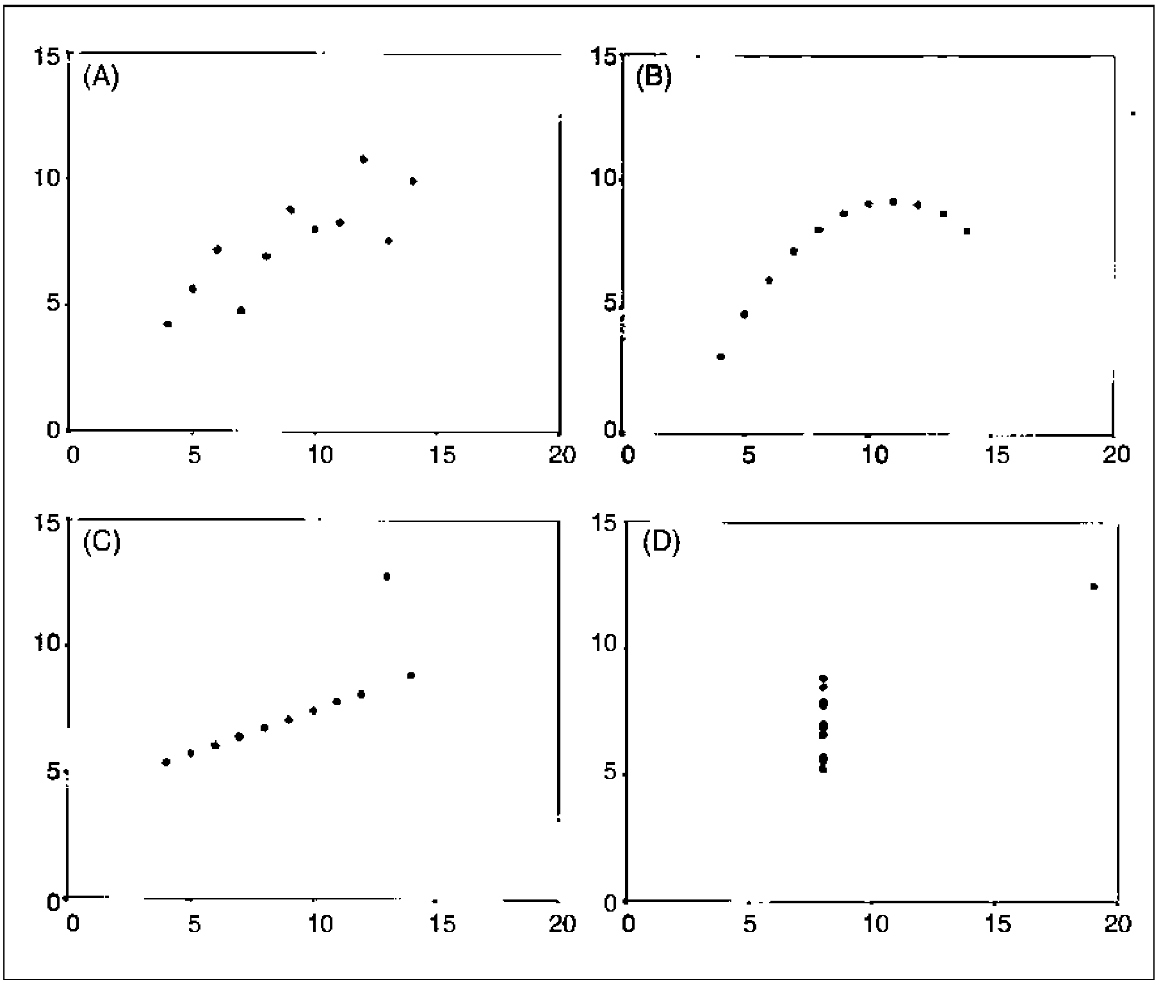
Table 11.1 ***Four columns of data***

A		*B*		*C*		*D*	
X	*Y*	*X*	*Y*	*X*	*Y*	*X*	*Y*
10.0	8.04	10.0	9.14	10.0	7.46	8.0	6.58
8.0	6.95	8.0	8.14	8.0	6.77	8.0	5.76
13.0	7.58	13.0	8.74	13.0	12.74	8.0	7.71
9.0	8.81	9.0	8.77	9.0	7.11	8.0	8.84
11.0	8.33	11.0	9.26	11.0	7.81	8.0	8.47
14.0	9.96	14.0	8.10	14.0	8.84	8.0	7.04
6.0	7.24	6.0	6.13	6.0	6.08	8.0	5.25
4.0	4.26	4.0	3.10	4.0	5.39	19.0	12.50
12.0	10.84	12.0	9.13	12.0	8.15	8.0	5.56
7.0	4.82	7.0	7.26	7.0	6.42	8.0	7.91
5.0	5.68	5.0	4.74	5.0	5.73	8.0	6.89

$N = 11$
Mean of Xs = 9.0
Mean of Ys = 7.5
Equation of regression line: $Y = 3 - 0.5X$
Standard error of estimate of slope = 0.118
$\tau = 4.24$
Sum of squares $x - \bar{x} = 110$
Regression sum of squares = 27.50
Residual sum of squares of Y = 13.75
Correlation coefficient = 0.82
$\tau^2 = 0.67$

Source: Anscombe (1973).

Figure 11.1 ***Four rows plotted***

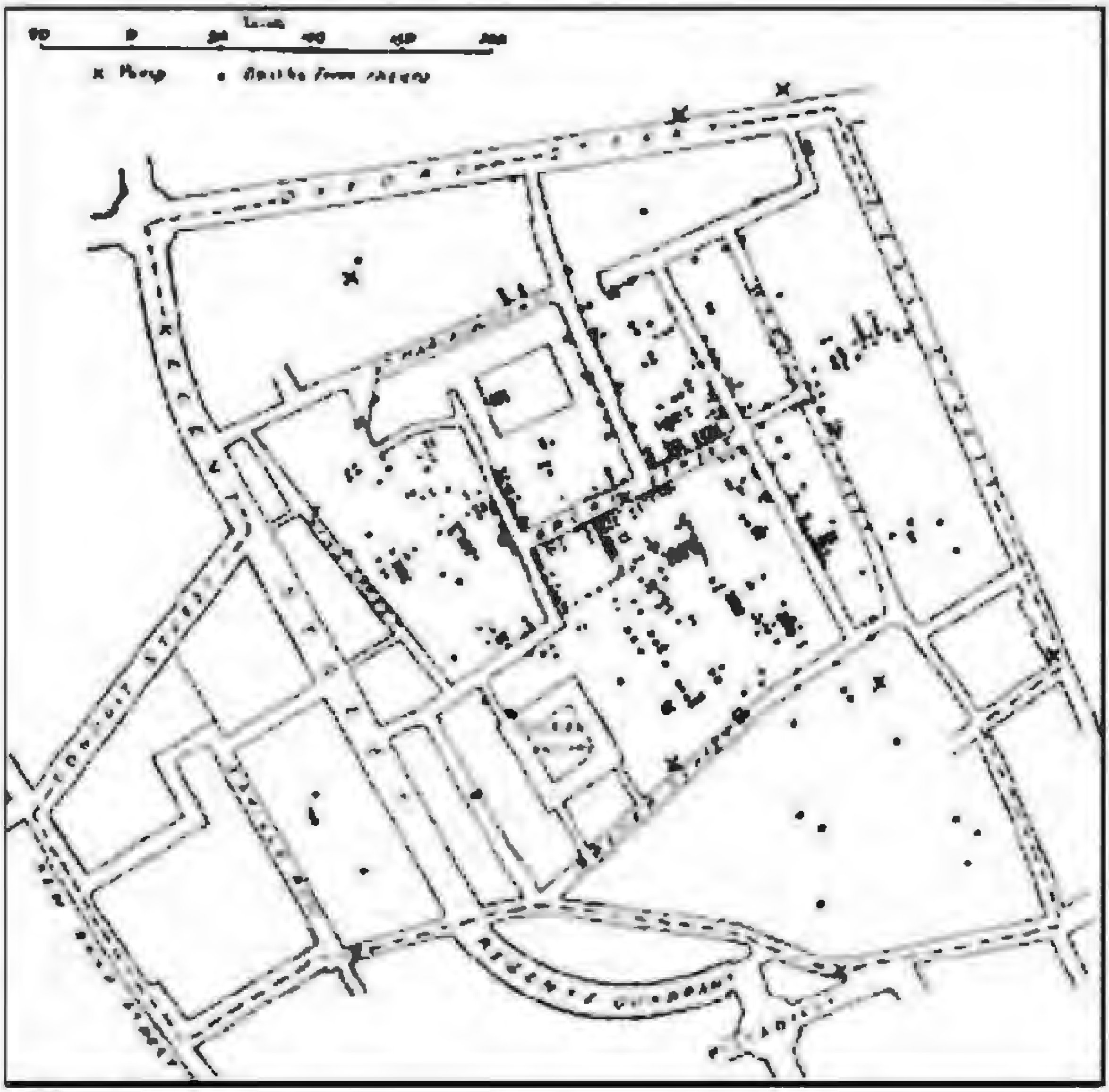


Source: Anscombe (1973).

London. Dr Snow collected cholera data and superimposed a tally of the number of deaths on to a city map of London, as shown in Figure 11.2. In so doing, he was able to show how most of the deaths were located in the vicinity of a common water pump on Broad Street. Consequently, Snow had the handle of the contaminated pump removed, and was credited with ending an epidemic that had already claimed more than 500 lives (see Snow, 1855; Gilbert, 1958). Though there are several ways that this information could have been conveyed to help determine the cause of the cholera outbreak, this graphical depiction provides strong inter-ocular (it hits you right between the eyes) support for the water-pump hypothesis.

Étienne Jules Marey's remarkable (1878) *La métode graphique* provides a phenomenal selection of graphs, two of which have become especially noteworthy. The first is a train schedule for the Paris to Lyon route in the 1880s, usually attributed to the French railway engineer Ibry. In this schedule, reproduced as Figure 11.3, we see arrivals and departures

Figure 11.2 ***Dr Snow's cholera map of London***



Source: Snow (1855).

from a station along the horizontal axes, and the length of a stop at a given station is shown by the length of the horizontal line. Individual stations are separated in proportion to their actual distance apart. As a consequence, the slope of the line reflects the speed of the train (in other words, faster trains have lines that are more vertical). When two trains pass each other going in opposite directions, this is indicated by the intersection of two lines, providing the time and place of the intersection.

Marey's graphical display provided the traveller with an enormous amount of information about the relationship of particular pieces of information in one simple drawing (for example, the time of departure of any given train, from any stop), in the light of the general pattern of rail connections between these cities. It is difficult to think of a more efficient way of capturing the complexity of detail that is exhibited in

Figure 11.3 *Ibry's Paris–Lyon train schedule*

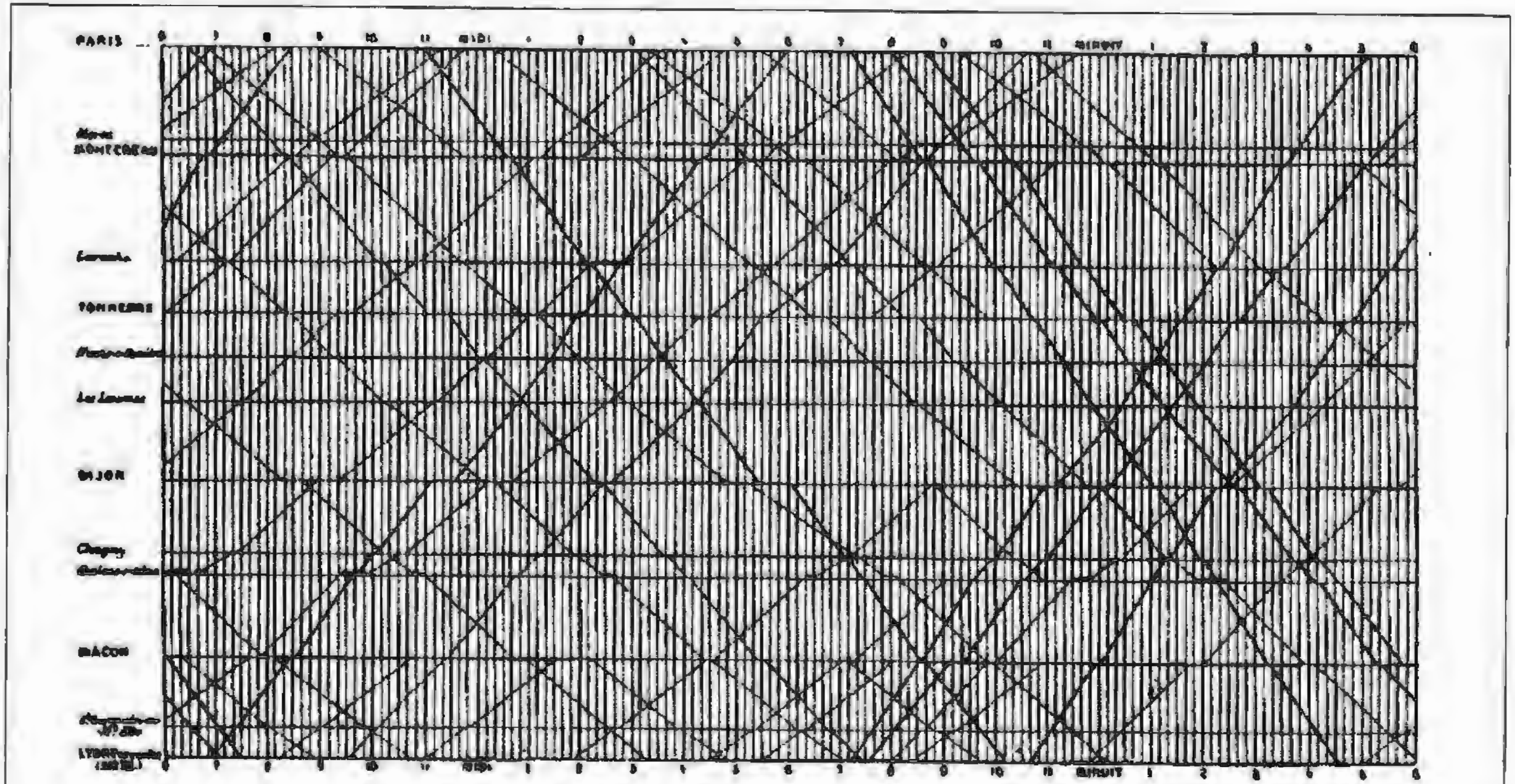


Fig. 7. Graphique de la marche des trains sur un chemin de fer, d'après la méthode de Ibry.

