

# **On Fact and Fraud**

Cautionary Tales

from the Front Lines

of Science

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# One

## Setting the Stage

Fraud in science is, in essence, a violation of the scientific method. It is feared and denigrated by all scientists. Let's look at a few real cases that have come up in the past.

Piltdown Man, a human cranium and ape jaw found in a gravel pit in England around 1910, is perhaps the most famous case. Initially hailed as the authentic remnants of one of our more distant ancestors, the interspecies skeletal remains were exposed as a fraud by modern dating methods in 1954. To this day no one knows who perpetrated the deception or why. One popular theory is that the perpetrator was only trying to help along what was thought to be the truth. Prehistoric hominid remains had been discovered in France and Germany, and there were even rumors of findings in Africa. Surely humanity could not have originated in those uncivilized places. Better to have human life begin in good old England!

As it turned out, the artifact was rejected by the body of scientific knowledge long before modern dating methods showed it to be a hoax. Growing evidence that our ancient forebears looked nothing like Piltdown Man made the discovery an embarrassment at the fringes of anthropology. The application of modern dating methods confirmed that both artifacts were not much older than their discovery date.

Sir Cyril Burt was a famous British psychologist who studied the heritability of intelligence by means of identical twins who had been separated at birth. Unfortunately there seem not to have been enough such convenient subjects to study, so he apparently invented thirty-three additional pairs, and because that gave him more work than he could handle, he also invented two assistants to take care of them. His duplicity was uncovered in 1974, some three years after his death.

That same year, William Summerlin, a researcher at the Sloan-Kettering Institute for Cancer Research in New York City, conducted a series of experiments aimed at inducing healthy black skin grafts to grow on a white mouse. Evidently, nature wasn't sufficiently cooperative, for he was caught red-handed trying to help her out with a black felt-tipped pen.

John Darsee was a prodigious young researcher at Harvard Medical School, turning out a research paper about once every eight days. That lasted a couple of years until 1981, when he was caught fabricating data out of whole cloth.

Stephen Breuning was a psychologist at the University of Pittsburgh studying the effects of drugs such as Ritalin on patients. In 1987 it was determined that he had fabricated data. His case was particularly bad, because protocols for treating patients had been based on his spurious results.

Science is self-correcting, in the sense that a falsehood injected into the body of scientific knowledge will eventually be discovered and rejected. But that fact does not protect the scientific enterprise against fraud, because injecting falsehoods into the body of science is rarely, if ever, the purpose of those who perpetrate fraud. They almost always believe that they are injecting a truth into the scientific record, as in the cases above, but without going through all the trouble that the real scientific method demands.

That's why science needs active measures to protect it. Fraud, or misconduct, means dishonest professional behavior, characterized by the intent to deceive—the very antithesis of ethical behavior in science. When you read a scientific paper, you are free to agree or disagree with its conclusions, but you must always be confident that you can trust its account of the procedures that were used and the results produced by those procedures.

For years it was thought that scientific fraud was almost always restricted to biomedicine and closely related sciences, and although there are exceptions, most instances do surface in these fields. There are undoubtedly many reasons for this curious state of affairs. For example, many misconduct cases involve medical doctors rather than scientists with Ph.D.s (who are trained to do research). To a doctor, the welfare of his or her patient may be more important than scientific truth. In a case that came up in the 1980s, for example, a physician in Montreal was found to have falsified the records of participants in a large-scale breast-cancer study. Asked why he did it, he said it was in order to get better medical care for his patients. However, the greater number of cases arises from more self-interested motives. Although the perpetrators usually think that they're doing the right thing, they also know that they're committing fraud.

In recent cases of scientific fraud, three motives, or risk factors, have always been present. In nearly all cases, the perpetrators

1. were under career pressure;
2. knew, or thought they knew, what the answer to the problem they were considering would turn out to be if they went to all the trouble of doing the work properly; and

3. were working in a field where individual experiments are not expected to be precisely reproducible.

It is by no means true that fraud always arises when these three factors are present. In fact, just the opposite is true: These factors are often present, and fraud is quite rare. But they do seem to be present whenever fraud occurs. Let us consider them one at a time.

*Career pressure.* This is clearly a motivating factor, but it does not offer us any special insights into why a small number of scientists stray professionally when most do not. All scientists, at all levels, from fame to obscurity, are pretty much always under career pressure. On the other hand, simple monetary gain is seldom if ever a factor in scientific fraud.

