How Scientists Explain Disease

PAUL THAGARD

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Preface

THIS BOOK is about the causes of disease and the causes of science. It is an attempt to answer the question: How do scientists learn about why people get sick? Explaining advances in medical science is similar to explaining diseases, in that both kinds of explanations require the assembly of complexes of interacting causes. Just as most diseases arise from the interaction of environmental and genetic factors, so medical theories arise from the interplay of psychological, physical, and social processes. I have written the book for two main audiences. The first consists of readers with a general interest in the development of medical knowledge about diseases such as peptic ulcer. The second consists of people studying the history, philosophy, psychology, or sociology of science, who will find here an investigation that combines and integrates all these approaches.

The case study at the core of this book is the development and acceptance of the theory that the primary cause of most peptic ulcers is infection by a recently discovered bacterium, *Helicobacter pylori.* I first encountered this case in 1993, when Dr. David Graham invited me to visit him at the Baylor College of Medicine in Houston. He had read my book *Conceptual Revolutions* and saw it as relevant to the rise of the bacterial theory of ulcers, which was first proposed in 1983 by two Australian physicians, Barry Marshall and Robin Warren. Initially, this theory was greeted with intense skepticism by medical experts, but by 1995 it had widespread support. Chapters 3 to 6 of this text provide an integrated explanation of these developments, discussing psychological processes of discovery and acceptance, physical processes involving instruments and experiments, and social processes of collaboration, communication, and consensus.

Chapters 1 and 2 set the stage for the ulcers case study by discussing the nature of explanations of scientific developments and of diseases, both of which are best described in terms of complex schemas that assemble multiple interacting causes. Chapter 1 presents explanation schemas that capture the main current approaches to the study of science, ranging from logical schemas favored by many philosophers to social schemas employed by sociologists. Chapter 2 reviews the most important medical explanation schemas in the history of medicine, from the Hippocratic theory of humors to very recent explanations based on molecular genetics. I argue that an integrated cognitive-social schema provides the most promising approach to explaining the growth of scientific knowledge, and chapters 3 to 6 fill out this schema in the case of the bacterial theory of ulcers.

Chapters 7 to 10 delve more deeply into cognitive mechanisms involving causality, analogy, and conceptual change. Chapter 7 discusses the meaning of the claim that bacteria cause ulcers and provides a general account of medical causal reasoning. Chapter 8 uses this account to explain why discovering the causes of diseases encounters many difficulties, which are illustrated by the development of ideas about scurvy, spongiform encephalopathies (e.g., mad cow disease), AIDS, and chronic fatigue syndrome. Analogical thinking has been important in many cases in the history of medicine that are described in chapter 9. Chapter 10 shows how the development of new medical theories can involve major kinds of conceptual change concerning diseases and their causes.

Chapters 11 to 13 investigate social processes that contribute to the growth of scientific knowledge. Collaboration was a major factor in the development and acceptance of the bacterial theory of ulcers, as it is in most current scientific work; chapter 11 provides a description and evaluation of this role. Chapter 12 describes a social process unique to medicine, the use of consensus conferences to reach authoritative conclusions that provide recommendations for medical practitioners. Increasingly, social interactions in science are being facilitated electronically by the various technologies available on the Internet, and chapter 13 discusses the contributions of these technologies to the development of scientific knowledge. In all three of these chapters, my concern is not only to describe social processes but also to evaluate their potential positive and negative effects on medical progress. Finally, chapter 14 uses ideas about distributed computing to portray science as a complex system of cognitive, social, and physical interactions. The book concludes with a defense of scientific rationality and realism.

Especially in chapter 13 but also in other chapters, I have referred to World Wide Web resources using universal resource locators beginning with "http." Web users can find live links for these references via my Web site at http://cogsci.uwaterloo.ca.

Acknowledgments

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For various chapters of this book, I have adapted parts of previous articles, and I am grateful to the respective publishers for permission to reprint some of their contents:

Thagard, P. (1993). Societies of minds: Science as distributed computing. *Studies in History and Philosophy of Science*, *24*, 49–67. [Used in chapter 14.] Reprinted with permission of Elsevier Science.

