CHAPTER 1

Science as a Social Enterprise

1.1 The Shaping of Knowledge in Science

We begin this introductory chapter with the case of a young scientist who made a revolutionary discovery. In September, 1983, Barry Marshall, an unknown internist at Australia's Royal Perth Hospital, presented his and pathologist Robin Warren's findings at the Second International Workshop on *Campylobacter* infections in Brussels. They had already published separate, technical "letters" together in the *Lancet* (Warren and Marshall 1983). In these letters they reported the presence of bacteria (later classified as *Helicobacter pylori [H. pylori]*) in the stomach lining of patients with gastritis. Following the presentation of their joint paper at this international conference, Marshall stood before doctors, researchers, and specialists in microbiology, gastroenterology, and infectious diseases. In response to a question from one of the experts, Marshall declared that he believed this bacterium was the cause of all stomach ulcer disease and that chronic ulcer recurrence could be eradicated in most, if not all, patients with a treatment of common antibiotics and bismuth such as Pepto Bismol (Chazin 1993; Monmaney 1993).

Marshall and Warren's theory that ulcers were the result of bacterial infection in the stomach lining was greeted with both intense interest and skepticism by experts in the field. After all, the claim that *anything* could live in the intensely acidic environment of the stomach was as unbelievable as it was revolutionary. If the claim were true, the implications for ulcer sufferers—and as Marshall himself later noted (2005b), for the major drug companies manufacturing acid blocker treatments—would be substantial. Although experts in the field were polite and respectful in the professional journals, they later told the popular press that when Marshall first presented the theory, they thought he was "brash" (SerVaas 1994, p 62), "a madman" (Chazin 1993, p 122), "a medical heretic" (Monmaney 1993, p 65), "a crazy guy saying crazy things" (Monmaney 1993, p 66). The experts were intrigued, however, and the problem was important enough to demand attention, as evidenced by letters and editorials in scientific journals devoted to gastroenterology and internal medicine.

Marshall and Warren started a revolution in the field of gastroenterology (Carey 1992). By 1993, more than 1500 studies around the world had lent support to Marshall's theory (Monmaney 1993). In a major shift in policy in February, 1994, a panel of the National Institutes of Health released a consensus statement in which it accepted that there is a relationship between *H. pylori* and ulcer disease and recommended that antibiotics be used in the treatment of stomach ulcers where the bacterium is present (NIH 1994). In April, 1996, based on Marshall and Warren's findings, the FDA approved the first drug specifically designed for treating ulcers. A sure sign of the acceptance of the theory, several books, including a handbook, have been published on the nature, identification, and manipulation of new species of *H. pylori* and their physical and genetic structure (Harris and Misiewicz 1996; Clayton and Mobley 1997; Heatley 1999; Mobley et al. 2001). Marshall himself edited a collection of firsthand accounts, spanning almost a century, of discoveries of *H. pylori* worldwide (Marshall 2002). And one of Marshall's most outspoken critics, David Graham, edits *Helicobacter*, a journal devoted exclusively to the subject. Dr. Marshall later was tenured at the University of Virginia School of Medicine, established and directs the Helicobacter Foundation, and runs a lab at the University of Western Australia. The testing for *H. pylori* and its treatment have been extended to the investigation of common dyspepsia (e.g., Chiba et al. 2002). Methods for more accurately and easily detecting the presence of *H. pylori*, such as a breath test for *urease*, continue to be developed (e.g., Alimenterics 1999; Harvard Medical School 2007; WebMD.com 2008 [see the Additional Resource for Chapter 9]). The *British Medical Journal*'s first Masterclass for General Practitioners was on the procedures for diagnosing and treating *H. pylori* infection (Shah 2007).

In 2005, Marshall and Warren won the Nobel Prize in Medicine. Yet, it had taken more than a decade for their theory to be accepted. And most of the development and support came from other physicians and scientists. Although Marshall had continued to publish technical letters, abstracts, reports, and some retrospective studies, he had great difficulty getting his clinical research published and felt his message about ulcer treatment was not being heeded (Chazin 1993; Marshall 2005d). The decade following his presentation in Brussels was a period of trial and tribulation for him.

Why was there such negative reaction to Marshall's presentation in Brussels? Despite all the positive editorials by scientists expressing interest in the theory and the subsequent research and confirmation of the claim, why did Marshall have difficulty getting his research published? Why wasn't his theory immediately embraced, as he, the press, and the public thought it should have been? A close examination of the answers to these questions can provide insight into the role of communication in the creation of scientific knowledge.

Certainly, part of the answer has to do with the somewhat shaky nature of Marshall's methods—with his science itself. In 1982, Warren, the Royal Perth pathologist, had observed the presence of the bacterium in the stomach and showed Marshall the pathology slides. Together they studied 100 patients suffering from peptic ulcers and found that *H. pylori* was present in 87 percent of

the cases. This was the study they presented in Brussels and later published as a research article in the *Lancet* in 1984 (Marshall and Warren 1984). But by his own accounts (Marshall 2005a) and those of experts such as David Graham, chief of gastroenterology at Houston's Veterans Affairs Medical Center, Marshall was "not the greatest researcher of all time" (Chazin 1993, p 123). One scientific editorial pointed to the 1984 Marshall and Warren study in the *Lancet* as "well planned" (Lancet 1984, p 1337), but in 1988 Marshall's large-scale study was rejected by the *New England Journal of Medicine* as "inconclusive" (Chazin 1993, p 123). Other editorials in scientific journals criticized the validity of his methods and conclusions (see Lam 1989), and Marshall admitted to the press that he was more interested in curing patients than in developing adequate methods and conducting large clinical experiments needed to support his claim; he had hoped that clinical success with patients would be enough to convince his colleagues (Chazin 1993). Thus, Marshall was faced with the problem that to some degree all scientists, not just the lucky ones who wind up doing important research, face in their career: How do you get other scientists to listen to you?

To gain the acceptance he thought his theory deserved, Marshall did something that most scientists would—and should—never do. Unable to convince his colleagues that *H. pylori* caused stomach ulcers, in 1984 Marshall created a potent mixture containing the bacteria and drank it, inducing a case of acute gastritis in himself (Marshall et al. 1985). As Marshall explains in his Nobel Prize lecture, "I felt that there was an urgency to solve this dilemma and do the experiment. The only person in the world at that time who could make an informed consent about the risk of drinking Helicobacter was me. So I had be to in my own experiment..." (Marshall 2005d). As he also points out in his Nobel lecture, he had at least the implicit cooperation of others, notably the lab chief who grew the precise strain and strength of bacteria for the mixture, and the endoscopists, one of whom was Warren. But at the time, Marshall's methods were seen not only as unorthodox but also as potentially dangerous. Although Marshall's experimenting on himself led to further research, the incident made his critics even more skeptical of his professionalism and less accepting of his theory (Morris et al. 1991; Carey 1992).