*Knowing the answer.* Scientific fraud is almost always a transgression against the methods of science, not purposely against the body of knowledge. Perpetrators think they know how the experiment would come out if it were done properly, and they decide that it is not necessary to go to all the trouble of doing it properly.

*Reproducibility.* In reality, experiments are seldom repeated by others in science. Nevertheless, the belief that someone else can repeat an experiment and get—or not—the same result can be a powerful deterrent to cheating. Here a pertinent distinction arises between biology and the other sciences, in that biological variability may provide apparent cover for a biologist who is tempted to cheat. Sufficient variability exists among organisms that the same procedure, performed on two test subjects as nearly identical as possible, is not expected to give exactly the

same result. If two virtually identical rats are treated with the same carcinogen, they are not expected to develop the same tumor in the same place at the same time. This last point certainly helps to explain why scientific fraud is found mainly in the biomedical area. (Two cases in physics offer an interesting test of this hypothesis. They are addressed in more detail later in this volume.)

No human activity can stand up to the glare of relentless, absolute honesty. We build little hypocrisies and misrepresentations into what we do to make our lives a little easier, and science, a very human enterprise, is no exception. For example, every scientific paper is written as if the particular investigation it describes were a triumphant progression from one truth to the next. All scientists who perform research, however, know that every scientific experiment is chaotic—like war. You never know what's going on; you cannot usually understand what the data mean. But in the end you figure out what it was all about, and then, with hindsight, you write it up as one clear and certain step after another. This is a kind of hypocrisy, but one that is deeply embedded in the way we do science. We are so accustomed to it that we don't even regard it as a misrepresentation. Courses are not offered in the rules of misrepresentation in scientific papers, but the apprenticeship that one goes through to become a scientist does involve learning them.

The same apprenticeship, however, also inculcates a deep respect for the inviolability of scientific data and instructs the neophyte scientist in the ironclad distinction between harmless fudging and real fraud. For example, it may be marginally acceptable, in writing up your experiment, to present your best data and casually refer to them as typical (because you mean

typical of the phenomenon, not typical of your data), but it is not acceptable to move one data point just a little bit to make the data look better. All scientists would agree that to do so is fraud. That is because experiments must deal with physical reality, a major point that can only be assured by an honest presentation of all the data.

In order to define as precisely as possible what constitutes scientific misconduct or fraud, we need first to have the clearest possible understanding of how science actually works. Otherwise, it is all too easy to formulate plausible-sounding ethical principles that would be unworkable or even damaging to the scientific enterprise if they were actually put into practice. Here, for example, is a plausible but unworkable set of such precepts.

1. A scientist should never be motivated to do science for personal gain, advancement, or other rewards.
2. Scientists should always be objective and impartial when gathering data.
3. Every observation or experiment must be designed to falsify a hypothesis.
4. When an experiment or an observation gives a result contrary to the prediction of a certain theory, all ethical scientists must abandon that theory.
5. Scientists must never believe dogmatically in an idea or use rhetorical exaggeration in promoting it.
6. Scientists must “bend over backwards” (in the words of iconic physicist Richard Feynman)<sup>1</sup> to point out evidence that is contrary to their own hypothesis or that might weaken acceptance of their experimental results.



7. Conduct that seriously departs from commonly accepted behavior in the scientific community is unethical.
8. Scientists must report what they have done so fully that any other scientist can reproduce the experiment or calculation. Science must be an open book, not an acquired skill.
9. Scientists should never permit their judgments to be affected by authority. For example, the reputation of the scientist making a given claim is irrelevant to the validity of the claim.
10. Each author of a multi-author paper is responsible for every part of the paper.
11. The choice and order of authors on a multi-author paper must strictly reflect the contributions of the authors to the work in question.
12. Financial support for doing science and access to scientific facilities should be shared democratically, not concentrated in the hands of a favored few.
13. There can never be too many scientists in the world.
14. No misleading or deceptive statement should ever appear in a scientific paper.
15. Decisions about the distribution of scientific resources and publication of experimental results must be guided by the judgment of scientific peers who are protected by anonymity.

Let's now look at each of our *diktats* in turn, beginning with principle 1. In a parallel case in economic life, well-intentioned attempts to eliminate the role of greed or speculation can have disastrous consequences. In fact, seemingly bad behavior such as

the aggressive pursuit of self-interest can, in a properly functioning system, produce results that are generally beneficial.