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Thagard, P. (1998). Ulcers and bacteria II: Instruments, experiments, and social interactions. *Studies in History and Philosophy of Science. Part C. Studies in History and Philosophy of Biological and Biomedical Sciences*, *29*, 317–342. [Used in chapters 5 and 6.] Reprinted with permission of Elsevier Science.

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Part One

EXPLANATIONS

CHAPTER 1

Explaining Science

IN THE 1950s, a doctor whose patient was diagnosed with a stomach ulcer would typically recommend that the patient relax and drink lots of milk. By the late 1970s, however, treatment had changed, and the doctor would probably prescribe Tagamet or one of the other acid-blocking drugs that had been developed. Today, in contrast, a well-informed doctor will prescribe a combination of antibiotics for an ulcer patient to kill the bacteria that are now thought to cause most stomach ulcers.

The change in medical practice is due to general adoption in the 1990s of the theory that most peptic (gastric and duodenal) ulcers are caused by *Helicobacter pylori*, a species of bacteria that was discovered only in the early 1980s. When Barry Marshall and Robin Warren suggested that these bacteria might be responsible for peptic ulcers, their proposal was widely viewed as implausible, particularly by the specialists in gastroenterology who usually treat ulcers. But by the mid-1990s, medical consensus panels in many countries had endorsed the bacterial theory of peptic ulcers and their treatment by antibiotics.

How did this change take place? Contrast the following two pictures of scientific development. In a traditional view held by many scientists and philosophers, scientists conduct careful experiments and use the resulting observations to confirm or refute explanatory hypotheses that can provide objective knowledge about the world. In a postmodern view held by some sociologists and culture theorists, scientists conduct experiments to support the hypotheses that best suit their personal and social interests, and they negotiate with other scientists to accumulate sufficient power to ensure that their theories prevail over those of their rivals. Whereas on the traditional view science is largely a matter of logic, in the postmodern view it is largely a matter of politics. The traditional view is exemplified by such philosophers as Hempel (1965), Popper (1959), and Howson and Urbach (1989), whereas the postmodern view is found among sociologists and cultural theorists (e.g., Aronowitz 1988; Latour 1987; Ross 1996).

Neither the traditional nor the postmodern account provides much of an explanation of the discovery and acceptance of the bacterial theory of ulcers. The logical view neglects the diverse psychological and social processes that contribute to scientific development, whereas the political view ignores the extent to which the growth of science is affected by experimental interactions with the world and by rational assessment of alternative hypotheses. By discussing the ulcers case and other important events in the history of science and medicine, this book develops a much richer view of science as an integrated psychological, social, and physical system.

Many philosophers, historians, psychologists, and sociologists of science are concerned about explaining the development of scientific knowledge, but the kinds of explanations they propose are very diverse. Some philosophers of science prefer *logical* explanations, in which new scientific knowledge derives logically (inductively or deductively) from previous knowledge. Researchers in cognitive science, including psychologists, computer scientists, and some philosophers, propose *cognitive* explanations, in which the growth of knowledge derives from the mental structures and procedures of scientists. Sociologists of science offer *social* explanations, in which factors such as the organization and social interests of scientists are used to explain scientific change.

Are these explanations competitive or complementary? During the 1980s and 1990s, since sociologists of knowledge staked claims to what had been the traditional philosophical territory of explaining the growth of scientific knowledge, there has been conflict between proponents of logical and social explanations (see, for example, Barnes 1985; Bloor 1991; Brown 1984, 1989; Collins 1985). In the meantime, cognitive approaches have emerged with explanatory resources much richer than those available within the logical tradition, but the relation between cognitive and social accounts is rarely specified. Some sociologists are intensely antagonistic toward psychological and computational explanations, even going so far as to propose a ten-year moratorium on cognitive explanations of science (Latour and Woolgar 1986, p. 280). In a similar vein, Downes (1993) attacks what he calls "cognitive individualism" and defends the claim that scientific knowledge is socially produced.