Source: Marey (1878, p. 20).

this graphic design. A verbal depiction of this display is almost entirely useless, as the reader would be overwhelmed by the details and remain blind to the general pattern (for example, which is the fastest train from Paris to Lyon?).

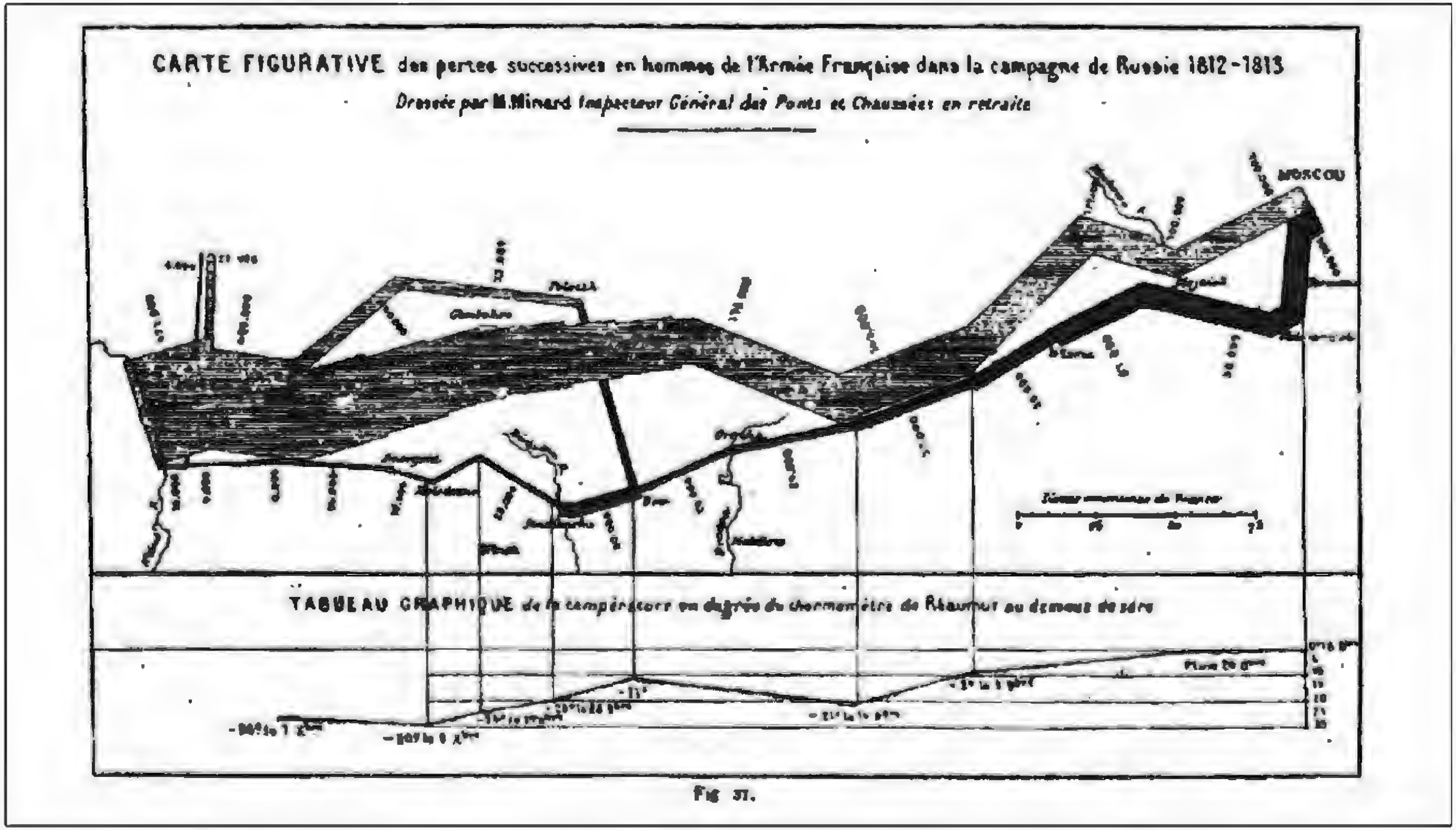
Another example from Marey is provided by Charles Minard's famous depiction of the fate of Napoleon's army in Russia, as shown in Figure 11.4. For Tufte (1983, p. 40), this 'may well be the best statistical graphic ever drawn'. Minard combines data maps and time series to depict Napoleon's costly campaign against Moscow. He includes information on a number of important variables, including diminishing troop sizes, troop movements over time, temperature and important dates. With one glance, the viewer gets a remarkable overview of the relationship between some of the most relevant factors to understand Napoleon's march on (and retreat from) Moscow. With a second glance the viewer can focus on a particular factor (for example, temperature) to see how changes in it are related to the size of the retreating army. Minard's figure is an excellent example confirming the old saying that a picture is worth a thousand words.

The marvel of maps and graphics is not restricted to the distant past. We remember the excitement we experienced when we saw social scientists beginning to employ new moving-picture techniques to the study of old problems. In 1997, Michael Ward and John O'Loughlin employed an early version of Shockwave Flash animation to illustrate the diffusion of democracy over time (from 1946 to 1994). While the technology appears dated now (the link is still active at www.colorado.edu/IBS/GAD/diffmov.exe), it is still wonderfully revealing to watch the pulsating movements of democratic zones over time. The data behind this film clip were not new; indeed, individual snapshots had long been available to the social scholar, but by showing how these pictures change over time, we see the complexity of the patterns as they develop.

On first viewing this film on the internet, we marvelled at the presentation: we played it over and over again, and saw developments that were much more rapid and startling than the literature (to date) had described. With apologies to Fukuyama (1992), the film provides no sense of the world's inevitable march of history toward liberal democracy. Here, instead, is a much more nuanced picture, capturing the ebbs and flows of the democratic tide (à la Huntington, 1991).

All these examples use descriptive statistics in the form of graphs or illustrations to allow the reader to interpret and digest a great deal of information. The nature of the presentation allows the critical reader to examine particular pieces of information (the location of individual cholera deaths, the time of departure from Lyon, the size of Napoleon's troops on a given date, whether a given state was democratic in 1978

Figure 11.4 *Minard's depiction of the fate of Napoleon's army in Russia*



Source: Marey (1878, p. 72).

and so on) in the light of the larger pattern that the graphics' authors wish to convey. The graphs encourage us to move back and forth between the individual data and the general patterns, of which they are an integral part.

It is also worth noting that each graphical presentation is unique. Each graph was designed with an understanding of the particular problem in mind. These are not off-the-shelf graphical depictions, and it is not certain that any of these presentations would work equally well to tell a different story. The authors' understanding of the phenomena in question provided them with a unique perspective from which to design a new form of presentation. It was their nearness to the phenomenon that allowed each author/designer to see that a graphical presentation would be more useful than, say, a verbal (or algebraic) depiction of the problem at hand.

The utility of these sorts of graphic approaches is spreading rapidly, as new technologies make it easier to generate sophisticated graphical depictions with easily accessible data. Indeed, the popularity of sites such as Hans Rosling's Gapminder (www.gapminder.org) provides evidence that students, policy-makers and academics are eager to exploit the utility of sophisticated statistical graphics to uncover new and insightful patterns in the world. To the extent that these sites and programs allow the researcher to tack back and forth between the general patterns evident in the evolving pictures, and the particular pieces of data that make up those patterns, they lend themselves to many types of constructivist projects.

Bayesian Statistics

The Reverend Thomas Bayes (1702–61) is credited with developing a method that has become increasingly popular in a wide variety of fields, ranging from archaeology to computing. The essence of his approach was to provide a mathematical rule for updating existing beliefs in the light of new evidence – or combing new data using existing knowledge or expertise. Constructivists find this approach useful as it allows them to incorporate prior knowledge about the subject (the whole) when first examining the particulars. Indeed, as Andrew Bennett (2008) has argued, Bayesian inference is akin, in many important respects, to process tracing. More to the point, Bayesian statistical approaches are quite unlike the statistics employed in a naturalist methodological approach (which can help to explain why there has been so much resistance to Bayesian statistics from naturalist scientists).

Bayes used the example of a newborn baby who works out the probability of the sun rising with each passing day (babies were apparently more intelligent in the 1700s...). Initially, the baby assumes that the chance of the sun rising the following day is 50:50. To signify this, she puts two marbles, one white and one black, into a bag. The following day, the sun rises again, and the child puts a white marble in the bag. The probability of picking a white marble (the chance the sun will rise) increases from one half to two-thirds, on the basis of the new information. As the baby grows, the number of white marbles in the bag increases. By the time the infant is a fully-grown human, she can be almost certain that the sun will rise every day. Bayes' point is simple – by mixing experience with prior expectations we are able to produce better predictions about the probability of future events occurring. Though the focus here is on prediction, it is also possible to say that the adult human has generated a better understanding of the likelihood of the sun rising the next day (than she did when she was just a baby).

Consider another example, from the realm of opinion studies. Opinion polling is often described to students in terms of a simple 'urn' model. The student is asked to imagine an urn filled with balls of two distinct colours. Red balls can be said to represent voters for the Red Party, and green balls represent voters for the Green Party. Choosing a sample from the electorate and asking them about their preferences (red ball or green ball?) is akin to choosing a ball, randomly, from the urn. In theory, the practice is repeatable, and the composition of the urn is uncertain. If, however, we can use information about how voters have tended to vote in other elections, this can give us important information about the 'contents of the urn'. (Remember, the urn is not filled randomly with balls; we simply don't know its contents.) In the same way that the baby discussed above could incorporate new information to improve predictions about future sunrises, opinion researchers can use prior information to make better predictions about the urn's likely content. In particular, the Bayesian scholar can incorporate this prior information into an *a priori* distribution about the electorate. This *a priori* distribution is combined (via Bayes' theorem) with the outcome of the sample. We can then produce what is called an '*a posteriori*' distribution – which provides us with a firmer foundation from which to make predictions.

Bayesian logic (or what some people call folk-Bayesianism) is being used increasingly by strategists and intelligence analysts whose job it is to assess threats to a nation's security. This was evident in an international affairs example from the early part of the twenty-first century. During the late 1990s, America's government was constantly frustrated by the Iraqi president, Saddam Hussein, who had played cat-and-mouse games with international weapons inspectors and successfully exploited UN sanctions for his own political advantage. When President George W. Bush took charge of US foreign policy in early 2001, he held the firm opinion that his country's policy toward Saddam Hussein was untenable: not only had containment proved a failure, but Saddam had undermined America's prestige and destabilized a volatile region. In time, the Bush administration concluded that the Iraqi dictator must be removed, and sought support among its allies to do so.

As most readers will be aware, many of America's European allies were not so eager. France, in particular, was reluctant to embrace a policy that aimed at toppling an established government in a terror-prone region. Besides, the French wanted to make sure that Saddam Hussein did, indeed, represent a threat to his neighbors. In order to do this, the French intelligence services adopted a two-step assessment program. The first step was to gain access to information about Iraqi weapons programs. This was done by using traditional diplomatic means: France made it clear (to both Bush and Saddam) that it would support the US policy if Saddam refused to admit UN weapons inspectors into the country. Thus, France pressured Saddam to allow inspectors into Iraq.

The second step was analytical and based on information gleaned from the reports of the UN weapons inspectors. The inspectors travelled around Iraq visiting military installations before delivering a series of largely negative reports. In one case after another, the inspectors found no obvious indication of nuclear weapons programs and, more surprisingly, they found no indication of biological or chemical weapons production either. Members of the UN Security Council had access to these reports, but they read them under different lights. Analysts from the Bush administration trusted neither Saddam nor the UN; they read the reports, but insisted stubbornly that Saddam Hussein had been toying with uncommonly naïve inspectors.

The French analysts read the reports in a different way: they applied a Bayesian logic of probability and continually upgraded their overall threat assessment. In other words, every time the French received a report of an inspection at a particular site that had failed to turn up any suspicious materials, the French analysts reduced their threat assessment by a small amount – they added another white ball to the urn, so to speak. After having read several negative reports from a substantial number of weapons sites, the French threat assessment had been so significantly reduced that they no longer believed the American assessments to be accurate.