Principles 2 and 3 derive from the following arguments. According to Francis Bacon, who set down these ideas in the seventeenth century, science begins with the careful recording of observations.<sup>2</sup> These should be, insofar as is humanly possible, uninfluenced by any prior prejudice or theoretical preconception. When a large enough body of observations is present, one generalizes from these to a theory or hypothesis by a process of induction—that is, working from the specific to the general.

Historians, philosophers, and those scientists willing to venture into such philosophic waters are virtually unanimous in rejecting Baconian inductivism as a general characterization of

Figure 1.1  
Engraved portrait of English philosopher and essayist Sir Francis Bacon, by Dutch engraver Jacobus Houbraken (1698–1780), Amsterdam, dated 1738, possibly after a portrait painting done circa 1731 by John Vanderbank (1694–1735). Courtesy of California Institute of Technology Archives.



good scientific method (adieu, principle 2). You cannot record all that you observe; some principle of relevance is required. But decisions about what is relevant depend on background assumptions that are highly theoretical. This is sometimes expressed by saying that all observation in science is “theory-laden” and that a “theoretically neutral” language for recording observations is impossible.

The idea that science proceeds only and always by means of inductive generalization is also misguided. Theories in many parts of science have to do with things that can't be directly observed at all: forces, fields, subatomic particles, proteins, and so on. For this and many other reasons, no one has been able to formulate a defensible theory of Baconian inductivist science. Although few scientists believe in inductivism, many have been influenced by the falsifiability ideas of the twentieth-century philosopher Karl Popper.<sup>3</sup> According to these ideas, we assess the validity of a hypothesis by extracting from it a testable prediction. If the test proves the prediction to be false, the hypothesis is also by definition false and must be rejected. The key point to appreciate here is that no matter how many observations agree with the prediction, they will never suffice to prove that the prediction is true, or verified, or even more probable than it was before. The most that we are allowed to say is that the theory has been tested and not yet falsified. Thus an important asymmetry informs the Popperian model of verification and falsification. We can show conclusively that a hypothesis is false, but we can never demonstrate conclusively that it is true. In this view, science proceeds entirely by showing that seemingly sound ideas are wrong, so that they must be replaced by better ideas.

Inductivists place much emphasis on avoidance of error. By contrast, falsifiability advocates believe that no theory can

ultimately be proved right, so our aim should be to detect errors and learn from them as efficiently as possible. Thus, a laudable corollary of the Popperian view is that if science is to progress, scientists must be free to be wrong.

But falsifiability also has serious deficiencies. Testing a given hypothesis, *H*, involves deriving from it some observable consequence, *O*. But in practice, *O* may depend on other assumptions, *A* (auxiliary assumptions, philosophers call them). So if *H* is false, it may be that *O* is false, but it may also be that *O* is true and *A* is false.

One immediate consequence of this simple logical fact is that the asymmetry between falsifiability and verification vanishes. We may not be able to conclusively verify a hypothesis, but we can't falsify it either. Thus it may be a good strategy to hang onto a hypothesis even when an observation seems to imply that it's false. The history of science is full of examples of this sort of anti-Popperian strategy succeeding where a purely Popperian strategy would have failed. Perhaps the classic example is Albert Einstein's seemingly absurd conjecture that the speed of light must be the same for all observers, regardless of their own speed. Many observations had shown that the apparent speed of an object depends on the speed of the observer. But those observations were not true for light, and the result was the special theory of relativity (so much for principle 3).

Both inductivism and falsifiability envision the scientist encountering nature all alone. But science is carried out by a community of investigators. Suppose a scientist who has devoted a great deal of time and energy developing a theory is faced with a decision about whether to hold onto it in the face of some contrary evidence. Good Popperian behavior would be to give it up, but the communal nature of science suggests

another possibility. Suppose our scientist has a rival who has invested time and energy developing an alternative theory. Then we can expect the rival to act as a severe Popperian critic of the theory. As long as others are willing to do the job, our scientist need not take on the psychologically daunting task of playing his own devil's advocate. In fact, scientists, like other people, find it difficult to commit to an arduous long-term project if they spend too much time contemplating the various ways in which the project might be unsuccessful (principle 4).