But we can appreciate science as a product of individual minds *and* as a product of complex social organizations. Not only can we see cognitive and social explanations as providing complementary accounts of different aspects of science, but we can also look for ways of integrating those explanations, bringing them together in a common approach. This chapter compares cognitive and social explanation schemas and shows how they can be brought together to form integrated explanations of scientific change. To illustrate the unification of approaches, I show how a cognitive account of the chemical revolution can be socially enriched, and how a social account of the early development of science and mathematics can be cognitively enriched. The social categories of Downes (1993) require similar enrichment. Finally, I sketch how a cognitive/social approach offers new perspectives on the question of scientific rationality.

EXPLANATION SCHEMAS

An explanation schema consists of an explanation target, which is a question to be answered, and an explanatory pattern, which provides a general way of answering the question. For example, when you want to explain why a person is doing an action such as working long hours, you may employ the following rough explanation schema:

Action Explanation Schema

Explanation target:

Why does a **person** with a set of **beliefs** and **desires** perform a particular **action**?

Explanatory pattern:

The **person** has the **belief** that the **action** will help fulfill the **desires.**
This **belief** causes the **person** to pursue the **action**.

To apply this schema to a particular case, we replace the terms in boldface with specific examples, as in explaining Mary's action of working long hours in terms of her belief that this will help her to fulfill her desire to finish her PhD thesis. Many writers in the philosophy of science and cognitive science have described explanations and theories in terms of schemas, patterns, or other abstractions (Darden and Cain 1989; Giere 1994; Kelley 1972; Kitcher 1981, 1989, 1993; Leake 1992; Schaffner 1993; Schank 1986; Thagard 1988, 1992b).

What are the explanation targets in science studies? The most straightforward is belief change, as when we ask why eighteenth-century chemists adopted Antoine Lavoisier's oxygen theory or why nineteenth-century physicians adopted the germ theory of disease. The focus of the general explanation target is why scientists abandoned their previously held belief in favor of a new theory. But there is much more to the development of science than belief change, for we can ask why conceptual changes took place involving the introduction and reorganization of whole conceptual systems (see chapter 10).

Another legitimate explanation target in science studies involves discovery. Why did Lavoisier discover the oxygen theory in the 1770s? Why did Louis Pasteur discover the germ theory of disease in the 1860s? Although such questions are not open to logical explanations, they are grist for the mills of cognitive and social theorists (see chapter 3). Similarly, cognitive and social explanations can be given for why scientists pursue particular scientific research programs. Pursuit is an intermediate stage between the initial discovery or proposal of concepts and beliefs and their eventual acceptance. Within that stage, there are many interesting questions to be answered, such as why scientists conducted particular experiments in particular ways. The remainder of a much broader range of structures and processes than logic describes (for deduction, see Johnson-Laird and Byrne 1991; for induction, see Holland et al. 1986).

Sociologists of science tend to focus on different features of science than on logical methods and mental procedures. They note that because of their social situations scientists have various interests, ranging from personal ambition to national sentiment. The also note that the development of science depends in part on the social connections that control information flow among scientists and the power relations that make some scientists much more influential than others in determining what science is done. Amalgamating ideas from various sociologists, we can roughly summarize various social explanations for belief change with the following:

Social Explanation Schema

Explanation target:

Why did a group of **scientists** adopt a particular set of **beliefs**?

Explanatory pattern:

The **scientists** had **previous beliefs** and **interests**.

The **scientists** had **social connections** and **power relations**.

Previous beliefs and **interests** and **social connections** and **power relations** lead to **acquired beliefs**.

The **scientists** adopted the **acquired beliefs**.

This schema is incompatible with the logical schema, which assumes that epistemic matters must be kept isolated from psychological and sociological ones. However, it competes with the cognitive schema only if one assumes that the best explanation of the development of science must be either purely cognitive or purely social. But open-minded cognitivists can easily grant that scientists have the interests, social connections, and power relations postulated by sociologists, and that these qualities play some role in the development of science. Similarly, open-minded sociologists can grant that psychological structures and processes can mediate socially affected belief changes. The cognitive schema is incomplete because it fails to note how social relations can affect the spread of beliefs through the group of scientists. The social schema is incomplete because it fails to show how individual scientists came to acquire their beliefs.