Another reason Marshall's theory wasn't readily accepted had to do with the prevailing assumptions about, and treatment of, gastrointestinal diseases. Warren and Marshall were not the first to observe bacteria in conjunction with gastric inflammation. Medical researchers as far back as the late 1800s had reported and even published images of bacteria living in the stomach lining (Blaser 1987; Marshall 2002). But these findings had been dismissed either as contaminants introduced during biopsy or as unrelated agents existing near ulcers; because of the presence of hydrochloric acid, gastroenterologists assumed that the stomach was "a sterile organ" (Warren and Marshall 1983, p 1273). As Monmaney reported, "Marshall's theory challenged widely held and seemingly unassailable notions about the cause of ulcers. No physical ailment has ever been more closely tied to psychological turbulence" (1993, p 64). Traditionally, ulcers had been attributed to weak stomach linings and/or to an increase in stomach acids caused by emotional trauma, tension, nervousness, or modern life itself. In his published Nobel Prize lecture, Marshall was unequivocal: "I realized then that the medical understanding of ulcer disease was akin to a religion. No amount of logical reasoning could budge what people knew in their hearts to be true. Ulcers were caused by stress, bad diet, smoking, alcohol, and susceptible genes. A bacterial cause was preposterous" (2005c, p 267).

The contrast between the initial response to Marshall's hypothesis and the subsequent success of his theory suggests several important points about how knowledge is shaped in science and the importance of communication in that process. We will highlight these briefly here by way of introduction and then will examine them in more depth in this chapter and throughout this book as you begin to explore them in your own field.

- Scientific experimentation and knowledge are governed by tacit beliefs and assumptions about what is factual, valid, and acceptable; these beliefs and assumptions are "social" in nature.
- Communication is central to the growth of scientific knowledge in each discipline, and thus to the advancement of science itself.
- Persuasion is an integral part of scientific communication; it includes the use of sound arguments and an appropriate style of presentation, as well as acceptable scientific theories, methods, and data.
- As social enterprises, scientific fields are also to some degree governed by explicit conventions about how and what to communicate, conventions that professional scientists expect each other to follow; failure to follow these can result in a failure to communicate and thus can hamper the advancement of scientific knowledge.
- Collaboration and cooperation both within and across disciplines and professions are essential to the development of scientific theories, research, and knowledge. As you will learn in your exploration of communication in your field, collaboration and cooperation are central to research and to the actual writing of research papers and proposals. Scientific knowledge is built and shared through collaboration and cooperation.

1.2 The Social Nature of Science

As you will see when you explore your own field, science is a social enterprise. In one sense, this means that science is a part of the larger society in which it is situated. Science is shaped by the values of the dominant culture in which scientists participate and live, sharing many of its assumptions, goals, biases, and problems (NAS 1995; Lyne 1998). Conversely, science also exerts a powerful influence on society. Think about decisions you've made recently concerning such practical things as medical treatment, diet, energy and fuel consumption, and weather. Indeed, at a deep level, our very way of thinking about the world is rooted in current scientific practices and beliefs.

Our primary focus here, however, is on the social nature of the activity within scientific communities and related settings, rather than on the general social context in which these communities are embedded; the challenges and implications of communicating science in the public realm will be considered in Chapters 3 and 8. Scientists in a discipline constitute a *community* in which knowledge is

built, is validated, and has meaning. Warren needed Marshall's clinical knowledge to understand how the bacterium he was observing was related to gastric symptoms (Chazin 1993; Warren 2005a,b). Marshall needed Warren in order to understand the biology of the bacterium he was observing in his patients (Marshall 2005a,c,d). And both Marshall and Warren relied on prior research to guide their own, and needed other scientists and the NIH to further test, validate, accept, and extend their work in theory and practice. In providing healthcare to their patients, physicians applying this research needed the pharmaceutical industry to develop tests and treatments for *H. pylori*, and the drug companies required FDA oversight and approval. We saw earlier that this can be a slow process. Like any society, scientific communities operate by a system of assumptions and beliefs that govern the perception and understanding of phenomena, the methods used, the research that is conducted, and the kinds of conclusions that can be drawn and treatments developed.

Before reading further, try the classic "nine-dot problem" presented in Figure 1.1. In the next five minutes, connect all the dots below with only four straight lines and without lifting your pencil from the page. If you have done this problem before, do what Einstein did—a *Gedanken* (thought) experiment: connect all the dots with only one line; there are several ways of doing this.

Often the difficulty is that the solution cannot be found within your current cognitive framework; your assumptions about the problem, the experimental and cultural context in which you are working, and your expectations about the solution all influence your perception. Thus, according to Kuhn (1996), finding the solution involves something like a "gestalt" switch, where you see the problem in a new context, a context that allows you to find the solution.

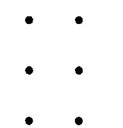

FIGURE 1.1 Nine Dot Problem (Adams 1976).

Notice that solving the nine-dot problem involves going outside the conceptual space you perceived and believed to be a square. The perception that dots form lines is, of course, one of the basic principles of geometry, which defines a *line* as the track made by a moving point, and so in some sense this perception is based on a social belief or convention. But the belief that the solution must fall within the square created from the dots prevents the perception of the easy solution. To solve the nine-dot problem, you must "violate" or ignore conventional beliefs and assumptions. This is what leading gastroenterologists had to do to accept Marshall's theory that stomach ulcers are caused by bacteria rather than by stress.

The important point here is that assumptions and beliefs influence scientists' perception of phenomena. In *The Structure of Scientific Revolutions*, Thomas Kuhn (1996) calls these sets of assumptions in science *paradigms*. Paradigms are learned tacitly through observation and imitation of scientists and practitioners in the field (Polanyi 1958); and they are learned explicitly through education, textbooks, and specific practices (Kuhn 1996). Thus, some of the assumptions in paradigms are only implicit or subconscious, and some are explicit in the form of rules and accompanying examples. In his Nobel lecture, Marshall explicitly refers to paradigms governing medical perception when he and Warren announced their theory, and in the years following: "Our results were rejected because they were outside the current paradigm" (Marshall 2005d). As students of science, you are currently engaged in learning the paradigms of your fields. As we will explore in this book, paradigms include conventions, assumptions, and rules about communication in science as well.

Paradigms constrain thinking, as illustrated in the nine-dot problem. Yet, at the same time, paradigms provide the support and context for discovery. Without the background of the square created by the pattern of the dots, there would be no clear way to connect the dots, nothing to break out of, no problem to solve. In the case of *H. pylori*, the discoveries that had been made—and the assumptions, examples, and knowledge about bacteria and about ulcers that already existed in the field—provided both the scaffolding and the backdrop against which to discover the connection between the bacterium and gastritis. Although Warren, Marshall's co-worker, stated that the discovery of the ulcer bacteria "was something that came out of the blue," he also admitted he "happened to be there at the right time, because of the improvements in gastroenterology in the seventies" (Monmaney 1993, p 68). Warren, in his Nobel lecture, says his role in this discovery wasn't brilliance, and it wasn't accident: "I think I was the right person at the right time, with the right interests, who would do more than just look at it and forget it" (Warren 2005b). We observe here that the discovery by an individual scientist that takes place outside the dominant paradigm nevertheless depends on that paradigm for the perception of it. Without paradigms, we would notice nothing at all. As a set of assumptions, conventions, and examples, the prevailing paradigm in a field helps define the problems in that field, specify methods that are allowed to solve the problem, and predict what results can be expected.