A certain tendency to exaggerate the merits of one's own approach and to play down contrary evidence may be necessary, particularly during the early stages of a project. Moreover, scientists like to be right and get recognition for being right. The satisfaction of demolishing a theory one has laboriously constructed may be small in comparison with the satisfaction of seeing it vindicated. All things considered, it's extremely hard for most people to adopt a consistently Popperian attitude toward their own work. In fact, part of the intellectual responsibility of a scientist is to provide the best possible case for important ideas, leaving it to others to publicize their defects and limitations. That is just what most scientists do (principle 5).

In a commencement address at Caltech some years ago, Richard Feynman endorsed the Popperian outlook by remarking, "It's a kind of scientific integrity, a principle of scientific thought that corresponds to a kind of utter honesty—a kind of leaning over backwards. For example, if you're doing an experiment, you should report everything that you think might make it invalid—not only what you think is right about it; other causes that could possibly explain your results; and things you've thought of that you've eliminated by some other experiment, and how they worked—to make sure the other fellow can tell

Figure 1.2  
Richard Feynman,  
Caltech commencement,  
1974. Courtesy  
of Floyd Clark,  
California Institute of  
Technology Archives.



they've been eliminated.”<sup>4</sup> That is a high-minded and laudable attitude to have, but it is far beyond the capacity of most scientists. Most scientists are content to present their results without calling attention to all the ways they could be wrong (principle 6). Nevertheless, it's important for scientists to be careful to point out what could be wrong if they know it.

It may be that merely verifying a hypothesis has little intrinsic value, but it is striking that the distribution of credit in science reflects a decidedly different view. Scientists win Nobel Prizes and other coveted accolades for detecting new effects or for predicting effects that are subsequently verified. It is only when a theory has become very well established that one receives significant credit for refuting it, and while such an achievement may burnish a scientist's reputation, it rarely, if ever, results in the type of rewards associated with an affirma-

tive breakthrough. Unquestionably, rewarding confirmations over refutations provides scientists with incentives to confirm theories rather than to refute them, but as we have been arguing, that is not necessarily bad for science.

Conventional accounts of the scientific method share the assumption that all scientists should adopt the same strategies. In fact, government agencies used to define scientific misconduct as “practices that seriously deviate from those that are commonly accepted within the scientific community” (principle 7).<sup>5</sup> But rapid progress will be more likely if different investigators have different attitudes toward appropriate methods. As noted above, one important consequence of the winner-take-(nearly)-all-the-credit system is that it encourages a variety of perspectives, programs, and approaches. Thus, attempts to define misconduct in terms of deviations from commonly accepted practices are doubly misguided: Not only will such commonly accepted practices fail to exist in many cases, but also it will be undesirable to enforce the conformity that such a principle would require. More generally, we can see why attempts to discover “the” scientific method fail. There are deep, systematic reasons why all scientists should not follow some single, uniform method.

But that doesn't mean that “anything goes.” The scientific community draws an important distinction between claims that are open to public assessment and those that are not, and a scientist who fabricates data will be judged far more harshly than one who merely extrapolates beyond the recorded data. The difference is that where there is no fabrication, nothing exists to obstruct the critical scrutiny of the work by peers. Since scientists must be able to trust that the data they are critiquing resulted from a legitimate experiment, fabrication of data

is a far more serious violation of the scientific method than extrapolation.

Conducting an experiment in a way that produces reliable results is not just a matter of following rules. Experimenters, some more so than others, possess skills that allow them to get their experiments to work, often without even knowing what those skills are. Assessing whether a particular experimenter has produced reliable results may require a judgment based on whether she or he has produced dependable results in the past. The often essential but hard to quantify role of craftsmanship in designing and carrying out successful experiments is another reason why general rules of method have proved so elusive (principle 8).

These facts about specialization, skill, and authority have a number of consequences for understanding what constitutes proper scientific conduct. For example, behavior that strikes an outsider as exhibiting irrational deference to authority may have a serious rationale. When a scientist discards certain data on the basis of subtle clues in the behavior of the apparatus, and other scientists accept his or her judgment, this should not be attributed to the operation of power relationships (principle 9).

Another consequence has to do with the extent to which scientists are responsible for misconduct or sloppy research on the part of their collaborators (principle 10). It is precisely the point of many collaborations to bring together people from different specializations, with the implicit understanding that their different backgrounds and diverse abilities mean that they may not always be in the best position to accurately judge the quality of one another's work. Setting up a policy of holding scientists responsible for the misconduct of coauthors and coworkers would discourage a great deal of valuable collaboration.



