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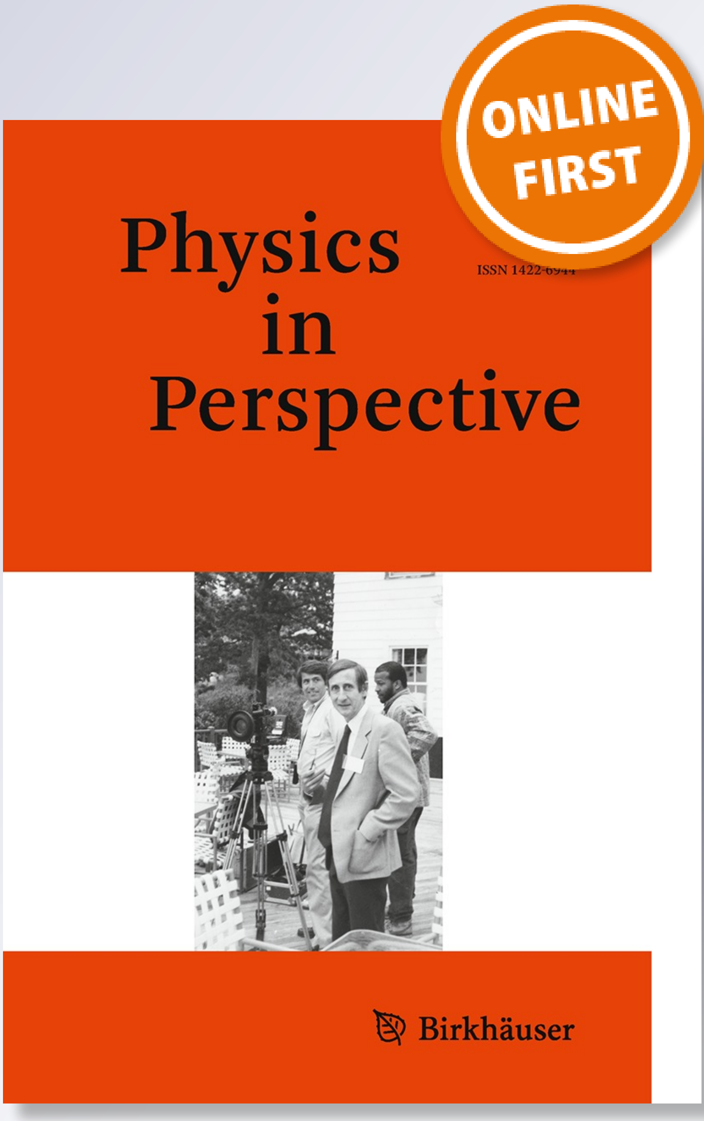

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Physics in Perspective

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# Politics, Morality, Innovation, and Misrepresentation in Physical Science and Technology

Jed Z. Buchwald*

The pressures of politics, the desire to be first in innovation, moral convictions, and the potential dangers of error are all factors that have long been at work in the history of science and technology. Every so often, the need to reach a result may require leaving out a few steps here and there. Historians think and argue best through stories, so what follows are several tales, each of which exemplifies one or more of these aspects, though some reach back nearly two hundred years. The first concerns the depletion of the ozone layer; the second involves the discovery of electric waves by Heinrich Hertz in 1888; the third concerns the controlled production of electromagnetic radiation by Guglielmo Marconi and John Ambrose Fleming in the early 1900s; the fourth portrays the circumstances surrounding Joseph von Fraunhofer's discovery and use of the spectral lines in the 1810s; our final case involves a bitter controversy between the physicist Hermann von Helmholtz and the astronomer Friedrich Zöllner in the 1890s.

*Key words:* Paul Crutzen; Michael Faraday; John Ambrose Fleming; Joseph Fraunhofer; Hermann von Helmholtz; John Herschel; Heinrich Hertz; Guglielmo Marconi; Mario Molina; Henry Rowland; Friedrich Zöllner.

## The Ozone Layer

In 1995 an MIT chemist by the name of Mario Molina shared the Nobel Prize in chemistry for his work on the atmospheric reactions that produce the depletion of the ozone layer.[1] Since they were first handed out at the beginning of the last century, the Nobel Prizes have been the gold standard by which scientists have judged success. When you win a Nobel, your work has been canonized by the priestly guardians of science, all of whom live in Sweden near the Valhalla of the old Nordic gods. With a few exceptions, one would not want to argue with the appropriateness of the awards handed out over the last century, though the losers might want to do so. One would certainly not gainsay the Prize awarded for the

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superb chemistry developed by Molina. But the research that led to the award bears discussion because it holds an interesting and timely lesson.