This is not to deny the role of the individual scientist's perception and judgment (cf. Ziman 2000). Some assumptions in science are personal, subjective, and even aesthetic (the scientific values of simplicity and elegance, for example, are aesthetic). The National Academy of Sciences calls attention to this dimension of science:

> Researchers continually have to make difficult decisions about how to do their work and how to present that work to others. Scientists have a large body of knowledge that they can use in making these decisions. Yet much of this knowledge is not the product of scientific investigation, but instead involves value-laden judgments, personal desires, and even a researcher's personality and style. (1989, p 1)

In general practice, this subjective dimension is balanced by social paradigms. Paradigms as social mechanisms act as a check on personal judgment and individual error, and so they make science as we know it possible (Merton 1973a; NAS 1995; White 2001). In fact, what is personal, private, and subjective must be validated socially by the community of scientists to count as "objective" scientific knowledge. Thus, while paradigms that operate in a particular discipline tend to influence perception and constrain scientific research and thought, they also serve to correct and enhance it.

Sometimes, as in the case of Drs. Marshall and Warren, the individual scientist or group of scientists opposing a dominant paradigm in their field turn out to be right. When other scientists begin to shift their beliefs and assumptions and work on the problems created by the new theory, the field undergoes a paradigm shift—what Kuhn calls a "scientific revolution" (1996). Marshall himself calls the paradigm shift in gastroenterology "a revolution" (Marshall 2005d). Scientific revolutions, such as the one that resulted in the shift from Ptolemaic to Copernican astronomy, or from the belief that the earth is flat to the belief that the earth is round, are very well-known paradigm shifts. Other scientific debates in contemporary society that may involve conflicting paradigms include the discussion of whether evolution proceeds gradually or by leaps (gradualism versus punctuated equilibrium); whether the universe is expanding and contracting or is a steady-state universe; and the cause of dinosaur extinction (volcanic activity and/or global climate change versus meteor storm). Can you think of any debates that may involve conflicting paradigms in your field?

In these special instances of revolutionary or extraordinary science, as in normal science, scientific knowledge must be validated by the community of scientists (e.g., see Toulmin et al. 1984; Kuhn 1996; Good 2000). As we will discuss next, communication is essential to that process and thus is central to science itself.

EXERCISE 1.1

Working in a group of students from your field, brainstorm about a theory that has been accepted by your discipline. Prepare to discuss what you know about its history: previous theories to explain the phenomenon, the personalities of the scientists involved, the debates surrounding the newer theory, and the evidence that made it acceptable or unacceptable in your field. Does the acceptance of the theory constitute a paradigm shift (i.e., a scientific revolution), or a working out of previously held assumptions or research?

1.3 The Centrality of Communication in Science

The centrality of communication in science? To the casual observer, the phrase may seem somewhat nonsensical if not patently false. After all, what matters in science is the science itself—hypotheses, research methods, results. Certainly, these are fundamental to good science. Yet, without the communication of those hypotheses, methods, and results to other scientists, no science would be possible. Scientists in and across fields would not be able to share or build knowledge on the results of other scientists. Science would become a private, redundant, and ultimately futile endeavor.

Some external factors exert enormous pressure on scientists *not* to share. Scientists who work for industry or for government agencies, for instance, often find there are restrictions on what they can talk about. But scientists generally agree that secrecy is bad for science (NAS 1989, 1995; see Huizenga 1992). One of the fundamental principles of science is free and open communication. The National Academy of Sciences puts it bluntly: "If scientists were prevented from communicating with each other, scientific progress would grind to a halt" (1989, p 10).

Barry Marshall's sharing of his theory prompted a rethinking of the field of gastritis research, ulcer treatment, and the curing of patients. His presentation in Brussels did "arouse much interest" (*Lancet* 1984, p 1336), and the desire and necessity to check out his theory propelled scientists in the fields of microbiology, gastroenterology, and internal medicine into a flurry of research. Hundreds of articles investigating the existence, nature, classification, detection, and treatment of *H. pylori* followed, eventually lending support to Marshall's contention.

For the National Academy of Sciences, communication is the engine that drives the "social mechanism" of science (NAS 1989, p 10). And as communication scholar William White puts it, "[C]onceptual innovation is both a fundamental scientific activity and essentially a communication phenomenon" (2001, p 290). We have already noted that the communication of hypotheses, research methods, and results—in journals, at conferences, over email—is essential for the growth of science. *Sharing ideas is essential to the evolution of every scientific field.* In this sense, it is communication that binds any discipline into a community, which makes science social. Language is the basis of any society. Without language, there could be no communication, no cooperation, no concerted research effort that we note in the investigation of *H. pylori* and other scientific endeavors.

Indeed, communication in science is so important that *the credit for a scientific discovery is awarded not to the scientist who discovers a phenomenon, but to the scientist who publishes its discovery first.* (Actually, even more telling, it is the date the paper is received by the journal that determines historical priority.) This is another way in which communication, and writing itself, is central to the conduct of science. Credit can be awarded only to the scientist(s) who write up their findings—and who write first! As astrophysicist Donald Clayton (2007) discusses, Hoyle's early equation for nucleosynthesis in young stars has been "underappreciated in comparison with later works. . . . [H]e did not put to paper the equation he envisioned and described verbally. Had he done so, unambiguous scientific visibility of his achievement would have followed more easily" (p 1876–1877).

The practice of attributing originality to the scientist whose paper first reaches the offices of the journal, along with the promise of quick turnaround time for publication and support by the institution and/or journal in disputes concerning the ownership of ideas and discoveries, was begun by the Royal Society of London in the 17^{th} century. By protecting the rights of the author, the Royal Society hoped to ensure open communication and the sharing of ideas in science

by alleviating the (real) fear among scientists that their ideas or results would be stolen by others. The National Academy of Sciences cites the example of Isaac Newton, who wrote in Latin anagrams so his findings could be on record but not publicly available (NAS 1989). Marshall and Warren were not the first to observe the presence of *H. pylori* in the stomach, but they were the first to recognize and write about its role in gastritis, and their joint letters to the *Lancet* thus mark the historical point of discovery of that phenomenon (Warren and Marshall 1983; Marshall 2005a).

Since historical priority is awarded to the scientist(s) whose manuscript reaches a publication first, scientists who do original work and want that work recognized and used by the field must write and publish as quickly as possible. Warren and Marshall presumably felt this need, for they, like many other scientists, submitted initial reports of their important discovery in the form of brief technical letters or preliminary notes rather than as fully elaborated research reports. (The Watson and Crick letter in *Nature* about the double-helix structure of DNA is a famous example of this genre.) Speed of publication is necessary not only because of the rapid advance of the field but also because of scientists' need to ensure their claim of originality (Merton 1973b; see Miller and Halloran 1993).

Thus, while scientific journals protect the science that is submitted and published, the necessary speed of publication in most sciences also creates competition: "researchers who refrain from publishing risk losing credit to someone else who publishes first" (NAS 1989, p 9). It is a matter of historical fact that eminent scientists such as Darwin, Watson and Crick, and others were pressured *to write up* and publish their results to beat the competition. Darwin didn't want to write at all, and only did so when he heard that Alfred Russell Wallace was about to publish a theory of evolution (Campbell 1975); Watson and Crick were hotly competing with another lab to be the first to announce the structure of DNA (Watson 1968; Halloran 1984). As you will read in Chapter 2, technology has only sped up the process of publication as well as response, which also has begun to affect communication conventions in multiple ways. Learning to write quickly and well is important in science.

There are other, less obvious but absolutely crucial ways in which communication has come to play a key role in science. The processes of writing and submitting papers, of giving presentations, and of writing grant proposals in a real sense *define the nature and activity of the field and the state of knowledge within it.* The acceptance or rejection of conference abstracts, scientific papers, and grant proposals by conference organizers, journals, funding agencies, as well as by peers, becomes a vehicle not only for the dissemination but also for the control of scientific research. As the National Academy of Sciences states, "At each stage, researchers must submit their work to be examined by others with the hope that it will be accepted. This process of public, systematic skepticism is critical in science" (1989, p 10).

