PREFACE TO THE PAPERBACK

COVID-19. Rarely does the world offer proof of an academic argument, and even more rarely in a single word or term. But there it is. COVID-19 has shown us in the starkest terms—life and death—what happens when we don't trust science and defy the advice of experts.

As of this writing, the United States leads the world in both total cases and total deaths from COVID-19, the disease caused by the novel coronavirus that appeared in 2019. One might think that death rates would be highest in China, where the virus first emerged and doctors were presumably caught unprepared, but that is not the case. According to *The Lancet*—the world's premier medical journal—as of early October 2020, China had confirmed 90,604 cases of COVID-19 and 4,739 deaths, while the United States had registered 7,382,194 cases and 209,382 deaths.[1] And China has a population more than four times that of the United States. If the United States had a pandemic pattern similar to China, we would have seen only 22,500 cases and 1128 deaths.

While COVID-19 has killed people across the globe, death rates have been far higher in the United States than in other wealthy countries, such as Germany, Iceland, South Korea, New Zealand, and Taiwan, and even than in some much poorer countries, such as Vietnam.[2] The Johns Hopkins University School of Medicine puts the US death rate per 100,000 people at 65.5.[3] In Germany it is 11.6. In Iceland, 2.83. In South Korea, 0.89. In New Zealand, 0.51. In China, 0.34. And in Taiwan and Vietnam? 0.03 and 0.04. If the American death rate had been similar to New Zealand's, instead of seeing more than 200,000 deaths in the first ten months of the pandemic, we would have seen fewer than 2,000. If we were like Vietnam, we would have seen a little over 100.[4]

Death rate is an imperfect guide to a pandemic, because it is affected by many factors, including population structure, access to health care, and the underlying health of the population. Death rates are also affected by reporting and testing. A country like China, with low transparency, may not be reporting everything accurately. A metropolis like New York City, caught by surprise with inadequate testing capacity in the early stages of the pandemic, probably underestimated the number of cases and therefore overestimated the death rate. (This could help to explain why the death rate in New York appeared to be much higher than elsewhere in the United States.) And since COVID-19 is very deadly to the elderly, a country with an aged population can be expected to see a higher death rate than one with a younger population, but by that measure, Germany should have done more poorly than the United States. In fact, it has done far better.[5] Perhaps the most compelling statistic is this: the United States has 4% of the global population, and it has had 20% of global deaths.

By any measure, the US response has been a disaster. But rather than ask why it has been so bad, it may be more instructive to ask: What is common to the countries that have done well? The answer is straightforward: The countries that have seen low death rates effectively controlled the spread of the virus, and they did so by trusting science.

In December 2019, when COVID-19 first emerged, public health experts raised the alarm that we were seeing a novel virus—of "unknown etiology"—that could pose a pandemic threat.[6] By the end of January 2020, the World Health Organization declared the coronavirus outbreak a PHEIC—a public health emergency of international concern.[7] This was only the sixth time the WHO had invoked this measure since the regulations under which it operates were established in 2005.

Public health experts immediately made recommendations about how to minimize the disease spread. These included frequent, thorough hand washing with soap and hot water; avoiding large public gatherings; and staying home at the first sign of illness. Admittedly, these recommendations were not 100% consistent—this was, after all, a *novel* disease, so there was much about it that was unknown—and the WHO offered contradictory advice on masks. But this was not because the organization did not have reason to think that masks might help. It was because it was afraid that people would hoard them, exacerbating an already serious shortage of masks for health care and other essential workers.[8] (The WHO's confusing mask guidance—which it later altered—was not a failure of scientific knowledge but a failure of scientific communication, grounded in expert distrust of lay people. But this distrust—a better word might be "caution"—was perhaps warranted, given how many people did, in fact, hoard toilet paper, disinfectants, and other essential supplies.) Other scientists felt that in the absence of convincing scientific evidence that masks would work to stop *this particular virus*, they could not recommend the use of them.[9] Overall, however, most of the public health advisories were consistent, based on existing scientific knowledge of how respiratory viruses spread.[10]

In the United States, a great deal of attention has focused on individual action—hand washing, staying home, wearing masks—but public health officials also recommended measures that prior epidemics had proved effective: testing, isolation of sick individuals, contact tracing, and where needed, quarantine. These measures had helped in past pandemics and therefore had at least some likelihood of working in this one. (The word "quarantine," after all, is a very old one, dating from fourteenth-century Italy, where incoming ships were required to stay in port for forty days: *quaranta giorni*.)

More important, a broad program of testing, isolation, and contact tracing was scientific common sense, because viruses do not spread by magic; they spread from sick people to well ones. If you can quickly identify the sick and separate them from the healthy, then you have a good chance of reducing the spread. The countries that can today boast of very low caseloads and death rates all took this scientific experience and expertise to heart.

Vietnam is a case in point.[11] Early in the pandemic the government implemented strict measures to test any symptomatic person, and, where results were positive, to trace, test, and isolate their contacts. The government also promoted the use of mobile apps by which people could record their symptoms and get tested promptly as needed. Passengers arriving from overseas were quarantined, and in a few cases—such as a man returning from a religious festival in Malaysia—the government ordered targeted lockdowns, in this case of a mosque he had visited in Ho Chi Minh City and of his entire home province.[12] The government also restricted travel and public gatherings, and ordered the shutdown of many non-essential businesses. By identifying and isolating the contacts of infected people, Vietnam was able in nearly all cases to stop the spread.

Vietnam is admittedly an authoritarian state, where mandatory measures are more easily implemented than in a democracy, and observers might be tempted to question data offered by its government. In fact, the Vietnamese success has not only been affirmed by independent medical sources; it has been touted by media outlets on both the right and the left of the political spectrum.[13] Ironically, some observers in fact attribute the country's success in part to prompt, effective, and transparent information and communication campaigns to keep the public updated.[14]

While future work will be needed to analyze the Vietnamese experience, it is already clear that it has much in common with the experiences of China, Germany, Iceland, New Zealand, South Korea, and Taiwan. Political leaders in these countries took the threat seriously, attended to the advice offered by scientific experts, and established public health approaches based on that advice. They trusted science, and science repaid that trust by saving lives.

And of course it is not just COVID-19 that illustrates the importance of having and using scientific information. While the COVID-19 pandemic was unfolding, climate change continued to progress as well. The 2020 Atlantic hurricane season has been among the worst on record, with so many named hurricanes that we went through not only the entire Latin alphabet from A to Z, but the entire Greek alphabet as well.[15] Hurricanes are not just an inconvenience. They are not something to which people simply "adapt." They kill people, destroy homes, and, in the worst cases, leave permanent social, psychic, economic, and environmental damage. Meanwhile, while citizens of the Gulf

Coast were suffering a surfeit of rain, deadly wildfires were ravaging California and the Pacific Northwest.

Scientists have known for decades that climate change had the potential to make hurricanes and wildfires worse, and we have known for some years now that climate change *is* making these events worse. It has been many years since climate change was just a "theory." And yet, our political leaders continue to stall, prevaricate, and even deny outright the scientific realities. They listen not to the experts who have studied the problem and subjected their findings to the open criticism of fellow scientists, but to "anti-experts" who tell them not what is true, but what they want to hear.[16]

And so people get hurt. Their homes are destroyed. They die.

Not all of these deaths could be prevented by trusting science. We have, after all, always had hurricanes and pandemics and likely always will. Public policy will never be only a matter of listening to science, nor should it be. Many factors weigh into the decisions we make about our personal lives and our public policies, and rightly so. All choices are trade-offs; all public policies involve costs and benefits. But we cannot judge the trade-offs—we cannot accurately calculate the costs and the benefits—if we ignore (or worse, are deliberately denied) the relevant scientific information.

Put positively, a great deal of pain and suffering can be avoided when we understand scientific knowledge and put it to appropriate use. Scientists are people who understand things in ways that we can use to our advantage. They know things that we need to know. And, as COVID-19 has tragically proved, they know things that we ignore at our peril.

Notes

1. Talha Burki, "China's Successful Control of COVID-19," *The Lancet: Infectious Diseases* 20, no. 11 (October 8, 2020), https://doi.org/10.1016/S1473-3099(20)30800-8.

2. Pablo Gutiérrez and Seán Clarke, "Coronavirus World Map: Which Countries Have the Most COVID Cases and Deaths?," *The Guardian*, October 16, 2020, https://www.theguardian.com/world/2020/oct/16/coronavirus-world-map-which-countries-have-the-most-covid-cases-and-deaths; "COVID in the U.S.: Latest Map and Case Count," *The New York Times*, July 20, 2020, https://www.nytimes.com/interactive/2020/us/coronavirus-us-cases.html; Henrik Pettersson et al., "Tracking Coronavirus' Global Spread," CNN, accessed October 19, 2020, https://www.cnn.com/interactive/2020/health/coronavirus-maps-and-cases.

3. "Mortality Analyses," Johns Hopkins Coronavirus Resource Center, accessed October 19, 2020, https://coronavirus.jhu.edu/data/mortality.

4. Vietnam has a population over 97 million, but it has seen a tiny number of COVID-19 deaths: 38, as of October 19, 2020. Since the US population is 3.33 times as large, 3.33 × 38 = 127. "Vietnam COVID 19 Deaths—Google Search," accessed October 19, 2020, https://www.google.com/search?q=vietnam+COVID+19+deaths&oq=vietnam+COVID+19+&aqs=chromVietnam%20has%20a%20population%20of%2095%20million,%20but%20it%20has%20seen%20a%20tiny%20number%20of%20covid. 0C19%20deaths:%2035e.1.0i457j0i20i263j69i57j0i20i263j0j69i60l3.5002j0j9&sourceid=chrome&ie=UTF-8.

5. "The Aging Readiness and Competitiveness Report: Germany," AARP, 2017, http://www.silvereco.org/en/wp-content/uploads/2017/12/ARC-Report-Germany.pdf. Cf. Deidre McPhillips, "Aging in America, in 5 Charts," *US News & World Report*, September 30, 2019, https://www.usnews.com/news/best-states/articles/2019-09-30/aging-in-america-in-5-charts.

6. "Pneumonia of Unknown Cause—China," WHO (World Health Organization), January 5, 2020, http://www.who.int/csr/don/05-january-2020-pneumonia-of-unkown-cause-china/en.

7. "Archived: WHO Timeline—COVID-19," WHO, accessed October 19, 2020, https://www.who.int/news/item/27-04-2020-who-timeline--covid-19.

8. Julia Naftulin, "WHO Says There Is No Need for Healthy People to Wear Face Masks, Days after the CDC Told All Americans to Cover Their Faces," Business Insider, accessed October 18, 2020, https://www.businessinsider.com/who-no-need-for-healthy-people-to-wear-face-masks-2020–4.

9. Ibid.

10. Since then, it has become clear that masks do work and perhaps even more effectively than some of their earlier advocates dared hope. See, for example, Stephanie Innes, "COVID-19 Cases in Arizona Dropped 75% after Mask Mandates Began,

Report Says," *The Arizona Republic*, accessed October 19, 2020, https://www.azcentral.com/story/news/local/arizona-health/2020/10/09/covid-19-cases-az-spiked-151-after-statewide-stay-home-order-and-dropped-75-following-local-mask-man/5911813002. As I have argued elsewhere, it was scientifically logical that masks should help at least somewhat; see Naomi Oreskes, "Scientists Failed to Use Common Sense Early in the Pandemic," *Scientific American*, November 2020, https://www.scientificamerican.com/article/scientists-failed-to-use-common-sense-early-in-the-pandemic.