A full account of the growth of scientific knowledge must therefore integrate the features of cognitive and social schemas, as is roughly illustrated by the following schema:

Integrated Cognitive-Social Explanation Schema

Explanation target:

Why did a group of **scientists** adopt a particular set of **beliefs**?

Explanatory pattern:

The **scientists** had a set of **mental representations** that included a set of **previous beliefs** and a set of **interests**.

The **scientists'** cognitive mechanisms included a set of **mental procedures**.

The **scientists** had **social connections** and **power relations**.

When applied to the **mental representations** and **previous beliefs** in the context of **social connections** and **power relations,** the **procedures** produce a set of **acquired beliefs**.

The **scientists** adopted the **acquired beliefs**.

As with the previous schemas I presented, considerable detail must be added to put this explanation schema to work. To fill in the cognitive side, we must specify the mental representations and procedures that operate on them, including logical methods. To fill in the social side, we must specify the relevant social interests, connections, and power relations. As chapter 5 shows, it is also crucial to take into account the instruments and experiments through which scientists interact with the physical world.

To make the integrated cognitive-social explanation succeed, we must provide a much fuller account of how the cognitive and social features of scientists together determine their belief changes. For example, sociological explanations that appeal to the interests of scientists should be able to draw on Kunda's account (1990) of the cognitive mechanisms by which goals affect the selection of evidence. Her experiments show that, in general, people do not simply believe what they want to believe, but rather, that what they want to believe can influence their recall and use of evidence in more subtle ways that influence but do not fully determine their conclusions.

The question of how to make such integrated explanations work cannot be pursued abstractly, since the balance of cognitive and social factors is different in different historical cases. If the explanation target is why T. H. Huxley accepted Charles Darwin's theory of evolution by natural selection, cognitive factors such as the explanatory coherence of the theory should predominate, although the social relations of the two friends should not be ignored. On the other hand, if the explanation target is why some nineteenth-century U.S. industrialists embraced Social Darwinism, social factors such as the mesh between their economic interests and the idea of survival of the fittest should predominate, although the cognitive mechanisms of motivated inference must not be ignored. Similarly, the explanation for acceptance of hormonal or sociobiological explanations of behavioral sex differences may have to weight social values more heavily than evidence evaluation (Longino 1990). I now look in more detail at two important cases of the development of scientific knowledge: the chemical revolution and the development of the mathematicalmechanistic world view. These cases illustrate the interactions of cognitive and social factors whose contribution to medical knowledge are discussed at greater length in later chapters.

LAVOISIER AND THE CHEMICAL REVOLUTION

In previous work, I offered a cognitive account of the chemical revolution in which Lavoisier's oxygen theory of combustion overthrew the phlogiston theory of Georg Stahl (Thagard 1992b). This account has two parts; a description of the conceptual changes that took place when Lavoisier developed an alternative to the phlogiston scheme, and an explanation, in terms of explanatory coherence, of why he viewed the oxygen theory as superior to the phlogiston theory. Both parts are cognitive, in that conceptual schemes are taken to be organized systems of mental representations, and judgments of explanatory coherence are specified as psychologically plausible computational procedures. My account of the chemical revolution thus instantiates the cognitive schema presented earlier.

I remarked, however, that my account omitted the social side of the chemical revolution and did not presume to tell the whole story (Thagard 1992b, p. 113). What would a social explanation of the chemical revolution look like? My aim in what follows is not to provide a full social account of the acceptance of the oxygen theory but merely to sketch enough that the compatibility and integrability of social and cognitive explanations become evident. From a social perspective, we can look at the developments of Lavoisier's own beliefs and also at how these beliefs spread to the larger scientific community. Social treatments of the chemical revolution include those of Levin (1984), McCann (1978), and Perrin (1987, 1988); other useful sources include Conant (1964), Donovan (1988), Guerlac (1961), and Holmes (1985).

