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Honest Error
or "Error Happens"

vestiges of error • celebrating errors • Nobel Prizes and noble errors • reflecting on the heroic mode • from acknowledgement to analysis

Artery.
Calorie.
Oxygen.
Vitamin.
Evolution.
Electric current.

Six ordinary scientific terms. And each bears historial witness to a former error in science. Each word is a vestige of a scientific theory once widely accepted, now abandoned as incorrect. The language reminds us of the mistakes.

Artery means "air duct." According to the early Greeks, that's what those blood vessels carried, along with the vital *pneuma*. Of course, arteries tend to drain in dead bodies, so when early investigators observed them, they *were* filled with air. The renowned physician Galen set that straight, but not without introducing a few errors of his own (more in Chapters 3 and /4/).

A calorie is now a unit of energy. Its name was derived from "caloric," Antoine Lavoisier's term for heat as a *substance*. But heat is molecular motion, not a form of matter. Lavoisier was wrong (more in Chapters 3 and 6).

Oxygen means "acid-former." It was part of Lavoisier's revolutionary theory of combustion. He also thought that the gas, in combination with other elements, yielded acids. Lavoisier got that wrong, too (more in Chapters 3,/8/). [Crosland, 1973]

Vitamin is short for "vital amine." That was Casimir Funk's new name for the set of micronutrients that caused scurvy, rickets and beriberi. In his analysis, they all contained nitrogen. He was wrong. Nor was this the only error in the history of discovering vitamins (more below and in Chapters 3/,4/).

Evolution means "unfolding." Originally, the term denoted an organism's embryological "unfurling" to its final intended form. With fossil discoveries, scientists learned that organisms change over geological time as well. They adopted the same term for the historical "development" of a species. Scientists assumed that the same causes governed the changes of form in both cases. And both followed a predetermined "plan." They were mistaken on both accounts. Development, guided by genetics, differs sharply from heritable changes in species, shaped primarily by opportunity and natural selection. Ironically, perhaps, Darwin rarely talked about "evolution." He referred, instead, to "descent with modification" and common ancestry. He wanted to avoid the confusing meanings, at the time, of the term "evolution."

[Richards, 1993; Browne, 2004, p. 59]

And electric current? Well, "current" because it *flows*. Electricity was originally thought to be a fluid. They even had jars — what we still sometimes call Leyden jars — to store the fluid. At one time scientists even debated whether electricity was one fluid or two. But not whether it was a fluid at all. That seemed obvious. All that was wrong, too. While we still sometimes talk informally about electricity behaving like a fluid, scientists do not see it that way anymore (more in Chapter 4). Ironically, the term "electric" itself comes from the Greek for "amber." Amber readily exhibits static electricity, but not "current." The flow of electricity was not observed clearly until almost 2 centuries after the term was introduced, when Alessandra Volta built the first batteries.

The stories of these common scientific terms embody an important lesson about science. Concepts change. Investigations can expose earlier errors. Theories may have relied on mistaken assumptions. Or incomplete information. Sometimes, apparently universal laws encountered unsuspected limits. We didn't look far enough. Or deep enough. Other times, complex patterns were just beyond our imagination. Or seemed implausible until more pieces of the puzzle emerged.

What is striking, of course, is that we have not just added to our knowledge. We have thrown out theories once deemed sound. Quite apart from modest udpating or revision. Science is not always a cumulative enterprise. Sometimes, the way we interpret nature undergoes a dramatic "gestalt switch." Old ideas are jettisoned. New ones embraced. Former "facts" can dissolve. We now know that arteries are *not* air ducts. Acids are *not* formed by oxygen. Heat is *not* a form of matter. Electricity is *not* a fluid. [Kuhn, 1970; Losee, 2005]

Such lessons are especially important in a culture that depends heavily on science. Consumers or citizens trying to assess the reliability of scientific claims need to be aware of the potential for flaws. Even more, they need to know how to assess the qualifications in particular claims. And gauge their significance. Scientists are still finding errors. They are surely still *making* errors. At the same time, they continue to expand and deepen our knowledge of the world. How, then, do we prudently interpret reliability in science with appropriate measure? This book offers an initial answer. Ultimately, by surveying the structure of error in science, I hope to inform our understanding of science, both philosophically and practically.

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Celebrating Errors

In large part this book celebrates errors in science. That may seem perverse to some. Science is about fact. And truth. Or so many people believe. Scientists do not err. When they do, something is wrong. Dangerously or pathologically wrong. The process of science — the scientific method — must have been grossly compromised. Some extra-scientific factor must have intruded. That is, in the popular perspective, any hint of error challenges the integrity of science. If errors do occasionally occur, we would certainly not want to "celebrate" them.

By contrast, as I hope will be clear by the end of the book, error may be essential to science. And an indicator of its health. Knowledge grows inevitably by trial and error, we often hear. But we may yet need to appreciate fully what that means. Are we being square and honest with ourselves? Do we accept the error along with the trials? Contrary to the public image, encountering — and accommodating — error is integral to scientific discovery and progress. To err is science, one might say. I hope this book lends sense and power to that apparent paradox.

Still, our intuitions may tell us that good scientists do not err. If so, then encountering occasions where good scientists endorsed wrong ideas would be important. We would have to rethink those initial impressions. We would have to consider how good science and error just might be compatible. It would not lessen our esteem of the great achievements. But it would certainly invite new ways of thinking about how science works.

So I begin here with some examples to inspire reflection. Because, yes, even great scientists have advocated incorrect ideas. Indeed, the more one delves into history, the more one finds surprises about famous discoveries and those who made them. It's almost as though those who write the histories and textbooks we typically encounter try to hide or downplay the errors. And perhaps they do. More about that later. First, let's consider a sampling of famous scientists, including Nobel Prize winners, and some of their noteworthy errors.

Who better to addresss in opening than perhaps the first champion of modern science, Galileo Galilei? Galileo, of course, discovered the principles of motion for bodies in freefall, epitomized in the (questionable) story of him dropping balls from the tower in Pisa, his home town. He cleverly devised experiments with balls rolling down inclined planes and methods for measuring time. With these innovations he could characterize the movement quantitatively. They also helped him establish experimentation as a new standard for developing knowledge. In addition, while he did not invent the telescope, he certainly applied his mathematical prowess in improving its optics. Then he deftly put the new device to practical work, examining the heavens. He discovered the mountainous surface of the moon, the moons of Jupiter, the rings of Saturn, and the phases of Venus. His observations of sunspots helped him interpret the motion of the Earth relative to the Sun. All remarkable achievements.

Galileo is best known today, however, for his *Dialog Concerning the Two Chief World Systems*. Here he argued vigorously for the Copernican system, assembling all the evidence then available, and using his potent rhetoric to persuade his readers. Of course, this is also the book that provoked his troubles with the Church. The story of his encounter with the Church (not really a trial in any modern sense) is a perennial favorite when proclaiming the triumph of science in the face of religious dogma and prejudice.