11. "Vietnam COVID 19 Deaths—Google Search."

12. Todd Pollack et al., "Emerging COVID-19 Success Story: Vietnam's Commitment to Containment," *Our World in Data*, June 30, 2020, https://ourworldindata.org/covid-exemplar-vietnam; Thi Phuong Thao Tran et al., "Rapid Response to the COVID-19 Pandemic: Vietnam Government's Experience and Preliminary Success," *Journal of Global Health* 10, no. 2 (July 30, 2020), https://doi.org/10.7189/jogh.10.020502020502.

13. George Black, "Vietnam May Have the Most Effective Response to COVID-19," *The Nation*, April 24, 2020, https://www.thenation.com/article/world/coronavirus-vietnam-quarantine-mobilization; "How Did Vietnam Become Biggest Nation without Coronavirus Deaths?," Voice of America, June 21, 2020, https://www.voanews.com/covid-19-pandemic/how-did-vietnam-become-biggest-nation-without-coronavirus-deaths.

14. Tran et al., "Rapid Response to the COVID-19 Pandemic."

15. National Oceanic and Atmospheric Administration, "2020 Atlantic Hurricane Season," https://www.nhc.noaa.gov/data/tcr/index.php?season=2020&basin=atl.

16. On anti-experts, see Adapt by Sprout Social, "Combating Anti-Expert Sentiment on Social," May 8, 2018, https://sproutsocial.com/adapt/anti-expert-sentiment.

A particularly damaging pandemic anti-expert is Scott Atlas, a radiologist and senior fellow at the politically oriented, conservative Hoover Institution, who, with no recognizable expertise in immunology, virology, epidemiology, or public health, has been advising Donald Trump on COVID-19 in a manner that conflicts with the views of most public health experts, including Drs. Anthony Fauci and Deborah Birx. See Yasmeen Abutaleb and Josh Dawsey, "New Trump Pandemic Adviser Pushes Controversial 'Herd Immunity' Strategy, Worrying Public Health Officials," *Washington Post*, August 31, 2020, https://www.washingtonpost.com/politics/trump-coronavirus-scott-atlas-herd-immunity/2020/08/30/925e68fe-e93b-11ea-970a-64c73a1c2392_story.html.

A related example of anti-experts muddying the intellectual waters around COVID-19 involves the "Great Barrington Declaration," organized by the American Institute for Economic Research; see "AIER Hosts Top Epidemiologists, Authors of the Great Barrington Declaration," October 5, 2020, https://www.aier.org/article/aier-hosts-top-epidemiologists-authors-of-the-great-barrington-declaration. The American Institute for Economic Research is, as its name suggests, an economic institute with no recognizable claim to biological or medical expertise. Like many such institutes, it promotes a particular political agenda, in this case "free trade, individual freedom, and responsible governance." Those may or may not be good things, but they are not matters of science. AIER also promotes anti-scientific discussion of climate change, much of which promotes the familiar canard that climate change will be minor and manageable. One recent piece, for example, which discounted scientific interpretations of the dangers of climate-induced sea level rise was written not by a scientist but by a "writer, researcher, and editor on all things money, finance and financial history" (Joakim Book, "The Tide-Theory of Climate Change," October 28, 2020, https://www.aier.org/article/the-tide-theory-of-climate-change).

While pandemics do, of course, involve economic matters, the Great Barrington Declaration focused on the *public health response*, urging a herd immunity approach, which most public health experts consider to be a euphemism for allowing people to sicken and die. Indeed, expert estimates suggest that if the United States had undertaken that approach, more than 200 million people would likely have become ill, with the potential for more than 2 million deaths. This, of course, is comparable to the argument that we can just "adapt" to climate change. Of course we can, but at what price?

Moreover, the concept of herd immunity is normally invoked in the context of vaccination: What percentage of a population needs to be vaccinated in order to protect the whole population? In the absence of a vaccine, herd immunity typically means that at least 70% of a population will need to get sick before the population as a whole is protected. See Christie Aschwanden, "The False Promise of Herd Immunity for COVID-19," *Nature* 587, nos. 26–28 (October 21, 2020), https://www.nature.com/articles/d41586-020-02948-4; and Kristina Fiore, "The Cost of Herd Immunity in the U.S.," Medpage Today September 1, 2020, https://www.medpagetoday.com/infectiousdisease/covid19/88401.

The clearest argument against the herd immunity strategy is provided by a comparison of Sweden and Norway. According to a report in *Nature*, drawing on statistics from Johns Hopkins University, "Sweden has seen more than ten times the number of COVID-19 deaths per 100,000 people seen in neighbouring Norway (58.12 per 100,000, compared with 5.23 per 100,000 in Norway). Sweden's case fatality rate, which is based on the number of known infections, is also at least three times those of Norway and nearby Denmark"; see Aschwanden, "The False Promise of Herd Immunity for COVID-19." And the Swedish economy suffered, anyway, because the global economy is, well, global.

Chapter 1

WHY TRUST SCIENCE?

Perspectives from the History and Philosophy of Science

The Problem[1]

Many people are confused about the risks involved in vaccination, the causes of climate change, what to do to stay healthy, and other matters that fall within the domain of science. Immunologists tell us that vaccines are generally safe for most people, have protected millions of people from deadly and disfiguring diseases, and do not cause autism. Atmospheric physicists tell us that the build-up of greenhouse gases in the atmosphere is warming the planet, driving sea level rise and extreme weather events. Dentists tell us to floss our teeth. But how do they know these things? How do we know they're not wrong? Each of these claims is disputed in the popular press and on the internet, sometimes by people who claim to be scientists. Can we make sense of competing claims?

Consider three recent examples.

One: In a 2016 presidential debate, Donald Trump rejected the position of medical professionals—including that of fellow candidate physician Ben Carson—on the safety of vaccination. Recounting the experience of an employee whose child was vaccinated and later diagnosed as autistic, Mr. Trump stated his view that vaccines should be given at lower doses and be more widely spaced. Few medical professionals share his view.[2] They consider delaying vaccination to increase the risk that infants and children will contract dangerous and otherwise preventable diseases such as measles, mumps, diphtheria, tetanus, and pertussis. Some of the children who contract these diseases will become gravely ill or die. Others will survive but pass on the infections to others. Yet, Mr. Trump is not alone in making this suggestion; prominent celebrities have made similar exhortations. Many parents now reject the advice of their physicians and choose to have their children vaccinated on a delayed schedule—or not at all. As a result, morbidity and mortality from preventable infectious diseases are on the rise.[3]

Two: The vice president of the United States, Mike Pence, is a young Earth creationist, meaning that he believes that God created the Earth and all it contains less than ten thousand years ago. The consensus of scientific opinion is that Earth is 4.5 billion years old, that the genus *Homo* emerged two to three million years ago, and that anatomically modern humans appeared about two hundred thousand years ago. While science cannot answer the question of whether God (or any supernatural being or force) guided the process, most scientists are persuaded that life on Earth evolved largely through the process of natural selection over the course of Earth's history, that humans share a common ancestor with chimpanzees and other primates, and that divine intervention is not required to explain the existence of *Homo sapiens sapiens*.[4]

Do Americans lean toward the scientific view or the Pencian view? The answer depends a bit on how you ask the question, but if you are a religious person in America who attends church regularly, the chances are high that you agree with Mike Pence: 67% of regular churchgoers believe that God created humans in their present form within the last ten thousand years. Some of us may think that these people are all Republicans, but we would be wrong. According to the Gallup polling organization, while 58% of Republicans agreed with the statement that "God created humans in their present form, within the last 10,000 years," so did 39% of independents and 41% of Democrats.[5] Given this popular support for creationism, it is perhaps unsurprising that in 2012, the state of Tennessee enacted what some have called a "twenty-first-century Monkey Law," empowering teachers to teach creationism in science classrooms.[6] Despite repeated rejection of previous laws of this type by US courts, many states continue to attempt to enact comparable laws.[7]

Three: The American Enterprise Institute (AEI) is a long-established and well-funded think tank in Washington, DC, committed to principles of laissez-faire economics, market-based mechanisms to social problems, limited (federal) government, and low rates of taxation. The Institute has long promoted skepticism about the scientific evidence for anthropogenic climate change and disparaged the conclusions of the scientific community, including the Intergovernmental Panel on Climate Change (IPCC).[8] AEI scholars have suggested that climate scientists are suppressing dissent within their community; the Institute at one point offered a cash incentive to anyone willing to search for errors in IPCC reports. Jeffrey Sachs, head of the Earth Institute at Columbia University from 2002–16 and special advisor to UN secretary-general António Guterres on the Millennium Development Goals, has said of one well-known AEI scholar that he "distorts, misrepresents, or simply ignores" relevant scientific conclusions.[9] In 2016, this particular scholar referred to scientists as an "interest group," demanding to know why "scientific analysis conducted or funded by an agency headed by political appointees buffeted by political pressures . . . [should] be viewed ex ante as any more authoritative than that originating from, say, the petroleum industry?"[10]

I am no fan of the American Enterprise Institute. With my colleague Erik M. Conway I have shown how they (along with other think tanks promoting laissez-faire approaches to social and economic issues) have persistently mispresented or mischaracterized scientific findings on climate change, as well as a variety of public health and environmental questions. (They are no fans of mine, either. Their scholars have attacked my work on scientific consensus.)[11] But the question raised is a legitimate one. Should a scientific analysis be viewed as ex ante authoritative? Is it reasonable to take the default position that the scientific community can in general be trusted on scientific matters, but the petroleum industry (to use his example) cannot?

Science in North American universities and research institutes is generally well funded and respected—typically much more so than the arts and humanities—but outside those hallowed halls something very different is transpiring. The idea that science should be our dominant source of authority about empirical matters—about matters of fact—is one that has prevailed in Western countries since the Enlightenment, but it can no longer be sustained without an argument.[12] *Should* we trust science? If so, on what grounds and to what extent? What is the appropriate basis for trust in science, if any?

This is an academic problem but one with serious social consequences. If we cannot answer the question of why we should trust science—or even if we should trust it at all—then we stand little chance of convincing our fellow citizens, much less our political leaders, that they should get their children vaccinated, floss their teeth, and act to prevent climate change.

Scholars' views on the answer to this question have changed dramatically and more than once in the past century. Moreover, some of the answers that scientists offer are manifestly contradicted by historical evidence. It is routine, for example, for scientists to insist that their theories must be correct, because they *work*. How else, they argue, would planes fly or medicines cure disease?[13] But utility is not truth: we can identify many theories in the history of science that worked and later were rejected as wrong. The Ptolemaic system of astronomy, the caloric theory of heat, classical mechanics, and the contraction theory of the Earth explained observed phenomena and made successful predictions, and are now on the scrap heap of history. Many scholars in the history and philosophy of science and science studies have, however, recently converged on a new view that does hold up to scrutiny: of scientific knowledge as fundamentally *consensual*. This consensual view of science can help us address the current crisis of trust.