No scientist is an island. Lavoisier had numerous teachers, friends, and associates who contributed to the development of his ideas. We can mention, for example, Guyton de Morveau, who demonstrated to Lavoisier in 1772 that metals gain weight when calcined; Joseph Priestley, who showed Lavoisier in 1774 his experiments that mercury when heated forms a red "calx"; and his wife, Marie, who translated English articles for him, made entries in his notebooks, and drew figures for his publications. Lavoisier was elected at a young age (25 years) to the French Academy and participated in its meetings. He also had a smaller circle of chemists with whom he could perform experiments and discuss the defects of the phlogiston theory uninhibitedly at a time when senior chemists such as Philippe Macquer would not have approved of the aggressive proposal of an alternative theory. Although he alone wrote his most important publications on the oxygen theory, he had various other joint publications, including the influential *Method of Chemical Nomenclature* (1787), written with Guyton de Morveau, Berthollet, and Fourcroy.

Lavoisier's broader social situation also contributed to his work. His substantial income as a tax farmer meant that he had ample resources and time to conduct his experiments (although this position ultimately led to his execution during the French Revolution). According to an early biographer, "His great wealth, his excellent education, his mathematical precision, his general views, and his persevering industry, all contributed to ensure his success" (Thomson 1813, p. 82). Understanding how the spread of oxygen theory differed between France and England requires an appreciation of the institutional differences between the two countries, which McEvoy summarizes:

> The difference between Lavoisier's corporate view of knowledge and Priestley's individualistic epistemology highlights the difference between the institutional organization of French and British science in the late eighteenth century. In the highly organized and centralized community of France, the pressures of formal education, centralized learned societies, employment opportunities, and a competitive system of reward and recognition meant that aspiring French chemists had little choice but to follow the intellectual lead of the academicians in Paris. In contrast, the organization of English science was much weaker, comprising fewer educational institutions, decentralized societies, little employment opportunity, and a looser congregation of amateurs with closer ties to entrepreneurial industry than their French contemporaries. Thus, whereas the highly integrated community of state-subsidized French theoreticians provided fertile ground for the flowering of paradigmatic conformity during the Chemical Revolution, the dissemination of Lavoisier's theory in England met with a more varied resistance. (McEvoy 1988, pp. 210–211)

Thus, a full explanation of the development of the oxygen theory should not be limited to conceptual development and belief revision, as in my cognitive account. Nevertheless, there is no incompatibility between that account and the relevant social information. No matter how much is said about how Lavoisier gained information from his associates or about how his social situation inclined him to act in certain ways, there remains the problem of describing how his conceptual system developed and changed as he formed and adopted the oxygen theory of combustion, rejecting the phlogiston theory that he had held as a young chemist. As is displayed in the Integrated Cognitive-Social Explanation Schema, cognitive and social explanations of conceptual change can coexist.

Both mind and society contributed to the development of the oxygen theory, but they do not tell the whole story either. The experiments of de Morveau, Lavoisier, Priestley, and others were an important part of the development of eighteenth-century chemistry: Neither mental nor social construction can fully explain why experiments on combustion and calcination gave the results they did. The growth of scientific knowledge is a function of mind, society, *and* the world. The difficult task for science studies is to create a synthetic account of how mind, society, and the world interactively contribute to scientific development.

The social side of the chemical revolution becomes even more prominent if one addresses the question of how scientists other than Lavoisier came to adopt the oxygen theory. Contrary to the common view that adoption of a revolutionary theory comes only when the proponents of the previous theory die off, the oxygen theory was almost universally adopted in France and (more slowly) in England by scientists who had to abandon their previous phlogiston beliefs. A cognitive explanation of this switch goes roughly like this. Through personal contact with Lavoisier or his disciples, or through reading his argumentative publications, scientists began mentally to acquire the new scientific conceptual scheme. The new mental representations enabled them to understand Lavoisier's claims and to appreciate that the oxygen theory has greater explanatory coherence than the phlogiston theory. This appreciation was part of a cognitive process that led them to accept the oxygen theory, abandoning the phlogiston theory and its conceptual scheme.