Yet the familiar stories leave out some key details. For example, the book was originally titled *Dialog on the Flux and Reflux of the Tides*. Yes, on the tides. The *tides*? Huh? How did that relate to Copernicanism? Well, for Galileo, the tides were caused by the combined daily and annual motions of the Earth. Each day, a point on the spinning Earth would first match the oribtal motion, then rotate to the opposite direction. As a result of going faster, then slower, then faster again, the seas should slosh back and forth in their basins, like water in a bowl in similar motion. This was the culmination, Day 4, of the dialog among his characters. Galileo knew that the challenge in persuading the astronomers *within* the Church was in providing physical arguments about the Earth's movements. Mathematical formalisms would not suffice. (Imagine that: the Church insisting on material proof.) Galileo thought that he had that very proof in the tides. That's why the Church censored his original title: a little too provocative. But Galileo was wrong. The tides are caused by the gravity of the moon, not the Earth's irregular motions. Kepler had proposed a role for the moon, but Galileo summarily dismissed that idea in the *Dialog*. Even mariners, who depended on knowing the tides, knew the importance of the moon. But Galileo did not trust forces acting mysteriously at a distance. So the Church astronomers were right: Galileo did not have solid material evidence of Copernicanism. It was an ingenious theory. But it nonetheless proved to be incorrect. Galileo's greatest work (and it remains great) centered on an error.

[Galileo 1632/19--; Brown, 1976; Drake, 1979; Conner, pp.206-209]

From Galileo, we might turn to Isaac Newton, another icon and hero of the Scientific Revolution. Newton's *Principia Mathematica* is certainly a scientific masterpiece. In it one finds the three laws of motion that now bear Newton's name. There, too, one finds the universal law of gravitation, associated with the vivid story (again, likely fabricated) where Newton watched an apple fall from a tree on his family's farm. Add, too, his development of calculus, what he called the method of fluxions. Newton's work, and his rigorous approach, guided scientists for at least the next century. One can hardly doubt Newton's supreme intellect.

Another remarkable achievement, generally less celebrated perhaps, was Newton's very first publication. In a short letter to the Royal Society, he presented the results of his years of research on prisms and the nature of light. His startling conclusion was that ordinary white light, or sunlight, is composed of many colors. A prism does not create colors. It separates them. And under appropriate arrangements, it can recombine them into white light again. [He used his theory to explain the chromatic aberration of glass lenses, and to design a reflecting telescope as an alternative, which he had presented to the Society shortly before.] But how many fundamental, or primary, colors are there? Newton was unclear. In one place, he said 5, another 7, yet another an indefinite number. He resolved that ambiguity years later in a larger book on the topic, another great masterpiece, his *Opticks*. There he likened the spectrum — erroneously — to a musical scale. He thus identified seven colors, one full "octave." And they were marked explicitly: sol, la, fa, etc. Five colors corresponded to whole steps on the scale, while orange and indigo were half-steps. This is the origin of the familiar mnemonic, ROYGBIV. It is also a handy reminder of Newton's error in conceiving colors as having harmonies, like musical sounds.

Newton's effort to fit color into a harmonic scheme, of course, reflected the same passion for mathematical order that guided his other famous works. And other, lesser known works, as well. Newton was obsessed with trying to understand a Divinely ordered universe. He compiled pages and pages of Biblical chronologies. Just glimpsing one of his manuscript sheets, one can appreciate the energy he invested in the small, but vigorous and densely packed writing. Newton's estimated end of the world in the mid-20th-century, however, has proven false. It appears. His passions also extended to alchemy and the search for a universal elixir. Indeed, when he died, Newton left more pages of notes on alchemy than on any other topic. Newton's brilliance ultimately included some pursuits and beliefs we would hardly accept as scientific today. The take-home lesson? Well, if someone as exceptional as Newton can err, then surely the rest of us can. We can expect no more from any scientist. Error will always be an inescapable part of science, for better or worse. [Newton, 1672, 1704; White, *Last Sorcerer*]

In the biological sciences Newton is matched in esteem by Charles Darwin. Darwin's theory of evolution by natural selection is surely the most important benchmark in biology, the foundation for unifying all the life sciences. It was another brilliant accomplishment, of extraordinary scope and synthesis. But that hardly exhausted Darwin's insights. He completed four volumes of barnacle taxonomy, studied the growth of plants towards light, elucidated the formation of soil by worms, documented the extraordinary varation of domesticated animals, explored the effects of plant hybridization, explained the unusual form of many orchids, and theorized about how emotions are expressed anatomically and interpreted by others. Ironically, in Darwin's own estimation, one of his greatest achievements was explaining why different flowers of the same species have female parts with different lengths (called "heterostyly"). This ensures cross-fertilization by pollinators, he noted. "No little discovery of mine ever gave me so much pleasure as the making out the meaning of heterostyled flowers," Darwin reminisced proudly in his autobiography (1958, p. 134). Darwin would surely be remembered as a great scientist even if he had never written the *Origin of Species* or *Descent of Man*.

But Darwin erred on several occasions. And those errors are worth celebrating, too. For example, unable to explain the origin of heritable variation, Darwin retreated to a theory that an individual organism's behavior could change or imprint itself on the hereditary material. He did this even while the co-discoverer of natural selection, Alfred Russel Wallace, cautioned him against it. The genes, we know more definitively now, are not susceptible to such influences.

Darwin, along with many others, also regarded a fossil known as *Eozoon* as a form of primitive life. It later turned out to be just a mineral formation. The organic appearance had been misleading. Darwin, to his credit, conceded his mistake. He also acknowledged another major error. Early in his career, he was enjoying professional recognition for his theory of the geological formation of coral atolls. The island rings were evidence of former mountains which had since eroded. Flush with confidence, Darwin then applied the same style of thinking to a geological formation in Scotland. The valley of Glen Roy was lined with stony ledges. Darwin claimed that they were geological evidence of, in this case, a gradually receding ocean. Namely, they were very old shorelines. Only later did Louis Agassiz introduce his account of glaciers and former Ice Ages. "The parallel roads of Glen Roy," as they were called, were due to retreating rivers of ice, not oceans. Darwin later referred to his own explanation as a "great blunder." So Darwin was wrong here, too. But Darwin's errors hardly discount his achievements. Rather, they give us deeper insight into Darwin, and into the nature of science more generally.

[Darwin 1859, 1877; Ghiselin, 1969; Gould, 1980; Rudwick, 1974; Browne, 1995, pp.376-378, 431-433; Allchin, 2009]

From biology, we may move to chemistry, and the renowned author of the periodic table, Dmitri Mendeleev. Like Darwin's theory of evolution, the periodic table is the discipline's great central organizing concept. It is an indispensable guide for intepreting the properties and relationships of the elements and their chemical interactions. While earlier many chemists had proposed similar such tables, Mendeleev focused on atomic weight as the fundamental feature. Based on his arrangement, he saw "holes" in the table, and thus predicted elements that no one knew to exist. In the succeeding 15 years, Mendeleev's new eka-aluminum, eka-boron, and eka-silicon were indeed found (now gallium, scandium and germanium, respectively). And they largely exhibited the properties, such as weight, density and melting point, that Mendeleev had assigned to them. Mendeleev also made bold predictions about atomic weights, contradicting established values. Later measurements bore out those claims: beryllium was revised from 14 to 9 (over 1/3 its value) and uranium doubled from 120 to 240.