The Dream of Positive Knowledge

Throughout the eighteenth and the early nineteenth centuries, most scholars located the authority of science in the authority of the "man of science."[14] The results of scientific investigations were trustworthy to the extent that the people who undertook them were. This is one reason why scientific honor societies, such as the Royal Society or the Académie des Sciences, were created: to acknowledge and identify the "worthies" whose opinions on scientific matters should be sought, trusted, and heeded.[15] These societies served to identify the individuals whose work was considered worthy of acceptance. In the United States, this ideal was instantiated in the creation of the US National Academy of Sciences during the Civil War to advise President Lincoln. Identifying these "great men" of science would enable the president to get the reliable advice he needed.

However, in the mid-nineteenth century, a substantive intellectual shift occurred, driven to a significant extent by the work of Auguste Comte (1798–1857), variously credited as the founder of sociology, the founder of philosophy of science in its modern form, and the founder of the philosophical school known as positivism.[16] Comte's work is abundant and complex and has been subject to various considerations and reconsiderations, refutations and restorations, but the most important aspect, for our purposes, is his commitment to the idea of *positive knowledge*. Science, Comte believed, was uniquely able to provide positive—which is to say *reliable*—knowledge. While the term "positive knowledge" is no longer much used apart from academics discussing it, most often as a discredited concept, the idea persists in our linguistic conventions. We still retain the notion of something being "absolutely, positively true." In English we can ask: "Are you positive?" To which you may reply: "Yes, I'm positive."

For Comte, the key element in the concept of positive knowledge is *method*, which he contrasted with *doctrine*—whether religious, superstitious, or metaphysical. The doctrines of religion and metaphysics, he argued, were forms of bias and blinkering that impeded intellectual and social progress, which the method of science, by contrast, could provide. By applying method to the pursuit of knowledge, science had the potential to liberate men and women from the shackles of religion and superstition.

Comte's philosophy (like many in the nineteenth century, including famously Marxism) was teleological: he saw human history as being characterized by three stages: the theological or fictitious, the metaphysical or abstract, and the scientific or positive. These were not necessarily sequential—they might coexist within a society or even with a person—but overall the direction of progress was from theology to science, with metaphysics serving as a necessary transition.[17] In the "positive stage" of human development, theology and metaphysics are replaced by scientific reasoning. And scientific reasoning is rooted in observation.

It has been argued that Comte was seeking to replace conventional religion with a new religion of science, and there is some justice to this claim. Teleology is a common feature of many religions. He accepted that people had a need for moral principles but thought those principles could be found in the humanistic ideals of truth, beauty, goodness, and commitment to others. He also believed that people had a need for ritual and proposed to replace the veneration of Christian saints with a set of positivist heroes. In his own life, he set aside time for meditation and affirmation of his central values.[18] But whether his views were quasi-religious or not, the key point for our discussion is that for Comte—and generations of those who followed him, knowingly or not—science was reliable because of its commitment to method. This leads one to ask: what is that method?

Comte was sensitive to the variety of scientific disciplines that were developing at that time. He did not assert that their practices were uniform, but he did believe that they shared a fundamental characteristic of the "positive" state of human existence. He wrote:

> In the positive state, the human mind, recognizing the impossibility of obtaining absolute truth, gives up the search after the origin and hidden causes of the universe and a knowledge of final causes of phenomena. It endeavours now only to discover, by a well-combined use of reasoning and observation, the actual laws of phenomena—that is to say their invariable relations of succession and likeness. The explanation of facts, thus reduced to its real terms, consists henceforth only in the connection established

> between different particular phenomena and some general facts, the number of which the progress of science tends more and more to diminish.[19]

In stressing the importance of empirical regularities, Comte was making an argument similar to the British empiricists, particularly David Hume.[20] He acknowledged his debt to British empiricism, particularly the work of Francis Bacon, writing, "All competent thinkers agree with Bacon that there can be no real knowledge except that which rests upon observed facts."[21] But he was not the "naïve positivist" that some later commentators made him out to be. He was a sophisticated thinker who recognized that our theories structure our observations as much as our observations structure our theories:

> If we consider the origin of our knowledge, it is no less certain that . . . [as] every positive theory must necessarily be founded upon observations, it is, on the other hand, no less true that, in order to observe, our mind has need of some theory or others. If in contemplating phenomena we did not immediately connect them with some principles, not only would it be impossible for us to combine these isolated observations and, therefore, to derive any profit from them, but we should even be entirely incapable of remembering the facts, which would for the most part remain unnoted by us.[22]

We can understand, therefore, why primitive humans had need of religion, superstition, and metaphysics: these early concepts were a step toward apprehending the world around us. We need not disdain or disparage these early stages in human development, we simply need to recognize and accept that to move forward—to identify the true laws that govern nature—our thinking needs to be grounded upon observation. In his words: "we must proceed sometimes from facts to principles [and] at other times from principles to facts," but ultimately we will establish "as a logical thesis that all our knowledge must be founded upon observation."[23]

Comte was also a fallibilist: he recognized that our views would grow and change and that his own vision would in time be modified. (Indeed, if his basic concept was correct, then the progress of knowledge would necessarily modify our views, and we might note that the persistence of religion has falsified a key element of his teleology.) But, to his credit, Comte was consistent insofar as he insisted that future change in our thinking would be the outcome of our observations.

Comte was also reflexive, recognizing that the practices of observation must themselves be subject to observation. An improved knowledge of positive method must come, therefore, not by *theorizing* it but by studying it; we must observe science in order to understand it. Comte thus anticipated Bruno Latour and his anthropological studies of laboratory science by more than a century when he held: "When we want not only to know what the positive method consists in, but also to have such a clear and deep knowledge of it to be able to use it effectively, we must consider it *in action*."[24]

Comte's key move was to insist that science is reliable not by virtue of the character of its practitioner, but by virtue of the nature of its practices.[25] We need to attend to these practices by studying them empirically. The key questions, then, for those who took up the Comtean program were: What exactly are those practices? Is there a scientific method?

Varieties of Empiricism

For twentieth-century empiricists, which we have come to call logical positivists or logical empiricists, the answer to the question of the method of science was the principle of verification.[26] The concept was developed most extensively by a group of German-speaking philosophers and scientists, known as the "Vienna Circle." The most famous English language articulation of the verificationist program came from the Oxford philosopher A. J. Ayer (1910–89). In his 1936 book, *Language, Truth and Logic,* which is still in print, Ayer summarized the principle by framing it in terms of the problem of meaning: A statement can be considered meaningful if and only if it can be verified by reference to observation. Put another way, "some possible observation must be relevant to the determination of [the statement's] truth or falsehood."[27] Science is the practice of formulating meaningful statements, and using observations to judge whether a meaningful statement is correct.

Verification gives us the basis for evaluating what is or is not justified true belief. If a claim can be verified through observation, and if it has in fact been so verified, then we are justified in believing it, which is to say, justified in accepting it as true. If a claim cannot be so verified, then it is meaningless and need not detain us further. Thus, in one fell swoop did Ayer dispense with religion, superstition, and various forms of political ideology and theory that were unverifiable. The principle of verification provided a means of demarcating scientific knowledge from non-scientific knowledge: scientific claims were verifiable thorough observation; claims that were not verifiable were not scientific.

Like Comte, Ayer was ambitious but not naïve. He understood that in practice any observation necessarily entails background assumptions. But, like his Vienna Circle colleagues Rudolf Carnap and Otto Neurath, he insisted that verification through observation was the key component to meaning, hence the moniker *verificationism*. In order to test a statement, one had to be able to deduce an observable consequence from it and express that deduction as a *statement*, and that deduction had to be specific to the statement under investigation for the verification to be dispositive. Ayer wrote: "A statement is verifiable, and consequently meaningful, if some observation statement can be deduced from it in conjunction with certain other premises, without being deducible from those other premises alone."[28]

Ayer and his colleagues recognized that any program that foregrounded observation necessarily faced the problem of induction: namely, how many observations are needed to conclude that a statement is true? Following Hume, his answer was that inductive knowledge was necessarily probabilistic, and he suggested that one needed to allow for weak and strong forms of verification, based on the quantity and quality of available relevant observations. These sorts of concerns underpinned research on the character of scientific observation, which quickly led to various complications regarding the formulation of observation statements, the meaning of terms, and the identification of what, precisely, was being verified by any particular observation or set of observations.

These issues detained many logical empiricists for the rest of their lives. Carl Hempel, in particular, paid attention to the role of hypothesis in generating testable observation statements; Carnap focused on the observation statements and the language in which they were rendered, and famously argued with Willard Van Orman Quine over whether observations could really confirm or refute beliefs. (Quine concluded they could not, a point we will take up.) This work did not resolve the issues it entailed.[29] For our purposes, the important point is that the logical empiricists sustained the central Comtean idea that the core of scientific method is verification through experience, observation, and experiment.

Challenges to Empiricism

While logical empiricism is often attacked as the ruling dogma of twentieth-century philosophy of science, it never really ruled. Even in its heyday, several important challenges were already underway.[30]

Karl Popper and Critical Rationalism

The most well-known critic of logical empiricism is Karl Popper (1902–94). Popper rejected several key tenets of logical positivism. First, he denied that induction was the method of science. Second, he argued that what distinguishes science from other forms of human activity is not its activities, but its attitude. Great scientists are notable for the critical attitude they take toward their work, which is an attitude of skepticism and disbelief. Third, he insisted that the goal of science is not to prove theories—since that cannot be done—but to disprove them. He introduced his now-famous notion of *falsifiability*, concluding that what distinguishes a scientific claim from a non-scientific one is not that there is some observation by which it can be verified, but that there is some observation by which it can be refuted.

These three ideas are linked in the following way. There may be habits or practices or even principles of induction, but there is no rational rule of induction. Inductive inferences cannot be justified based on any purely logical rule, and therefore cannot be established with logical necessity. This is what nowadays is referred to as the black swan problem. I may have observed one hundred swans, or one thousand, or ten thousand, and found that they have all been white, as have all the swans observed by my scientific colleagues. Therefore, my colleagues and I conclude (seemingly with robust warrant) that all swans are white. Yet, one day I travel to Perth, Australia, where I see a black swan.

Thus, we see that observations cannot prove that a theory is true, no matter how extensive or comprehensive. Refutation may be lurking around the corner (or the antipodes). If science is to be a rational enterprise, induction therefore cannot be its method.

Because observation alone cannot give us *logical* grounds to support inductive generalizations, verification cannot be the basis of scientific method. However, the observation of the black swan did prove that my inductive generalization was false, so there is a logic of *refutation*. There is a logical asymmetry between verification and falsification: verifications are always necessarily provisional, whereas falsifications (Popper held) can be dispositive. Given this, as a scientist I should not be seeking observations that confirm my theory, but observations that might refute it. The method of science, Popper therefore concludes, is neither generalization from observation nor verification by observation, but *falsification*. Put another way, the key activity of science is not the gathering of observations, but the formulation of conjectures and the pursuit of specific observations that may refute them. Thus the title of his famous collection of essays and lectures: *Conjectures and Refutations*.