From a social perspective, we want to know more about how information spread from scientist to scientist. Diffusion of the oxygen theory was slow, even in France (Perrin 1988). Members of Lavoisier's immediate circle, such as Pierre Laplace, were fairly quick to adopt his views, but the majority of French chemists came around only in the late 1780s and early 1790s. According to Perrin, nearly all converts initially resisted Lavoisier's theory but underwent a conversion that lasted several years. The duration of conversion has both a cognitive and a social explanation. The cognitive explanation is that developing a new conceptual system and appreciating its superiority to the old one is a difficult mental operation; the social explanation is that information flow in social networks is far from instantaneous. Lavoisier and his fellow antiphlogistinians worked to improve the flow—by giving lectures and demonstrations, by publishing articles and books, and by starting a new journal, *Annales de Chimie.* It is also possible that different scientists had different interests that made them resistant to the new theories, although I know of no documentation of this. It is certainly true that different scientists had different initial beliefs and cognitive resources. My cognitive account of Lavoisier cannot be automatically transferred over to all the other scientists, since they had different starting points and associated beliefs. In principle, we would need a different cognitive account for each scientist; but these accounts would have a great deal in common, since the scientists shared many concepts and beliefs, not to mention similar underlying cognitive processes.

Thus, there is much more to a social account of the chemical revolution than

ALTERNATIVES TO COGNITIVE INDIVIDUALISM

Downes (1993, p. 452) accuses me and others of *cognitive individualism*, "the thesis that a sufficient explanation for all cognitive activity will be provided by an account of autonomous individual cognitive agents." Obviously, I do not hold this position and in fact have given a battery of arguments for why psychological reductionism in science studies is bound to fail (see chapter 14). But the kind of anticognitive view that Downes seems to prefer in alliance with Latour, Woolgar, and Collins is also bound to fail. Downes distinguishes three levels of social aspects of science, each of which can be shown to have an essential cognitive component.

The first level is the "public embodiment of scientific theories," which includes the textbooks, research papers, instruments, and other shared property of the scientific community. These things clearly exist outside the mental representations of individual scientists, and naturalistic science studies cannot ignore their significance. But part of this significance is cognitive: The use of textbooks, papers and instruments by scientists presupposes scientists' mental capacities to read, write, plan, design, and in other ways produce and use such tools. The public embodiment of scientific knowledge would be pointless if scientists lacked the cognitive processes to understand and produce the embodied objects. Use of external representations such as books and diagrams means that the thought of each scientist does not have to rely entirely on his or her own internal mental representations; but internal representations are needed to comprehend the external ones.

Downes's second level is social interaction, such as is found in complex laboratory work in which no one researcher is entirely responsible for the ultimate result. This level is indeed of great importance, as is clear from research in fields such as psychology, in which most research is collaborative, and experimental physics, in which almost all work is collaborative. But the importance of collaboration and social interaction speaks only against the most implausible forms of psychological reductionism and provides no support for purely social accounts (see chapter 11). Understanding how scientists work with each other in part requires understanding how they communicate with each other, which in turn requires cognitive theories of how they represent information and use language and other means, such as diagrams to convey information to each other. Level 2 is undeniably social, but it is also undeniably cognitive.

Downes's third social level depends on the claim that the activities of scientists make sense only when taken in the context of a broader scientific community. The difference between someone performing an experiment and someone else doing the same physical motions in a play lies in the fact that the former is part of a community of experimenters. We can grant this social distinction,