The background to this work goes back quite far, to 1879 in fact, when ozone in the upper atmosphere was first recognized. Soon thereafter, it was realized that ozone shields the surface of the Earth from ultraviolet radiation. When it was also discovered that ozone is substantially present only in an altitude range of fifteen to fifty kilometers, and that it exhibits patterns of annual variation, scientists began to investigate what sort of chemical processes might be involved. Then, in the 1930s, a new industrial development took place when it was found that chlorofluorocarbons were an ideal refrigerant—these CFCs, as they were soon widely called, were non-toxic and non-reactive, and so seemed to be perfectly suited for widespread use in the rapidly growing world of commercial refrigeration. Although scientists noted quite early that CFCs could easily accumulate in the stratosphere, at the time this raised no alarms. In fact, the first public discussion of the possibility that human activity might compromise the ozone layer occurred only in the 1960s and then did not involve the effects of CFCs, but rather of the high altitude flights of the proposed supersonic transport (SST). This was one element (though certainly not the only one) in the debates of the day that led the US Congress to stop funding for the SST.

This set the stage for the three events that led to Molina's Nobel, which he shared with Paul Crutzen and Sherwood Rowland: research in the early 1970s identified a sequence of chemical reactions by which CFCs could gradually deplete the ozone layer. Then, surprisingly late, in the mid-1980s the worsening development each spring since the mid-1970s of an ozone "hole" over the Antarctic was announced; this was followed by a highly organized effort to gain data that revealed, among other things, that the size of the Antarctic hole varied over the course of a year.

And here we come to a surprising and significant point. The chemistry that had been proposed for ozone depletion, and for which the Nobel was granted, requires ultraviolet radiation, and the various models that were built using this chemistry had predicted about a 5% general depletion of the layer near the equator by the 1980s. But the ozone hole occurred at the Antarctic. Not only is ultraviolet radiation comparatively small at the poles, the depletion effect there was far greater than the meager 5% predicted for the equator. Clearly something was up. Competing speculations were proposed by different groups of scientists. Then, as often happens, new experimental data altered the discussion. In the late 1980s, the Antarctic Airborne Ozone Experiment produced what amounted to "smoking gun" evidence that chlorofluorocarbons were the culprit, after which efforts focused on the detailed chemistry involved. In 1987, the Montreal Protocol for eliminating the use of CFCs was signed, but only in 1994 was a widely accepted chemical process that could explain the full depletion cycle evolved. The process of elaborating the chemistry continued thereafter.

Despite the failures of the original chemical system that had linked high atmospheric concentrations of CFCs to low concentrations of ozone, no chemist today thinks that the first theory was actually false; it was instead only incomplete, for the principal reactions singled out did remain part of the story. Moreover, the original chemistry had been instrumental in focusing the research community's attention on CFCs. This holds a lesson for scientific research that goes beyond the overly simple notion that a theory just makes predictions that either fit the data or do not, and that, if the fit is bad, then the theory must be tossed out. On the contrary, here we see that the fit between theory and experiment was not only bad, it was pretty much the reverse of what it should have been. Nevertheless, research scientists had no workable alternative but to continue pushing the original chemistry until it had been sufficiently fine-tuned to accommodate observation.

There are at least two lessons in this story, one scientific and the other political. The scientific lesson is this: good researchers are stubborn; they do not simply cave in when faced with discrepant data. The data may be complex, the connections between model and data even more so, so that when the foundations of a theory are otherwise firm it is not a good idea to discard it too quickly. This is all the more true when dealing with something like the atmosphere, for here scientists encounter the difficult question of whether large scales require different ways of thinking than do the small scales that had been dealt with until the middle of the twentieth century. After all, the atmosphere cannot be put into a tabletop device. But it can be modeled on a computer, and a very great deal of recent science, as well as engineering, is done with computer models either because of computational complexity, scale, or both. This trend raises important questions about just what the relation between a model and a natural system might be, and how discrepancies between data and computational outcomes should be dealt with, particularly when the system in question cannot actually be manipulated. Not only can we not yet play around with the atmosphere, one hopes that we would not want to do so even if we could, given the possibility of disastrous consequences, which brings us to the moral and political lesson of the ozone episode.