We can now begin to appreciate how a Bayesian approach attracts constructivists. By incorporating new information, the analyst's picture of the 'whole' is continually evolving. This understanding of the whole is used to update interpretations of the (new) particular data that are always coming in. McKeown (1999, p. 180), referring to Gooding (1992), describes Bayesianism in terms of the researcher who 'move[s] back and forth between theory and data, rather than taking a single pass through the data'. This process is very similar to the hermeneutic approach, as described in previous chapters, and is quite different from traditional statistical approaches.

Bayesian inference is different from traditional (statistical) inference in at least two ways. First, Bayesian inference is built on the concept of subjective probabilities. By introducing subjective probability, the analyst is allowed to include his own degree of belief about an uncertain event in the estimation – it is not a fact describing the real world, but rather a personal statement about the analyst's level of certainty or confidence. These subjective probabilities are then added to the sample data to produce 'posterior probability' statements about the parameters of a statistical model. In doing so, analysts blur the solid line that usually separates the naturalist's facts from his values. These posterior probability statements express the researcher's degree of belief in the parameters (given the data and the prior subjective probabilities). By limiting the variation in parameters in this way, the analysis can focus on areas where there is disagreement or less understanding.

Second, Bayesian inference allows for the introduction of prior information (in addition to the sample) when making inferences. Both of these differences present radical challenges to the way in which naturalists assume we should approach our subject matter (though it is not at all unlike the way much statistical work is actually done). More to the point: an awareness of the fact that analysts approach their research with preconditions (influenced by normative positions as well as *a priori* knowledge), and that these preconditions frame our understanding of the world under study, are two central characteristics of constructivism.

Bruce Western and Simon Jackman (1994) show how Bayesian inference can address some of the most common prohlems of statistical inference in comparative political research. They do this by applying a Bayesian approach to evaluate the competing claims of two comparative statisticians (Michael Wallerstein and John Stephens) – in a context characterized by the relative absence of information. Using Bayesian inference, Western and Jackman replace the regression coefficients estimated from Wallerstein and Stephens' data set with a set of regression coefficients that Western and Jackman believe are most probable (*a priori*). They then nse this *a priori* information, combine it with the sample information, and produce a multivariate normal posterior distribution for the coefficients.

Such *a priori* beliefs can be developed on the basis of information that is not easily quantifiable. For example, Western and Jackman suggest that Stephens' deep historical grasp of the Swedish conditions allow him to generate realistic priors. Indeed, '[t]he Bayesian approach allows the information informally introduced into the analysis by Stephens and Wallerstein to enter formally through a prior distribution' (Western and Jackman, 1994, p. 417). This has radical methodological consequences. By allowing for this sort of interpretive variation, we can expect that no two researchers will prefer one prior probability distribution over another. In other words, the analyst no longer needs to try to understand a (singular) Real World – the world itself is allowed to vary in line with these *a priori* expectations. In effect, Bayesian statistics allow individual interpretations to re-enter the statistical project. In addition, the emphasis on prior information encourages Bayesian statisticians to familiarize themselves with local contexts before setting off on any statistical journey.

It is important to emphasize that Bayesianism is not a panacea, either for the naturalist or for the constructivist. For the constructivist, Bayesian approaches suffer from many of the shortcomings that are common to other statistical projects. Not only do many Bayesians strive for stronger predictions and application to causal analysis, but the approach suffers from a number of operational and philosophical shortcomings. Primary among these is the distance necessarily created between the researcher and the context she studies. To most constructivists, Bayesian statistics are still a long way from the serendipitous storytellers described in Chapter 9.

For the naturalist, however, the integration of subjective priors spoils the scientific credentials of the Bayesian approach. It is for this reason that the renowned statistician, Ronald Fisher, believed an experiment interpreted with prior information 'would carry with it the serious disadvantage that it would no longer be self-contained, but would depend for its interpretation from experience previously gathered. It could no longer be expected to carry conviction to others lacking this supplementary experience' (1953, p. 69). Similarly, Leamer (1994, p. xi) finds the Bayesian approach less attractive because '[i]t may in fact increase the burden by requiring analysts to think consciously about their "priors"'[!!].

Thinking consciously about one's priors, or one's presuppositions, is – of course – a foundational component of the constructivist's approach. Applying a Bayesian statistical approach forces the analyst to be explicit about his normative and epistemological priors, and requires him to consider the general nature of the phenomenon when studying its particular parts. This, in turn, allows for the possibility of having various, even competing, interpretations of a given event.

Social Patterns of Interest

The previous two examples have considered ways of expanding the statistician's toolbox so that it can be more responsive to constructivists' priors. This is one response by constructivists to the growing influence of statistical techniques. A second response is to employ traditional regression techniques on issues that are particularly important to constructivist scholars. Here the analytical focus tends toward explaining the contextual setting that creates the patterns we see (rather than simply documenting the patterns themselves). As these sorts of statistical applications have already been described in Chapter 4, there is no need to discuss further the specific techniques employed. A short illustrative example should suffice.

One of the fundamental insights of constructivism is the role that socialization can have on how we perceive the world. A central instrument in that socialization process is education, and we know that professional training plays a significant part in influencing how an individual assesses the appropriateness of a given observation.

Professional training does more than simply transfer technical knowledge; it actively socializes people to value certain things above others. Doctors are trained to value life above all else. Soldiers are trained to sacrifice life for certain strategic goals. Economists, ecologists and lawyers all carry different normative biases systematically instilled by their professional training (Finnemore and Sikkink, 1998, p. 905).

Knowing this, we should not be surprised to find that economists view the world differently than do doctors, soldiers, lawyers, and even political scientists. There are differences too among economists. Consequently, we can expect that an economist (type 1) trained at an institution that emphasizes neo-classical approaches will see the nature of the economic world differently than an economist (type 2) who trained at an institution that reflected another (say, Keynesian) world view. Should type 1 economists come to dominate influential policy positions, we might expect the nature of the policies they influence to change in the light of the approach they learned in training.

This way of thinking generates a relatively straightforward empirical test, where traditional regression techniques can be used to measure the influence of education on policy outcomes, controlling for other potential explanations. Indeed, this is just the sort of work that Jeffrey M. Chwieroth does: he employs standard regression techniques and innovative new databases to map the educational background of national (2007a) and international policy-makers (for example, at the International Monetary Fund: Chwieroth, 2007b) to explain change in policy outcomes over time. In his study of 29 emerging markets from 1977 to 1999, Chwieroth (2007a, p. 445) shows how the 'formation of a coherent policymaking team of neoliberal economists significantly influenced the decision to liberalize' the capital accounts in those countries.

In examples such as these, the constructivist statistician discounts the disadvantages inherent to regression analysis in order to exploit its potential to map different types of patterns. The focus now is not on documenting the neo-liberal turn in economic policy-making, but to understand this turn in terms that are close to the constructivist's heart: by way of contextual influence (here, education).

Quantifying Discourse

A final example considers the efforts to generate empirical measures and statistical approaches that can map the nature of political discourse. As we learned in Chapter 9, constructivists employ discourse analysis to identify the rules and reasons behind all kinds of dialogue, and these analyses are most commonly conducted in ways that facilitate contextual interpretation. In other words, discourse analyses tend to lie very close to the empirical ground, so that the analyst can gain insight and empathy regarding the meanings being conveyed in the discourse.

But many scholars also realize the utility of mapping larger and longer discourses. For example, much of contemporary democratic theory focuses on the role and scope of deliberation, where this is understood to be a public form of reasoning, or what Jürgen Habermas (1989 [1962], p. 249) refers to as a 'public sphere'. After all, it is through the process of deliberation that interests are formed, and it is through public deliberation that we develop our understanding of collective rationality and inter-subjective reality (Habermas, 1984; for a good review of the competing forms of deliberation, see Bächtiger *et al.*, 2010). Describing how these interests and common understandings are generated is an important part of a constructivist research agenda. But discourses at this level of aggregation are difficult to map with the sorts of narrative approaches described in Chapters 9 and 10.

In recent years, a number of competing quantitative approaches have been developed to capture the nature and scope of deliberation in larger contexts (see the special issues of *Acta Politica* (2005, vol. 40, nos 2 and 3) on 'Empirical Approaches to Deliberative Democracy'; Curato, 2008; Midthjell, 2010). The most prominent of these is probably the Discourse Quality Index (DQI) developed at the University of Bern's Center for Interdisciplinary Deliberation Studies (Steenbergen *et al.*, 2003; Steiner *et al.*, 2005). This index has been used to measure, among other things, the level of deliberation in parliamentary debates in Germany, Switzerland, the UK and the USA (Bächtiger, 2005; Steiner *et al.*, 2005); the level of deliberation in the European Parliament (Roger, 2010); and the effects of different institutions on the development of deliberative ideals (Bächtiger and Hangartner, 2007).

While many constructivists are critical of the distance that must necessarily separate the indexes used to capture such large-scale forms of deliberation, the existence of such indicators allow constructivists to better understand how different types of institutions affect the quality of deliberation or discourse, or how the nature of discourse can affect various outcomes. In short, these sorts of approaches allow us to measure the integrity of the deliberation process at higher levels of aggregation.

As with the previous example, of measuring the influence of education on policy outcomes, constructivists are willing to accept some of the inherent limitations of regression analysis (given their ontological priors) in exchange for the opportunity to map out how discourse and deliberation affects political understanding. Regression analyses can provide that sort of mapping.

Conclusion

We have come a long way from the narrative approaches described in Chapter 9. As a consequence, most constructivists don't feel very comfortable in the foreign world inhabited (and created) by statisticians. For this reason, we have tried to illustrate how some forms of statistics can be employed in a way that is consistent with constructivists' core beliefs about the social world, and how it can be understood. By extending the way that constructivists use comparisons – in particular, by using them to establish associations and patterns for further enquiry, and by recognizing explicitly the source of these patterns – we can begin to recognize how statistical tools can be used by constructivists. After a brief attempt at contextualizing the history of statistics, our discussion has focused on four different examples. Two of these – quantitative graphics and Bayesian statistical approaches – show how statistical techniques can be transformed in ways that make them more sensitive to constructivist beliefs about the nature of the social world. Our discussion of the other two examples – mapping social patterns and discourses – was much shorter, as we needed only to show how traditional statistical techniques (described in Chapter 4) can be employed on the types of questions that motivate much constructivist analysis. In the next chapter we move one step further away from constructivists' home territory to look at how experiments can be used to further our understanding of social phenomena.

Recommended Further Reading

For those who read French, we recommend returning to the original source of much modern graphical study: Étienne Jules Marey's impressive *La métode graphique* (1878). Edward Tufte's *The Visual Display of Quantitative Information* (1983) and his *Visual Explanations* (1997) provide more contemporary examples. Howard Wainer's (2005) *Graphic Discovery: A Trout in the Milk and other Visual Discoveries* tells an engaging history of graphic presentation. For criticisms of the statistical world view and its influence in social science, read Timothy McKeown's 'Case Studies and the Statistical Worldview' (1999). As mentioned in the text, Western and Jackman's 'Bayesian Inference for Comparative Research' (1994) provides a nice glimpse of the power of a Bayesian approach.

Chapter 12

Interpretive Experiments

In an experiment, researchers control the conditions under which their study takes place, as well as the variables they explore. As we saw in Chapter 3, it is this type of control that allows researchers greater certainty about the nature of the causal relationships they test. This, in turn, produces firm predictions about the nature of the Real World. As Kathleen McGraw (1996, p. 770) notes: 'Structurally, experiments are marked by a deliberate intervention in the natural, ongoing state of affairs.'

This willingness to intervene deliberately and to manipulate the empirical context of a given social phenomenon is a hallmark of naturalist science. The constructivist, by contrast, wants to avoid muddying the contextual waters. She would argue that when an experimenter manipulates the contexts surrounding a phenomenon, she undermines the very ground in which interpretation and meaning are anchored.

Thus, in theory, it would seem rather preposterous to consider experimental methods from a constructivist perspective. In social science practice, however, we find much experimental activity that is consistent with constructivist approaches – and is used to support them. This activity can be organized in three sections.

As we saw with statistical studies in Chapter 11, it is possible to use traditional naturalist-based methods to uncover contextual features that are of interest to constructivists. Thus some constructivists have been willing to use traditional experimental methods (as described in Chapter 3) to secure knowledge about how different contexts affect our perceptions of the world. Indeed, this movement mirrors a broader interest by philosophers in using experiments in ways that help us to understand how people ordinarily think about foundational concepts in philosophy (see, for example, Phillips, 2011). In this way, experiments are being used instrumentally, to make a constructivist point.

But constructivists are also willing to use their insight, tempered with an experimental attitude, to develop new ways of understanding the world. This second type of approach employs imaginative thought experiments, based on deep local knowledge, to direct the scholarly project in new directions.

Finally, constructivists have employed innovative experimental designs, a commitment to contextual familiarity, and explicitly normative agendas to improve local conditions in the context under study. The final part of this chapter considers experimental action-research projects with explicitly normative objectives.

Seen in this light, experimental projects are useful means of linking constructivist and naturalist research agendas: they highlight the advantages and shortcomings of both methodological traditions, and provide common ground for future collaboration.