Even more urgently than his logical positivist colleagues, Popper held science to be the model of rationality, insisting that critical rationality is not only the appropriate basis for intellectual inquiry, but also for politics and civil society, as it empowers resistance to authoritarianism of both the right and the left. Therefore he labeled his approach *critical rationalism*. His project was both epistemological and political: he sought an epistemology that would enable not just scientific rationality but also political rationality in democratic forms of governance. Among other things, Popper sought to refute Marxism by showing that "scientific socialism" was an oxymoron, because problems in Marxist theory were never taken as refutations but only as elements to be explained or accounted for in some way.[31]

Popper's critical rationality ironically opened the door for a form of radical skepticism that he abhorred. Popper pushed fallibility further than his predecessors, insofar as he insisted that refutation is not merely an inevitable feature of science, but the goal of it; it is through refutation that science advances. But if our scientific views are not only soon to be refuted, but *should* be refuted, then why should we believe any of it?[32] Popper's answer was to develop the notion of corroboration: that we can have good reason to believe theories that have passed severe tests, such as the deflection of starlight as a test of the general theory of relativity. Successful empirical tests corroborate theories, even if they do not prove them. In making this move, Popper helped to explain why theory testing plays such a major role in scientific practice, but he also radically weakened the otherwise strict tenor of his work: we are now left with having to make subjective judgments as to what constitutes a "severe" test and how many such tests we need.

Ludwik Fleck and Thought Collectives

The various forms of positivism that developed from the mid-nineteenth to the mid-twentieth century were all concerned with method, paying less attention to the people who were pursuing that method or the institutional structures within which they operated. Popper paid some heed to the character of the individual scientist, insofar as he stressed the importance of a critical investigative attitude. But Popper's epistemology (like his political theory) was individualistic; he vested the advance of science in the actions of the bold individual who doubted an existing claim and found a means to refute it. Popper paid less attention to the institutions of science, and was actively hostile to suggestions of collectivism, redolent as they were of the Marxist philosophy and Communist politics that he loathed.[33]

The recognition of science as a collective activity thus laid the grounds for a radical challenge to received views of science that would flourish in the second half of the twentieth century. Whether one had read Comte or Ayer or Popper, one could have come away with the impression that scientists, like Descartes in his room staring at melting wax, lived, worked, and thought alone. Yet anyone who studied science in action—as Comte instructed us to do—or who participated in scientific research knew that wasn't so. Yet somehow this had escaped sustained scholarly attention.

Ludwik Fleck (1896–1961) changed that. A microbiologist who made the social interactions of scientific life a centerpiece of analysis, in hindsight he is credited with developing the first modern sociological account of scientific method. In his 1935 work, *The Genesis and Development of a Scientific Fact: An Introduction to the Theory of Thought Style and Thought Collective*, Fleck shifted attention from the individual scientist to the activities of communities of scientists, and proposed that scientific facts are the collective accomplishment of communities. In doing so, he pioneered the analysis of the social interactions that yield scientific facts.

Fleck was aware of the logical positivists' work; he sent his work to the Viennese positivist Moritz Schlick seeking help to get it published.[34] He was also in contact with historians and philosophers of medicine and mathematics in Poland at that time. But scholars have mostly concluded that his work was primarily influenced by his experience as a researcher and his attention to developments in science, particularly the rise of quantum mechanics in physics, which (he believed) had led to the emergence of new styles of thinking.

Fleck's key point was that scientists worked in communities in which styles of thought became shared resources for future work, including the interpretation of observations. He labeled these communities "thought collectives." Groups of scientists within any particular discipline—biology, physics, geology—constituted thought collectives whose common ways of thinking made it possible for them to work together, share information, and interpret that information in meaningful ways. Without a thought collective, science could not exist. He wrote:

> A truly isolated investigator is impossible . . . Thinking is a collective activity. . . . Its product is a certain picture, which is visible only to anybody who takes part in this social activity, or a thought which is also clear to the members of the collective only. What we do think and how we do see depends on the thought-collective to which we belong.[35]

The term "thought collective" may invoke the specter of thought police, and Fleck recognized that collectives could be conservative or even reactionary—as he believed religious thought collectives were. But a thought collective could also be democratic and progressive, and this was the key to understanding science. Science (unlike most European religion) has a democratic character: all researchers can participate in an equitable way, and through their interactions with each other, refine and change the views of the whole.

Fleck had a radical view of how far such change could go, stressing that over time changes could be so great that the meanings of terms changed, that problems that were previously seen as central could now be dismissed as irrelevant or even illusory, and new issues would emerge that previously went unrecognized. While the increments of change were small—the pathways of change more evolutionary than revolutionary—eventually the thought style may have changed so much that the old view is essentially unrecognizable, even indecipherable.

> Thoughts pass from one individual to another, each time a little transformed, for each individual can attach to them somewhat different associations. Strictly speaking, the receiver never understands the thought exactly in the way that the transmitter intended it to be understood. After a series of such encounters, practically nothing is left of the original content.[36]

Scientific ideas, like evolution itself, may change dramatically over time, but they do so by the accumulation of small transformations and differing interpretations.

"Whose thought is it that continues to circulate?" Fleck asks. His answer: "It is one that obviously belongs not to any single individual but to the collective."[37] As Helen Longino would later put it in a slightly different context, "Of course, Galileo and Newton and Darwin and Einstein were individuals of extraordinary intellect, but what made their brilliant ideas *knowledge* were the processes of critical reception." Fleck would say: of reception and transformation.[38] Newtonian mechanics is not equivalent to the contents of the *Principia,* nor is evolutionary biology coincident with the contents of the *Origin of Species*. The ultimate outcome is the result of Newton and Darwin's work and the diverse ways in which over time it has been interpreted, adjusted, and altered.

Scientific progress in this view is inextricably connected with the institutions of science such as conferences and workshops, books and peer-reviewed journals, and scientific societies through which scientists share data, assess evidence, grapple with criticisms, and adjust their views. Scientific research is organized, it is cooperative and interactive, it creates shared worldviews, and observations are interpreted in accordance with these worldviews. Progress, Fleck holds, consists of the revision and adjustment of worldviews as the community deems appropriate, and over time these adjustments may be so great as to constitute a new worldview, a new style of thought, even a new reality.[39] What the thought collective previously recognized as physical reality may no longer be viewed as reality. Fleck is unambiguously anti-realist on this point: what members of a collective call truth is merely what the thought collective has settled upon at that point. He is also unambiguously anti-individualist and anti-methodological: the agency of scientific progress is located not in the individual but in the group, and the core of science lies not in a particular method but in the diverse interactions of that group.

Under-determination: Pierre Duhem

Fleck's work received some attention when first published, but became much more famous in later years when it came to be viewed as anticipating and influencing the work of Thomas Kuhn. Something similar may be said about Pierre Duhem (1861–1916), whose work was recognized by the Vienna Circle but is now seen as influential primarily because of its uptake by the American philosopher W.V.O. Quine (1908–2000).

To scientists, Duhem is known as a founder of chemical thermodynamics, but he was also a sedulous historian and acute philosopher of science.[40] To philosophers and historians of science today, he is best known for his 1906 book, *The Aim and Structure of Physical Theory,* with its refutation of the notion of a critical experiment and its articulation of what has come to be known as the principle of under-determination.[41]

Duhem's central argument was simple: The Baconian idea of a crucial experiment is mistaken, because if an experiment fails there are many reasons why that might be, so we don't necessarily know what has gone wrong. Conversely, if an experimental test of a theory succeeds, other consequences of the theory may yet be shown to be incorrect. The support for a theory must in principle include all the potential tests of it, and its refutation must be considered in light of all the possible elements that were necessary to perform the experiment in the first place. As the physicist Louis de Broglie put it in 1953 in the preface to the English edition:

> According to Duhem, there are no genuine crucial experiments because it is the ensemble of a theory forming an individual whole which has to be compared to experiment. The experimental confirmation of one of its consequences, even when selected among the most characteristic ones, cannot bring a crucial proof to the theory, for . . . nothing permits us to assert that other consequences of the theory will not yet be contradicted by experiment, or that another theory yet to be discovered will not be able to interpret as well as the preceding one the observed facts.[42]

Put simply: any test of a hypothesis is simultaneously a test of the specific hypothesis under consideration and of the experimental setup, auxiliary hypotheses, and background assumptions. A failed experiment does not necessarily reveal where the failure lies, and a successful experiment does not preclude that a different experimental arrangement or other auxiliary hypotheses would have revealed some difficulty. Duhem wrote: "Any experimental test [in physics] puts into play the most diverse parts of physics and appeals to innumerable hypotheses; it never tests a given hypothesis by isolating it from the others."[43]

Nor does experimental evidence exhaust the range of possible theoretical options open to us: Duhem was explicit that hypotheses are not simply inductions from observation. It is impossible, he asserted without equivocation, to "construct a theory by a purely inductive method."[44] Both theory and experiment have a role in science, and it is mistaken to view experiments as more crucial than theory, mistaken to view them as the source of theory, and above all, mistaken to view them as the final arbiter of theory.

Duhem was not rejecting experimentation. On the contrary, he argued that "the sole purpose of physical theory is to provide a representation and classification of experimental laws."[45] Experiment is essential both to discovering those laws in the first place and to testing the general physical theories that we develop to account for them. The "only test permitting us to judge a physical theory and pronounce it good or bad is the comparison between the consequences of this theory and the experimental laws it has to represent and classify." This view is essentially probabilistic: an experiment can neither verify nor refute a theory; rather it simply tells us whether a theory is "confirmed or weakened by the facts."[46]

De Broglie suggested that a key to Duhem's thought was his interpretation of Léon Foucault's famous experiment in which he demonstrated that the speed of light in water is less than its speed in a vacuum, taken by many as a crucial experiment validating the wave (as opposed to particle) theory of light. Duhem disagreed. Even if Foucault's experiment contradicted Newton's corpuscular theory, other forms of corpuscular theory might yet be consistent with the result.[47]

Yet Duhem did not adopt the radical holism with which his name later became associated. (Holism is the idea that theories stand or fall in their entirety and that a challenge to any one component is potentially a challenge to the entire intellectual fabric.) In places, it may appear that he is on the verge of radical holism, as when he writes of the "radical impossibility [of separating] physical theories from the experimental procedures appropriate for testing these theories," or that an "experiment in physics can never condemn an isolated hypothesis but only a whole theoretical group."[48] But elsewhere he makes clear that he believes some elements of our belief structure are so well established that we are unlikely to doubt them, and rightly so. Some elements of our work are well confirmed through other sources, or strongly linked to principles that we have little doubt are correct. Basic instruments such as thermometers and manometers, for example, are unlikely to be distrusted, as are the concepts that accompany them, such as temperature and pressure. Indeed, he insists that in testing the accuracy of a proposition, a physicist must make use of a whole group of theories that are accepted by him as "beyond dispute." Otherwise he would be paralyzed; it would be impossible for him to proceed. (One may suppose that basic principles of thermodynamics, such as conservation of mass and of energy, are in his mind.) Likewise if an experimental test fails, it does not tell us where the failure lies. It tells us only that somewhere in the system "there is at least one error."[49]

> In sum, the physicist can never subject an isolated hypothesis to experimental test, but only a whole group of hypotheses; when the experiment is in disagreement with his predictions, what he

> learns is that at least one of the hypotheses constituting this group is unacceptable and ought to be modified; but the experiment does not designate which one should be changed.[50]