Mendeleev's predictions are widely celebrated today. But Mendeleev made just as many incorrect predictions. He predicted heavier analogs of titanium and zirconium. Only to retract them soon after. He predicted eka-molybdenum, eka-cadmium, and eka-iodine. They never materialized. Neither did eka-niobium, eka-cesium, or eka-cerium, also predicted. The noble gases came as a complete surprise. Afterwards, Mendeleev became convinced that there were two noble gases lighter than helium or hydrogen. He named them simply elements X and Y. Later, he decided one of them was coronium, earlier postulated based on an unexplained spectra line from the sun. He later characterized the other as the physicists' ether, naming it newtonium and calculating its expected weight based on presumed relationships with other elements. In 1904 he also predicted six new elements between hydrogen and lithium. As you may well know (or have guessed), none of these many elements — coronium, newtonium, the unnamed series — were ever observed. Nor was Mendeleev's work with atomic weights free from error, either. Also, based on chemical properties, Mendellev switched the positions of tellurium and iodine, putting their atomic weights "out of order" in his system. He thus challenged the known atomic weights, asserting one at least would later be changed. He was wrong on that, too. (The ordering today is based on atomic number, not weight, and this is an exceptional case where the two sequences do not coincide.) Mendeleev's many errors certainly invite reflection. What is the relative meaning of successful versus unsuccessful predictions, or of discovery versus error? In doing so, however, one need not question the ultimate value of Mendeleev's insights in establishing the modern periodic system.

[Ciparik?; Brock, 1992, 321-325; <Brush 1996; Scerri & Worrall, 2001; Scerri, 2007, 131-143]

In the Earth Sciences, the figure of greatest renown is probably Alfred Wegener, the pioneering advocate of continental drift. Wegener was an astronomer and amateur meteorologist, but his interests led him to consolidate and extend the evidence that the continents were once joined together in a single land mass. That challenged the commonplace view that the Earth's crust, once formed, had remained relatively stable. Impressively, Wegener drew together information from quite different disciplines. He looked at the jigsaw puzzle fit of coastlines. He drew on the position of mountain chains along coasts. He considered paleontological evidence of climate, especially deposits of coal that indicated once-tropical latitudes. He cited matching rock types. He addressed fossils, of both animals and plants, on different continents. And he synthesized it all into a coherent view, showing that the major land masses had once been joined, and since moved. North America has once been next to Europe. South America to Africa. Antarctica to India and Australia. The Earth was surprisingly dynamic. It was a paradigm of what philosopher William Whewell once called the "consilience of inductions," expressing the unity in observations from disparate domains. While Wegener did not explain *how* contintents moved, his description of the historical pattern was significant and raised a new set of questions. Answers to those questions in the mid-1960s ushered in new platetectonic perspectives and a revolution in geological thinking.

Ironically, perhaps, Wegener's 1915 book, *The Origin of Continents and Oceans*, was not universally well received. Indeed, to us now, the book often seems a half-century ahead of its time. The noteworthy error was (according to conventional stories) the failure of scientists at the time to recognize the virtue of Wegener's arguments. Yet Wegener's original proposals were not without fault. For example, Wegener noted alignment of glacial morraines across the split continents. Yet that would imply that the last Ice Age occurred before the continents split. That does not fit the geological timelines. Wegener also suggested the continuity of geological folds (orogenic fold-belts) across the split continents. But no one now considers such continuities to exist. Indeed, the geological forces at work would tend to work in directions that would make such alignments unlikely. Most importantly, perhaps, Wegener claimed that contemporary geodesic measurements and the timing of radio communications showed continents still drifting. Greenland was apparently still drifting away from Europe at a rate of --- cms per century. Washington, DC was drifting from -------. ; and China from North America -----. The errors in these measurements — due to flawed calculations of longitude — soon became clear. The very simple errors further diminished Wegener's already marginal credibility. Finally, Wegener's lack of a viable explanation for how the continents moved was more an omission than an outright error. But the implied contradictions with current geophyscial understandings of the magnitude of various forces were certainly considered a decisive flaw by many of his critics. Wegener never solved that puzzle. While today we concur with many of Wegener's conclusions, his original presentations were not completely without fault.

[Hallam, 1989, 144-153; LeGrand, 1988, 55-69; Oreskers, 1999]

Another scientist of enduring renown is Gregor Mendel, the monk who elucidated the basic principles of inheritance. Learning Mendel's story is almost a ritual for every introductory biology student. Working with garden peas, Mendel selected several pairs of traits. He showed that, contrary to many widespred assumptions, these traits did not merely blend in the offspring. Rather, by following the generations further, he showed that the heritable factors remained discrete and in each successive generation separated and then recombined. There was an identifiable pattern to inheritance, based on paired elements for individual traits. Several decades later, Mendel's experimental approach provided a fruitful model for other investigators of heredity. They justly expressed their debt by dubbing the study of genetics "Mendelism."

Still, Mendel also partly misled subsequent geneticists. Most notably, perhaps, his traits were unrepresentative. For clarity, he had deliberately chosen traits that had only two forms. When they combined in one individual, a "dominant" trait eclipsed a "recessive" one, masking its presence. As early geneticists discovered, however, many traits (indeed, the majority, we now know) do not follow this pattern. While the unseen genetic factors do indeed remain distinct, the physical expression of features in hybrids is frequently intermediate. That is, they do not exhibit dominance. Early geneticists thus pointedly renounced what they called Mendel's Law of Dominance. It was wrong. Also misleading was the interaction of traits according to Mendel. He apparently selected his seven traits because they functioned independently. He could thereby follow each clearly. Again, in later research geneticists encountered cases where the inheritance of traits were "linked." Mendel's principle of "independent assortment" was not universal, nor even justified as an assumption. It was wrong, too. Later, this exception to Mendel's model would, ironically, lead to another important discovery: that genetic traits are carried on the chromosomes and thus form "linkage groups." These two fundamental aspects of heredity — expression of both genes in hybrids and the existence of linkage groups — violate Mendel's principles. Indeed, regarding Mendel's work on peas as a benchmark seems to have obscured rather than facilitated their discovery. Mendel himself realized this to some degree when he later tried to generalize his results on peas to another species, hawkweed. He could not find the clear simple ratios found in his earlier studies. Mendel's principles of genetics apply only in some circumstances, forming an incorrect general model . [Darden, 1992; Allchin, 2005]

All these great scientists — Galileo, Newton, Darwin, Mendeleev, Wegener, Mendel — erred. But this does not diminish their greatness. Nor does their greatness diminish the significance of their errors. If even such important figures can err, then it seems hard to imagine science without error. We need to reflect on what error in science means.

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Nobel Prizes and Noble Errors

One may well be tempted to dismiss the particular set of cases above as an unfair sample taken from centuries gone by. Error, one might contend, is due largely to ignorance. We might view astrology, alchemy, or medicine based on the four humors — from even earlier eras — as representing the best that anyone at the time could do with the intellectual resources available. Wrong, but unavoidably wrong. Even great scientists of the past would thus be limited by the status of knowledge in their historical period. Now we know more. Modern science — say, of the 20th century — would seem beyond such missteps. Accordingly, one might more appropriately consider great scientists of the past century — those whose achievements have been marked by Nobel Prizes. Surely they are beyond error? But here, too, one finds ample examples of important ideas and claims that were eventually cast aside. The coupling of great discovery and error can certainly be fascinating.

[?on the progressivist perspective: Jastrow 1936; +Taton? +?]