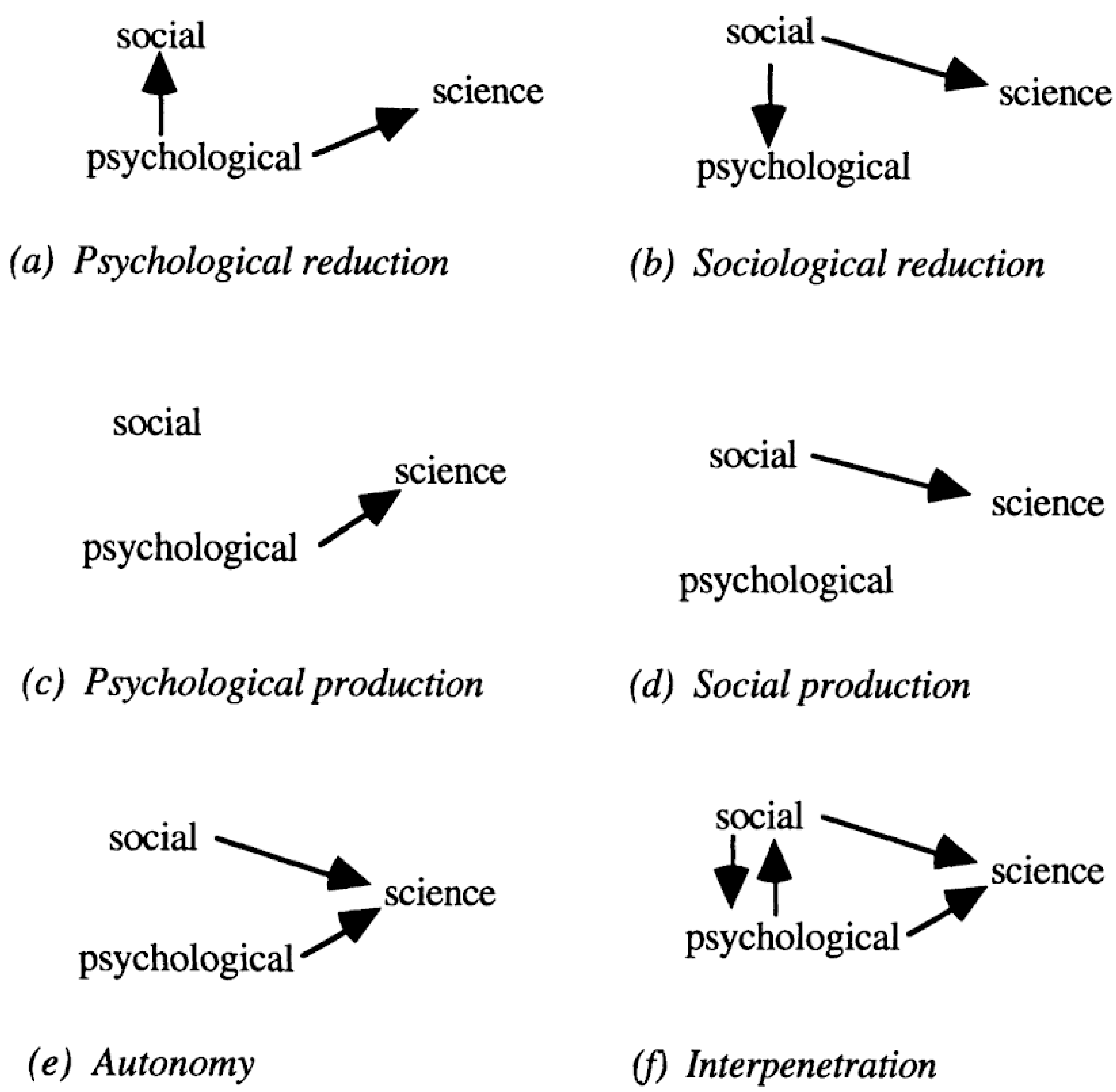

Figure 1.1. Six models of the relation of social and psychological explanations of science. The arrows signify "explains."

but we cannot help but notice that there are also obvious cognitive distinctions. The mental representations of the trained scientist are drastically different from those of the actor who is merely mouthing lines, since the scientists have absorbed an enormous amount of both declarative and procedural knowledge in the course of training. The ability of the experimenter to plan experiments and interpret the results cannot be explained purely in terms of social context but must also make reference to mental structures and procedures.

My arguments that Downes's three social levels each have a crucial cognitive aspect are in no way an attempt to explain them psychologically. We can appreciate social aspects of science at each of these levels while simultaneously appreciating relevant cognitive aspects. Figure 1.1 illustrates six possible relations between psychological and social explanations of science. Schemas *a* and *b* express extreme views about the dominance of a particular style of explanation. Psychological reductionism (*a*) is the view that everything about science, including social aspects, can be understood in terms of the psychology of the individuals involved. An analog of this view may survive in the economic doctrine of methodological individualism, which proclaims the reduction of macroeconomics to microeconomics, but I know of no one in science studies who holds this view. Sociological reductionism (*b*) is the view that everything about science, including its psychological aspects, can be understood in terms of social factors. In their most rhetorical moments, some Marxists and social constructivists approximate to this view. A slightly more modest view (*d*) advocates social explanations of science but does not purport to explain the psychological. Similarly, schema *c* proposes to simply ignore the social explanations while providing psychological explanations of science. The last two schemas (*e* and *f*) present less dogmatic views of the relation of mind and society. Schema *e* eclectically proposes that social and psychological explanations of science can proceed in relative autonomy of science, perhaps explaining different aspects of science, whereas schema *f* presents a potentially richer and more dynamic view of science studies, in which the social and the psychological are mutually informed. The task before us is to specify these interactions in much more detail, as chapters 3 to 6 do for the development and acceptance of the bacterial theory of ulcers.