Duhem did not conclude that for this reason we should be radically skeptical. Rather he argued that we should adopt an attitude of reasonable humility toward intellectual commitments. Following Claude Bernard, he reminds us to be antidogmatic, to maintain an openness to the prospect that our theories may need revision, and to preserve an essential "freedom of mind."[51] Hypothesis, theories, and ideas in general are essential for stimulating our work, but we should not have "excessive faith" in them.[52] We should not be too pleased with our own accomplishments. As Americans at that time might have put it, we should not become "auto-intoxicated."[53]

In the face of an apparent refutation, how does a scientist decide which element(s) of the relevant nexus of theory, instruments, experimental setup, and auxiliary hypotheses should be revised? On this point, Duhem is not entirely satisfactory, invoking Pascal that there are "reasons which reason does not know." In the end, he concludes that these decisions ultimately are matters of judgment and "good sense."[54] Duhem uses history to underscore this point:

> We must really guard ourselves against believing forever warranted those hypotheses which have become universally adopted conventions, and whose certainty seems to break through experimental contradictions by throwing the latter back on more doubtful assumptions. The history of physics shows us that very often the human mind has been led to overthrow such principles completely, though they have been regarded by common consent for centuries as inviolable axioms, and to rebuild its physical theories on new hypotheses.[55]

Yet at the same time, he makes equally clear his conviction that history gives us grounds for confidence in the processes of scientific investigation, so long as we do not become dogmatic. He concludes with the following passage:

> The history of science alone can keep the [scientist] from the mad ambitions of dogmatism as well as the despair of . . . skepticism. By retracing for him the long series of errors and hesitations preceding the discovery of each principle, it puts him on guard against false evidence; by recalling to him the vicissitudes of the cosmological schools and by exhuming doctrines once triumphant from the oblivion in which they lie, it reminds him that the most attractive systems are only provisional representations, and not definitive explanations. And, on the other hand, by unrolling before him the continuous tradition through which the science of each epoch is nourished by the systems of past centuries . . . it creates and fortifies in him that conviction that physical theory is not merely an artificial system, suitable today and useless tomorrow, but that it is an increasingly more natural classification and an increasingly clearer reflection of realities which experimental method cannot contemplate directly.[56]

W.V.O. Quine and the Duhem-Quine Thesis

Duhem's views became known to American audiences primarily through the Harvard philosopher Willard Van Orman Quine, and in the process came to be viewed as more radical than they arguably were. Quine took the problem of refutation and reformulated it under the rubric of what has come to be known as "under-determination." If theories are tested not in isolation but in whole theoretical groups, then how do we know which piece of the group is in need of revision when something goes awry? Duhem's answer was: We rely on judgment. Quine's answer is: We *don't* know. Knowledge, he insists, is a web of belief. When we encounter a refutation, there is a universe of potential adjustments we can make, a universe of threads that can be tightened or loosened to sustain the fabric or reweave it. In Quine's words: "our statements about the external world face the tribunal of sense experience not individually but only as a corporate body."[57]

Duhem would have agreed with that, but he also believed that evidence could lead us to reexamine and adjust parts of that corporate body appropriately. This is one of his two key purposes of experimentation—to strengthen or weaken the support for particular elements in physical theory. If saving the phenomena required us to abandon something that is very strongly held—such as conservation of energy—we would be unlikely to do it. We would conclude that the experiment revealed a problem somewhere else or that there was a problem with our instrumentation. For Duhem, the various parts of the whole theoretical group are not created equal and not equally up for grabs. But Quine thinks that they are, concluding, famously: "any statement can be held true, come what may, if we make drastic enough adjustments elsewhere in the system."[58]

Quine's radical holism came to be known as the Duhem-Quine thesis and is taken by many scholars to weaken the grip of evidence on theory, because if theories are under-determined by experiment—and we have a world of choices in how to respond to experimental failure—then what is the basis for our belief?[59] It appears that some additional component is necessary to explain how scientists come to the conclusions that they do. This became the foundation of a great deal of what followed:

some scholars have argued that the concept of underdetermination underpins the entire set of challenges to empiricist philosophy that developed in the second half of the twentieth century, including the work of Thomas Kuhn and emergence of the field of science studies.[60]

T. S. Kuhn and the Emergence of Science Studies

Thomas Kuhn's point of entry was to hoist the empiricists on their own petard: to assert that the empiricists have not been sufficiently empirical about science itself. His own work was grounded in the history of science through his early study of the Copernican Revolution—the topic of his first book—and his work at Harvard with James Conant developing a set of educational modules known the Harvard Case Histories in Experimental Science.[61] But Kuhn was also deeply engaged with arguments in philosophy of science and had read both Fleck and Quine, as well as works of the Vienna Circle.[62]

One of the central points of Kuhn's *Structure of Scientific Revolutions* was the same as Fleck's: scientists do not work alone but rather in communities that share not just theories about empirical reality—such as the theory of relativity or the theory of evolution by natural selection or the theory of plate tectonics—but also values and beliefs about how their science should operate. Together with models of exemplary scientific accomplishment ("exemplars"), these theories, values, and intellectual and methodological commitments collectively constitute the "paradigm" under which the community operates. This community aspect is paramount: in a 1979 forward to the first English translation of Fleck, Kuhn stressed that in the contemporary scientific world, a person working alone is more likely to be dismissed as a crank than accepted as a maverick.[63]

Most of the time, scientists do not question their paradigms, they work within them, solving problems and answering questions that the framework identifies as relevant. Kuhn called this "normal science" and asserted that its principal activities were a form of puzzle solving. Contra Popper, during normal science scientists do not attempt to refute the paradigm. In fact, they do not even question it—until a problem arises. This is where the engagement of science with reality becomes most evident: problems arise because some observation or experience of the world—some "technical puzzle"—does not fit expectation.[64] Kuhn calls these "anomalies." At first, scientists will attempt to account for the anomaly within the paradigm, perhaps making some modest adjustment in it. But if the anomaly becomes too great or too glaring, or the adjustments made to accommodate it generate new problems, this creates a crisis, which opens the intellectual space for reconsideration of the paradigm. Sometimes crises are resolved within the paradigm, but when they cannot be, a scientific revolution occurs: the governing paradigm is overthrown and replaced by a new one. It is like a political revolution, insofar as the new paradigm is in effect a new form of intellectual governance, with new rules and regulations. Kuhn thus argued that science advances neither by verification nor refutation, but by paradigm shifts.

Many scientists welcomed Kuhn's views insofar as they painted a picture of science that was recognizable to them, or at least more recognizable than the alternatives.[65] But what fired up the many readers who were not scientists was a claim that most scientists probably didn't understand and wouldn't have liked if they had (and what distinguishes Kuhn from Fleck): that successive paradigms are *incommensurable*. By this Kuhn meant, literally, that there was no metric by which a new paradigm could be compared to the one it proposed to replace. As Fleck had argued, the new paradigm—like the new thought-style—was not just a shift in thinking about a particular scientific question, it was also a shift in meanings, values, priorities, aspirations, and even the self-identity of the scientist. This opened still wider the question that Quine had posed: How do scientists decide which part of their belief structure needs to be revised in light of an anomaly? How do they decide whether a small adjustment is sufficient or a scientific revolution is in order? And if the new paradigm is incommensurable with the one it proposes to replace, on what basis do scientists make the choice to accept it?

Historians and philosophers have been debating these questions ever since. Philosophers were vexed by the incommensurability claim, insofar as it seemed to reduce paradigm choice to relativism and even irrationality.[66] Imre Lakatos, for example, opined that in Kuhn's theory, the scientific revolution is "a mystical conversion which is not and cannot be governed by rules of reason."[67]

Historians felt validated that Kuhn insisted on the detailed study of real science, but tended to find the incommensurability claim to be overblown, and noted that Kuhn had made a methodological error by sometimes comparing non-proximate scientific theories, such as Aristotelian physics and quantum mechanics. Yes, historians acknowledged, Aristotelian physics is inscrutable to a contemporary physicist, but there have been many intermediate steps between then and now; it does not work to try to understand the entire arc of the history of physics without tracing these intermediate steps. It would be like analyzing a relay race thinking that the baton had been thrown rather than passed.

My own view is that Kuhn was closer to the mark in his less famous earlier work *The Copernican Revolution*, in which he described a major scientific change as a bend in the road:

> From the bend, both sections of the road are visible. But viewed from a point before the bend, the road seems to run straight to the bend and disappear. . . . And viewed from a point in the next section, after the bend, the road appears to begin at the bend from which it runs straight on.[68]

Kuhn's work was itself a bend in the road of studies of science: away from method and toward practice; away from individuals and toward communities.[69] Scholars generally agree that the largest impact of Kuhn's work—besides adding the term *paradigm shift* to the general lexicon—was in helping to launch the field of science studies.

Away from Method

Philosophers from Comte to Popper attempted to identify the method of science that accounted for its success and therefore justified our acceptance of scientific claims as true—what is sometimes called "warranted true belief." Kuhn did not exactly say that there was no method, but he did say two things that displaced method from centrality. The first was the claim that under different paradigms, methods could change. The second was that most of the time, the methods of science amounted to not much more than puzzle-solving—working out details within the paradigm without questioning the larger structure—and that seemed pretty uninteresting. Moreover, whatever the methods were, they were done by groups of people working together, not individuals working alone.

This opened the door for an expanded sociology of science that not only examined the formal institutional structures of science, as previous sociologists had done, or the norms of scientific behavior, as the famous sociologist of science Robert Merton had studied, but addressed the *epistemological* question: What is the basis for scientific belief? If the intellectual action in science is in the paradigm shift, and if paradigms are incommensurable, then our traditional notions of scientific progress are clearly unsupportable. Perhaps science does not give us warranted true belief. Perhaps we should *not* trust science. If scientists can abandon one view and replace it with another incommensurable one, that does not inspire confidence in the idea that the processes of science necessarily provide us with a reliable view of the world. In any case, someone needs to explain the grounds on which scientists accept the claims they do.

Sociology of Scientific Knowledge and the Rise of Science Studies

Sociologists who took up the gauntlet thrown down by Kuhn called further attention to the social elements responsible for scientific conclusions, or what has come to be known as the *social construction* of scientific knowledge.[70] While they saw themselves as epistemological radicals, they were building on what had come before, particularly Quine's formulation of under-determination. They now asked: On what grounds do scientists decide what to believe and what to reject? How are these decisions articulated within the frameworks of scientific communities? To what degree, if any, should we respect the claims that emerge from this process?