Credit tends to go to those who are famous at the expense of those who are not. A paper signed by Nobody, Nobody, and Somebody will almost invariably be referred to as “work done in Somebody’s lab.” There are so many papers in so many journals that few scientists have time to read more than a fraction of those relevant to their work. Famous names tend to identify those works that are worth noticing. In certain fields, particularly biomedical fields, it has become customary to include the head of the lab as an author, even when the head of the lab didn’t participate in the research. Some people refer to this practice as “guest authorship” and regard it as unethical (principle 11). However, the practice may be functionally useful and involve little deception, since it will be well known to all participants in a field. Physics is not such a field. Most physicists recoil at the thought of guest authorship.

This brings us to a view of science called the Ortega hypothesis. It is named after the Spanish philosopher José Ortega y Gasset, who wrote in his 1930 classic, *The Revolt of the Masses*, that “experimental science has progressed thanks in great part to the work of men astoundingly mediocre, and even less than mediocre. That is to say, modern science, the root and symbol of our actual civilization, finds a place for the intellectually commonplace man and allows him to work therein with success.”

Ortega’s assertion (principle 12) is probably based on the empirical observation that there are, in every field of science, many practitioners doing more or less routine work. Less empirically, it is also supported by the idea that knowledge of the universe is a kind of limitless wilderness to be conquered by the action of many hands relentlessly hacking away at the underbrush. An idea supported by both observation and theory has a very firm basis in science.

The Ortega hypothesis was named by two sociologists, Jonathan R. Cole of Columbia University and Stephen Cole of SUNY–Stony Brook, when they set out to demolish it in a 1972 article in *Science*. They wrote:

It seems, rather, that a relatively small number of physicists produce work that becomes the basis for future discoveries in physics. We have found that even papers of relatively minor significance have used to a disproportionate degree the work of the eminent scientists.<sup>6</sup>

In other words, according to the authors, a small number of elite scientists are responsible for the vast majority of scientific progress. (The authors base these conclusions on their observations of the physics community, while contending that they are valid for all branches of science.) Seen in this light, the reward system in science is a mechanism that has evolved for promoting and rewarding the star performers.

If the Ortega hypothesis is correct, science is best served by producing as many scientists as possible, even if they are not of the highest quality (principle 13). However, if the elitist view is right, it is best to restrict production to fewer and better scientists. In any case the question involves ethical issues (What is best for the common good?) as well as policy issues (What is the best route to the desired goal?).

Scientific papers often misrepresent what actually happened in the course of the investigation(s) they describe. Misunderstandings, blind alleys, and mistakes of various sorts will fail to appear in the final written account. Nevertheless, the practice is nearly universal, because it is a more efficient means of transmitting results than an accurate historical account would be. Contrary to normal belief (principle 14), this type of mis-

representation is condoned and accepted in scientific publications, whereas other transgressions are harshly condemned. This practice may not be ideal, but it is an inherent way in which science is done.

Peer review has an almost mystical significance in the community of scientists. Published results are considered dependable because they've been peer reviewed, and unpublished data are not dependable because they have not been. (The last decade has seen a growing number of papers "published" in pre-press on the Web, without the advantage of peer review. These are naturally regarded as less reliable by most scientists.) Many consider peer review the ethical fulcrum of the whole scientific enterprise. For most small projects and nearly all journal articles, peer review is accomplished by sending the manuscript or proposal to referees whose identity will not be revealed to the authors.

The peer-review process is very good at separating real science from nonsense. Referees know the current thinking in a field and are aware of its rules and conventions. But it is not at all good at detecting fraud, as the cases of compromised papers that have successfully passed through peer review amply demonstrate (principle 15).

**¶** It has become fashionable in recent decades for scholars from the social sciences and other disciplines to visit the exotic continent of Science and send back reports of their observations of the behavior and rituals of the natives. The resulting dialogues have not always been entirely amicable and have, in fact, sometimes been referred to as "the science wars." Let us extend an olive branch by offering an entirely unjaded, unbiased insider's view of this curious terrain.