Peer review is the primary mechanism through which such gatekeeping is accomplished. In addition to the dissemination of scientific research, it is the function of research journals to ensure "quality control" by deciding what is acceptable to publish in the field. These decisions are typically accomplished via a "peer-review" system in which journal editors send the manuscripts they receive from researchers to other experts working in the same field for evaluation. After soliciting evaluations on a given manuscript from several such experts, an editor uses their assessment to make a decision about whether the paper merits publication in the journal. (Authors also receive the comments of the reviewers, which influence subsequent revisions.) Through the process of peer review, journal editors and reviewers determine what gets published and thus influence what scientists read and, to some extent, what scientists work on (Bazerman 1983; NAS 1989; Rowland 1997; Relman 1999). (Warren notes that disbelieving reviewers delayed the publication of their first paper in the *Lancet* [Warren 2005a, p 302]. Marshall's three-year clinical study did not pass peer review because it was inconclusive [Chazin 1993], but his smaller studies and technical notes were allowed into print.) Similarly, research funds are typically allocated using a peer-review process. Funding agencies such as the National Science Foundation, the Department of Energy, and the National Institutes of Health evaluate proposals or requests for funding by assigning them to appropriate groups of experts for peer review; then, based on those recommendations, the funding agency decides which studies to fund, thus again determining what kind of research can proceed in the field (Myers 1985; Seiken 1992). (We consider related ethical issues in Chapter 3, and discuss mechanisms of peer review in more detail in Chapters 4, 6, and 7.)

Most scientists believe that peer review, as the social system of checks and balances that works against personal bias and ensures quality control, is at the heart of good science. Scientists therefore take this system of checks and balances very seriously and believe that violating or circumventing it makes for risky science. One of the ways the system can be circumvented is by releasing scientific studies or results to the public prior to peer review and publication in professional journals; this prior release has been dubbed "pre-publication" (Ingelfinger 1977). Except for presentations at scientific meetings and conferences (Angell and Kassirer 1991), journal editors become concerned when scientific results are released prematurely or independently of scientific publication. The National Academy of Sciences summarizes this concern:

> Bypassing the standard routes of validation can short-circuit the self-correcting mechanisms of science. Scientists who release their results directly to the public—for example, through a press conference called to announce a discovery—risk adverse reactions later if their results are shown to be mistaken or are misinterpreted by the media or the public. (1989, p 10)

It is because of this gatekeeping function of the peer-review process, the need and desire to control the flow of information and ensure "quality control," that email and Internet discussion lists, which in the past have had fewer quality control mechanisms in place, are a concern for professional scientific organizations (NAS 1995). However, as we will see in Chapter 2, some of the parameters of "pre-publication" are evolving with the advent of new technological media.

These traditional gatekeeping mechanisms, and the entire consensus process by which theories and results are verified and accepted as knowledge in the scientific community, depend on the fair, accurate assessment of research. *Papers must be written in a way that makes the science accessible, testable, and acceptable* to journal editors and other colleagues in the field. As proposed many years ago by philosopher of science Karl Popper (1959) and is still widely believed, a scientific hypothesis or theory must be susceptible to falsification in order to be verified; that is, a hypothesis or theory must be wholly testable (as opposed to tested) before it can be accepted by the scientific community as a valid hypothesis or theory (but cf. Ziman, who argues that in reality there are too many hypotheses to all be tested [2000]). One way in which findings are tested is through replication, in which scientists in a later study repeat or build on the methods and results of an earlier one. The practice of replication as standard procedure was recommended at the beginning of modern science by Francis Bacon to address the untrustworthiness of the senses and mind in interpreting what we see, playing tricks with our perception and understanding of reality (Bacon 1605).

In addition, *the development of knowledge in science depends on the willingness and ability of scientists to share information after publication.* Research reports must provide enough information for readers to evaluate the plausibility and rigor of the researchers' theory, methods, and results; but scientists can't and don't include every detail of their experiments in their reports (Berkenkotter and Huckin 1995). Researchers must be willing to make further details available to others working in the field. This is especially important when you consider that scientists cannot be present at each other's observations and experiments. Rather, scientists must rely on how observations and experimental results are presented in writing. Here again we see the centrality of writing and communication in science. As Popper (1959) pointed out, the statements contained in a research report come to embody and represent the science itself. Hypotheses, theories, experiments, and results are primarily presented, obtained, and critiqued through publication (Latour [1979] 1986; Bazerman 1988; Winsor 1993; White 2001).

EXERCISE 1.2

1. Carefully read the summary contained in Figure 1.2. Describe the sequence of events. Now think about the discussion in this chapter. Based on the information given here, why do you think Pons and Fleischmann did what they did? Explain the reaction of the scientific community. What principles of science were involved? What principles of scientific communication did Pons and Fleischmann violate? Were they justified in doing so? Can you think of a scenario in which scientists would be justified in doing so?
2. Now compare the story of Pons and Fleischmann with that of Marshall and Warren. What are the similarities between these two cases? What are the differences? Do you think the differences between these two cases have anything to do with the eventual acceptance of Marshall and Warren's theory, or with the rejection of Pons and Fleischmann's? Explain. Why do some scientists still argue in favor of cold fusion and seek to duplicate the results that Pons and Fleischmann said they achieved? What should Pons and Fleischmann (or Marshall) have done differently? Why?

On March 23, 1989, at a Salt Lake City press conference called by the University of Utah, two electrochemists, Dr. B. Stanley Pons (University of Utah) and Dr. Martin Fleischmann (University of Southhampton, England) announced to the world that they had achieved cold fusion. They claimed that their electrolysis experiment produced four times the amount of energy required to run the experiment—not by the tremendous heating and smashing and splitting of atoms (fission), but by bringing together positively charged atomic (deuterium) nuclei at normal room temperatures (fusion). The benefits of their method of achieving cold fusion would be that deuterium is available in seawater and produces much less dangerous radioactivity, and thus that the process would not require nuclear reactor facilities. Pons and Fleischmann, both chemists, thought they had made a major breakthrough in nuclear physics, where research into the possibility of cold fusion had been going on for years without much hope of success; they thought their breakthrough would benefit the entire world (Crease and Samios 1989; Maddox 1989). A flurry of experiments followed, with major research labs around the world (MIT, Cal Tech, Harwell in Britain, for example) diverting attention and money to cold fusion projects.