The most influential of these early efforts came from the group of scholars we have come to know as the Edinburgh school, particularly Barry Barnes, David Bloor, and Steven Shapin. Barnes concentrated on "interests" as a driving force in theory choice. These "interests" could be professional, in the sense that the success of a favored theory would benefit the career of its promoter, or there could be an interest in a particular value set or a theory that was consistent with one's political, religious, or ethical views.[71] (In hindsight, interest theory seems oddly individualistic, but that is another matter.) Bloor insisted that the methods of science studies should be "symmetrical," meaning that "the same types of cause would explain, say, true and false beliefs."[72] Shapin attended particularly to the interrelationship between knowledge production and social order, arguing memorably, with historian Simon Schaffer, that "solutions to the problem of knowledge are solutions to the problem of social order."[73]

The arguments of the Edinburgh school were often taken to be ontologically anti-realist, and for that reason dismissed by many scientists as ridiculous.[74] To be sure, some scholars wrote in a manner that suggested a disregard for, if not outright disbelief in, the significance of empirical evidence in formulating scientific knowledge. It was easy to slip from the claim that empirical evidence does not by itself determine our conclusions to the suggestion that empirical evidence plays no role. But the argument was not so much anti-realist as it was *relativist*: if empirical evidence cannot determine decisively what we should believe and what we should reject, it does seem to suggest that our views are framed in relation to some set of standards and concerns that cannot be deduced from, nor reduced to, empirical evidence. And if social interests and conditions play a determinative role, then our knowledge must be at least in part relative to those interests and conditions. This was a very serious challenge. As Barnes explained in the 1970s, the approach of the Edinburgh school is

> sceptical since it suggests that no arguments will ever be available which could establish a particular epistemology or ontology as ultimately correct. It is relativistic because it suggests that belief systems cannot be objectively ranked in terms of their proximity to reality or their rationality.[75]

This was not the same as denying that our encounters with reality play a role in our convictions (much less to claim that there is no physical reality). Rather, it was to argue that the role of empirical evidence in shaping them was not nearly as determinative as most philosophers and scientists thought. Later commentators have generally allowed that the Edinburgh school was correct in stressing that evidence alone does not account for the conclusions to which scientists come.[76] The question, however, was whether Edinburgh theorists were suggesting that it played little or even no role. As Barnes allowed, "Occasionally, existing work leaves the feeling that reality has *nothing* to do with what is socially constructed or negotiated to count as natural knowledge, but we may safely assume that this impression is an accidental by-product of over-enthusiastic sociological analysis."[77]

This claim may be too generous; my own feeling is that some sociologists associated with or influenced by the Edinburgh school deliberately created this impression. When Karin Knorr-Cetina, for example, insisted in the 1980s that scientific knowledge was a "fabrication," when Harry Collins asserted that "the natural world in no ways constrains what is believed to be," and when Bruno Latour declared that science was "politics by other means," these terms and phrases were clearly chosen to unsettle what the historian John Zammitto has called the "ambient idolatry of science" that had prevailed under positivism.[78] Moreover, by saying that "belief systems cannot be objectively ranked," Edinburgh scholars seemed to imply that objectivity did not play the role in science that scientists typically asserted, and perhaps played no role at all. These assertions were not accidental; they were deliberate provocations.

But not all provocations are illegitimate, and the more important point, stressed recently by David Bloor, is that if we feel the need to contrast *relativism* with something, we should contrast it not with objectivity—which is the opposite of subjectivity—nor with truth, which is the opposite of falsehood—but with *absolutism*. The opposite of relative knowledge is absolute knowledge, and no serious scholar of the history or sociology of knowledge can sustain the claim that our knowledge is absolute. Nor can we sustain the claim that empirical evidence alone suffices to explain scientific conclusions. Far too much evidence refutes that hypothesis. Bloor has always been clear that he wants to be scientific in his study of science, and to be scientific about science means to take seriously the empirical evidence about the role of empirical evidence! And that empirical evidence reveals the limits of empiricism. Bloor's point has always been that when we look at science carefully and with an open mind, we see both empirical and social factors at play in stabilizing scientific knowledge, and we cannot assume a priori which ones are more important in any given case.[79]

A different critique of the notion of empirical method came from the philosopher Paul Feyerabend (1924–94). Born in Vienna, Feyerabend completed a PhD in philosophy on the topic of observation sentences and spent much of his life in conversation with Karl Popper and Imre Lakatos, laying the groundwork for what might have been a career as a leading light of logical empiricism. But he later rejected not just logical empiricism, but any attempt to define or prescribe the method of science. In his most famous work, *Against Method* (published in 1975), he argued that there was no scientific method, nor should there be. Scientists have used a diversity of methods to good effect; any attempt to restrict this would hamper their creativity and impede the growth of scientific knowledge. Moreover, falsification as a rule is clearly falsified by the facts of history: few if any theories in the history of science ever explained all the available facts. Often scientists ignored facts that didn't fit or didn't seem significant, or set them aside to worry about at a later date.[80] (Popper might claim that those scientists were bad scientists, but if so then most scientists have been bad scientists, including some of our most celebrated.)

Like the science studies scholars quoted above, Feyerabend embraced a deliberately provocative style, and perhaps because he described his position as "theoretical anarchism" he is often quoted as having claimed that in science "anything goes." But that was not his claim. The actual quotation is this:

> It is clear then, that the idea of a fixed method, or a fixed theory of rationality, rests on too naïve a view of man and his social surroundings. To those who look at the rich material provided by history, and who are not intent on impoverishing it in order to please their lower instincts, their craving for intellectual security in the form of clarity, precision, "objectivity," [and] "truth," it will become clear that there is only one principle that can be defended under *all* circumstances and in all stages of human development. It is the principle: anything goes.[81]

Feyerabend was saying that if you *pressed* him to define the method of science, he would have to say that anything goes—which is to say that there is no unique method or principle of science. This was not an abdication of the responsibility to demarcate science from non-science, as Popper might have argued, but a recognition that methodological and intellectual diversity characterized the history of science, and this was a good thing: it made communities stronger, more creative, more open-minded, and nicer.[82] Absolutism—whether in science, politics, or anything else—was generally objectionable.[83] Like Popper (and Duhem and Comte), Feyerbend believed in progress; he just disagreed about whence it came. He summarized: "Theoretical anarchism is more humanitarian and more likely to encourage progress that its law-and-order alternatives . . . [and the] only principle that does not inhibit progress is: anything goes."[84] When we look seriously at what scientists do, we find that they are nothing if not creative, flexible, and adaptive.

Feyerabend was a philosopher, not a sociologist, and he accepted that science was progressive in a way that most of his sociological colleagues did not. But his work did support the sociological trend emerging strongly in the 1970s of focusing on the practices of scientists—in their labs, in the field, in clinical trials. If we cannot state a priori what the method of science is (or methods are), then the only way to find out is through observation.

The person who since then has done the most in that regard is unquestionably Bruno Latour, who turned the techniques of anthropology to science and in doing so drew particular attention to the practices that scientists employ to persuade their colleagues to accept any particular claim. Latour's great impact on the field was to establish ethnography as a key methodology in science studies, and to insist on the importance of privileging what scientists do over what they say.[85] While the work that has followed in his wake defies easy summation, one thing is clear: it confirms earlier arguments about scientific methodological diversity. After the work of the Edinburgh school, of Feyerabend, of Latour and his colleagues, and of the diverse historians who have documented the ways scientific methods have changed over time, it is no longer plausible to hold to the view that there is any singular scientific method.[86]

This is not an entirely negative finding, but it does commit us to the conclusion that the dream of positive knowledge has truly ended.[87] There is no identifiable (singular) scientific method. And if there is no singular scientific method, then there is no way to insist on ex ante trust by virtue of its use. Moreover, despite the claims of prominent scientists to the contrary, the contributions of science cannot be viewed as permanent.[88] The empirical evidence gleaned from the history of science shows that scientific truths are perishable. How can we tell then if scientific work is good work or not? On what basis should we trust or distrust science?

Getting Unstuck: Social Epistemology

Despite the challenges of science studies, there have still been many attempts to salvage scientific rationality. In my view, the most successful of these have come from a direction that most scientists would have least suspected: feminism.

Since the 1960s, feminists have asked: How could science claim to be objective when it largely excluded half the population from the ranks of its practitioners? How could science claim to be producing disinterested knowledge when so many of its theories embedded obvious social prejudices, not just about gender but also about race, class, and ethnicity? These questions were not necessarily hostile. Many of them were raised by female scientists who were interested in the natural or social world and believed in the power and value of scientific inquiry to explain it.

Sociologists of scientific knowledge stressed that science is a social activity, and this has been taken by many (for both better and worse) as undermining its claims to objectivity. The "social," particularly to many scientists but also many philosophers, was synonymous with the personal, the subjective, the irrational, the arbitrary, and even the coerced. If the conclusions of scientists—who for the most part were European or North American men—were social constructions, then they had no more or less purchase on truth that the conclusions of other social groups. At least, a good deal of work in science studies seemed to imply that.

But feminist philosophers of science, most notably Sandra Harding and Helen Longino, turned that argument on its head, suggesting that objectivity could be reenvisaged as a social *accomplishment,* something that is collectively achieved.[89] Harding mobilized the concept of *standpoint epistemology*—the idea that how we view matters depends to a great extent on our social position (or, colloquially, that where we stand depends on where we sit)—to argue that greater diversity could make science stronger. Our personal experiences—of wealth or poverty, privilege or disadvantage, maleness or femaleness, heteronormativity or queerness, disability or able-bodiedness—cannot but influence our perspectives on and interpretations of the world. Therefore, *ceteris paribus,* a more diverse group will bring to bear more perspectives on an issue than a less diverse one.[90]

In her groundbreaking 1986 book, *The Science Question in Feminism,* Harding argued that the objectivity practiced by most scientific communities was weak, because of the characteristic homogeneity of those communities. The perspectives of women, people of color, the working classes, and many others were lacking, and the consequences were plain to see when one considered the obvious sexism, racism, and class bias of many past scientific theories. But there could be less obvious forms of bias at work as well. She argued for what she labeled *strong objectivity*: an approach that acknowledged that an individual's beliefs, values, and life experiences necessarily affect their work—scientific or otherwise—so the best way to develop objective knowledge is to increase the diversity of knowledge-seeking communities. Objectivity was not a 0/1 proposition: communities could be more or less objective and greater objectivity in scientific research achieved—or at least made more likely—by greater heterogeneity in the scientific community.[91]

Like Feyerabend, Harding tended toward the deliberately provocative—as when she compared Newton's *Principia Mathematica* to a rape manual—and this made her an easy target of right-wing critics.[92] It also made her the target of scientific critics, such as Paul Gross and Norman Levitt, who failed to understand that the central point of her critique was that science could be made stronger through inclusion. This point was made a bit more diplomatically—albeit equally forcefully from an intellectual standpoint—by the feminist philosopher Helen Longino.