There are undoubtedly many reasons why people choose to become scientists. Simple greed, however, is not high on the list. The reason is that the rewards for success in science are not primarily monetary (although a certain degree of material well-being does often follow in their wake). If you are a scientist, each success is rewarded by the intoxicating glow that comes from knowing or believing that you have won at least one small round in the endless quest for knowledge. That glow fades quickly, however, unless it brings with it the admiration and esteem of your peers and colleagues (who are, after all, the ones capable of understanding most fully what you have done and are frequently the only ones who care). The various means by which scientists express their admiration and esteem for their colleagues are so subtle and complex that they beggar the etiquette of a medieval royal court. We will call these means collectively the Reward System of science.

Closely linked to the Reward System is a second organization that we may call the Authority Structure. The Authority Structure guides and controls the Reward System. Moreover, certain positions within the Authority Structure are among the most coveted fruits of the Reward System. Nevertheless the two are not identical. The pinnacle of the Reward System is scientific glory, fame, and immortality. The goal of those in the Authority Structure is power and influence. Scientists distinguish sharply between the two. They will sit around the faculty lounge or the lunch table lamenting the fate of a distinguished colleague who has become the president of a famous university. "He was still capable of good work," they will say, sounding much like saddened warriors grieving the fate of a fallen comrade. The university president is a kingpin of the Authority Structure but a dropout from the scientific Reward System.

The Reward System and the Authority Structure are both rooted in the institutions of science. These institutions vary somewhat from one discipline to another and from one country to another, but the broad outlines will be recognizable to all. Our discussion is most influenced by the physical sciences as they are practiced in the United States, but it will apply broadly to all science, in all countries.

Scientific research is performed in universities, and to a lesser extent in colleges that do not grant doctoral degrees. It is also performed in national laboratories and in industrial laboratories. The universities and colleges may be public or private. The national laboratories may be run directly by government agencies or managed for the government by universities or consortia of universities. Industrial laboratories are usually, but not always, operated by a single company.

Scientific societies, such as the American Physical Society or the American Chemical Society, have members from all of the above types of scientific institutions. The societies organize national and regional scientific meetings, publish journals, and administer the awarding of certain prizes and honors. They are private organizations, whose officers are elected by their members and whose costs are paid by the dues of their members and by other related sources of income. There are a few scientific societies (such as the American Association for the Advancement of Science) that are not tied to a particular scientific discipline but still hold meetings and publish journals.

There are also purely honorary societies, typified by the National Academy of Sciences (NAS). The NAS holds meetings, publishes a journal, and serves certain needs of the government through its research and consulting arm, the National Research Council. However, by far the most important thing the NAS

does is to elect its own members. Election to the NAS is one of the highest rungs on the Reward System ladder.

These are the elements of the institutions of science. We have left out a few crucial items, such as the Scandinavian bureaucracy (the Royal Swedish Academy of Sciences and the Royal Caroline Institute) that awards Nobel Prizes, and the inscrutable college of historians and journalists that somehow decides which scientists shall become famous outside of science itself. However, even within the elements described, there are infinitely subtle layers of influence and prestige.

Behind a carefully cultivated veneer of cordiality, colleges and universities wage a fierce, endless struggle of titanic proportions for positions of honor in a peculiar contest. No one is quite sure who's keeping score, but everyone knows roughly what the score is. The contest ranks each university against others, each college against others, and within a single discipline, departments against one another. (Similar rivalries exist among national laboratories, industrial laboratories, and even federal funding agencies.)

To the aspiring academic scientist, the steps on the perilous ladder to fame and glory look something like this:

1. Be admitted to a prestigious undergraduate college or university (useful but not essential).
2. Graduate with a B.S. degree (essential).
3. Be admitted to a prestigious graduate department (very important).
4. Graduate with a Ph.D. (essential).
5. Get a postdoctoral appointment or fellowship at another prestigious university (this almost always ranks lower in

the invisible hierarchy than the university where you did your graduate work).