Contextual Experiments

Traditional forms of experimentation can be used to support larger constructivist projects. Such experiments can be seen as attempts to bridge the naturalist and constructivist traditions – generating the sort of knowledge that naturalists respect about the important role that context plays in influencing how we perceive and understand the world. This section will consider how experiments have been designed to measure three types of contextual influences on our perceptions of the world: the influence of groups, people in authority, and the passing of time (or generations). The complexity uncovered in these experiments calls into question some basic methodological assumptions of naturalist social science.

Group Effects

One of the core tenets of constructivist social science is an explicit recognition of the role that context plays in influencing how we come to perceive the nature of the world we study; patterns that are evident in one context may not be apparent in another. Context matters, and constructivists are interested in documenting the size and nature of contextual effects.

These sorts of contextual effects are perhaps most evident in the topsy-turvy world of social trends. As parents of teenagers, we have on more than one occasion pondered why a particular pop song (for example) becomes a runaway best seller. From our particular vantage point, it is difficult to see that the extreme popularity of a given song is related to its unique musical qualities.

The development of novel social media has opened up new possibilities. Matthew Salganik *et al.* (2006) have used innovative experiments to track the effects of external social conditions. They established an artificial music market, in which 14,341 subjects were exposed to controlled stimuli in the form of downloadable songs. The researchers then observed the participants' reactions to the stimuli, looking carefully for patterns in the way they downloaded songs.

They did this by creating a fictitious cultural market ('Music Lab') in the form of a website, where users could listen to, rate and download 48 songs from unknown bands. As users entered the site, they were randomly assigned to one of two experimental groups (one treatment, one control). The two groups varied only in one respect: the availability of information on the previous choices of others. In short, users in the control group were exposed to the music (that is, the names of the bands and their songs), without knowing what other users thought of it. When listening to the music, these users were asked to rank songs (one star for 'I hate it' to five stars for 'I love it'), before being given an opportunity to download the song.

Members of both groups were presented with the same list of band names and songs, but only the treatment group was told how many times each song had been downloaded by other users. Thus the members of the treatment group could gain some insight into what other users thought was good music (as indicated by their willingness to download that music). What the investigators discovered was that both the inequality and the unpredictability of success increased concomitantly with the strength of the social influence. In other words, a song's success was determined only partly by its quality: the best songs rarely did poorly, and the worst songs rarely did well, but any other result was possible.

In conducting studies such as these, we learn that social patterns are not simple mechanistic aggregations of individual preferences. In the words of one of the study's co-authors, 'you could know everything about individuals in a given population – their likes, dislikes, experiences, attitudes, beliefs, hopes, and dreams – and still not be able to predict much about their collective behavior' (Watts, 2011, p. 79).

We hope it is evident that the design described here follows closely the design of the classical experiment. We hope it is equally clear that the reasoning that guided this experiment is quite foreign to the reasoning that lies beneath most naturalist approaches. Salganik *et al.* (2006, 2009), who conducted these experiments, do not describe themselves as constructivists, and their research design is firmly grounded in naturalist terrain. Yet their results provide fodder for constructivist arguments about the crucial role that social context plays in our perception of the world, and how individual pieces of the social world do not aggregate in simple, fixed or mechanistic ways.

Pygmalion Effects

There is a story about Claude Monet who, on a rainy day, remained inside his house at Giverny and painted what he saw through his living room window. The result was a beautiful naturalistic painting of his garden. He depicted his object accurately, with one exception: he omitted a large tree because, according to traditional rules of composition, it would have destroyed the balance of the picture. Increasingly, Monet grew dissatisfied with the asymmetry between the garden and his depiction of it. One day he yielded to his aesthetic urges, went out into the garden and cut down the tree.

The story may not be true. But if it is fabricated, it is invented in the constructivist spirit. Given their emphasis on author–context relations, constructivist scholars tend to be especially aware of the effect that the author or investigator might have on the object observed – and on the outcome of his work. The experiments conducted in 1927 by the psychologist, Elton Mayo, for the Western Electric Company at its Hawthorne plant – discussed in Chapter 3, above – is a famous example of this. However, the phenomenon predates the Hawthorne case. These effects have a long and distinguished pedigree in Western thought, and their influence has been mapped through many different channels.

In the classic narrative poem, *Metamorphoses*, which describes the history of the world since its creation (in fifteen books!), the Roman poet Ovid (43 BCE – AD 17/18) introduces Pygmalion, a sculptor and prince of Cyprus. Pygmalion, we are told, created an ivory statue of his ideal woman, Galatea (see Figure 12.1). As it turns out, Pygmalion was quite the sculptor: his efforts were so successful, and Galatea was so captivating, that Pygmalion begged Venus, the golden goddess, to breathe life into the statue – so that Pygmalion could make it/her his own. Venus obliged, and the Pygmalion Effect was born.

Ever since, the Pygmalion Effect has become shorthand for the way that the author of an account can influence its outcome. A similar sort of effect has been documented in experimental studies aimed at capturing teacher influence on student performance. The most famous of these are associated with the work of Robert Rosenthal and Lenore Jacobson (1968), to document the way that teacher expectations can generate self-fulfilling prophesies. In these experiments, teachers were (falsely) told that they were using a test which could identify students who would likely be late bloomers (that is, show a sudden and dramatic improvement in their learning). But in the test, the late bloomers were actually selected at random. This had the effect of controlling for (unconscious) teacher influence.

The Rosenthal and Jacobson experiments revealed that those students who were identified as late bloomers gained more IQ points than did the control students. In fact, the investigators found that the teachers were somewhat hostile toward students in the control group. In the words of Rosenthal and Jacobsen (1968, p. 70): 'The difference between the

Figure 12.1 *Section from* Metamorphoses

The golden Goddess, present at the pray'r,
Well knew he meant th' inanimated fair,
And gave the sign of granting his desire;
For thrice in chearful flames ascends the fire.
The youth, returning to his mistress, hies,
And impudent in hope, with ardent eyes,
And beating breast, by the dear statue lies.
He kisses her white lips, renews the bliss,
And looks, and thinks they redden at the kiss;
He thought them warm before: nor longer stays,
But next his hand on her hard bosom lays:
Hard as it was, beginning to relent,
It seem'd, the breast beneath his fingers bent;
He felt again, his fingers made a print;
'Twas flesh, but flesh so firm, it rose against the dint:
The pleasing task he fails not to renew;
Soft, and more soft at ev'ry touch it grew;
Like pliant wax, when chasing hands reduce
The former mass to form, and frame for use.
He would believe, but yet is still in pain,
And tries his argument of sense again,
Presses the pulse, and feels the leaping vein.
Convinc'd, o'erjoy'd, his studied thanks, and praise,
To her, who made the miracle, he pays:
Then lips to lips he join'd; now freed from fear,
He found the savour of the kiss sincere:
At this the waken'd image op'd her eyes,
And view'd at once the light, and lover with surprise.
The Goddess, present at the match she made,
So bless'd the bed, such fruitfulness convey'd,
That ere ten months had sharpen'd either horn,
To crown their bliss, a lovely boy was born;
Paphos his name, who grown to manhood, wall'd
The city Paphos, from the founder call'd.

Source: Ovid (1717 [1], Book x).

children earmarked for intellectual growth and the undesignated control children was in the mind of the teacher.'

Whatever we call them – Hawthorne Effects, Pygmalion Effects or Rosenthal Effects – a good deal of experimental evidence has been collected to document the way in which the actions and beliefs of authority figures can inadvertently influence the behaviour of others. As with the previous example of group effects, these experiments led us to question some of naturalism's fundamental assumptions about the nature of the social world: the social world does not reflect some sort of fixed (albeit patterned) reality, but responds to a host of contextual stimuli, including those stirred up by the investigator.

Generational Effects

In Chapter 9 we learned about the ethnomethodological approaches of Harold Garfinkel (1984 [1967]); approaches designed to understand how people make sense of the world around them. Ethnomethodologists are interested in the problem of cultural persistence, or how social knowledge becomes institutionalized as facts, and is transmitted across generations as such.

There are many ways to account for cultural persistence. Traditional accounts tend to reference the specific characteristics of institutions: how actors comply with the action prescribed by the institution, and where the actors are motivated by a sense of functional necessity, self-interest or internalization. In the 1970s, Lynne Zucker developed a series of experiments to test whether an ethnomethodological approach could provide a better account of cultural persistence. This account recognizes how 'individual actors transmit what is socially defined as real and, at the same time, at any point in the process the meaning of an act can be defined as more or less a taken-for-granted part of that reality' (Zucker, 1977, p. 728). In Zucker's account we find a third example of how experimental techniques are being used to uncover the nature of social patterns in the world, as perceived by constructivists.

In particular, Zucker focused on three aspects of cultural persistence (transmission, maintenance and resistance to change), and developed a series of experiments to test the effects of institutionalization on each (in a context where all the actors in a situation were committed to obtaining an appropriate understanding of the situation). The experiments were designed to capture the variance in degrees of institutionalization (high to low), and how this level of institutionalization affected each of the three aspects of cultural persistence.

For example, to test for the transmission effect, 180 subjects were divided into four groups of 45 members: three treatment groups and one control group. Three generations were used, with fifteen replications in each condition. The subjects were directed to a dark room in which a light appeared to move either smoothly or erratically on the wall. Earlier studies had revealed that subjects to this sort of autokinetic experience develop common understandings of why the light appeared and moved as it did. Zucker's experiment was then expanded to include 'generations', where new subjects were paired up with subjects who had already developed an understanding based on previous experience (in an earlier round, or generation). The nature of the pairing was done to vary the strength of the institutional effect, so that some subjects were exposed to a 'personal influence condition' (that is, a weak institution), others to an 'organizational context condition' (a medium institution), while the third treatment group was exposed to an 'office condition' (a strong institution).

On the basis of these experiments, Zucker was able to demonstrate that the persistence of cultural understanding tends to vary with the degree of institutionalization. Indeed, the degree of institutionalization was shown to affect each of the three aspects of persistence (transmission, maintenance, and resistance to change). This experimental evidence is consistent with an ethnomethodological (read constructivist) understanding of how actors perceive and describe social reality by enacting it, and transmitting it to other actors in the social system: 'The young are enculturated by the previous generation, while they in turn enculturate the next generation. The grandparents don't have to be present to ensure adequate transmission of this general cultural meaning. Each generation simply believes it is describing objective reality' (Zucker, 1977, p. 728).

In this way, experimental approaches are used to complement the sort of in-depth ethnomethodological studies described in Chapter 9, and the path dependency approaches described in Chapter 10. While these experiments are designed to manipulate the interpretive contexts which constructivist usually hold sacred, they provide an opportunity to zero-in on the transmission, maintenance and persistence mechanisms that constructivists expect to find influencing the social patterns we study. In effect, they are generating the sorts of proofs that naturalists require, for an argument that challenges naturalist beliefs about the nature of the social world.

Imaginary Journeys

Throughout the first part of this book, we have maintained that experimentation is the basic method of naturalist science. In the examples above, we have seen how this very naturalist method can be employed to address important ontological questions that are near to the constructivist's heart. While important and useful knowledge is being generated by these sorts of experiments – and it is the sort of knowledge that can build bridges between the constructivist and naturalist camps – these experiments do not really exemplify the constructivist approach, because of their relatively cavalier attitude toward manipulating interpretive contexts.

We think that a second sort of example is truer to the constructivist ideal: the use of thought experiments. After all, much scientific discovery begins in the thin air of imagery. When we run mental experiments – and all scientists do – the laboratory is situated close to home. It is coloured by the world that the scientist knows best and is informed by the experiences the scientist has accumulated during the course of her life. In short, our familiarity with local contexts is an important ingredient when imagining scientific progress.

Three examples can illustrate this point in more detail. The first is provided by a Norwegian lawyer and fur-trapper, who became one of the most controversial archaeologists exploring Norse history. The second example intrudes a Texan cowboy and banker who became a contentious archaeologist of Mayan affairs, and a third set of examples can be found in the deep reservoir of Western social theory.

Discovering Vinland

For decades, Norwegians were convinced that Vikings had crossed the Atlantic Ocean and landed in North America. Indeed, by some of the more enthusiastic accounts, the Vikings had sailed along America's east coast all the way down to Florida and the Bahamas (Prytz, 1991). These claims had one main source: the Viking sagas – which Nordic scholars read with extreme care, every sentence being scrutinized carefully for suggestions and clues. However, as many critics pointed out, the saga texts were too general to support the claims made by Scandinavian historians. Most significantly, no archaeological evidence had been found in the Americas to substantiate the claim that the Vikings had been there.

Helge and Anne Stine Ingstad set out to search for such evidence. While Anne Stine's background was in archaeology (she had turned to it rather late in life), Helge's background was less orthodox: he had originally studied to become a lawyer, but instead struck out across the North Atlantic to try his hand as a trapper, polar explorer and historian. Undoubtedly, this singular combination of life experiences provided the Ingstads with a unique vision of scholarly endeavour.

In the 1950s, the Ingstads began to map the Norse settlements on Greenland. While participating in the archaeological digs there, Helge Ingstad was in the habit of sitting on the front step of an old Viking stone house, looking out over the ocean. From that doorstep he would wonder how the sea and the landscape might have looked a millennium ago, when the house was built and its front step laid. In short, Ingstad tried to imagine himself in a Viking settlement a thousand years earlier. In this imagined context, he came to wonder where *he* would have set sail, had he been Leif Erikson at the end of the first millennium.