Longino transformed a common scientific assumption—that science *is* self-correcting—into a pressing intellectual question—*How* is it that science is self-correcting? After all, the claim that science corrects itself might be viewed as highly implausible—a sort of epistemic magic trick. Longino's suggested that it is not so much that *science* corrects *itself*, but that *scientists* correct *each other* through the social processes that constitute "transformative interrogation." It is through the give and take of ideas—the challenging, the questioning, the adjusting and amending—that scientists integrate their colleagues' work, offer up criticisms, and contribute to the growth of warranted knowledge. She wrote:

> The objectivity of individuals in this scheme consists in their participation in the collective give-and-take of critical discussion and not in some special relation (of detachment, hardheadedness) they may bear to their observations. Thus understood, objectivity is dependent upon the depth and scope of the transformative interrogation that occurs in any given scientific community.[93]

Longino urged us to accept (rather than lament) the fact that individual scientists invariably bring biases, values, and background assumptions into their work. The scientist entering the laboratory cannot hang up her personal values, preferences, assumptions, and motivations like an overcoat, as Claude Bernard once supposed.[94] What can happen, however, is that in a diverse community subjective elements can (and most likely will) be challenged by others, and to the extent that they may be inappropriately informing evidential reasoning and theory choice, that can be challenged, too.[95]

Longino's account of transformative interrogation solves the problem of how science, as a whole, can be objective even when individual scientists are not:

> If scientific inquiry is to provide knowledge, rather than a random collection of opinions, there must be some way of minimizing the influence of subjective preferences and controlling the role of background assumptions. The social account of objectivity solves this problem. The role of background assumptions in evidential reasoning is grounds for unbridled relativism only in the context of an individualistic concept of scientific method and scientific knowledge. . . . Values are not incompatible with objectivity, but objectivity [emerges] as a function of community practices rather than as an attitude of individual researchers.[96]

This perspective reinforces Harding's position that objectivity is not a matter of either/or, but of degree. The greater the diversity and openness of a community and the stronger its protocols for supporting free and open debate, the greater the degree of objectivity it may be able to achieve as individual biases and background assumptions are "outed," as it were, by the community. Put another way: objectivity is likely to be maximized when there are recognized and robust avenues for criticism, such as peer review, when the community is open, non-defensive, and responsive to criticism, and when the community is sufficiently diverse that a broad range of views can be developed, heard, and appropriately considered. On this view, it is not surprising that when scientists were almost exclusively white men, they developed theories about women and African Americans that were at best incomplete and at times pernicious—theories that have now been rejected. Nor is it surprising that many of the logical and empirical flaws of these earlier theories were pointed out by women and people of color.[97] (This point is addressed further in chapter 2.)

The key point here is that often "assumptions are not perceived as such."[98] They are so embedded as to go unrecognized *as* assumptions, and this is most likely to occur in homogeneous communities. Longino continues:

> When, for instance, background assumptions are shared by all members of a community, they acquire an invisibility that renders them unavailable for criticism. They do not become visible until individuals who do not share the community's assumptions can provide alternative explanations of the phenomena without those assumptions, as, for example, Einstein could provide an alternative explanation of the Michelson-Morley interferometer experiment [because he did not share the assumption of the variable speed

> of light]. . . . From all this it follows again that the greater the number of different points of view included in a given community, the more likely it is that its scientific practice will be objective . . . [and] it will result in descriptions and explanations of natural processes that are more reliable . . . than would otherwise be the case.[99]

Transformative interrogation can empower us to decide whether those background assumptions are, in a given context, appropriate and helpful or inappropriate and unhelpful. This is most likely to occur in a diverse community for the simple reason that diverse communities will have diverse background assumptions. Diversity does not heal all epistemic ills, but *ceteris paribus* a diverse community that embraces criticism is more likely to detect and correct error than a homogeneous and self-satisfied one.[100]

Feminist epistemology soundly refutes the claim that the social character of science makes it subjective. On the contrary, we can now see that scientists who were offended by the social turn in science studies—as well as science studies scholars who thought they could debunk science by exposing its social character—got it wrong. The feminist account of the social character of science can make a stronger case for the objectivity of scientific knowledge than previous accounts by identifying both sources of bias and remedies to it. And consider this: in their dyspeptic polemic of the 1990s, *Higher Superstitions: The Academic Left and Its Quarrels with Science,* scientists Paul Gross and Norman Levitt accused feminists of being anti-science. But neither Harding nor Longino were anti-science.[101] Both were discussing ways to strengthen and improve it. Gross and Levitt could have used feminist philosophy of science in their defense of science had they not been so busy taking offense.

In Diversity There Is Epistemic Strength

Feminist philosophy of science salvages science from the claim that its social character makes it subjective, but it does leave us with a view of science that makes some people uncomfortable: that science is fundamentally *consensual*. Longino summarizes: "To say that a theory or hypothesis was accepted on the basis of objective methods does not entitle us to say it is true but rather that it reflects the critically achieved consensus of the scientific community. [And] it's not clear we should hope for anything better."[102] I agree. But where does that leave us?

To recapitulate: There is now broad agreement among historians, philosophers, sociologists, and anthropologists of science that there is no (singular) scientific method, and that scientific practice consists of communities of people, making decisions for reasons that are both empirical and social, using diverse methods. But this leaves us with the question: If scientists are just people doing work, like plumbers or nurses or electricians, and if our scientific theories are fallible and subject to change, then what is the basis for trust in science?

I suggest that our answer should be two-fold: 1) its sustained engagement with the world and 2) its social character.

The first point is crucial but easily overlooked: Natural scientists study the natural world. Social scientists study the social world. That is what they do. Consider a related question: Why trust a plumber? Or an electrician? Or a dentist or a nurse? One answer is that we trust a plumber to do our plumbing because she is trained and licensed to do plumbing. We would *not* trust a plumber to do our nursing, nor a nurse to do our plumbing. Of course, plumbers can make mistakes, and so we get recommendations from friends to ensure that any particular plumber has a good track record. A plumber with a bad track record may find herself out of business. But it is in the nature of expertise that we trust experts to do jobs for which they are trained and we are not. Without this trust in experts, society would come to a standstill. Scientists are our designated experts for studying the world.[103] Therefore, to the extent that we should trust anyone to tell us about the world, we should trust scientists.

This is not the same as faith: We do (or should) check the references of our plumbers and we should do the same for our scientists. If a scientist has a track record of error, underestimation, or exaggeration, this might be grounds for viewing his or her claims skeptically (or at least judging their results with this information in mind.) If a scientist is receiving financial support—directly or indirectly—from an interested party, this may be grounds for applying a higher level of scrutiny than we might otherwise demand. (For example, an editor might send the paper for additional review, or a reviewer might pay extra attention to study design, where subconscious bias may slip in.)[104]

No doubt individual scientists, like individual plumbers, may be stupid, venal, corrupt, or incompetent. But consider this: the *profession* of plumbing exists because in general plumbers do a job we need them to do, and in general they do it successfully. When we evaluate the track record of science, we find a substantial record of success—in explanation, in prediction, in providing the basis for successful action and innovation. We have a world of medicines, technologies, and conceptual understandings derived from science that have enabled people to do things they have wanted to do. (As already noted, that success does not prove that the theories involved are necessarily true, but it does suggest that scientists are doing something right.) This might be the one point on which the diverse scholars I have discussed agree: philosophers, historians, sociologists, and anthropologists have all been interested in science because of its *success*—both culturally and epistemologically. The question of this lecture is of interest at least in part because the success of science as a source of stable epistemic authority has been called into question, and its future success as a cultural enterprise appears to be at least somewhat in doubt.

This consideration—that scientists are in our society the experts who study the world—is a reminder to scientists of the importance of foregrounding the empirical character of their work—their engagement with nature and society and the empirical basis it provides for their conclusions. As I have stressed elsewhere, scientists need to explain not just what they know, but how they know it.[105] Expertise as a concept also carries with it the embedded idea of specialization, and therefore the limits to expertise, reminding us why it is important for scientists to exercise restraint with respect to subjects on which they lack expertise.

However, reliance on empirical evidence alone is insufficient for understanding the basis of scientific conclusions and therefore insufficient for establishing trust in science. We must also take to heart—and explain—the social character of science and the role it plays in vetting claims. Here it is worth reiterating my point that scientists who were offended by the "social" turn in science studies got it wrong: much of what we identify as "science" are social practices and procedures of adjudication designed to ensure—or at least to attempt to increase the odds—that the process of review and correction are sufficiently robust as to lead to empirically reliable results.[106] Again, Longino: "Socializing cognition is not a corruption or displacement of the rational but a vehicle of its performance."[107]

Peer review is one example of such a practice: it is through peer review that scientific claims are subjected to critical interrogation. (This is why, in my own work, I have stressed the importance of evaluating scientific consensus through analysis of the peer-reviewed literature and not the popular press or social media, and why these chapters were subject to peer review.) This includes not only the formal review that papers go through when submitted to academic journals, but also the informal processes of judgment and evaluation that research findings undergo when scientists discuss their preliminary results in conferences and workshop and solicit comments from colleagues prior to submitting them for publication, as well as the continued process of evaluation that published claims endure as fellow scientists attempt to use and build on those claims.[108]

Tenure is another example: we evaluate scholars' work in order to judge whether they are worthy of joining the community of scholars in their fields, in effect to be certified as experts. Tenure is effectively the academic version of licensing. The crucial element of these practices is their social and institutional character, which work to ensure that the judgments and opinions of no one person dominate and therefore that the value preferences and biases of no one person are controlling. Of course, within any community there will be dominant groups and individuals, but the social processes of collective interrogation offer a means for the less dominant to be heard so that, to the maximum degree possible, the conclusions arrived at are non-partisan and non-idiosyncratic.[109] The social character of science forms the basis of its approach to objectivity and therefore the grounds on which we may trust it.

In recent years, this insight has been implicitly incorporated into scientific practices, particularly in just those domains where scientific claims are likely to be viewed as controversial.

The US National Academy of Sciences works to ensure that the panelists who perform its reviews are diverse and represent a range of viewpoints. Scholars have called this approach the "balancing of bias."[110] The Intergovernmental Panel on Climate Change—now one of the world's largest aggregations of scientists—makes a particular point of seeking geographical, national, racial, and gender diversity in its chapter-writing teams. While the motivations for inclusivity may be in part political, the widespread character of practices of inclusion suggest that many scientific communities now recognize that diversity serves epistemic goals.

Caveats

My arguments require a few caveats. Most important is that there is no guarantee that the ideal of objectivity through diversity and critical interrogation will always be achieved, and therefore no guarantee that scientists are correct in any given case. The argument is rather that, given the existence of these procedures and assuming they are followed, there is a mechanism by which errors, biases, and incompleteness can be identified and corrected. In a sense, the argument is probabilistic: that if scientists follow these practices and procedures, they increase the odds that their science does not go awry. Moreover, outsiders may judge scientific claims in part by considering how diverse and open to critique the community involved is. If there is evidence that a community is not open, or is dominated by a small clique or even a few aggressive individuals—or if we have evidence (and not just allegations) that some voices are being suppressed—this may be grounds for warranted skepticism. In this respect, each case must be evaluated on its own merits.

An interesting recent case is the "extended evolutionary synthesis" (EES) concept, which challenges the primacy of genetic control in inheritance and calls increased attention to developmental plasticity, environmental modification by organisms (including niche construction), epigenetics, and social learning.[111] Some advocates of EES have been disturbed by resistance they have encountered among "traditionalists" in the evolutionary biology community, who argue that the existing evolutionary synthesis is adequate and no extension is needed.[112] The ensuing arguments have sometimes become hostile and personal.[113] To a historian familiar with past major debates in science, it is not surprising that there is resistance to new ideas that threaten the stability of past scientific achievements or the social position of their adherents, nor that this resistance at times gets heated.[114] When people's life work is being questioned, they may get testy. No one likes to be told that they are wrong. The important question here is whether the advocates of EES have been able to publish their views in respected journals and to obtain funding for their research. The answer is yes. Despite the flaring of tempers, the evolutionary biology community as a whole has proved open to the introduction of new ideas and the critical interrogation of old ones.