On March 24, the day after Pons and Fleischmann's press conference, *Nature* received a paper by a team of physicists, led by Stephen E. Jones, working on cold fusion at Brigham Young University. This paper made much more modest claims about cold fusion. Pons and Fleischmann apparently had a prearranged agreement with Jones, made at a March 6 meeting between the scientists and the presidents of their respective universities, to submit their papers simultaneously to *Nature* on March 24 (Huizenga 1992). However, on March 11, unknown to Jones, Pons and Fleischmann submitted a paper to the *Journal of Electroanalytical Chemistry* (Fleischmann and Pons 1989); the revised version was received on March 22, the day before the press conference on March 23, and appeared in the *Journal of Electroanalytical Chemistry* on April 10, 1989. The paper had been faxed around the world so many times that only the words "Confidential—Do Not Copy" were legible (Huizenga 1992, p 24). This paper was later followed by the publication of extensive corrections, called "errata," including the omission of the third author, Marvin Hawkins. Contrary to what was widely believed and reported in the press, Pons and Fleischmann never submitted a paper to *Nature* (Huizenga 1992). On April 26, 1989, the University of Utah asked for $5 million from the Utah state legislature, and Pons and Fleischmann appeared before the U.S. Congress to ask for an additional $25 million immediately (and $125 million later) for a Cold Fusion Institute to continue their research. The paper by Jones et al. (1989) was published in *Nature* on April 27. At the American Physical Society meeting in Baltimore on May 1 and 2, other groups reported negative results from cold fusion experiments, and at the American Electrochemical Society meeting in Los Angeles on May 8, Fleischmann reported flaws in some of Pons's and his original results. On May 18, the first full-fledged critique of Fleischmann and Pons's paper appeared in *Nature*. Petrasso et al. (1989a) criticized the research on the grounds that it lacked adequate controls and that the equipment may have been miscalibrated, and attributed the reports of energy production and other by-products to those errors. Even though it was "only" a "preliminary" or "technical note," many scientists thought that Fleischmann and Pons's paper should have been revised again before being published. As Huizenga comments:

> When the paper was finally available for examination by an anxious scientific community, most readers were shocked by the blatant errors, curious lack of important experimental detail and other obvious deficiencies and inconsistencies. David Bailey, a physicist at the University of Toronto, said the paper was "unbelievably sloppy." He was quoted as saying, "If you got a paper like that from an undergraduate, you would give it an F." (1992, p 24)

Scientists also complained that there wasn't enough information in the paper for others to replicate the experiment. Pons and Fleischmann refused

FIGURE 1.2 Chronology of communication events in cold fusion. Among other sources, we are indebted to Huizenga (1992) for the basis of the chronology here.

to answer criticisms directly or to provide crucial details of their experiment (Petrasso et al. 1989b; Huizenga 1992). As reported by *The New York Times:*

> Drs. Pons and Fleischmann offered little help to the unfortunates struggling to repeat their work; they declined to provide details of their techniques and refused to send samples of their equipment to laboratories for analysis. . . . When someone claimed that it was not possible to produce cold fusion, the two Utah [sic] scientists would add more instructions. As Robert Park, head of the Washington office of the American Physical Society, remarked, "Anytime someone did the experiment with no results they would say, 'You didn't do the experiment right,' and offer up another tidbit." (Crease and Samios 1989, p 3D)

When asked for more information, Pons and Fleischmann claimed "that they preferred to press on with more urgent work rather than stop to handle the reviewers' criticisms" (Crease and Samios 1989, p 3D).

As researchers failed in their attempts to test or reproduce Fleischmann and Pons's results, many of the big laboratories terminated their expensive cold fusion experiments. By early July, a special advisory panel to the Department of Energy, co-chaired by John Huizenga, had recommended against awarding special funds for cold fusion research. In 1996, a judge ruled against Pons and Fleischmann in a libel suit they had brought against an Italian journalist who had reviewed their work in a book on scientific fraud. In his decision, the judge ruled that the journalist's review was justified, citing "important opposition from the scientific community, not just against the theory of the research and the way the experiments were conducted, but also the way the data were divulged and the conclusions reached about the future direction of research" (Abbott 1996). For many scientists, subsequent research, including a paper published by Pons and Fleischmann in 1993 (Pons and Fleischmann 1993), added little to what was already known (Amato 1993; Dagani 1993). The field split between "believers" and "nonbelievers," both of whom continue to try to convince each other of their positions (Dagani 1993; Greenland 1994). In 2004, DOE established a second committee to evaluate research in cold fusion, and once again arrived at the conclusion that the evidence wasn't persuasive enough to warrant federal funding of this research program. "While significant progress has been made in the sophistication of calorimeters since the review of this subject in 1989, the conclusions reached by the reviewers today are similar to those found in the 1989 review" (DOE 2004, p. 5).

Despite the lack of funding from DOE, cold fusion research continues and has some advocates. The U.S. Navy's Space and Naval Warfare (SPAWAR) Systems Center in San Diego remains understandably interested in this technology and the development of this potential fuel source (Van Noorden 2007). At the Cold Fusion session of the American Physical Society (APS) Meeting in March 2006, thirteen papers sponsored by the Naval Research Laboratory were delivered (see Chubb 2006). Research also continues in industry and some governments, as well as groups of scientists (Feder 2005). An "invited symposium" on cold fusion was featured at the 2007 American Chemical Society's (ACS) conference (Van Noorden 2007). In 2008, India, having abandoned cold fusion research for sixteen years, began revising its effort again after the former chairman of the Indian Atomic Energy Commision recommended reviewing the research program (Jayaraman 2008; Srinivasan 2008). Authors, complaining about a lack of funding, continue to research and publish on this controversial subject (see Anderson 2007; Szpak et al. 2007). Pons and Fleischmann and their supporters continue to stand by the discovery and to work on cold fusion (see Moore 2000; Van Noorden 2007). But most scientists continue to be skeptical about claims of excess power produced from low-energy nuclear reactions and critical of poorly documented experiments and the unrepeatability of results (DOE 2004). A website of ongoing research has been developed and is available online (see http://www.std.com/~mica/cftrefs.html).

FIGURE 1.2 *(continued)*

1.4 The Role of Persuasion in Scientific Communication

We have already discussed the importance of sharing information in science. But facts do not speak for themselves. Rather, facts are interpreted and presented as evidence in scientific arguments contained, for example, in research reports, conference presentations, or grant proposals. In the two cases we have examined, that of Marshall and Warren and that of Pons and Fleischmann, the initial failure to gain acceptance for a theory can be directly attributed to a failure to convince colleagues of the validity of the work. The problem is not only a matter of methods and data; it is also a matter of the accessibility, quality, and presentation of evidence—the persuasiveness and style of the argument made.

Neither Marshall and Warren nor Pons and Fleischmann did a particularly good job of persuading their colleagues. In the end, Marshall's critics were persuaded by the results of other researchers, made possible in part because Marshall and Warren provided enough information to make their theory testable; Pons and Fleischmann apparently did not. While Marshall and Warren's theory has revolutionized the field of gastroenterology, Pons and Fleischmann's theory is still hotly disputed, as is their professional credibility.

Persuasion is central to scientific communication. Persuasion tends to be a dirty word in our culture, and a tricky subject in science, which traditionally prides itself on objectivity. But in addition to acceptance by editors and reviewers associated with journals and funding agencies, the work of scientists must ultimately be accepted by the scientific community at large. As mentioned previously, science consists of those findings that have survived the scrutiny of the community and the test of time. Individual findings take on the status of scientific knowledge as they are accepted by more and more members of the field. Thus, the process of building scientific knowledge is best described not through individual facts, but through the achievement of consensus about what counts as fact (Kuhn 1996; Good 2000).