6. Get a position as assistant professor. The caliber of the university and department is crucial, since you are unlikely ever to move up from there in the invisible rankings. National and industrial laboratories also have positions analogous to assistant professor, and some people prefer the risky course of starting in an industrial lab with the hope of being successful enough to be called to a university later.
7. Bring in outside research support (mostly from federal agencies), attract graduate students of your own, get papers published in the best journals (that usually means the ones published by the professional societies—but there are exceptions, such as *Nature*, which is privately published), get invited to speak at national or (even better) international meetings sponsored by professional societies, and generally become visible among active scientists in your field outside your own institution. It is useful, but not essential at this stage of your career, to teach well and to participate in academic committees and the like. All of these demanding and challenging steps are to be taken honestly, without the remotest hint of scientific misconduct or fraud.
8. Get tenure (as a result of doing number 7 very well).
9. Get promoted to full professor.
10. Your colleagues darkly suspect that you will now rest on your laurels, and you must prove them wrong. Get more funding; expand the size of your research group (graduate students, postdocs, technicians, etc.). Get yourself

appointed to national boards, panels, and committees, secure more invitations to speak at more meetings, and so on. If at all possible, get something (a discovery, a technique, a program, and a piece of hardware are all acceptable options) named after yourself. This is the most effective way of getting noticed, but it's also tricky, since someone else must do it for you, and then it has to catch on among workers in the field. Once again, there must be not the slightest whiff of scientific misconduct here. You might do all of these things motivated purely by the thrill of discovery, but do them you must.

11. The following are now available if you work hard enough to get them and manage to have a little luck in your research:

- Awards and prizes from your professional society,
- A named professorship,
- Membership in a National Academy,
- Major national and international prizes up to the Nobel itself, and
- Immortality.

At each of these various steps, you have faced gatekeepers from the Authority Structure of science. They are generally people who have ascended a few rungs above that level but then stepped out of the competition (remember the university president mentioned earlier). For example, the faculty of an undergraduate college (where you may choose to attempt steps 1 and 2) will generally have reached step 4 (a Ph.D.), and perhaps 5 (a postdoc), but opted out of the research competition at step 6 (by taking a position in a college rather than a research university). They may very well never have intended



to climb any higher than necessary to reach their positions as college faculty, but it would have been unwise for them to admit as much while they were climbing. Each of the gatekeepers they faced probably had to be convinced that they were aspiring to the very pinnacle. These are the people who will now decide your fate. They are most likely to be impressed if they believe you are aspiring to that same pinnacle.

At the graduate school level, your Ph.D. thesis advisor, a very important person in your life, will probably (had better be) still climbing and may very well have climbed quite high already, but decisions about you will be made also by department chairs, deans, and others who have traded their places on the ladder for positions in the Authority Structure of science.

Once you pass the Ph.D. hurdle, the rules for scaling successive steps become increasingly less well defined. The rules are often unwritten, and the people you must impress are further afield. Each promotion will require confidential letters of recommendation from people outside your own institution, solicited not by you but by the chair of a committee. You will thus be expected to be known by people you have not met, merely because of your growing scientific reputation. Your reputation will be based on published papers whose fate will be in the hands of journal editors and anonymous referees chosen by them. The research reported in those papers will be possible only if you can win financial support on the basis of research proposals submitted to the granting agencies. Your proposals will be handled by project officers (either permanent or temporary refugees from the race up the research ladder) and judged once again by anonymous referees or a panel of active scientists. Finally, even if you manage to finance and publish your work, it will be little noticed unless you manage to get invited to speak at national meetings organized by your profes-

sional society. The staff of the society will generally have dropped out of the race, but decisions about who speaks will most likely be made by committees of active scientists.

Notice that at each point of decision, there tend to be two kinds of gatekeepers. One kind is an administrator (department chair or dean, journal editor, project officer, professional society staff) and the other kind an active scientist (writer of letters of recommendation, anonymous referee, member of panels and committees). The first kind of gatekeeper has often stepped out of the race (the position itself is generally the reward for having reached a certain level), while the other is still very much in the race. The people in this latter group are not only your judges, they are also your competition. Furthermore, you have become one of them. People in the other group, if they are no longer in competition with you, have often forgotten the fierce struggle you face, and moreover they tend to have the curious view that you are working for them.

It should be clear from this discussion that scientific score-keeping is no simple matter. The issue of who will emerge as successful and famous in science depends in large measure on who has the best ideas and who works the hardest. In that sense, science is a true meritocracy. However, there are very clearly other elements at play here. One of the most important is being in the right place at the right time. For example, the discovery of quantum mechanics early in the twentieth century swept a whole generation of theoretical physicists to fame and glory. The very best made truly fundamental contributions, but even those of more modest talent found untouched problems ready to be solved with the new theory. Another example is supplied by World War II's mega-science projects, chief among them the Manhattan Project and MIT's Radiation Lab, which swept yet another generation of physicists to power and influence.