One of my personally favorite cases is Christian Eijkman, who shared the 1929 Nobel Prize for the discovery of a new class of nutrients, vitamins. Eijkman had investigated the cause of the disease beriberi in Java in the 1890s. Through some patient sleuthing, conjoined with a few key unplanned circumstances, he traced the disease to a difference in rice diet. Chickens and humans who ate just white rice succumbed to the disease. Those who ate whole-grain rice did not. The white-rice diet was deficient in thiamine, vitamin B1, found in the rice cuticle. But this was not Eijkman's original conclusion. He had approached this problem just as germ theory was emerging, with astounding discoveries every few years. Beriberi often appeared in institutions, suggesting contagion. Eijkman had thus been searching for the bacterium that caused the disease. In the dietary difference, he thought he had isolated the cause. The white rice contained the elusive bacterium, which produced a neurotoxin absorbed by the body. The rice cuticle contained an anti-toxin. Paradoxically, perhaps, all his evidence fit the germ-based scheme, even a study of diet in over a quarter million prisoners in Java. His conclusion was entirely reasonable. Yet it was also wrong. All the evidence also fit the concept of a nutrient deficiency. That notion was pursued by his successors. Ironically, Eijkman even rejected the role of vitamins in causing beriberi when the idea was first introduced. The Nobel Prize Committee clearly found his work significant enough to earn an award, despite this ironic error. Today, one may certainly ponder on how a great scientific work could be coupled with a "great" error.

[Carpenter, 2000, Ch. 3-4]

Consider, too, Charles Glover Barkla, recipient of the 1917 Nobel Prize in Physics, for discovering how each atomic element emits characteristic X-rays. When X-rays were still relatively new, Barkla studied how elements subjected to such radiation then re-emit their own, secondary X-rays. Akin to fluorescent materials. First, he found that such rays could be polarized. That indicated importantly that X-rays were indeed a type of wave, not a particle. Then Barkla documented the secondary rays' angles, wavelengths, and strengths. He systematically went through all the elements. His exhaustive study revealed how each element's emissions were unique, while also exhibiting a serial pattern based on atomic weight. Later, this information was linked to the number of protons in each atom's nucleus. The classification of elements on the periodic table soon shifted from atomic weight to atomic number — thus resolving some of the uncomfortable exceptions in Mendeleev's arrangement method (noted above). Later still, the characteristic X-ray emissions were used to help confirm the discovery several new elements, including hafnium and rhenium. All in all, work appropriately celebrated in the Nobel award.

Barkla's work continued. Originally, he had documented two types of X-ray emissions, labeled the K and L series. Later, using the same form of analysis, Barkla announced the presence of another series, the J series, or J radiation. He mentioned them, for example, in his Nobel acceptance speech. At first, others replicated the results. As Barkla proposed, they seemed to indicate another, undocumented dimension of atomic structure. But in 1923, Arthur Compton announced the effect that now bears his name. The J radiation could largely be explained as a secondary effect, derived from the forms of radiation that Barkla had already documented. But the effect was also based on viewing light as quantized particles, a new idea that did not conform to Barkla's classical worldview of continuities. Barkla rejected that interpretation, which soon became the community consensus. He continued to investigate the phenomenon, noting unexplained effects. But the technology of producing X-rays was rapidy becoming more precise (by focusing on specific wavelengths). Barkla opted not to adopt the newer technologies, which did not resonate with his "holistic" worldview. Without the clarity of precision on X-ray wavelengths, Barkla could not resolve the experimental inconsistencies that emerged between his data and others', or even within his own results. He was unable to meet the burdens of proof. Within a few years, Barkla's work was peripheralized as experimentally crude and theoretically ill informed. Yet he continued his investigations on the J phenomenon for over two decades, writing 20 papers, and characterizing it as a discovery "of the very greatest importance." Barkla's reputation diminished among his peers. Despite his Nobel Prize, he has since drifted into relative historical obscurity. His K and L series are frequently noted in the historical section of textbooks, his corresponding J series discretely omitted. For many people, it seems, Barkla's errors eclipse his notable achievements. [Allen, 1947; Wynne, 1976]

The image of Enrico Fermi, by contrast, has remained favorable historically. Fermi received the 1938 Nobel Prize in Physics "for his demonstrations of the existence of new radioactive elements produced by neutron irradiation, and for his related discovery of nuclear reactions brought about by slow neutrons." The discovery of slow neutrons (whose energy has been lowered, for instance by passing through paraffin) has proved an important method. They help produce radioisotopes and make nuclear power plants possible. At the time, they helped in the study of nuclear fission, eventually leading to the atomic bomb. By contrast, the award for the discovery of new elements beyond uranium (already dubbed ausonium and hesperium) proved premature. In 1934 German chemist Ida Noddack had criticized Fermi's claim. Noddack had co-discovered rhenium, and so she was familiar with the elements in Group VII of the periodic table, which would include the proposed element #93. For her, the reported new properties did not indicate such an element. Instead, she proposed, the uranium that he had bombarded with slow neutrons had undergone fission. That is, Fermi may have split his atom into elements with substantially smaller nuclei (lower atomic number). He had not ruled out such elemental products. However, Noddack's caution was brushed aside. There was no evidence for fission. Fermi – and others – assumed that nuclei could only absorb or release a few protons. Yet within a year of the Nobel award, nuclear fission was documented. Fermi's transuranic claims dissolved. He had produced barium and krypton, not elements #93 and #94. Just as Noddack had suggested. If scientific results must stand the "test of time," six years apparently had not been enough on this occasion. The Nobel Foundation had recognized Fermi for what, in part, turned out to be an error. Yet we still (justly) celebrate his many discoveries.
[Noddack, 1934; Darden, 1998; Galison, 2005]

Another striking error belongs to Alexander Fleming, who shared the 1945 Nobel Prize for the "discovery of penicillin and its curative effect in various infectious diseases." In a story widely retold, in 1928 Fleming noted the antibiotic properties of the fungus *Penicillium* in a discarded bacterial culture. Rather than dismiss the strange observation, he investigated it further. Might it be an important substance for curing or controlling infections? Ultimately, Fleming judged the antibiotic as too weak for treating human diseases. He deemed it appropriate only as a topical antiseptic, at best. Still, Fleming did find it useful. It could limit the growth of bacteria that would otherwise contaminate his vaccine production, one of his duties at the hospital. So, the history of penicillin might well have ended there. Years later, however, Ernst Chain and Howard Florey picked up the thread and pursued the production and concentration of penicillin more aggressively. They ultimately shared the Nobel Prize with Fleming. When they published their first animal studies, however, Fleming remained relatively aloof. He was not persuaded of penicillin's efficacy until the first successful clinical trials. Ironically, Fleming failed initially to recognize the potent "curative effect" that has since made his modest discovery famous and, in a sense, exceptionally meritorious. [Macfarlane, 1985, pp. 177-180, 187-189]

Error is found, too, in the work of James Watson and Francis Crick, whose model of the structure of DNA was recognized with a Nobel Prize in <1959>. Having created a model of DNA in 1953, they went on to probe the relationship between DNA and proteins. What was the "genetic code" that converted the hereditary information into functional molecules in the cell? In 1958 Crick proposed a theoretical guidepost: "Once information had passed into protein it cannot get out again." That is, cells cannot create new functions and then encode them in new DNA. The flow of information is one way only. Crick dubbed it "the central dogma." Watson soon expressed the "dogma" in his own way in his 1965 book, *Molecular Biology of the Gene*:

◯ DNA → RNA → protein

Watson's simple formula gradually eclipsed Crick's notion. It gained widespread currency as expressing a family of truths beyond doubt. First, the cellular roles of information (inheritance) and enzymatic catalysis (metabolism) were embodied in distinct molecular types: nucleic acids (DNA and RNA) and proteins. Second, only DNA could self-replicate. Third, information flowed irreversibly from DNA nucleotide sequences through RNA to amino acid sequences. All three principles later yielded to exceptions — although not without controversy. Indeed, it is a measure of the depth of this set of errors that each counter-discovery itself earned a Nobel Prize. In 1975 Howard Temin and David Baltimore were honored for discovering reverse transcriptase — which produces DNA from RNA. In the case of some viruses, the flow of information went "backwards." In 1989 Sidney Altman and Thomas Cech were recognized discovering ribozymes — RNA that can fold on itself and catalyze certain reactions, much like proteins. RNA could both carry information *and* have enzymatic function. And in 1997, Stanley Pruisner received the award for characterizing prions — proteins that can "reproduce" (or at least provide the "information" to transform similar proteins into new, disease-causing agents). Self-replication was not limited to DNA. In addition, the 2006 Nobel Prize announcement for RNA interference implied that it, too, violated the central dogma — by interrupting the "normal" transfer of information from RNA to protein. Watson's simple formula of the "central dogma" was thus wrong in many ways.

Francis Crick, for his part, never really advocated all the wrong ideas implied by Watson's expression. He was not completely free of error himself, however. He chose the term "dogma" thinking he was just labeling an unjustified belief. In common parlance, however, a dogma is an inviolable tenet. Crick erred, it seems, by not fully understanding the meaning of the word "dogma." Crick tried to clarify his meaning in 1970. But by then the label was entrenched and unchangeable. Others (through Watson) had learned to regard the central dogma as — well, inviolable dogma. What seems like a trivial error of a misconstrued definition ultimately had profound effects.

Crick earns note for yet other, more substantive errors. He investigated the structure of chromosomes, and published an structure for chromatin which turned out to be quite wrong. He became increasingly impressed by the complexity of the cell's protein-making process. He could not imagine the circumstance under which it could have originated here on Earth. Thus, in 1981 he endorsed the notion of panspermia — that life originated elsewhere and arrived here by deliberate (though unspecified) means. Quite understandably, scientists did not receive this "maverick" idea with the same esteem and respect as the double helix model of DNA. Crick's case indicates, perhaps, that we might want to reassess how we think about maverick ideas, not confusing original context and ultimate success. Errors might be noble, even while wrong.

[nobelprize.org/nobel_prizes/medicine/laureates/2006/illpres/2_central_dogma.html; Judson, 1979, p. 337; Olby, 2009]

Consider next Linus Pauling, the master protein chemist. Applying his intimate knowledge of bond angles, he deciphered the alpha helix structure of proteins in 1950, which earned him a Nobel Prize in 1954. He also reasoned fruitfully about sickle cell hemoglobin, leading to molecular understanding of its altered protein structure. However, months before Watson and Crick proposed their double helix model of DNA, Pauling proposed a triple helix model. That was more speculation than error, perhaps. Yet Pauling later came to believe that megadoses of vitamin C could cure the common cold. His Nobel status surely helped persuade others. However, the evidence indicates no such benefit. Even so, Pauling's legacy lingers in popular culture. His unqualified advocacy eventually led to him losing sources of funding. Pauling sometimes described the origin of good ideas as having lots of ideas, and throwing away the bad ones. That may well characterize science. Yet it also highlights the question of how one recognizes bad ideas. How long may they linger, and with what effect, before being thrown away?

[Pauling, 1970; Darden, 1998; Nye, 2007; Magner, 2002, pp. 357-359; Hurd, 2007]

Pauling's ideas about vitamin C partly echoed another Nobel Prize winner, whom he called "the most charming scientist in the world": Albert Szent-Györgyi. Szent-Györgyi isolated vitamin C and helped identify it as ascorbic acid. Later, he buoyed research by showing how vast quantities of it could be extracted cheaply from the paprika peppers of his native Hungary. He also claimed, erroneously, that vitamin C participates as an intermediate in mitochondrial reactions and that it could cure various medical conditions. Szent-Györgyi received a Nobel in 1937 "for his discoveries concerning the biological combustion processes." He had helped resolve a debate about those reactions — showing how oxidations leading to proton transfers could be reconciled with electron flow and the use of oxygen. He also helped elucidate the role of fumaric acid — although he identified it incorrectly as a catalyst, rather than an intermediate. Szent-Györgyi went on to contribute to muscle physiology, demonstrating the role of ATP in actin and myosin interaction. Yet he also promoted many spurious claims, such as having discovered yet another vitamin (vitamin P), and treating diabetes with succinic acid and cancer with ultrasound or mushroom juice! For every fruitful idea Szent-Györgyi offered, it seems, there was at least another that was equally mistaken. Given his heroic renown, of course, the errors often remain in shadow. [Allchin, 2007]

Another fascinating case is found in John Eccles, who earned a Nobel Prize in 1963 for his discoveries related to the "ionic mechanisms ... of the nerve cell membrane." Eccles helped characterize the transmission between nerve cells. At some junctions, he found, an incoming impulse makes it more difficult to trigger the successive impulse in the next nerve cell. At others, by contrast, the signal lowers the threshold for an impulse, making stimulation more likely. When impulses from multiple nerves converge at one junction, the next cell can start its own impulse spontaneously. This all indicated that the signals between nerve cells are chemical. The variable influence of the chemicals is critical to how the brain works. Individual nerve cells can only fire or not fire. They function in a simple binary mode. They do not transmit complex "information," as DNA does in its molecular sequence. How, then, can the brain generate the complex and nuanced responses we observe? Eccles discovery provided the answer: the chemicals between nerve cells can combine excitatory and inhibitory signals in varying proportions. That is, Eccles helped elucidate the biological basis of mind. As one might easily imagine, the experimental work was technically demanding and precise. One may puzzle, then, why Eccles considered the mind and body wholly distinct. Even more, in many publications, he argued that the existence of a divinely created soul was grounded in science. One might localize the phenomenon of consciousness to a particular part of the brain, he acknowledged, but still held that there were connections with another (non-material) entity to be described. That gap accounted for free will. For Eccles, biology could not explain that. Nor could Eccles reconcile a deterministic science with the concept of moral responsibility. He applied his dualist view to the evolution of the brain, asserting that "there can be no physicalist explanation of this mysterious emergence of consciousness and self-consciousness in a hitherto mindless world." Eccles effort to deploy naturalistic science in non-naturalistic contexts was, of course, never generally adopted. The irony, here, is that he rejected, in a sense, the special power of his own field of expertise, neurophysiology, to describe the intimate relationship between mind and body.