After two years of such imaginings, Helge Ingstad came to draw a probable sea route from Greenland to America – to L'Anse aux Meadows, on an island off the east coast of Canada. Visiting the area, Ingstad felt that the North Newfoundland landscape and scenery suited the few geographical descriptions that can be found in the sagas:

> I looked out over the plains towards the islands, and north over the ocean, where Belle Isle looked like a fairy-tale castle, towards the distant blue coast of Labrador, along which the Vinland voyagers of old had sailed south. It was almost a *déjà-vu*, so much was reminiscent of what I had seen in the Norse settlements of Greenland, and on the west coast of Norway – the houses built on ground higher than the surrounding land, with a view of the ocean, the green fields and meadows, the rippling brook in the open landscape, and perhaps also something else, less easily grasped. People from Greenland must have felt at home here. (Ingstad and Ingstad, 2001, pp. 126–7)

Thus, by reading the ancient Norse sagas about Vinland, by studying Viking culture and technology, by considering other prominent theories of Vinland's location, the Ingstads began to form an idea of where the Vikings might have settled in the New World. But a very important piece of this intellectual puzzle was an effort by Helge Ingstad to *imagine* how the Vikings would have evaluated different landing sites in America. In the early 1960s, the couple arrived at L'Anse aux Meadows, after marvelling over how well it fitted the sagas' description of Vinland.

It must have been difficult for the couple to raise funds for a research expedition on the basis of such flimsy evidence. (Just imagine the exasperated reply of a potential funding agent: '*What?!!* You *imagined* that the Vikings would have liked to settle there?'). They persevered, however, and eventually managed to fund an archaeological expedition to the site. Counter to the forecasts of many sceptics, who saw this as yet another wild goose chase, they soon made important discoveries. Before long, their archaeological team uncovered conclusive evidence in the shape of a soapstone spindle-whorl, a Viking ring pin and the remains of a dozen Viking buildings.

By Ingstad's account, the site at L'Anse aux Meadows is, in all probability, the Viking camp mentioned in Leif Erikson's *Saga*. It was here, Ingstad claims, that sailors from an Icelandic trading ship had landed around AD 985, and were the first to describe these new lands to the west. Fifteen years later, Leif Erikson had sailed from Greenland, and wintered at a settlement which the saga refers to as Straumfjord. In the years following, members of his family and a group of colonists had visited the camp. They had built timber and sod longhouses and several smaller buildings – in one of which they made the first iron tools in the New World. It was the remnants of these houses and activities that the Ingstads believed they had found.

Ingstad argued that members of the camp ventured as far south-west as New Brunswick. But it would seem that conflict with the indigenous population obliged them to withdraw from the area and they returned to Greenland within a decade. Norse contacts with the New World continued and knowledge of the new lands likely remained with European sailors, facilitating the reopening of the Atlantic sea lanes in the 1490s.

Which methods did Helge Ingstad use to find Straumfjord? It would be grossly unfair to say that he made a wild stab in the dark. He guessed; but it was an educated guess. In effect, Ingstad's thought process is akin to what Charles S. Peirce described as retroduction, introduced in Chapter 8. His insight is like Kikki Rouget's, born of familiarity. Similarly, Karl Popper might have claimed that Ingstad had made a 'conjecture', informed by deep knowledge of his subject. After all, Ingstad was a trapper and a sailor: he knew the winds and the waters of the Arctic, and drew on his own expert knowledge when he made his conjecture. He was helped by the realization that the Vikings would have thought deeply about where to settle – and he could tap into that thinking/reflective process. Ingstad's conjecture was, in a sense, the result of a series of mental experiments he had run over and over in his mind during the course of several years.

Ingstad started with a few surviving artifacts – the Norse sagas and the stone remnants of Viking settlements in Greenland. He then retroduced a series of probable events that could have produced them. Then, on the basis of the properties that marked this process, he ran mental experiments with the aid of knowledge, logic and a good deal of imagination.

Mayan Decline

A similar process appears to have guided Richardson B. Gill. When he visited the Mayan ruins of Southern Mexico in 1968, Gill was deeply affected by the people and the place. As a result, he resolved to find the reason behind the collapse of the Mayan civilization.

Mayan society had flourished for more than 2,000 years, evolving into an empire before disaster struck around AD 800. Rather abruptly, the construction of pyramids halted and whole cities were abandoned. The most advanced civilization of the Western hemisphere suddenly unravelled, allowing the jungle to reclaim its cities and fields. Like the ancient city of Troy, the sudden collapse of the Mayan civilization presented a tantalizing mystery for generations of archaeologists.

Prevailing theories ascribed the decline of major civilizations to human error. Accordingly, historians and archaeologists tended to blame the Mayans themselves for their destiny – laying the blame, for example, on slash-and-burn agricultural techniques, religious doctrines, invasion and warfare, rebellions, diseases, foolish administrative practices and so on. Gill believed none of them. He resolved that climate, not the Indians themselves, was to blame: drought might have caused the Mayan civilization to topple.

At this point, of course, the critical reader will note how Gill's perception of the problem is remarkably similar to the one that dogs his own, late-twentieth century, context. Is it really just coincidence that the Mayans were struggling with the same sorts of problems as we are? Gill was aware of this uncomfortable parallel and wrestled with his bias face to face, turning it to his favour. In doing so, Gill's eventual explanation resulted less from books or careful study, and more from his own personal history: it came from recollecting the devastating droughts in the 1950s in the US state of Texas, when farmland was parched and fires raged. 'Being a Texan,' he explained later, 'I'm very aware of drought. It's something we deal with on an annual basis; we never know from one year to the next if we'll have enough rain' (Wong, 2001).

In the early 1980s, Texas was hit by a financial crisis and Gill's family bank collapsed. Dick Gill went back to college to study anthropology and archaeology so that he could study Mayan history more systematically. He studied fragments of pottery and counted Mayan farmsteads to estimate the demographic trends of the region. From these he drew a dramatic conclusion: Mayan society could have counted as many as 15 million inhabitants around AD 800, but this number had dropped to less than 1.5 million by AD 900. In other words, more than 90 per cent of the Mayan population simply disappeared during the course of the ninth century. While Gill was aware that several factors could account for such an enormous drop in population, he was convinced that drought was among them.

In order to make his case, Gill needed to reconstruct the climate of ninth-century Yucatán. Since the Mayans weren't in the habit of recording exact rainfall and temperature, Gill had to use whatever evidence he could find. First he turned to the national archives in Mexico City. They revealed that a severe, three-year-long drought had devastated Mayan society between 1902 and 1904. This suggested that his basic proposition was reasonable: droughts *had* occurred in Yucatán at least once in the past. If it had occurred a hundred years ago, Gill reasoned, it *could* have occurred a thousand years ago. But had it?

Searching further, Gill investigated older, colonial records from Spanish authorities in the Yucatán province of Mexico. Here he found evidence of repeated droughts. For example, a particularly bad drought had destroyed the harvests in 1795, and a document from that year contained a plea to Spain for help: the region was running out of grain and they feared the consequences. Now Gill had proof that devastating droughts had occurred more than once in Yucatán's past. This increased the likelihood of his argument. But he was unable to find any archival sources with enough detail to allow him to reconstruct the peninsula's weather patterns accurately. Nor did he find any evidence that a great drought had actually occurred during the ninth century.

To reconstruct the weather patterns and provide evidence of his hypothesized drought, Gill began – Miss Marple-like – to pursue new sources. He turned from archives to trees. Tree trunks grow fast in warm, wet weather; and their growth is arrested in extremely dry weather. Thus, by measuring the width of the rings in Yucatán tree trunks, Gill hoped to reconstruct the peninsula's precipitation pattern. On the basis of tree-ring records, Gill could identify the droughts of the Yucatán peninsula over the past few centuries. However, to support his case, he needed to analyse 1,200-year-old tree trunks – of which there were none to be found.

In search of a solution, Gill began to read meteorology papers. After ploughing through hundreds of dead-end sources, he finally stumbled across a paper on 'Dendrochronology, Mass Balance and Glacier Front Fluctuations in Northern Sweden' (Karlén, 1984). This paper allowed him to count the rings in a 1,200-year-old pine tree from Arctic Sweden. In this process of counting he made an astonishing discovery: that the pattern in the outer part of the old Swedish pine trunk matched exactly the 200-year-old record he had made of drought and disaster in Yucatán! Not only did the Yucatán droughts (of 1902–4 and 1795) match perfectly with severe cold snaps in Sweden; every time there had been a drought in the Mayan lowlands, there had also been severe cold in Sweden!

Meteorologists were able to tell Gill that he had tapped into the effects of a well-known weather system called the North Atlantic High. The term refers to an area of high pressure that travels eastward annually across the Atlantic – from the Caribbean toward northern Europe – and back again. When the high pressure moves toward Europe, it brings balmy temperatures to Sweden and pulls moist air in over Central America. However, once in a while, the North Atlantic High – for reasons that are unclear – doesn't fully complete its eastward journey; it stops a little short of Europe. In these years, Sweden becomes bitterly cold and the Yucatán peninsula suffers a drought.

Because it connected Scandinavian and Central American weather patterns with clockwork regularity, the North Atlantic High allowed Gill to use old Swedish tree trunks as operational indicators to reconstruct ancient weather patterns in Yucatán. As he patiently inspected Swedish wood, and filled in the missing centuries of his ancient weather charts, he found indications of a string of cold winters in ninth-century Sweden – which would indicate a similar string of dry years in Central America. Gill's proposition thus developed from plausible to highly possible.

The circumstantial evidence was growing stronger, but Gill still lacked direct proof of a devastating drought in ninth-century Yucatán. He finally got this evidence from a team of American researchers who had collected mud samples from the Yucatán Lake Chichancanab. The research team drove long, hollow tubes deep into the bottom of the lake and collected samples of mud from thousands of years ago – the deeper the mud, the older it was (and the seeds and shells trapped in it). The surfaces of shells from times of high rainfall are dominated by a particular type of light oxygen. If the rainfall is sparse, the water in the lake will evaporate and produce a dominance of heavy oxygen in the shells. Core samples from the ninth century showed an exceptional surge of heavy oxygen, indicating that it was an extremely dry period. Indeed, comparative analyses showed that it was the driest century in the region for over 7,000 years (Gill, 2000).

Like the Ingstads, Gill retroduced or conjectured a series of probable events that were consistent with his hunch about the decline of the Mayans. His hunch was propelled by the insight gained from personal experience of Texas farming and drought. Through a long and tenacious period of exploration, Gill juggled a number of mental experiments: experiments fuelled by knowledge, logic, detective work and a good deal of imagination. These experiments led him to search for particular pieces of a vast puzzle. The patterns that constituted the entire puzzle existed first in Gill's mind; he then tried to locate the individual pieces in the real world. Gill could not hope to find all the missing puzzle pieces – but he searched for individual pieces, here and there, to anchor his dream in terra firma.

Social Thought Experiments

In the examples above we have shown the important (if underappreciated) role that thought experiments can play in scientific discovery. We have then shown how scientists employ retroduction to anchor these thought experiments to their empirical foundations. Social theorists also employ thought experiments to construct imagined communities or contexts which can then be juxtaposed against the real world (at certain empirical points of tangency), or used as ideal types. As such, the thought experiments used in traditional social theory are remarkably similar to the examples described above. To illustrate this we can consider three prominent examples from Western social thought: Plato, Hobbes and Rousseau.

When Plato is asked to describe his ideal state, he begins with a discussion about human nature. 'Society originates,' we are told, 'because the individual is not self-sufficient, but has many needs which he can't supply himself' (Plato, 1987 [c.360 BCE], p. 59, *369b*). In addition, Plato adds that 'no two of us are born exactly alike. We have different natural aptitudes, which fit us for different jobs' (ibid., p. 59, *370b*). Thus Plato shows us how the construction of a social community can be imagined on the basis of the (interrelated) needs of its inhabitants (such as to provide food, shelter and clothing).

Indeed, it is quite clear (implicit, but clear) from Plato's Book II of *The Republic* that his community did not exist historically; it was devised (imagined) to help us understand the nature of the individual citizens who might inhabit it, and the justice that will characterize both. When Glaucon criticizes Socrates' first attempt as 'founding a community of pigs' (ibid., p. 63, *372d*), Plato imagines an even more complex social arrangement. But this second, grander, community remains nothing more than a mental construct.

A similar argument was developed some 2,000 years later by Thomas Hobbes. In his *Leviathan*, Hobbes writes that it is possible (if not very attractive) to consider human beings as divorced from their community, in an imagined state of nature. In contrast to Plato, Hobbes begins by assuming that humans are – on balance – equal, and that each person is driven by a number of *passions*, including a powerful lust for power: 'a perpetual and restless desire of power after power that ceases only in death' (Hobbes, 1958 [1651], p. 86). This lust for power among equals, in the context of a hypothesized state of nature, produces lives that are (most famously) 'solitary, poor, nasty, brutish and short' (Hobbes, 1958, p. 107). For Hobbes, the solution to this living hell is escape to an imagined sovereign community: the *Leviathan* – a fanciful creature on loan from the Bible.