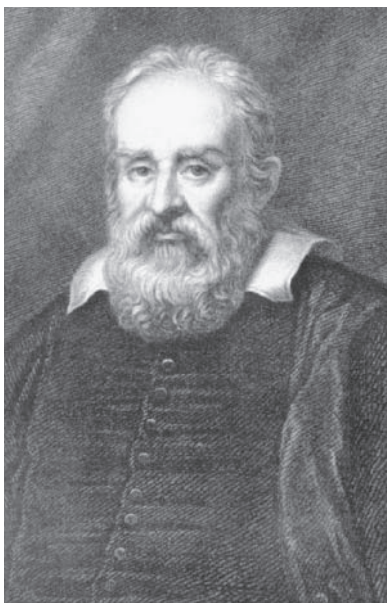
In addition to the factors we have just outlined, there are others that have been observed and documented, that arise out of the behavior and customs of scientists as a group. The late sociologist Robert K. Merton called one of them the Matthew effect, following this passage in the Gospel according to Matthew: “For unto every one that hath shall be given, and he shall have abundance: but from him that hath not shall be taken away even that which he hath.”<sup>7</sup>

The Matthew effect in science is the observation that credit tends to go to those who are already famous, at the expense of those who are not. For example, if a paper is written by a team of researchers, only one of whom is well known in the field, readers will tend to refer to the article by the alpha scientist’s name even if it is far back in the authorial pack.

The roots of this scientific Reward System and the Authority Structure date back to the seventeenth century, almost to the birth of modern science itself. It is probably fair to say that experimental physics was invented by Galileo Galilei (1564–1642), who discovered the law of falling bodies and the law of inertia by means of experiments using ingeniously crafted instruments. The scientific research laboratory was first created not much later, by English chemist Robert Boyle, who set up a team of assistants, specialists, technicians, and apprentices to carry out systematic chemical investigations. Both Galileo and Boyle belonged to scientific societies that still exist (*L’Accademia dei Lincei* and the Royal Society, respectively). Boyle supported his research by means of his own wealth, but Galileo spent much of his time and energy seeking what we would today call government and private sponsorship. (It is not for nothing that the discoverer of the moons of Jupiter named them the *Sidera Medicea*—the “Medicean stars.” Patronage by the Medici no

Figure 1.3

Galileo Galilei. Photo reproduction of Robert Hart's stipple engraving published by Charles Knight of London in 1834, after a 1757 oil on canvas portrait done by Scottish portrait painter Allan Ramsay (1713–84) and presented to Trinity College, Cambridge, in 1759, where it hangs in the Master's Lodge; Ramsay was inspired by Flemish portraitist Justus Sustermans's (1597–1681) oil on canvas portrait of Galileo painted circa 1640, which hangs in the Pitti Gallery in Florence. Courtesy of California Institute of Technology Archives.



longer being what it once was, they are today more commonly called the Galilean satellites.) Both Galileo and Boyle also engaged in fierce struggles with others over priority for scientific discoveries. In other words, the basic outlines of the social organization of science emerged almost as soon as science did, and it was firmly in place by the time Isaac Newton (who became a named professor at Cambridge and the first president of Great Britain's Royal Society) wrote his *Principia*. It is difficult to avoid the conclusion that science cannot exist—and certainly cannot flourish—without the Reward System and the Authority Structure.

Of course, professional societies, prizes, and awards, to say nothing of department chairs and deans, are by no means limited to the sciences. One can detect the basic elements of the Reward System and the Authority Structure in virtually every

academic discipline. Nevertheless, it seems better developed and more highly organized in the sciences than elsewhere. The reason is undoubtedly to be found both in the nature of science and in human nature, since it is we humans who must pursue science. Science is basically a collaborative enterprise to discover important truths about the world, carried out by individuals who are generally more strongly motivated by their own interests than by the collective good. The Reward System and the Authority Structure serve to regulate and channel this collaboration-*cum*-competition to produce useful results. So long as it succeeds in doing so, this system of ours seems likely to remain firmly in place.

In all of this, scientific misconduct plays a peripheral role, lurking quietly in the shadows: a temptation, perhaps, for some at each stage, but never a central point. The mountains described here must be scaled without a hint that any untoward activity has contributed to the ascent. Anything else is utterly unacceptable—but, as we are about to discover, not always unthinkable. With that in mind, we turn to some illuminating episodes in the history of modern science.