[Shepherd, 2007; Eccles, 1952, pp. 271-286; quote: Eccles, 1989, pp. xiii, 236-245]

Finally, we may conclude with Albert Einstein, hailed as one of the most insightful scientists in history. Few are not familiar with his famous equation, $E=mc^2$, that describes the relationship of energy and matter. Einstein received the 1921 Nobel Prize in Physics for his work on the photoelectric effect and the quantized nature of light. But, of course, he was equally renowned for his revolutionary work on Brownian motion, special relativity and general relativity. Einstein's wild white hair has thus become an iconic image of genius itself. Yet Einstein was not infallible. He believed (at first) that the universe was static, its size constant. To reconcile this status with general relativity, in 1917 he introduced a new factor in his equations: the so called cosmological constant. Ten years later, when the contrary idea of an expanding universe was proposed, he called it "abominable." Yet soon thereafter, Edwin Hubble presented evidence for a red shift in some galaxies. They were receding. The universe was indeed expanding. Einstein reversed course, calling the cosmological constant both "theoretically unsatisfactory" and unnecessary. Later in life he confided to colleagues that it was his "biggest blunder." Einstein's work on general relativity also led to the concept of gravitational waves. However, in a co-authored paper submitted for publication in 1936, he concluded that they could not exist. To Einstein's dismay (and fury), an anonymous reviewer noticed that an assumption of his about coordinate systems was mistaken. After several months Einstein amended the assumption, reversing his conclusion in the published version. Einstein also vehemently rejected quantum physics as incomplete. Hence, his famous dictum, "God does not play dice with the universe." Unfortunately, the quantum view has since become consensus, and the paradox that Einstein co-presented in 1935 as an inherent contradiction has since been resolved. Even the great Einstein could be wrong on occasions.

[O'Raifeartaigh & Minton, 2018]

So: error in science is not isolated to the remote past. Modern, well informed scientists have made mistakes. Even the most notable, award-winning researchers. Einstein, Eijkman, Eccles, Fleming, Fermi, Pauling, Watson, Crick, Barkla, Szent-Gyorgyi. One could add others, of course. And significant errors still occur, one may find by just following the news media (as detailed in subsequent chapters).

One might well feel overwhelmed by all these examples. Why dwell on so many cases of error? Of course, the wealth of illustrations itself conveys a lesson. Error is prevalent, even among the greatest scientists. The celebrated discoveries, equally part of the story, remind us that we are still deeply within ordinary scientific practice. One might begin to appreciate, then, just how important error is in understanding science as a process. Science and error seem paradoxically coupled.

Indeed, awareness of wrong ideas and failed efforts among our scientific heroes might even inspire us, on those occasions when we encounter our own errors. The occurrence of error certainly does not discount the great discoveries. Rather, it adds to them. Notably, it adds depth and realism. It restores a familiar humanness to these sometimes caricatured legends. Knowing that their accomplishments were indeed human in scale, not due to some supernatural infallible genius, makes them all the more remarkable and, at the same time, realistic. Yes, they could have failed. It might have been otherwise. That sense of fragile contingency gives the actual discoveries a sense of human monumentality.

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Reflecting on the Heroic Mode

What do we learn from these many examples of errors among some of history's most renowned scientists? First, perhaps, that we should accept errors as an integral feature of science. If so many famous scientists have all erred, trying to challenge or reassess their heroic status based on their errors seems inappropriate. We may acknowledge the errors, secure in the knowledge that their greatness is not lessened in any way. The errors are thus not indicators of scientific incompetence, ineptitude, or impoverished mental faculties. Or worse, credulity or self-delusion. Even great scientists can err, honestly. Errors must reflect, instead, something about how science works. Error is not a "failure" in the science or the scientist. It is an inescapable part of science. *To err is science*, we might say. But what does that mean for scientific practice and for interpreting its conclusions? That is the challenge addressed in this book.

Finding errors among great scientists might further indicate that error in science is not only present, but also common. Or at least not rare. Nor so infrequent that it is hard to find. If even these great scientists can err, then surely anyone can. We need to multiply the handful of examples above to imagine the scope of error among the scientific community as a whole. The emergence of error is not due to just "one bad apple" in the bunch. We thus need to adopt an image of science where errors happen. And we need to interpret them. That is, we need to accommodate error in a model of science, making sense of it without wholly unraveling the very concept of scientific knowledge or the methods for ensuring its reliability. We must find a way to understand how science and error coexist. And perhaps adjust a common preconception of them as being at odds with one another.

That is, error in science is "normal." Many scientists and philosophers who have reflected on and written about error in science have portrayed it as "pathological." They see the process of science as gone awry. An "aberration." Sometimes a "perversion." For them, error is disease-like. Science has been polluted with unscientific thinking. Worse, they fear, one "bad apple" can spoil the whole barrel. Of course, these commentators do not engage cases of error among the most respected scientists. That would challenge their portrayal of science and its authority as unimpeachable. If one can render error as "external" to science proper (they seem to think) science — and scientists — retain a pristine image. Any scientist who errs implicitly demonstrates that he was not really a scientist after all! The many cases described above show how this view is artificially blinkered, incomplete, and thus ill informed. Error is far too pervasive in science to frame it as exceptional or extraordinary. In an honest appraisal, one must come to terms with error as an integral part of science.

[Langmuir, 1989; Rousseau, 1992; Dolby, 1996; Turro, 1999; Kohn, 1988; Youngson, 1998; Gratzer, 2000, pp.viii-ix; Fritze, 2009; Pigliucci, 2010; see also critique by Bauer, 2002]

Thinking clearly about error in science also challenges us to distinguish carefully between idealized science and authentic science. One may wish that science was error-free. But that does not make it so. We need to guard against confusing *normative* and *descriptive* accounts. One depicts science as it *ought* to be. The other depicts science as it *is*. Both may be important. But we err ourselves if we mistake the idealized (normative) version for how science really happens. The fully rendered cases of error in authentic scientific practice are important benchmarks. In particular, they can help one assess the idealizations of science. What is realistically possible? Perhaps our expectations of science are not only idealized, but also utopian, beyond the reach of human capabiities? Historical cases of scientific error, especially, can help us frame "realistic ideals" — if that is not an inherent contradiction. In the same way, the historical remedy of errors can help us discern and articulate concrete methods or strategies for finding and resolving errors. It may be worth remembering too, that our current scientific knowledge is the result of “real” science, not idealized science. We should not discount it, errors included.

The tendency towards idealized interpretation is especially problematic in historical writing. Popular stories about scientific discoveries tend to focus on the discovery itself. We seem chiefly interested in how the discovery happened, not how it did not happen. Even if these two processes were intertwined at the time. We thus tend to disregard the error. It seems “wasted effort,” not part of the relevant story. We streamline the history. We idealize it. We reconstruct the past as it “should” have happened, rather than how it *did* happen. We may not even be aware that we are thereby distorting the image of the nature of science. Popular historical accounts of science thus tend to become idealized. Error is purged as a “distraction.” As a result, we rarely get the story of trial *and error*. Just the trials. And the eventual triumph. This book, again, is an effort to help restore a healthy view of science as it is authentically practiced. Ultimately, we might appreciate scientific discoveries all the more if we understood the role of errors encountered along the way.