In the same way that Plato imagines an ideal state (*The Republic*) as a just solution to the purported nature of man, Hobbes offers an imagined community (governed by an almighty *Leviathan*) to liberate humankind from the state of nature (also imagined). The attractiveness of the *Leviathan* or the *Republic* can only be understood by knowing how each author hypothesized the nature of humans.

Rousseau provides yet another take on the same theme. In what became known as his Second Discourse [1754], when addressing the *Origin and Foundations of Inequality Among Men*, Rousseau begins by:

> setting all the facts aside, for they do not affect the question. The researches which can be undertaken concerning this subject must not be taken for historical truths, but only for hypothetical and conditional reasonings better suited to clarify the nature of things than to show their true origin. (Rousseau, 1964 [1754], p. 103)

Rousseau is explicitly critical of Hobbes' depiction of human nature, and the more timid pictures painted by Richard Cumberland and Samuel Pufendorf. Rousseau has a different conception of human nature, which he introduces by placing his imagined human in a state of nature (like Hobbes, only very different). Unlike Plato (but like Hobbes), Rousseau has no difficulty in imagining a pre-social human. But unlike Hobbes, Rousseau's human in the state of nature is a noble savage: 'most of our ills are our own work, and ... we would have avoided almost all of them by preserving the simple, uniform, and solitary life prescribed to us by nature' (Rousseau, 1964, p. 110).

For Rousseau, it is society that is corrupt – not humankind. He illustrates this argument in a similar way as the other authors: by having his readers imagine a human being in a state of nature and contrasting that imagined creature with the one more familiar to us from everyday experience. In doing so, Rousseau reminds us to be careful 'not to confuse savage man with the men we have before our own eyes' (ibid., p. 111). For Rousseau, as for Hobbes, the state of nature is an imagined condition – a condition 'which no longer exists, which perhaps never existed, which probably never will exist, and about which it is nevertheless necessary to have precise notions in order to judge our present state correctly' (ibid., p. 93).

Drawing from authors as different as Plato, Hobbes and Rousseau, we have aimed to illustrate the way in which thought experiments in social theory share several traits with constructivism, as described in the preceding chapters. In particular, there are two points worthy of note.

First, none of these authors, regardless of their point of departure, used thought experiments to justify a fixed ontological perspective (a Real World). We are asked to imagine that human beings are equal (or unequal), cooperative (or competitive), and reasoned (or not) in a world – a state of nature – that is otherworldly. Similarly, we are asked to imagine these fictional characters in a society that is also a product of the author's imagination. This sort of ontological flexibility is one of the ballmarks of a constructivist perspective.

Second, the very question being asked lends itself to a hermeneutic approach. Simply asking the reader to imagine humankind in this state of nature requires that we imagine: (i) humans; (ii) society; and (iii) their interaction/juxtaposition. Plato cannot imagine humans prior to the community. Hobbes can (imagine the unimaginable), but it is not very attractive: humans in the state of nature were a desperate lot. Finally, Rousseau not only imagines, but also dreams of, humans uncorrupted by society. By hopping back and forth frequently between the imagined individual and the imagined community, we come to understand each in the light of the other.

Action Research

In the first set of examples, we showed how naturalist-based experiments can be used to support some of the foundational assumptions of constructivist social science. We saw how the complexity of social relationships delivers a rich and varied world of social inquiry – one that changes in response to contextual features. Here we saw the rigorous application of method overpowering the need to protect interpretive contexts. In the second set of examples we showed how a scientist's familiarity with a particular context plays an essential part in the progress of science. To solve difficult puzzles, social scientists employ imaginative thought experiments, born of contextual familiarity. In these examples, contextual familiarity is allowed to prevail over rigorous application of method. In this final section, we look at an example that combines these different features: in action research, scholars often embrace naturalist-based experimental designs; they anchor their understanding in familiar contexts; and they are explicitly committed to using the resulting knowledge to change the world for the better.

Action research is made up of a remarkably broad range of forms, inspired by different methodological and professional standards (Cassell and Johnson, 2006). The most famous of these may be participatory action research (PAR), associated with the work of (among others) the Brazilian philosopher, Paulo Freire (1921–97). Freire developed critical approaches to helping the poor by transforming the nature of education. In *Pedagogy of the Oppressed*, Freire (2006 [1970]) criticizes the way that traditional forms of education (what he calls the banking concept of education) support oppressive relationships in society, and encourages educators to embrace the learner/student as a co-producer of knowledge. In fostering creativity and knowledge, education can liberate the oppressed; the educator and researcher become allies in social transformation.

What different breeds of action research share is a desire to act out in the world (with an eye to improving it), and to study that action as it takes place (Coghlan and Shani, 2005, p. 533). In doing this, action researchers challenge the naturalist's commitment to demarcating clearly between the observer and the world being observed (and between normative and factual statements concerning that world). Consequently, action research has found some difficulty in being accepted into the naturalist church; critics have argued that action research is incompatible with the epistemological norms of (naturalist) science (see, for example, Susman and Evered, 1978; Argyris, 1980; Stone, 1982).

In this light, it is somewhat odd to consider action research in the context of experiments, because experimentation (we have argued throughout the first part of the book, and earlier in this chapter) is the quintessential method of naturalist science. But the man credited with coining the term 'action research', Kurt Lewin, was a keen advocate of experimental techniques: he was strongly committed to a naturalist ontology and epistemology (Cassell and Johnson, 2006, p. 790). Even if much subsequent action research has moved away from Lewin's explicit commitment to naturalist science and methods, the resulting tradition forces us to reflect on the tensions inherent in naturalist social science research.

As with Karl Marx's (1978 [1845]: 145, emphasis in original) *11th Thesis on Feuerbach* ('The philosophers have hitherto only *interpreted* the world, in various ways; the point, however, is to *change* it'), Lewin recognized that the point of conducting social science was to contribute, in a particular manner, to the betterment of society and its institutions.

> The research needed for social practice can best be characterized as research for social management or social engineering. [Enter Galton's Ghost, stage right] It is a type of action-research, a comparative research on the conditions and effects of various forms of social action, and research leading to social action. Research that produces nothing but books will not suffice. (Lewin, 1946, p. 35)

In this article, Lewin was particularly concerned with the problem of intergroup relations, especially race relations, and how research could help communities to improve these relationships. What is interesting about this piece is its explicit recognition of the need to adapt social research techniques and outlooks, to make them more suitable for application in particular contexts. For Lewin (1946, pp. 36–7):

> It is important to understand clearly that social research concerns itself with two rather different types of questions, namely the study of general laws of group life and the diagnosis of a specific situation.
>
> Problems of general laws deal with the relation between possible conditions and possible results. They are expressed in 'if so' propositions. The knowledge of laws can serve as guidance for the achievement of certain objectives under certain conditions. To act correctly, it does not suffice, however, if the engineer or the surgeon knows the general laws of physics or physiology. He has to know too the specific character of the situation at hand. This character is determined by a scientific fact-finding called diagnosis. For any field of action both types of scientific research are needed.

Lewin coaches us to follow the effects of a given change as they play out over time, under relatively controlled conditions. His approach involves a spiral of steps, 'each of which is composed of a circle of planning, action, and fact-finding about the result of the action' (Lewin, 1946, p. 38). The researcher proceeds up a long spiral of actions: he begins by identifying an idea or problem; he then tracks down facts/solutions; plans; takes action; evaluates; amends the plan accordingly; and takes action again. In this problem-solving process we can see a clear parallel with John Dewey's conception of learning from experience.

To secure firm knowledge from this learning/experience, Lewin recognizes the importance of developing experiment-like conditions for testing posited relationships. For example, in his work on race relations in the US state of Connecticut (ibid., pp. 39–41), Lewin focused on the difficulty of transmitting values learned in leadership-training workshops to the community at large (when workshop attendees returned to their communities). To test the utility of different transmission mechanisms, Lewin set up something akin to control and treatment workshops: some of the workshop delegates attended as individuals representing a particular town; other towns were allowed to send a group of delegates who worked as teams when they returned home; and a third group of delegates was provided with expert help when they went home from the workshop. In the process, the researchers were able to transmit information about how to improve race relations in the home communities, while also experimenting with different ways to ensure that the information was transmitted effectively from the workshop attendees to the communities they represented.

Once started, action research spread quickly. The work in Connecticut was quickly followed by a project to integrate black and white sales staff in New York department stores (Marrow, 1969), and the creation of the National Training Laboratories in 1947 (Burnes, 2004, p. 980). Lewin was also influential in the establishment of the Tavistock Institute in Britain, which used action research to improve managerial competence and efficiency in the newly nationalized coal industry. After this initial blossoming, action research experienced a decline in popularity because of its association with radical political activism (Stringer, 1999, p. 9), but has since enjoyed a renaissance with the launch of the journal *Action Research* in 2003 (Brydon-Miller *et al.*, 2003).

From our perspective, and following Reason and Bradbury (2001), we can understand action research in terms of both ideology and methodology. As an ideology, action research is grounded in a democratic tradition that promotes humanism and individual welfare: there is a clear political agenda here. As a methodology, action research distinguishes itself from naturalist approaches in prescribing different ways to collect and interpret data. While the naturalist strives to formulate general laws in a neutral manner to predict behaviour, the action researcher aims to meet two objectives: to accumulate data in a scientific and systematic manner, and to develop interventions or practical solutions to problems experienced by people and their communities.

This novel combination of naturalist-grounded techniques, familiarity with context, and explicit normative objectives provides a neat example of the promise of constructivist social science. It also illustrates the benefit of drawing from a broad spectrum of methodological tools and insights.

Conclusion

We have endeavoured to argue that constructivists do not rank their methods in an explicit hierarchy in the way that naturalists tend to do. Even so, it is clear that experimental approaches are not the most frequently used tools in the constructivist's toolbox. Still, if seen in the right light, experimental approaches can play an important role in constructivist research. This chapter has aimed to provide that light, by showing how the constructivist's reliance on experiments can be instrumental, innovative and liberating.

In their most basic (naturalist) form, experiments are able to provide firm knowledge that can support some of the foundational assumptions of constructivist social science. Experiments are useful tools for testing hypotheses about the constructed nature of social reality: about the roles of authority, context, generations and peer-groups in deciding how the world is patterned. These sorts of experiments provide grounds for questioning some of the basic underlying assumptions of naturalist social science: that there is a Real World out there, patterned by nature and independent of the observer.

But these sorts of experiments, where contexts are manipulated in order to control for expected outcomes, tend to irritate the constructivist. Context, meaning, interpretation and seeing the big picture are simply too important for most constructivists to find much utility in conducting experimental research. They may employ the evidence generated by experiments in an instrumental way, but they are not especially motivated to study the world with rigid experimental designs.

If we tweak the definition of experiment to include thought experiments, however, we can see how social scientists use a constructivist approach to leverage our understanding of the human condition. In these imaginary journeys, constructivists rely on their familiarity with contexts and employ hermeneutic techniques to produce ground-breaking scholarship. It is these sorts of creative thought experiments that lie at the centre of the academic traditions known as political or social theory.

By closing this chapter with a reference to action research, we want to show how it is possible, if not yet common, to combine the naturalist's commitment to comparison and control with the constructivist's embrace of local knowledge to generate research that is explicitly aimed at changing the world for the better. While it is not impossible to use the knowledge generated by naturalist social science to influence the world, naturalists want to segregate the production of knowledge from its application. This is done to distinguish clearly between value-laden and factual statements. For the constructivist social scientist, by contrast, this distinction makes little sense, as the social facts we describe are themselves subjective (value-laden). Methodological concerns needn't stand in the way of the constructivist who hopes to use his or her research to change the world (and our perceptions of it) for the better.

Recommended Further Reading

For a recent introduction to the promise of contextual experiments grounded in new social media, see Duncan Watts' (2011) *Everything Is Obvious Once You Know the Answer*. To access a broad overview of the exciting work being done in experimental philosophy (x-phi), see http://experimentalphilosophy.typepad.com. For those who want to learn about less traditional forms of experiments, as we saw in the imaginary journeys, see Roy Sorensen's *Thought Experiments* (1992). The promise of counterfactual thought experiments is explored in Tetlock and Belkin's (1996) *Counterfactual Thought Experiments in World Politics*. A recent debate on counterfactual history is summarized by Martin Bunzl (2004). The classic example of participatory action research, Paulo Freire's (2006 [1970]) *Pedagogy of the Oppressed*, comes highly recommended, but those who want to get up to speed with more recent developments in action research should check out the journal *Action Research* (http://arj.sagepub.com).

Chapter 13

Conclusion

Detectives, such as Sherlock Holmes and Miss Marple, have played an important and recurring role in this book on social science methods and methodologies. There is a reason for this. Karl Popper once noted that science is about solving mysteries, and we tend to agree. Science is all about solving problems and answering riddles; and scientific riddles are often derived from some observed regularity. Realizing this provides the key to opening our text: all scientists are concerned with patterns or regularities, but some social scientists argue that these patterns are part of the social world, whereas others argue that they are contingent.

On Design

The design of this book reveals much of our intent. It depicts two extremely different ways of studying social phenomena: one half of the book is dedicated to naturalist approaches; and the other to constructivist approaches. Each approach should be understood as an ideal type: they are caricatures of traditional approaches to social science, not actual descriptions of any particular piece of work. Each half of the book describes the different roles played by similar methods. More to the point, we have noted a strong methods hierarchy in the naturalist approach, as scholars in this tradition tend to prioritize experiments and statistics over comparisons and case studies (in that order). Naturalists are willing to subscribe to a strong demarcation principle that can be used to establish such a hierarchy of methods.