A second caveat is that my argument is by no means a call for blind or blanket trust, much less a slavish adherence to scientists' recommendations on non-scientific matters. It is a call for *informed* trust in the consensual conclusions of scientific communities, but not necessarily in the views or opinions of individual scientists, particularly not when they stray outside their domains of expertise. Indeed, the track record of scientists outside their specialties is not particularly impressive. One need only think of physicist-mathematician John von Neumann claiming in the 1950s that within a few decades nuclear energy would be as free as the "unmetered air," or physicist William Shockley's insistence that African Americans were genetically inferior to whites and should be paid to undergo "voluntary" sterilization.[115] Werner von Braun thought that by the year 2000, the first child would have been born on the moon.[116] Physical scientists, particularly in the United States, have tended to be technofideists, exaggerating the rate at which new technologies would be developed or the degree to which they would improve our lives. Both physical and life scientists have an unhappy record of insensitivity to social and ethical concerns, as witnessed by the widespread support among biologists in the early twentieth century for eugenics programs that in hindsight appear both scientifically erroneous and morally noxious (see chapter 2). Outside their domains of expertise, scientists may be no more well informed than ordinary people. Indeed, they may be less so as their intense training in one area can lead them to be undereducated in others.[117]

The claim that scientists have expertise in particular domains is not, moreover, to insist that this expertise is exclusive. Many lay people—farmers, fishermen and women, patients, midwives—have expertise in their particular domains.[118] Patients may have considerable understanding of the progression of their disease or the side effects of pharmaceuticals; midwives may be able to recognize problems in pregnancies as well or better than some obstetricians. There was extensive scientific knowledge in India before the arrival of the British, particularly about matters that the British would label "natural historical" (but locals might not have labeled this way).[119]

We have a considerable literature on indigenous expertise: the knowledge that both lay people and experts may have about plants, animals, geography, climate, or other aspects of their natural environments and communities. In recent decades we have come to understand more fully the empirical knowledge systems that have developed outside of what we conventionally call "Western science"—what anthropologist Susantha Goonatilake has called "civilizational knowledge." These systems may involve highly developed expertise, and may be quite effective in their realms.[120] For example, Traditional Chinese Medicine (TCM), acupuncture, and Ayurvedic medicine can be efficacious in treating certain diseases and conditions for which Western medicine has little to offer.[121] Civilizational knowledge traditions have authority in their regions of origin by virtue of track records of success, and in some cases (e.g., acupuncture) have demonstrated efficacy beyond those regions as well. Moreover, the study of civilizational knowledge has highlighted the values embedded in Western science that often go unrecognized or are even denied by its practitioners.[122]

There are also lay knowledge traditions based on sustained empirical and analytical engagement with the world. Hunter-gatherer societies, for example, typically have detailed empirical knowledge of plant distributions and animal migrations; anthropologist Colin Scott, for example, has demonstrated that Cree hunting traditions are highly empirical, and argues that they are therefore rightly viewed as scientific.[123] Where lay knowledge overlaps with scientific knowledge, one should not assume that the latter is necessarily superior to the former.[124] We know, for example, that Polynesian navigators were far more effective in plying the Pacific than their European counterparts until at least the time of Cook in the late eighteenth century.[125]

There is an important distinction to be made here: respecting indigenous, lay, and "Eastern" knowledge that has demonstrated empirical adequacy or clinical efficacy is a very different thing from accepting popular claims that are ignorant, erroneous, or represent motivated disinformation. The claims of an actress that vaccines cause autism or an oil executive that recently observed climate change has been caused by sunspots do not come out of established knowledge traditions; the individuals promoting them do not have a credible claim to expertise. An actress is not an immunologist; a petroleum industry CEO is not a climate scientist. And in these particular cases, we have abundant empirical evidence that their claims are untrue. The claim that climate change is caused by sunspots has had its day in scientific court: it has been vetted by evidence and shown to be incorrect.[126] Autism is no more common among children who have been vaccinated than those who have not.[127] Respecting alternative knowledge traditions does not mean that we suspend judgment, either about those traditions or our own.

It is also important to distinguish between the scientific and the normative questions that get mooted in contemporary society. To be sure, the interrelations between the various sciences and the politics, economics, and morality that surround and embed them are often complex, intercalated, and not easily disentangled; some scholars have argued that they cannot be disentangled.[128] I believe that, however imperfectly, we can distinguish between the scientific and normative aspects of many questions—and that we continue to need to. Whether manmade climate change is underway is a different sort of question from what we should do about it; I may have reasons for declining vaccination that have nothing to do with its alleged relation to autism.[129] These distinctions matter, because if I understand that some of my fellow citizens reject vaccinations on religious grounds, I may respect that opinion without succumbing to the fallacy that vaccines cause autism; depending on my own religious views, I might join them or I might not. Similarly, I can respect the fact that many people have had adverse reactions from pharmaceuticals and know that iatrogenic illness is a real thing, without accepting the allegation that it is the drug AZT, rather than a virus, that causes HIV-AIDs.[130] Pope Francis rejects genetically modified organisms as an inappropriate interference with God's domain; if I were Catholic I might choose to follow his views irrespective of the scientific evidence as to whether those products are safe to eat.[131] Distinctions between the scientific and the social matter, because they rightly affect our choices, and because us they help to distinguish between arguments that may be persuasive to our audiences and arguments that are doomed to fail because they don't address their underlying concerns.

Comte argued long ago that the basis for the success of science was experience and observation. We now know that that is only part of the story, albeit an important part. Nevertheless, we can use this argument to remember that the basis for *our* trust in science is, in fact, experience and observation—not of empirical reality, *but of science itself*. It is what Comte argued long ago: that just as we can only understand the natural world by observing it, so we can only understand the social world by observing it. When we observe scientists, we find that they have developed a variety of practices for vetting knowledge—for identifying problems in their theories and experiments and attempting to correct them. While these practices are fallible, we have substantial empirical evidence that they do detect error and insufficiency. They stimulate scientists to reconsider their views and, as warranted by evidence, to change them. This is what constitutes progress in science.

Coda: Why Not the Petroleum Industry?

We can now answer the question raised at the outset of this chapter of ex ante trust. Why should the conclusions of climate scientists about climate change be viewed ex ante as more authoritative than those originating from the petroleum industry? Or arguments about cancer and heart disease from the tobacco industry? Or about diabetes or obesity from Coca-Cola?[132]

The answer is simple: conflict of interest. The petroleum industry exists to explore for, find, develop, and sell petroleum resources, and by doing so to make a profit and return value to shareholders. It relies heavily on science and engineering to do this, and company scientists and executives have considerable expertise in the domains of sedimentary geology, geophysics, and petroleum and chemical engineering, as well as sales and marketing. But recent scientific findings about the reality and severity of anthropogenic climate change—and the role of greenhouse gases derived from fossil fuel combustion in driving it—threaten not only the industry's profitability, but even its existence. The fossil fuel industry as we know it is fighting for its survival. Rather than accept the necessity of change, certain elements in the industry have misrepresented the scientific evidence that demonstrates that necessity.[133] Exxon Mobil may be a reliable source of information on oil and gas extraction, but it is unlikely to be a reliable source of information on climate change, because the former is its business and the latter threatens it.[134]

We may say the same about the tobacco industry. For decades, the tobacco industry refused to accept the scientific evidence that tobacco products caused cancer, heart disease, bronchitis, emphysema, and a host of serious conditions and fatal diseases, including sudden infant death syndrome. It worked to challenge, discredit, and suppress known information, and it paid scientists to engage in research that was in other respects legitimate, but whose purpose (from the industry standpoint) was to distract attention from the adverse effects of tobacco use. The chemical industry has done much the same with respect to pesticides and endocrine disrupting chemicals; in recent years we have seen some of the same strategies and tactics taken up by elements of the processed food industry.[135] The tobacco, processed food, and chemical industries face an essential conflict of interest when discussing scientific results that bear on the safety, efficacy, or healthfulness of their products. They are not engaged in good faith in the open, critical, and communal vetting of evidence that is crucial for the determination of the reliability of scientific claims. This is why, ex ante, we have reason to distrust them.

This is not to say that an individual scientist or team of scientists is necessarily discredited simply because they work in or for a potentially conflicted industry or have received funding from it. Scientists within an industry may participate in the scientific enterprise by doing research and submitting it for publication in peer-reviewed journals, and there are many fine examples of this, particularly in the early twentieth century when many corporations ran large industrial research laboratories. (Full disclosure: my own PhD work was partly funded by the mining company for whom I worked before going to graduate school, and this was disclosed in my relevant publications.)

When industry-funded scientists attend conferences and publish in peer-reviewed journals, they are acting as parts of scientific communities, participating in the norms of those communities and subjecting themselves and their work to critical scrutiny. As long as they do—so long as the norms of critical interrogation are operating and conflicts of interest are forthrightly disclosed and where necessary addressed—these scientists may well make fine contributions.[136]

But it is scarcely a secret that the goals of profit-making can collide with the goals of critical scrutiny of knowledge claims. We know from history that industrial research can be of high quality, but we also know that it exists—and is subject to external scrutiny—at the discretion of the industrial sponsor. Excellent research has emerged from the precincts of American business and industry, but so has disinformation, misrepresentation, and distraction. Science done within industries has won Nobel prizes; it has also been subject to suppression and distortion. Moreover, as Robert Proctor, Allan Brandt, David Rosner, Gerald Markowitz, Miriam Nestle, Erik Conway, and I have documented, a substantial amount of industry research has been *designed* to be a distraction.[137] Empirical reality tells us that we are right to be suspicious when the petroleum industry makes claims about climate science or the soda industry offers up nutritional claims, just as we should have been suspicious when the tobacco industry told us that Luckies were good for us and Camels would aid our digestion.[138]

The checkered history of scientific research in American industry that was designed to distract, confuse, and/or misinform also helps us to address one of the more nefarious strategies of industry—doubt-mongering: the claim that they are instantiating the spirit of scientific inquiry when they pose skeptical questions and that it is *scientists* who are being dogmatic. This is an intellectually noxious move, because it takes the strength of science and turns it into a weakness, and falsely imputes scientific motives to activities that are intended to undermine science. Moreover, when scientists are unfairly attacked, they may become defensive and therefore less open to warranted critique than they should be. In this regard doubt-mongering is doubly damaging: it undermines public trust in science and it has the potential to undermine science itself.

The processes of critical interrogation rely on an assumption of good faith: that participants are interested in learning and have a shared interest in truth. It assumes that the participants do not have an intellectually compromising conflict of interest. When these assumptions are violated—when people use skepticism to undermine and discredit science rather than to revise and strengthen it, and to confuse audiences rather than to inform them—the entire process is disabled.[139] It can lead scientists to want to shut down criticism rather than embrace it. After all, it is challenging to maintain a spirit of openness in the face of dishonesty. The critics of science do not strengthen it, as they sometimes claim; they damage it.

For this and many other reasons, there is no guarantee that the methods of scientific scrutiny will operate as intended. In the next chapter, I examine historical examples where, in hindsight, we may say that scientists went astray. We will see what we may be able to learn from those examples as to when we are justified in not trusting science. But for the purposes of the present argument, the key point is this: We have an overall basis for trust in the processes of scientific investigation, based on the social character of scientific inquiry and the collective critical evaluation of knowledge claims. And this is why, ex ante, we are justified in accepting the results of scientific analysis by scientists as likely to be warranted.