And this consensus is created through scientific argument (Prelli 1989; Lyne 1998; Ziman 2000). In the cases of Marshall and Warren and of Pons and Fleischmann, we get a glimpse of the importance of persuasion, argumentation, and debate in the construction of scientific knowledge. In later chapters you will explore this dimension of science in your own field. To briefly illustrate the role of argumentation in science here, let's return to the case of Barry Marshall. In the exchange of technical letters in the *Lancet* that followed Warren and Marshall's initial announcement in their joint letters, scientists focused not only on methods and data, but also on proving or disproving the validity of Marshall's argument. Debate raged not so much about data, but about reasoning from the data. Veldhuyzen van Zanten et al. (1988), Lam (1989), Marshall et al. (1989), Loffeld et al. (1989), Bell (1991), and others commented on Marshall and Warren's logic, evidence, classification, terminology, beliefs, and judgment. In most cases, it was the *reasoning* of the scientists—and thus the persuasiveness of their argument—that was being called into question. Commenting on a 1989 paper by the Marshall team, Walter Peterson (1989, p 509) pointed to "a number of problems *with this paper* that compel me to urge that *its recommendations* not be accepted" (emphasis ours). Once published, the letters themselves became the object of critique.

Persuasion is created not only by the logic of arguments but also by presentation and style (Myers 1985; Montgomery 1996; Fahnestock 1999). Earlier, we said Marshall had trouble getting people to listen to him because of the way he answered questions at the conference in Brussels. Certainly, a part of the problem was Marshall's position as a young internist speaking before seasoned experts in gastroenterology, an outsider working against the dominant assumptions in the field. Marshall himself admitted to the press that the odds were stacked against him. But critiques by his colleagues in the popular press indicated that they were skeptical not only because of his youth and casual appearance but also because of how he presented himself. Beyond one's past "reputation," the persona one projects through language—what Aristotle called *ethos*, the persuasive character of the speaker or writer created in and through language—can be understood to operate in scientific communication as well (see Halloran 1984; Miller and Halloran 1993; Constantinides 2001). Although the contents of his conference paper may have been appropriately qualified and cautious, Marshall struck listeners as brash and reckless because of his presentation style and the way he answered questions:

> Unschooled at such presentations and filled with boyish eagerness, he refused to respond to questions in the measured, cautious manner of most researchers. Asked whether he thought the bacteria were responsible for some ulcer disease, Marshall replied, "No, I think they're responsible for *all* ulcer disease." Such blanket statements, backed only by small studies and anecdotal case histories, alarmed many researchers. (Chazin 1993, p 121–122)

Pons and Fleischmann too, were criticized for overstating their claims. Physicist Stephen Jones, on the other hand, made more modest claims in his report in *Nature*, and so he was more believable. As *The New York Times* reported, Jones's "colleagues took him seriously not because he was one of their own, nor even because he showed up at all the important meetings to defend his work. Rather, it was because his work betrayed an awareness of potential pitfalls" (Crease and Samios 1989, p 3D). As we will see in subsequent chapters, that awareness is reflected not only in what is said, but also in how it is said. Sociological research has shown that the kinds of arguments and styles employed in formal scientific communication often differ from those in informal settings. Much gets said in the lab that would not be said in more formal forums such as the research report or grant proposal. In formal communication, scientists employ a style that subordinates their personal preferences and professional allegiances (Merton 1973a; see Couture 1993). Regardless of the validity of his claims, Marshall's enthusiasm seemed inappropriate in this formal context. Even Walter Peterson, one of Marshall's most staunch opponents, says, "We scientists should have looked beyond Barry's evangelical patina and not dismissed him out of hand" (Chazin 1993, p 124). Of course, the Nobel Prize vindicated Marshall and Warren—but as Marshall humorously points out in his Nobel Prize speech (2005a), Nobel Prizes are not easy to come by, and most scientists don't receive them. So the question is: Can most scientists look beyond style of argument, appearance, and delivery when this is how science is presented?

The reaction of scientists to Marshall's and to Pons and Fleischmann's presentacommunication. To be persuasive, scientists must make the claims of their research believable in the context of the previous research and the existing paradigm of the field; and they must present these arguments in professional forums and styles that are acceptable in the scientific community.

1.5 Scientific Communication and Convention

As illustrated by the preceding discussion, the forums and styles a scientist chooses can make a difference in how well the results of his or her research are heard and understood. The conferences you attend and in which you participate, the publications to which you submit your work, the funding agencies to which you apply, even the institutions for which you work—all can make a difference in how well your research is received and whether it is used by other scientists.

As you will discover in working through subsequent chapters, different types or genres of writing follow different implicit and explicit "conventions"—that is, different organizational, evidentiary, and stylistic patterns that have come to characterize that genre's use in a particular community. Understanding what these conventions are and how to use them to demonstrate the nature and significance of your research to other scientists in your field is one of the things that distinguishes a professional scientist from a student scientist.

As we join a research community, we gradually acquire knowledge of the ways in which that community develops and communicates knowledge. This happens naturally, and usually without conscious effort, as we read papers written by other researchers, attend conferences, and talk informally with new colleagues in the classroom, the field, the development lab, or other research workplace. We are gradually *socialized* into the communication patterns the community has adopted. (We will discuss ethical issues related to this process in Chapter 3.)

You have probably already started thinking as a biologist or soil scientist or wildlife researcher. If you've taken a course or two or have pursued a personal interest in the field, the socialization process has begun. Not only have you begun developing *content knowledge*—the principles, concepts, and terminology that members of the field take for granted; but you've also begun to acquire *procedural knowledge*—knowledge of how to do things in this research area: how to solve problems, how to test hypotheses, how to use the basic methods of your field, and how to communicate your concerns, questions, and findings to others in the community. The more familiar you become with the ways of thinking, speaking, and writing in your field, the easier it is for you to quickly understand written texts, to grasp important concepts and recognize issues that matter to the field, and to contribute to written and spoken conversations—that is, to participate in the development of new knowledge.

Your content and procedural knowledge develop together, though not necessarily at the same rate, and are mutually reinforcing. You can't write like a physicist if you don't know anything about physics. And you cannot acquire knowledge of physics if you're not able to read the texts written by physicists, apply the experimental procedures and theorems that physicists have developed, and communicate with others in the field about your questions and your

developing understanding. Thus, the process of socialization involves learning the subject matter of your particular branch of science and also learning how to reason and communicate as a member of your research community. In Chapter 4 you will take a close look at reports published in research journals in your field. You'll be looking at how the authors of those reports have introduced the problem motivating their research, how they've described their methods and results, and how they've explained and defended the conclusions they've drawn. Our emphasis on exploring such forms of scientific discourse is grounded in the assumption that this knowledge is both generative and constraining (Carpenter & Krest 2001). In studying the genres of a discipline, students entering that discipline learn *what* can be said as well as *how*, and they also learn what is *not* said in particular forms or forums.

For example, when science is "written up" in a typical research report, it is not presented to the scientific community as a personal narrative or story of "what happened" in the actual order that it happened. Although the report may include a chronological description of methods, this description is embedded in a broader argument in which a claim or hypothesis is supported or refuted. Thus, several scholars have pointed out that the traditional report does not accurately represent the processes of scientific research (Medawar 1964; Bazerman 1988; Gross 1990; Ziman 2000), which might include emotions, beliefs, intuition, mistakes, missteps, accidents, serendipity, and other "non-scientific" factors (see Holton 1973; Kuhn 1996; Feyerabend 1978; NAS 1989, 1995; Harris 2005). In his Nobel Prize lecture, Robin Warren (2005b) settles on serendipity as perhaps the most accurate term to describe his part of the discovery. The Delphic oracle (Chapter 11) also illustrates how science initially may be "rooted in serendipity, hard work, and productive dreaming" (Broad 2002, p D1). Two ground faults—one of them exposed by a widening of the road to accommodate the need for tourist buses to turn around!—were first discovered at the Delphi site by Jelle Zeilinga de Boer, a geologist hired by the Greek government to determine the geological conditions for constructing nuclear reactors in the area (Broad 2002). When De Boer discussed his puzzling finding over a bottle of wine in Portugal with archeologist John Hale, the two hypothesized that, contrary to accepted belief in their respective fields, these fissures may have been the source of the gas that inspired the Delphi priestesses' prophetic pronouncements and visions. None of this was included in their published report, however (compare Broad 2002, p D1, and De Boer, Hale, and Chanton 2001 [both in Chapter 11]).