The constructivist part of the book is more circular – at least, nonlinear – in design. Lacking a clear demarcation principle, constructivists tend to be less catholic with regard to questions of method. Not only are decisions regarding method often left implicit, they are also seldom used in a way that limits opinion or voice. Personally, we find this lack of a demarcation principle somewhat worrisome as we have no clear-cut measure for distinguishing between good and bad constructivist designs. This lack of a clear standard can easily lead to the (unfortunate) impression that 'anything goes' in the name of constructivism. This is clearly not the case; there is a point at which an analysis can lose both utility and credibility – for example, where deconstructing a text becomes more of a playful act than any sort of useful analytical device. The problem is that this demarcation line is a delicate one and its location seems to differ for each one of us: we have different sensibilities with respect to these things.

Even if they avoid an explicit demarcation principle, constructivists do find some methods more reliable than others. Most obviously, constructivists tend to draw on thick narratives, or storytelling techniques. This emphasis on narration and the importance of context and contingency is extended to the way in which constructivists employ comparisons: as a tool for developing associations that can leverage meaning. Consequently, comparisons play a central role in constructivist studies (as they do in naturalist studies). Because statistical and experimental studies are seen to violate the very context that constructivists hold dear, these methods are used more cautiously; for example, to support foundation assumptions, or to highlight the role of context.

We have at least two reservations in emphasizing this methodological divide. First, we are concerned that any dichotomy has the potential to split scholars into two disparate (and autonomous) camps. As we note below, we began to write this book because we were concerned about the effect of another divide on our students: we wanted to bridge what was commonly seen as a divide that separated quantitative from qualitative approaches. With this caveat in mind, we have proceeded with caution, as we believe that it can be very useful to re-survey any terrain from a different angle. New perspectives – even if they result in new divisions – can be useful if they challenge our presuppositions and make us think anew about old problems.

Our second reservation concerns simple typologies: in dividing social phenomena into two groups we risk dividing some research projects down the middle. We also risk marginalizing different research traditions or particular authors. To resolve this potential dilemma we have tried to emphasize how these two approaches needn't be seen as exclusive or exclusionary. As we noted in the introductory chapter, this attitude is probably most familiar to students of International Relations – a subdiscipline of Political Science that has developed around a recognition of the value of maintaining different traditions, approaches, perspectives or paradigms. For generations, students have been taught to understand the world in terms of wearing different-coloured lenses – that different perspectives provide different understandings of the international context, and that each is legitimate.

On Methodological Bridge-building

The most important lesson a student can take away from reading this text is a willingness to recognize and distinguish between the different methodological traditions used to understand and interpret social phenomena. Our intention in providing this methodological smorgasbord is to emphasize the need for students to be able to read, critically, contributions from both traditions. The social science literature (in general), and our own experiences (in particular), provide ample evidence of methodological misunderstanding. Much contemporary social science is beset by problems that have arisen from a lack of philosophical reflection.

In short, we hope to encourage students to understand social science in a way that is sensitive to the methodological presuppositions of the authors they read. Only in this light can we truly appreciate how methods are used in such disparate ways. We also hope to encourage readers to consider their own methodological priors before beginning any research project. We fear that both objectives are discounted or ignored in traditional introductions to social science methods: ergo this book.

In recognizing two distinct ontological points of departure, and acknowledging that these positions influence different methodological approaches to studying the world, it is possible to conceive of at least four different paths along which social science might proceed. The first two paths are the most travelled, least imaginative, and most dangerous. Along one path are the hard-science scholars advocating a naturalist consilience; and along the other are the constructivists, with a similar hegemonic mission. Unfortunately, it is all too easy to find colleagues who are supremely confident in the sole appropriateness of their approach, and who feel a calling for methodological proselytization. While we can appreciate the allure of a unified scientific vision, we are sceptical that the social world lends itself to such simple methodological reductionism (on one side of the methodological divide or the other).

We find the other two alternatives more appealing, if only because they honestly address the complexity at hand, and our ability to deal with it adequately. The third path can be depicted in terms of a strategic synthesis; and the fourth in terms of bridge-building.

Scientific realism represents the third path we are describing. This approach was introduced briefly in Chapter 1 as an increasingly popular attempt to meld naturalist methods with a more pliable ontological base. Scientific realism offers itself as a unified position of scientific inquiry and does so by burying the most important ontological differences that separate what we have called the naturalist and constructivist perspectives. The result is a bewildering, many-layered ontological vision, where a philosophical ontology is in opposition to a scientific ontology, and where reality is stratified into domains of the empirical, the actual and the real (Bhaskar 1997 [1975], p. 13). In the words of Roy Bhaskar (1999), the founder of critical realism:

> The sort of ontology I was arguing for was the kind of ontology in which the world was seen as structured, differentiated and changing. And science was seen as a process in motion attempting to capture even deeper and more basic strata of a reality at any moment of time unknown to us and perhaps not even empirically manifest ... Through and through critical realism has been critical of what we call the nature of reality itself. Not the nature of absolute reality, or the absolute structure of being – to be critical of that is to put oneself into the position of God or the creator of the universe – but rather it is to be critical of the nature of actual, existing, social reality, or of our understandings of social and natural reality.

To simplify, scientific realism recognizes that patterns in the social world are fixed in nature, but that they can sometimes be hidden under many ontological layers, each of which can be haunted by misperceptions and obfuscation. For these scholars, a Real World *does* exist, but it resists our immediate inquiries as it lies at the end of a long chain of intermediaries; a chain in which each particular link complicates our relationship to the Truth. This perspective reminds us of the guru from our introductory chapter, who held that the world we experience rests on the back of a tiger, which stands atop an elephant, which stands on a giant turtle – which, in turn, stands on a stack of additional turtles that go 'all the way down'. The strategy of inquiry inherent to scientific realism is convenient, effective and increasingly popular. However, these advantages are secured at the expense of the simple ontological duality we have employed above. We hope that the second half of this book has helped to convince readers that social science loses too much when it sacrifices the constructivist's view of the world.

This leaves us with the fourth (and our chosen) path forward: that of methodological bridge-building. This strategy begins by acknowledging the usefulness of maintaining different ontological points of departure and embracing the methodological diversity that results from interacting across that ontological divide. At the start of a bridge-building exercise one needs to secure strong foundations on each side of the chasm to be crossed. Once constructed, bridges unite different communities in ways that can encourage synergy and exchange, as well as celebrating differences.

But isn't this what scientific realism seeks to do? Not exactly. Bridge-building is about connecting different positions, each anchored firmly on one side of a divide, by using a manufactured device that is both strong and appealing. Scientific realism, in contrast, might be understood to be an attempt to fill in the chasm that separates the two banks. By burying the ontological divide (with truckloads of turtles, as it were), scientific realism allows constructivists and naturalists to work together on common ground – but in doing so they are likely to forget that this common ground is mainly back-fill, not the solid rock from which they began their inquiries.

In building these bridges, social scientists often find themselves working alongside rather surprising collaborators. Growing from foundations that are well anchored in one methodological tradition, scholars may be surprised to see how their work is being embraced by others who started from very different, even opposing, positions. The result is an exciting sort of pluralism, holding the promise of building new alliances and approaches to studying the social world. We can illustrate the potential of such bridge-building efforts with reference to two very different examples: rational thought experiments and within-case approaches.

Rational Thought Experiments

The first bridging effort aims to show how two projects, each working in isolation, share a long-term affinity for rationalist epistemologies. As the first part of this book aimed to illustrate, naturalists have found it necessary and useful to buttress their largely inductive approaches with theory. After all, much scientific discovery has proceeded on the basis of thought experiments, or reason-based arguments. Naturalists often employ reason in lieu of a natural world that does not always lend itself to manipulation (or fit into the scientist's laboratory).

Some of the most influential natural scientists in history (especially those occupied with the biggest thoughts) could not depend on observations derived from physical experiments to form or test their theories. Instead, they had to dream up another (imagined) world, in which they developed scenarios and examined what was necessary to support them. In short, much of the progress of science has occurred in the minds of great scientists.

In acknowledging this, we find ourselves in territory familiar to the constructivist: we are not far from Benedetto Croce's (1921) recognition that all history is contemporary history, or R. G. Collingwood's (1956, 1999) argument that the historian's past is inseparable from his present. As noted in the previous chapter, mental experiments draw inescapably on the context surrounding the experimenter – they reflect the surrounding conditions. In this way, the method employed by natural scientists (such as Charles Darwin) is not unlike the method employed by the Ingstads in their imaginative journey (see Chapter 12), or in social theory more generally. Here, buried deep in the naturalist tradition, we find a bridgehead that can be built on.

On the other side of the methodological divide, among constructivists and social theorists, we have already uncovered a similar bridgehead in Chapter 12: the use of thought experiments is more celebrated on these methodological banks. But the objective of constructivist thought experiments reflects their ontological priors; for example, they can be used to understand the nature of cooperation, community and context, and the individual's relationship to these.

Like most constructivists, Rousseau believed that the key to human behaviour lies in its context. Human beings are malleable creatures (their nature is not fixed), Rousseau averred. If individuals are to behave in an orderly manner, and live moral lives, it is crucially important that they are brought up in an orderly way in a moral society – that they are moulded by the protective framework of a tender, just and nourishing state. Within the ordering presence of such a state, the inculcation of public feeling imparts to each citizen a spirit of devotion toward the welfare of the whole, and equality prevents the development of partial interests that might be fatal to civic order and the unity of the state.

Thus, if people are treated firmly and fairly, society will be unified (and conflict can be eliminated):

> If children are brought up in common in the bosom of equality; if they are imbued with the laws of the State ... if they are taught to respect these above all things; if they are surrounded by examples and objects which constantly remind them of the tender mother who nourishes them ... we cannot doubt that they will learn to cherish one another mutually as brothers. (Rousseau, 1950 [1755], p. 309)

By the same token, Rousseau recognized that if individuals live in a corrupt society, they will themselves become corrupt. In the absence of an ordering and moral state, individuals cannot develop the skills and social graces that order a community. In the early state of nature, Rousseau argues, individuals were egotistical: they thought only of themselves and their own advantage. They did this not out of malice, but out of need and ignorance. Under such conditions, human behaviour is governed by the two principles that human beings possess before society endows them with reason: well-being and self-preservation. Under these primitive conditions, humans do not associate, let alone cooperate, very easily.

In Rousseau's state of nature, human beings are good and empathetic. But as they possess no developed reason, cooperation is possible only in the most dire and peculiar of circumstances. Rousseau imagines a situation in which natural men have been driven into cooperation by a common threat of hunger:

> That is how men could imperceptibly acquire some crude idea of mutual engagements and of the advantages of fulfilling them, but only insofar as present and perceptible interest could require; for foresight meant nothing to them, and far from being concerned about a distant future, they did not even think of the next day. Was it a matter of catching a deer, everyone clearly felt that of this purpose he ought faithfully to keep his post; but if a hare happened to pass within reach of one of them, there can be no doubt that he pursued it without scruple, and that having obtained his prey, he cared very little about having caused his companions to miss theirs. (Rousseau, 1964, p. 145)

The hunger of all can be satisfied if these natural men cooperate together in hunting down a deer (a deer is difficult to catch singlehanded, but yields good meat for many). At the same time, the hunger of each individual can be satisfied with a hare (a hare can be caught by one person alone, and provides poor – but sufficient – meat for one). Thus individuals have to decide which is better: to trust and cooperate with others in the hope of a tasty venison dinner; or defect from the group and secure a rabbit for oneself. In effect, the story of the stag hunt is the story of the social contract; it surveys the groundwork for community.

Rousseau's point is that no individual is strong enough to subdue a deer single-handedly, whereas it takes only one hunter to catch a hare. Everyone prefers deer to hare. But they also prefer rabbit meat to nothing at all (which is what the hunting party will end up with if too many members run off chasing hares).

It is at this point that these two distinct projects – one emanating from the naturalist's side, the other from the constructivist's – are conceptually close enough to connect and form a bridge. Modern game theorists, such as William Poundstone (1992, pp. 218ff), envision Rousseau's deer hunt as a game between two rational people – let's call them Robinson and Friday. Poundstone then adds that the value (or payoff) of catching a deer is 3, the value of catching a hare is 1, and there is no value (0) in going home empty-handed. The outcome of the game is summarized in Figure 13.1. To clarify, the conditions in the upper right corner can be described thus: Friday chooses to hunt the deer, while Robinson chooses to chase a hare, with the corresponding payoffs: 0 for Friday; 2 for Robinson.

Figure 13.1 ***Stag hunt payoff matrix***

		Robinson	
		Hunt deer	Chase hare
Friday	Hunt deer	3,3	0,2
	Chase hare	2,0	1,1

Note: To understand this game, consider the incentives from Friday's perspective. If Friday assumes that Robinson will hunt deer, Friday must then decide if it makes more sense for him to cooperate (and hunt deer) or to defect (and chase bunnies). In the first case (cooperate), he can expect a reward of 3; in the latter (defect), he can expect a 2 – it is clear that it is best for Friday to also hunt deer. However, when Friday begins by assuming that Robinson might chase rabbits, he finds that it is now more rational for him to also chase rabbits.
Source: Authors.