Not the least of the idealizations are the portrayals of the scientists themselves. They are often rendered as extraordinary persons. As heroes. Of legendary status. Icons. Emblems of the virtues of science. In such a role, they become “perfect.” Perfect in the sense of error-free. Our notion of perfection — even human perfection — seems not to include making mistakes. A telltale symptom of these romanticizations of scientists is thus how they treat instances of error — when they are not wholly ignored. When they do acknowledge an error, it is promptly “excused” or explained away. The author inevitably appeals to extenuating circumstances. Faulty equipment, say. Or the error is attributed to the historical context or social factors. For example, limited available background knowledge. Or inadequate technology. Or a personality quirk that is not “really” relevant to scientific progress. Or the error is deftly transformed into an example of bold theorizing and fearless risk. Wrong, perhaps, *but clever* or *an expression of creative intelligence*. Or (when all else fails) the error is brushed aside as (obviously) insignificant when compared with the magnitude of the celebrated discovery. Through all these rhetorical tropes, the scientist himself is kept free of any hint of blemish. The hero remains a hero. Ironically, these same “external” factors are rarely considered relevant when discussing the discovery process itself. And they seem not to count as mitigating explanations for scientists who did not ultimately reach the “right” answer. Only the hero gets sympathetic treatment. Through these various asymmetries and patterns of heroic rhetoric, then, the prevalance and normality of error in science, even among renowned scientists, is typically hidden or disguised. [Allchin, 2003; Waller, 2003]

Yet there is a cost to these idealizations. Whatever we may gain by romanticizing these scientists, we lose in understanding how science works. We get an incomplete portrait of the process of science. We fail to appreciate the role of error. Indeed, we fail to fully appreciate the heroes. Thus, when scientists today err, many people view them as not living up to the heroic model. They become disillusioned. They casually dismiss science — all science — as arbitrary and worthless. Cynicism eclipses the opportunity for critical analysis. The result is a culture that fosters ill informed lawsuits against scientists for their published mistakes or even their “failure” to predict an earthquake. This is the unwelcome (and largely hidden) consequence of the scientific hero syndrome. A fresh (or honest) appreciation of error in science, then, has potentially profound cultural value. [Hall, 2011; Steinbach, 1998]

The psychology of the heroic mode certainly invites reflection. Why are the images of scientific heroes so engaging? Why so potent? Why do we make superhuman heroes out of ordinary humans? Are we projecting our own desires to transcend the commonplace? Why do we create role models from historical figures, especially if we must corrupt the history to yield the idealized status? Why are we so primed to dismiss or peripheralize their errors? Why must a hero be error-free? Can we envision a type of monumentality that still accommodates error? Can we be inspired by persons who achieved but also erred, such as the famous scientists above? Working through the psychology of scientific heroes may well be a first step towards understanding error in science. The short portraits above, juxtaposing discovery and error, are intended, in part, to invite and encourage such reflection.

At the very least, we may benefit from acknowledging the sometimes deeply *emotional* dimension of thinking about error in science. For many, it is not just a intellectual exercise. Discussion of error in science seems to pose an implicit threat. To admit error seems to challenge the edifice of (idealized) science as a trusted ultimate authority. Error violates the sacrosanct realm of (idealized) science, viewed as a supremely secure foundation for knowledge. Never mind that the standards of assessment are idealizations. They evoke feelings of allegiance and emotional investment. Accordingly, while we might view distorted romanticizations as "misconceptions," one may more appropriately call them *myth-conceptions*. They are stories whose chief function is to justify (through history) an ideological or political perspective or value. [Bauer, 1992; Milne, 1998; Allchin, 2003; Allchin & Werth, 2012]

The emotionality of myth-conceptions can express itself in striking ways. When someone mentions a scientist's error, for example, others who regard him as a hero may instantly rise to his "defense." The sense of indignation can be palpable, even in printed remarks. This is the cryptic normative conception of science erupting at the surface. Thus, a simple descriptive comment about an error may be grossly reinterpreted in *value-laden* terms. Even a sympathetic analysis of an error may be re-construed as "disparaging" criticism or as an "allegation." The apparent fear is that the hero has been cast as a bad apple. The reputation of the hero — and f the science — (the core values) are to be protected at all costs. Among geneticists, for example, Gregor Mendel is a classic hero. Yet, as noted above, while his work was critical in helping to launch modern genetics, its claims were also incomplete, and sometimes misleading, or wrong if improperly generalized. But try telling this to some geneticists! They bristle at these historical observations. They do not consider the claims or the historical evidence. Rather, they target the "offending" author with harsh *ad hominem* comments. In their hyperbolic response to profiles of error, these geneticists illustrate the widespread emotional dimension of error and idealized science. Venturing into error in science may thus require, for some, an investment of courage. The cases noted above might illustrate how we can approach error without sacrificing or diminishing our scientific heroes. [Sapp, 1990; Fritz, 1999; Allchin 1999; Westerlund & Fairbanks, 2004; Allchin, 2004]

The puzzling — indeed troublesome — aspect of most errors is that they were not perceived as errors at the time. Even when they seem glaringly obvious to us now, in retrospect. To truly understand the error, we must see things the way that the scientists originally saw them historically. Historians call this *historicism*. Respecting the historical perspective. Sometimes they seek to recreate it through "historical imagination." We want to understand how a claim that is now unjustified once *seemed* justified. Of course, that flirts with justifying the error. Delving into cases of error can thus, emotionally, seem treacherous. We "risk" sympathizing with the error, drawing us precipitously close to "accepting" or endorsing the error. We seem close to erring ourselves. But this ability to entertain alternative perspectives is the challenge of any analytical discourse. "Listening" effectively to a position we do not accept. Skills of historical perspective thus seem essential for exploring and understanding error. By exercising them, however, we might transform our images of scientists-as-heroes and science-as-a-romanticizedideal. [Collingwood, *Idea of History*; Kuhn, 1970]

As I see it, we desperately need to replace our inauthentic scientific role models with *real* models. We need to bring humanity back to science. Scientists are human. They are not omniscient. Nor cognitively perfect. They use short-cuts in their reasoning. Their preconceptions filter their perceptions. Their theoretical commitments bias their interpretations of evidence. Emotions shape their assessments of concepts. Scientists are real persons, not decontextualized or idealized minds. The institution of science, too, is human. It is not perfect. It inevitably generalizes from a limited set of examples. It adopts methods of investigation that are pragmatic, if not ideal. It relies on assumptions. In all these ways, science and scientists are subject to error. To err is human, we often hear. And science is a thoroughly human enterprise. Should we not also humbly acknowledge, to echo the central theme yet again, that "to err is science"?

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From Acknowledgment to Analysis

Acknowledging error, while essential, is only an opening. A portal to further exploration. What does all that error *mean*? In the late eighteenth century, Benjamin Franklin reflected:

> Perhaps the history of the errors of mankind, all things considered, is more valuable and interesting than that of their discoveries.

Here, Franklin posed a provocative challenge, of sorts, to philosophers of science and others concerned with how knowledge develops. Do we learn more about the process of science from its achievements or its mistakes? Which are more informative in interpreting how science works: discoveries or errors? At the very least, Franklin, the source of so much wit, considered the errors more *interesting*!