In writing a report, the creative process of scientific imagination and discovery is reconceptualized along the empirical and mathematical lines of the argument needed to justify the science to a particular scientific community (see Carnap 1950; Holton 1973; cf. Feyerabend 1978; Fuller 2000; Ziman 2000). The four-part structure of the conventional research report (introduction, methods, results, discussion) requires the writer to begin not with the first step in the experiment but with an argument for the significance of the research question. Personal narrative does play a role in science; it is often employed to communicate science to general audiences (see Katz 1992a; Jorgensen-Earp and Jorgensen 2002; Samson 2006), particularly in general-interest magazines and television programs, and often in email, where discussions are more informal. We will explore this important dimension of science in

Chapter 8. But in a formal research report in most fields, the narrative form probably would be considered inappropriate in convincing other scientists of the validity of research and might actually undermine that attempt. As you continue to read and study research reports in your field, you'll become more sensitive to the distinctive ways in which scientists in your research community persuade each other that their questions are important, their methods sound, and their results carefully interpreted.

It is important for students contemplating becoming professional scientists to know the conventions of their fields, to understand the underlying assumptions and attitudes that give rise to those conventions, and to understand how to work within them in order to be heard. Theoretically, the more you know about the community you want to join and about the socialization process itself, the easier your initiation into the community will be. Thus, you can simply let the socialization process take its course, or you can actively seek out the distinctive patterns of communication and interaction that characterize your field. The discussion and activities in this book are intended to help you recognize the communication patterns that have developed in the scientific community you are joining.

1.6 The Role of Collaboration in Scientific Communication

One of the most striking features that becomes evident when examining scientific communication is its collaborative nature. Science itself tends to be more collaborative than work in some other fields "because of the specialization and sophistication of modern research methods," and "the increased emphasis on interdisciplinary research being prompted by funding agencies" (Macrina 2000, p 157, 158). Given this trend, the role of communication becomes even more important. (In our citing of scientific research in this book, note the number of times "et al.," designating multiple authorship, has been used; even the shorter technical letters to the editor are often written collectively or represent a group of scientists.) The discovery of the role of *H. pylori* in the development of stomach ulcers clearly has a collaborative history. Marshall drew heavily on earlier studies to support his claim that there is a connection between bacteria and ulcers by showing that bacteria in the stomach lining already had been noted but had been overlooked as a cause of ulcers; Marshall and Warren wrote joint letters and a research report together to argue this claim (see Chapter 9); finally, other scientists validated, confirmed, and extended Marshall and Warren's research and began testing and developing treatment. The creation of scientific knowledge is truly a collaborative process.

In the Marshall and Warren case, we see how scientific progress involves collaboration within the discipline of gastroenterology. In addition to in-field work, collaboration increasingly occurs across disciplines. A good example of this kind of interdisciplinary collaboration can be found in the case we present in Chapter 11 on the Delphic oracle. In this research, an archeologist, a geologist, a chemist, and a clinical toxicologist eventually teamed up to work on a theory that the legendary trances and visions and occasionally violent frenzies of the prophesying priestesses at the Temple of Apollo at Delphi were caused by vapors that rose from a subterranean fault beneath the temple floor. First postulated by writers in antiquity, this theory was virtually dismissed in the early 20th century by archeologists and geologists who first excavated the site and found no large faults or volcanic activity in the region (Spiller et al. 2002). What is of particular interest in this case is that collaboration occurred not only across the boundaries of disciplines, but across the geography of time as well. This diverse team of scientists relied not only on each other, but also on the evidence provided by the testimony of another group of diverse authors: ancient Greek and Latin philosophers, historians, poets, orators, geographers, travel writers, and biographers (Spiller et al. 2002, p 190), particularly Plutarch (see De Boer et al. 2001; also see Plutarch in the Additional Resources, Chapter 11). The keen observations and reports of these ancient writers provided not only the questions that guided the hypothesis of the team of contemporary researchers, but also some compelling evidence (e.g., see De Boer et al. 2001, p 710). Also drawing on ancient literature as well as modern science is the refutation of the "ethylene-intoxication hypothesis," by Foster (a philosopher) and Lehoux (a classicist). In their 2007 article, published in *Clinical Toxicology*, we see that collaboration again crosses what C.P. Snow once called "the Two-Cultures"—the humanities and the sciences—thus dissolving another artificial boundary.

Another example of the collaboration of scientists, who together can truly be said to be attempting to cross the vastness of time and space, is the range of experts working on the Kepler supernova project. The range of expertise represented on this team, as detailed in the final paragraph of the Reynolds et al. proposal in Chapter 12, clearly illustrates that collaboration is necessary for the development of theory and the conduct of research in contemporary science. This is especially true in "big science" projects. Based on the roles designated at the end of the Reynolds et al.'s Chandra Satellite Observatory proposal, imagine the range of expertise and professions involved in the construction, deployment, repair, and maintenance of the Hubble Space Telescope, and in the continuous processing and publishing of images from it. Or the number of scientists and engineers from a variety of countries involved in the construction of the International Space Station. Or the Human Genome Project! In physics, biomedicine, and other fields, the effect of collaboration is vividly illustrated by the growing (and problematic) number of authors appearing on articles (McDonald 1995; CBE Task Force on Authorship 2000).

John Ziman (1968) has called the schools of thought that emerge when researchers regularly work and/or publish with each other "invisible colleges." But not all collaborations have to be large, and sometimes the professional interaction can be quite varied. Thomas Edison relied on an array of lab assistants and workers in all sectors of society (including the press) to develop and promote his inventions, especially electric power and light (see Bazerman 1999). Uglow (2002) describes the close and productive friendship of James Watt (inventor of the steam engine), Erasmus Darwin (Charles's grandfather and a physician and evolutionary theorist in his own right), potter Josiah Wedgwood, and chemist Joseph Priestly, who together created an informal "society" to share ideas and provide mutual support. As a result of this alliance, for example, Wedgwood sculpted the ceramic equipment Priestly needed to eliminate contamination in his work on gases. In all of these examples we see the benefits of multiple skills and perspectives that scientists from different fields can bring to an exploration or problem.

And although perhaps less obviously than in the case of the Delphic oracle, the use of historical research occurs in other fields as well. For example, note how Marshall's letter in the *Lancet* (page 231) situates his study of bacteria in the stomach lining in the context of prior research going back more than half a century, and how he uses that history to build a case for the significance of his own findings. (This is one of the purposes of Marshall's 2002 book as well.) In his Nobel Prize lecture, Marshall makes a point of discussing the importance of prior research in his ability not only to form a hypothesis, but even to see the phenomenon! (2005d). In recognizing the role history can play in scientific collaboration, we also more clearly see the "situatedness" of collaborative research. A research program develops in a specific time and place. History, politics, economics, and culture all influence the shape of science, just as science influences each of them (e.g., see Bazerman 1983, 1999; Lewontin 1993; Lyne 1998; Fuller 2000; Shea 2008). To begin to test the feasibility of their hypothesis, De Boer and Hale had to request that the Greek government allow their team to take samples from this ancient site (see pages 324–325). For the Greek government and the Greek state, this is not exactly a context-free request: throughout its history, and often without permission, explorers and collectors from other countries have carried away pieces of ancient Greece. (This controversy can be perused in the websites we have listed in the Chapter 12, Additional Resources.) This is yet another kind of collaboration, between scientists and governments—in this case, a foreign government. Just as specific research projects are planned and carried out in the context of a paradigm and fields, science as a social enterprise is always situated. History is ongoing and contemporaneous.