In modern game theory, a stag hunt game is one which describes a choice between social cooperation and individual safety. Hunting deer (large and very tasty) represents social cooperation; chasing hares (small, but hits the spot) is the safe bet. Because Friday and Robinson don't trust or talk to one another, they don't really know what the other is going to do. Under these conditions, each has to find out what makes most sense under different possible scenarios.

In this game, what is rational for Friday depends on his belief of what Robinson will do (this is, by the way, quite different from a Prisoner's Dilemma). Both stag hunting and bunny chasing represent *equilibria* (in other words, that it is best for Friday to hunt deer if Robinson hunts deer and it is best for Friday to chase hares if Robinson does so). If Friday chooses to hunt deer, he takes a bigger chance of going hungry (as he takes a chance that Robinson will not join in the deer hunt). Alternatively, if Friday chooses to chase hares, he runs no such risk, since his payoff does not depend on Robinson's choice of action. Weighed against all these considerations is the realization that everyone prefers venison steaks to bunny stew. In this game, rational players are pulled in two different directions: at one end is a consideration of mutual benefit; and at the other a consideration of personal risk.

This is the essence of Rousseau's dilemma for natural men. Also, as game theory makes clear, it is a dilemma for all rational actors who live outside an ordered society, or who think exclusively of themselves.

In this example we have aimed to illustrate how constructivists and naturalists share an affinity for rationalist approaches. But there is little point in hiding the fact that this shared legacy is an awkward one. Game theorists and rational choice scholars tend to think of themselves as high priests in the naturalist church. Their mathematical sophistry allows them to tap directly into the rationalist legacy we traced back to Descartes in Chapter 2; their methodological acumen allows them to make sophisticated predictions about the nature of social behaviour in a complex, but patterned world. We do not think it unlikely that scholars of this ilk will take offense by being associated with a constructivist tradition.

And yet much of this tradition connects directly with a bridgehead that is easily excavated from discussions in Chapter 12: the imaginative thought experiments employed by social theorists. By the same token, it is difficult to find constructivists who openly reveal an admiration or affinity for the mathematical prowess of game theorists or formal modelers. There is much that separates these two bridgeheads, but this does not detract from the utility, or appeal, of a bridge that connects them.

Indeed, there are enough commonalities between these two positions to promise hope and insight. As constructivists approach the naturalist position, they are driven to question the subtle axioms that naturalists embrace about the way the world is patterned: constructivists want to explore where rationality comes from, if it varies by context, and how.

This interest is clearly evident in the work of contemporary social theorists, such as Alessandro Pizzorno, who examine the conditional nature of rationality – how it is a product of social relationships, not some sort of neutral or autonomous antecedent to it:

> the self-interest of an individual cannot be considered as a reality that precedes and motivates the action. It is instead the outcome of a process whose full content is not known by the subject, and hence by the non-knowledgeable observer when the actor engages in social action. Self-interest, as defined by the actor as well as by the observer, represents the outcome of the action rather than its premises. (Pizzorno, 2008, p. 173)

But naturalists, too, are approaching this middle part of the bridge: they are examining the same sorts of questions, but with one foot still firmly planted in the naturalist tradition. For example, in a 2001 paper published in the *American Economic Review*, Joseph Henrich *et al.* described the results of a series of behavioural experiments conducted in fifteen small-scale societies (three foraging societies; six that practice slash-and-burn horticulture; four nomadic herding groups; and three sedentary, small-scale agriculturalist societies). After exposing these very different subjects to a series of ultimatum, public good and dictator games, the authors found 'considerably more behavioral variability across groups than had been found in previous cross-cultural research, and that the canonical model [that individuals are entirely self-interested] fails in a wider variety of ways than in previous experiments' (Henrich *et al.*, 2001, p. 73). Rationality, it seems, can vary significantly across contexts.

Scholars from both methodological traditions are beginning to close in on common problems and queries, employing approaches and perspectives that reflect their different methodological and ontological points of departure. Constructivists and naturalists alike are examining the contextual and contingent nature of rationality – each from their respective methodological vantage point. It is here, then, high above the chasm that once separated naturalist and constructivist thought, that we can see the promise of methodological bridge-building.

Within-Case Approaches

A similar promise can be found in the new work employing within-case approaches. Path dependency and process-tracing are both examples of within-case-study approaches, where the analyst's focus is trained on the nature of developments internal to a particular case or object of study. As we noted in Chapter 6, naturalists have traditionally down-played the utility of lowly case-studies; these offer little more than the hope of plausibility – the chance that this case may prove to be part of a larger (and more interesting) social phenomenon. By contrast, the naturalist's aim has been to produce reliable general conclusions by focusing on cross-case patterns of variables, in search of neo-Humean (or Hempelian) forms of causal relationships. It is this focus that explains the utility of experiments and large-N, cross-case comparisons for naturalist social scientists.

By contrast, constructivists have traditionally embraced path-dependent and process-tracing approaches (and case studies in general); and they have tended to celebrate the uniqueness of the cases at hand. As a general rule, constructivists have not embraced these techniques for their ability to address methodological concerns (such as their access to any underlying causal claims, for example). Their approach to causality has been less explicit, less mechanical, yet still 'scientific'. Thus Collingwood (1962, pp. 22–3) would have us distinguish between

> the desultory and casual thinking of our unscientific consciousness and the orderly and systematic thinking we call science. In unscientific thinking our thoughts are coagulated into knots and tangles; we fish up a thought out of our minds like an anchor foul of its own cable, hanging

> upside-down and draped in seaweed with shellfish sticking to it, and dump the whole thing on deck quite pleased with ourselves for having got it up at all. Thinking scientifically means disentangling all this mess, and reducing a knot of thoughts in which everything sticks together anyhow to a system or series of thoughts in which thinking the thoughts is at the same time thinking the connexions between them.

Collingwood, that dean of constructivist argument, advises us to think in terms of complex connections and systems of interaction. He might have appreciated Foucault's effort to identify the constituent elements of human discourse and to lay out the logic of their systemic interaction. He might also have appreciated Fernand Braudel's attempt to identify the elements of the modern capitalist world economy and the huge system that they form. Likewise, we expect he would have been intrigued by Immanuel Wallerstein's observation that the capitalist world system is unique – that world history has seen only *one* such world system.

This tradition looks for causal connections in the complexity of *the* case. For them understanding does not arise from comparing similar cases with an eye at controlling for variation. Rather, understanding comes from untangling the complex knot of connections: from unfouling the causal cable. This focus on internal (and complex) causal mechanisms is clearly evident in Wallerstein's endeavour, as the subject of his inquiry (the world system) was limited to a single case:

> There has only been one 'modern world.' Maybe one day there would be discovered to be comparable phenomena on other planets, or additional modern world-systems on this one. But here and now, the reality was clear – only one. It was here that I was inspired by the analogy with astronomy which purports to explain the laws governing the universe, although (as far as we know) only one universe has ever existed.
>
> What do astronomers do? As I understand it, the logic of their arguments involves two separate operations. They use the laws derived from the study of smaller physical entities, the laws of physics, and argue that (with perhaps certain specified exceptions) these laws hold by analogy for the system as a whole. Second, they argue a posteriori. If the whole system is to have a given state at time *y*, it most probably had a certain state at time *x*. (Wallerstein, 1974, p. 7)

In short, Wallerstein proposes to study the world system in a way that is akin to within-case analyses. To understand the nature of complex systems, we have to take them apart as units to examine complex relationships and mechanisms internal to the case under study.

Herein lies the rub. Many of the most prominent scholars of within-case analysis are fiercely committed to the naturalist tradition. Indeed, some of them might take offense at our placing their approaches (for example, process tracing and path dependency) firmly on the constructivist bank (as Chapters 9 and 10 suggest). We are prepared to argue, however, that such within-case approaches have adherents in both methodological camps, and that the tension which results from pigeonholing them in one methodological tradition (or the other) reveals the continued existence of a methodological divide – and hence the need for a bridge.

There are similarities in these versions of within-study approaches. The chief difference lies in varying conceptions of causality. In recent years, the rise of within-study approaches has managed to challenge the link between naturalism and its traditional embrace of a Humean search for cross-case patterns of variables. Whereas naturalist methods tend to evaluate explanations by comparing patterns in co-variation of variables *across* observations, within-case analyses assess explanations by examining evidence from *within* the observed cases. In questioning the traditional linkage between naturalists and their favourite form of causation, within-case study approaches have revealed different ways of thinking about causation and opened up new venues for inquiry and collaboration. Within-case-study approaches provide new ways of untangling that complex knot of connections to which Collingwood referred.

> There are several different phenomena that exhibit causal complexity, including tipping points, high-order interaction effects, strategic interaction, two-directional causality or feedback loops, equifinality (many different paths to the same outcome), and multifinality (many different outcomes from the same value of an independent variable depending on context). The possible presence of these kinds of complexity affects how knowledge statements can be most usefully constructed and verified. We conclude ... that qualitative methods, particularly the combination of within-case analysis and cross-case comparisons, are useful approaches toward addressing these kinds of complex causation even when scholars study only one or a few cases. (Bennett and Elman, 2006, p. 251)

This is potentially revolutionary stuff (even if within-case practitioners are not always aware of its explosive impact). Within-case analyses are letting social scientists get their hands on (different types of) causal inference without employing large-N correlational studies. Should this logic take hold among naturalists, it would clearly jeopardize the methods' hierarchy as outlined in the first part of this book.

Within-case analyses bring us to the forefront of a new, more humble social science – one that is more aware of contextual settings and the way it can affect our understanding of the world (and the way it is patterned). Just as important, the difficulty of placing these methods in one methodological camp or the other reveals the novelty and utility of a bridge-building strategy. These approaches belong in both camps, and it makes little sense to limit their appeal to just one side of the methodological divide.

At the same time, these sorts of bridge-builders need to be cognizant of their underlying ontological and methodological differences; and so much small-N research has failed to do this in the past (Bevir and Kedar, 2008). From a bridgehead firmly anchored in the naturalist tradition, and with a strong foundation set on the other methodological bank, within-case-study approaches show us the remarkable promise of crossing what was once a significant methodological divide.

In addition to these larger bridging projects, the preceding chapters have introduced a number of equally promising (if smaller) collaborative ventures. This book has aimed to underscore this potential for cross-methodological collaboration. In the constructivist half of the book in particular we have aimed to gather examples that illustrate how constructivists have employed naturalist-styled experiments and statistical studies to underscore the importance of contextual factors in explaining the changing patterns we study.

On the Numerology Divide

It was noted above how this book grew out of a dissatisfaction with the traditional dichotomy of 'quantitative' and 'qualitative' research methods – and with dismay at the number of students who enrolled in our class on historical and comparative methods for the sole reason of avoiding statistics and numbers. Alert readers have probably noted that, throughout the text, we have avoided any reference to 'quantitative' and 'qualitative' methods. Some of these readers may wonder if our dichotomy of 'naturalist' and 'constructivist' approaches might just be another way of expressing this quantitative/qualitative divide. For them we have a clear and simple answer: No, absolutely not! The dichotomy delineated in this book (naturalist/constructivist) *does not* dovetail or harmonize with this older, harmful and counter-productive divide.

The quantitative/qualitative divide is a relic of the past. It was a singularly counter-productive divide, and it was offensive to boot: after all, this divide implies that quantitative work lacks quality. By continually harping on a division between quantitative and qualitative methods, we end up doing more harm than good: we reproduce this detestable division, keep a useless debate artificially alive, and undermine attempts to build bridges across what has become a divide (even if it is an imagined one). Indeed, by dwelling on the qualitative/quantitative divide we are doing just the opposite: rather than building bridges, we facilitate the digging of trenches. By constantly presenting quantitative and qualitative methods as alternative approaches, we are not only flogging a dead horse, but we are also re-creating the chasm that has so long divided social scientists into rival camps. This quantitative/qualitative schism has been the incubus of social science for almost 100 years. We can live with it no more.

In today's field of research one can find new methods and techniques that have already wiped out any meaningful differences between quantitative and qualitative studies. The growing appeal and sophistication of Qualitative Comparative Analysis (QCA), as described in Chapter 5, is evidence of this trend. Likewise, as we have endeavoured to show in Chapters 11 and 12, constructivists are employing some of the most sophisticated empirical tools on the market to help them approach and understand the patterns they study. At the same time, naturalist social scientists are increasingly aware of the importance of employing narrative and context-supporting approaches when uncovering their own (different) patterns, or in examining critically some of their own foundational assumptions. It is high time for methods teachers to take heed and relegate concepts such as 'qualitative research methods' to the dustbins of history. All of us are capable of producing quality research.

To erase this false distinction, we have emphasized the way in which small-N studies actually require more methodological reflection and sophistication than large-N studies (as there is less analytical leeway, fewer degrees of freedom, and more problems to face). In addition, we have used the last couple of chapters to show that some of the most sophisticated pea-counting strategies can be associated with the constructivist methodology.

There is no reason to encourage students to consider research projects in terms of a qualitative/quantitative divide. Similarly, we caution against making too much of the difference between the naturalist and constructivist approaches introduced above. Our hope is that readers of this text will take us seriously when we encourage them to embrace methodological pluralism and bridge-building. It is in choosing this path that we can best exploit the manifold ways of knowing.

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