The occasion for Franklin's comment was a report to the King of France on whether "animal magnetism" was authentic. Franz Anton Mesmer had claimed that this was a unique force in nature and that it could be harnessed for healing all sorts of bodily ailments and mental afflictions. Mesmer presented himself as a master practitioner and was making a good business of it. For several years the social elite of Paris gathered around fancy electrical devices in his salons to experience, and hopefully benefit from, the much vaunted Mesmerism. People felt the prickly induced shocks. They experienced Mesmer's touch and intense gaze. Some screamed. Some laughed, or vomited, or convulsed. Some fainted. Marie-Antoinette, the queen herself, endorsed Mesmer's wonders. The Marquis de Lafayette wrote to George Washington to advise him that Mesmer's secret was "a great philosophical discovery." Something remarkable was happening. But what was it? Despite its enormous popularity and pretensions, Mesmerism was never accepted among those that studied electricity, magnetism, and other natural phenomena. The French Academy of Science denounced it (to Mesmer's dismay) as "destitute of foundation and unworthy of the smallest attention." So the King had commissioned Franklin and several other renowned experts to investigate and report on the phenomenon.

The commission devised some simple yet clever experiments. They blindfolded some subjects and placed them near magnetized objects, but with no reported effect. Others were not treated but asked whether they had not felt something, and they testified that indeed they had. Others (again, blindfolded) could not report which area of the body had been targeted. Some, when told that they were in the presence of the mesmerist, succumbed to the familiar convulsions. Another individual, who had been susceptible to earlier treatments, was presented with a series of china cups, having been told that one was magnetized. By the fourth unmagnetized cup, she exhibited the usual symptoms. But when given a drink to recover — now, with the cup that had been magnetized — she was, ironically, calmed. The phenomenon was clearly psychological or, as the commission concluded, due to “sensitive excitement, imagination and imitation.” The effects were quite real, of course, but not based on any physical force or the so called animal magnetism. That was the error that Franklin and the commission reported. But they had also made an important complementary discovery, in a sense. They had articulated what was happening. The behavior associated with animal magnetism could be explained, not as an “error.” The commission had vivid evidence for a significant cognitive phenomenon: the power of suggestion and the placebo effect. The “error” had a definite, identifiable cause.

Yet Franklin did not dwell on what the commission had documented about the nature of mental processes. For Franklin, Mesmer’s subjects’ beliefs were “just” wrong. His commentary on the nature of error continued:

> Truth is uniform and narrow; it constantly exists, and does not seem to require so much an active energy, as a passive aptitude of soul in order to encounter it. But error is endlessly diversified; it has no reality, but is the pure and simple creation of the mind that invents it. In this field the soul has room enough to expand herself, to display all her boundless faculties, and all her beautiful and interesting extravagancies and absurdities.–

That is, for Franklin, one need not explain the emergence of knowledge. That happened naturally. Or automatically, without effort. He thereby epitomized the optimistic spirit of the Enlightenment. Today, however, our perspective might be precisely opposite. Error seems easy to come by. The untutored mind will fall easily into error. As did those who believed Mesmer’s claims. The sources of error are many. One needs education and keen awareness to avoid all the potential pitfalls. Scientific evidence, on the other hand, must be assembled mindfully and carefully. Appropriate methods and controls are essential. Scientific claims demand rigorous scrutiny and review. Indeed, they require the very sort of diligent work and critical acumen that Franklin and his commission exhibited. Detailed knowledge of error in its various forms and of their effects thus seems very important. The modern view seems to echo and even add significance to Franklin’s view on the value of the history of the errors. But unlike Franklin, we might want to treat those errors as more than mere curiosities or arbitrary personal deviations. Intimate knowledge of error seems essential for understanding what makes science effective.

[Franklin, 1784; Gould, 1991; Turner, 2006]

The challenge of error in science, then, is not only to acknowledge that errors occur, but also to analyze them fully. This is the challenge in the succeeding chapters. For example, what precisely do we mean by error? Do we include minor slips, soon detected and remedied? Do we count deficits that, historically, a scientist could never have known about? Is a missed opportunity or oversight equivalent to an outright error? Does the scientist’s level of knowledge or intent matter? Do we include fraud, misconduct, or other ethical offenses? Do we include the deliberate adoption of false models as a tool in research? Do we include hypotheses that are entertained and pursued, but never seriously endorsed? Do we include “failed” experiments that yield only ambiguous results, where discussion of “right” *or* “wrong” hardly makes sense? What is an error? By whose account? In short, we need to define, or clearly conceptualize, error as a topic of discussion (see Chapter 2).

Second, what are the various sources of error? How do scientists make (well meaning) mistakes? We can certainly turn to history for informative cases. What can we learn from that history, appropriately abstracted and generalized? Ultimately, we want to compile an inventory of error types. And then a taxonomy, or a way to organize them. By documenting and analyzing errors from the past, one builds an important prospective resource for recognizing or diagnosing possible errors in the future (Chapter 3).

Third, how are the errors ultimately remedied? One often hears that science is “self-correcting.” But how? By what mechanism? How are the errors detected, or found? How are they then isolated to particular error types, and then fixed? Reproducibility and replication are often mentioned as key to reliability in science. Are these principles key to reducing or eliminating errors? Organized skepticism is another feature commonly regarded as a hallmark of science. What does doubt or a critical attitude contribute to the process? How effective is peer review of scientific publications at catching errors? Should it be reformed? Again, we might look profitably to history for guidance. What processes helped resolve error? In what contextss? Can we equally catalog strategies for remedying error? (Chapter 4)

Next, we might consider the ultimate consequences of encountering and remedying error. Do errors impede the progress of science, as many contend? If so, in what ways? Are errors just a waste of time and resources? Do we ever *learn* anything of lasting value? If so, what? Is it even logically possible to consider a positive role for “negative” knowledge? (Chapter 5)

From these analyses of error, more profound questions about the nature of error, knowledge, and the process of scientific discovery emerge. For example, if errors are common and can occur almost anywhere, how is it possible for scientific knowledge ever to develop or grow? How could trustworthy knowledge be built using false theories or assumptions? Or be reliable if based on unreliable foundations? Again, history offers many provocative examples (Chapter 6).

We seem to prize error-free knowledge. So we might strive to purge error from the process of science itself. Having identified the many sources, or causes, of error, what are the forseeable implications for trying to eliminate errors from science entirely? What is the relationship between error and discovery? The cases of Nobel Prize winners and other famous scientists might be especially helpful here. How can insights and blind spots ever be coupled? And here, too, we might find some very surprising and counterintuitive conclusions indeed (Chapter 7).

Also, having surveyed the spectrum of errors in science, can we discern anything about which errors are most common? Or most significant? Which mistakes are the hardest to perceive or to root out? Some scientists, borrowing from the ideas of philosopher Karl Popper, profile the virtues of falsification as the core of good science. Should scientists strive only to expose errors in their concepts or theories, rather than confirm them? What is the role of actively searching for errors? Here, we might hope to find some recommendations for science administration and education. How do we manage science most effectively to accommodate its errors? Also, how might we both train better scientists and inform consumers of science in our society? (Chapter 8)

Finally, where does all this exploration of and reflection on error lead? How should it influence science policy? How should it shape science education, both for future scientists and non-scientists? Franklin suggested that probing the history of errors might be fruitful. But at the end of the day, is there anything worthwhile to say? Can the analysis of errors lead to better science, better science policy, or better science literacy? (Chapter 9)