This brief overview of collaboration illustrates several important features of the conduct of scientific research: that professional scientists help each other with both the thinking and the writing of science; that readers as well as authors can come from a variety of fields, countries, and even times; and that readers, whether peer reviewers or other professionals, also influence scientific communication (see Gragson and Selzer 1993). Science is truly a social enterprise.

Activities and Assignments

1. In Chapter 9 (pages 231–233) we have reprinted the original letters Marshall and Warren sent jointly to *Lancet*, in which they announced their discovery of *H. pylori*. Write a one- to two-page analysis in which you compare and contrast Warren's letter with Marshall's. Which arguments are similar and which are different? Do you detect any difference in emphases and purpose between them? How do the letters build on each other? Why do you think Warren and Marshall decided to submit these letters together?
2. To demonstrate the value of collaboration as a means of generating ideas and discovering insights, do the NASA simulation exercise in Figure 1.3.
3. Write a one-page introduction to the "business" of your field for outsiders who are unfamiliar with that branch of science. What do botanists (or soil scientists or organic chemists) do? Where do they work? What do they study? What kinds of questions do they ask? What kinds of methods do they use? Why is their research important? Who uses the results of this research? Who is affected by the results?

Your spaceship has just crash-landed on the lighted side of the moon. You were scheduled to rendezvous with a mother ship 200 miles away on the lighted surface of the moon, but the rough landing has ruined your ship and destroyed all the equipment on board, except for the 15 items below.

Your crew's survival depends on reaching the mother ship, so you must choose the most critical items to take on the 200-mile trip. Working alone, your first task is to rank the 15 items in terms of their importance to your crew in reaching the rendezvous point. In the column labelled "Your Rank" place a number 1 by the most important item, number 2 by the second most important, and so on through number 15, the least important.

When you have finished, your instructor will give you additional directions for working in groups, and then for calculating errors based on the NASA rank.

	Your Rank	Group Rank	NASA Rank	Your Error	Group Error
Box of matches					
Food concentrate					
50 feet of nylon rope					
Parachute silk					
Solar-powered portable heating unit					
Two .45 caliber pistols					
One case of dehydrated Pet milk					
Two 100-pound tanks of oxygen					
Stellar map (of the moon's constellations)					
Self-inflating life raft					
Magnetic compass					
5 gallons of water					
Signal flares					
First-aid kit containing injection needles					
Solar-powered FM receiver-transmitter					

FIGURE 1.3 NASA simulation exercise. Adapted from Hall (1971).

4. This multipart activity involves creating and possibly presenting a profile of communication patterns in your research field. The purpose of this activity is to have you further explore the major organs and structures in your own field, and how they function. Your instructor will let you know which parts of this activity to complete.

   A. Interview a member of your research community to learn the kinds of writing, reading, speaking, and listening he or she does in professional life. A good way to elicit this kind of information is to ask your interviewee to describe a typical day or couple of days in the lab or office: With whom did he or she talk? What meetings did he or she attend? In what professional reading or writing activities did she or he engage, in either print or electronic formats (e.g., recording data, writing notes, drafting part of a report abstracts, reviewing manuscripts or proposals, participating in online discussions)? Your interview subject also should be able to help you identify the major research journals, professional conferences, funding agencies, online forums, and databases in your field. If you are working with a group, decide together on an appropriate interview subject, develop a set of questions, and arrange for one or more members of the group to conduct the interview. Feel free to interview more than one person, particularly if your group includes students from different subfields. Your instructor may ask you to summarize your findings for the class in an oral presentation, or to present them in writing, as described below.

B. Use the research you've conducted in Activity 4A as the basis for an oral or written profile of communication patterns in your research community. Your goal is to describe the distinctive practices that have developed in your community. Include the names of the most important journals, professional associations, funding agencies, and websites as examples, but also focus more broadly on how, why, and with whom scientists in your field communicate. Include written, spoken, and digital channels of communication. Tell your readers what kinds of knowledge or information are exchanged via the various channels, to whom that information is directed or from whom it is received, and what form it must take in each case.

    Your profile should be directed toward readers who are not familiar with your particular field of study. Begin the presentation or paper with a brief introduction to the field (as described in Activity 4A) to give readers some idea of the type of research that is carried out in this field. Then go on to describe the channels of communication that have developed to facilitate this research.

C. As an alternative or addition to the written profile, draw a comprehensive map of your field. Your map should identify the major journals, associations, and funding agencies that are active in your field and illustrate specific interrelationships among them.

D. In groups, exchange your written or graphic profiles with other students in the class. (If profiles were presented orally, use your notes from the oral presentations.) Ideally, each member of the group should represent a different research field or subfield. After all members of the group have read the set of profiles, develop a list of similarities and differences across fields. Prepare a brief group presentation for the class in which you compare and contrast the conventional ways of communicating in this set of research fields. For example, consider whether scientists in these fields ask similar or different types of questions, whether their objects of study are similar or different in significant ways, and whether and how their methods differ. Determine whether differences in research goals or methods have led to different patterns of communication in these communities. For example, you might examine the importance of conferences versus publications in different fields, print versus digital publication, the amount of interaction researchers in each area have with outside audiences, the amount of collaborative work in each field and the mechanisms for supporting that work, and so forth.

CHAPTER 2

Exploring Technology in Scientific Communication

2.1 Science and Technology

The interrelationship of science and technology has a long history. The ancient Greeks regarded science as a higher form of knowledge (*episteme*), and technology as a craft (*techne*) (see Toulmin and Goodfield 1962; Stiegler 1998). Thus, for a long time, technology was considered "a handmaiden" of science. In *The Structure of Scientific Revolutions*, Thomas Kuhn talks about technology as the concluding stage of a science, the application of a science being its natural "end" (Kuhn 1996). In a reversal of fortune, with the rise of the Industrial Revolution, technology became a driving force in science as well as culture. In fact, following the scientific revolution of the seventeenth century, technology played decisive roles in the development of the sciences (see Gillispie 1960; Toulmin and Goodfield 1962). As technology became more powerful it came to be considered another form of knowledge in its own right, equal if not superior to science (see Skolimowski 1966; Stiegler 1998), and in the 1960s even a kind of consciousness of the world (Habermas 1970; Heidegger 1977; Miller 1978). Most scholars now agree that science and technology work in tandem in the discovery or creation of new knowledge, as well as its propagation (e.g., see Bazerman 1999): science is essential for the development of technology (e.g., Markoff 2008b), and developments in technology are essential for the growth of science (e.g., Eisend 2002).

When we think of technology in science it's natural to think of the tools or methods with which scientific work gets done. From molecular probes for detecting microscopic organisms to satellite observatories exploring the reaches of space, new technologies are continually changing how, and therefore what, scientists can