The best strategy for naturalistic studies of science is neither psychological reductionism nor sociological reductionism but an integrated approach that takes both the cognitive and the social seriously. To conclude this chapter, I argue that such an approach can be normative—prescritive of how science should be done—as well as descriptive of how it is done.

MIND, SOCIETY, AND RATIONALITY

When the sociology of scientific knowledge arose in the 1970s with its implication of supplanting logical explanation schemas with social ones, philosophers were aghast. Philosophers in the analytic tradition have viewed incursions of psychology into epistemology as assaults on rationality. Incursions of sociology seemed even worse, especially given the rampant relativism of sociologists such as Woolgar (1988), who think that scientific objectivity is an illusion. However, as epistemology and philosophy of science have come to take psychology more seriously, it has become obvious that psychologism requires new theories of rationality but need not embrace irrationalism or relativism. For example, Giere (1988), Goldman (1986), Harman (1986), and Thagard (1988, 1992b) all use psychology to challenge traditional logic-based conceptions of rationality while opening up new territory for rational appraisal.

Similarly, taking the social context of science seriously does not entail relativism. Goldman (1992, p. 194), Kitcher (1993), and Solomon (1994) have outlined how social practices, like cognitive processes, can be subject to rational appraisal, for example, concerning the extent to which they promote reliable beliefs. Logical explanation schemas carry rationality with them for free, since any beliefs that are inferred logically are presumably warranted. With cognitive and social explanations, the matter is more complicated. We have to ask first what is the best cognitive and social account of a scientific development and only then raise the question of whether the cognitive and social processes invoked are ones that promote the ends of science. In pursuit of the first question, philosophers of science can ally themselves with psychologists, sociologists, and historians of science who, lacking an appetite for the second question, may choose to leave concern for rationality in philosophy, its traditional home. But rational appraisal of social practices and organizations has barely begun (see Goldman 1992, and chapters 11 to 13 of this text).

Solomon (1994) has made the audacious proposal that the scientific community, rather than the individual scientist, should be taken as the important unit of cognitive processing. She contends that a scientific community may reach a consensus that can be judged to be normatively correct from an empirical perspective, even though not one individual scientist in the community made an unbiased judgment. Although the view that she calls "social empiricism" is a useful antidote to past neglect of social aspects of rationality, it swings too far in that direction. My Integrated Cognitive-Social Explanation Schema allows various cognitive and motivational biases to influence the judgments of scientists. But if these biases are as dominant as Solomon suggests, it becomes mysterious how the community collectively reaches a consensus based on empirical success rather than on communal delusion. On the other hand, if scientists share cognitive processes such as those postulated by my theory of explanatory coherence (Thagard 1992b), then their convergence on the empirically successful theory despite their disparate individual biases becomes intelligible. Individual evaluations of the merits of competing theories are not all there is to rationality, but they are an indispensable part of it.

A key conclusion to draw from the interdependence of cognitive and social explanations of scientific change is that the appraisal of cognitive and social strategies must also be linked. Cognitive appraisal should consider the fact that much scientific knowledge is collaborative, and we should therefore evaluate particular cognitive strategies in part on the basis of how well they promote collaboration (see chapter 11). Conversely, social appraisal should take into account the cognitive capacities and limitations of the individuals whose interaction produces knowledge. Determining how to facilitate the growth of scientific knowledge, like the more descriptive task of explaining this development, depends on appreciating the complex interdependencies of mind and society. The next five chapters, however, are primarily descriptive and attempt to explain the development and acceptance of the bacterial theory of ulcers. I return to the question of social rationality in chapters 11 to 14.