Morality and politics are not words that easily go together, and not because politicians are inherently untrustworthy, though most Americans think with good reason that they probably are. Politics is and always has been the art of persuading others to do what the politician wants done, and that for a complicated variety of reasons. Some of those reasons may even be moral, however unlikely that may seem. But most of them are, and must be, pragmatic, which does not mean that they are necessarily immoral. Pragmatism comes in at least two forms: the pragmatics of the moment, and the pragmatics of vision. What might be expedient and perhaps even useful in the short term may have unfortunate consequences as the years go by.

The ozone layer discussions that took place in the US House and Senate exhibit these characteristics. There was originally considerable talk that the strength of evidence supporting the connection between the ozone hole and CFCs was much too weak to support drastic action, which was probably true in the first few years. But data and theory were pushed further, and soon the connection between them became quite close—though certainly not of the same order as, say, the statement that if you let go of an apple it will fall to earth. Precisely because the shrinking ozone layer could have drastic ecological consequences, the connection did not need to be so tight as that in order to mandate a policy with vision. Because policy must be developed, decisions must be made in the light of a balance of risks against potential gains. Science can shed light on the likelihood of risk and gain, but not always with the unbreakable certainty that policymakers might wish because many questions call for decisions that cannot wait until the finished scientific scheme is in place. Standards of evidence when risk is central can and must be very different from standards of evidence in either comparatively finished science or science in the earliest stages.

These differing standards can be a serious source of continuing confusion in disputes over such matters as ozone depletion. Indeed, they clearly have been in respect to global warming, which, unlike the ozone debates, has not prompted sufficient action by a recalcitrant and scientifically ignorant US Congress, despite an overwhelming consensus among scientists—a consensus that is no less strong than the one that prompted action against CFCs. Those whose vested interests may be adversely affected by policy decisions made on the basis of this evidence can always invoke the standard of a perfectly finished science to argue for delay. All the while, the policy question is best viewed as a balance of risks against gains, given all currently available information.

## Hertz's Experiments on Electric Waves

Let us turn now to something rather different, something that will take us into the heart of the research enterprise as it was practiced over a century ago. Our story here has two dimensions: one concerns the character and meaning of what appear to be misrepresentation in science; the other concerns the thrill that comes with a new discovery. The two topics are, we shall see, not unconnected. Claims concerning scientific misrepresentation have, in the last two decades, become increasingly common and increasingly shrill, more often than not because government money—and thus politics—is involved. Depending on the circumstances, such a thing might even be considered fraud.

What, though, is fraud? Definitions can become rapidly, and legalistically, complicated, but an assertion that some claim is fraudulent presupposes at the least that a deliberate attempt has been made to lead others to believe something the defrauder knows to be untrue. But more than mere deception is necessary for a claim to be truly and properly fraudulent: the purpose of the deception must be venal; its goal must be to produce a significant benefit to the defrauder or to those for whom he is acting. Without this element of personal benefit, we are not dealing with true fraud but rather with something quite different, namely misrepresentation, and there can be many motives for that.

Fraud and its sibling, willful misrepresentation, have existed in every area of human endeavor that involves persuasion. Science is certainly no exception, since claims made by its practitioners are crafted to convince others. Those who persuade many Americans, apparently by the millions, that they will soon by transported rapturously to heaven in their family cars certainly do no less, this being a major theme of several very successful recent books sold widely in airport kiosks.[2] But there is a difference. The scientist knows that any attempt to persuade may come to shipwreck on the shoals of a future observation, experiment, or calculation, and that this does not lie altogether under his or her powers of persuasion. Whereas if the Rapture does not happen tomorrow, then the true believer will just wait for another day. Belief is utterly and essentially impervious to evidence. Therein lies a critical difference between those who foolishly attempt scientific fraud and those who merely misrepresent, because the power of evidence can eventually destroy a scientific fraud, whereas a misrepresentation may be designed to convey just how powerful the evidence for a particular claim may be.

Consider the case of the German theoretician and experimentalist, Heinrich Hertz (figure 1). Born in 1857, Hertz undertook the first series of experiments to demonstrate the existence of electric waves in the late 1880s. He began with wires. In those early years of telegraphy and telephony, the highest frequency that had yet been produced was about 15 kilohertz—of course the very word for frequency, hertz, refers to our discoverer. He was the first to produce waves in wires in the megahertz range, and to show how to detect and to control them. Along the way, he also discovered how to design a spark-switched oscillator, which was a device

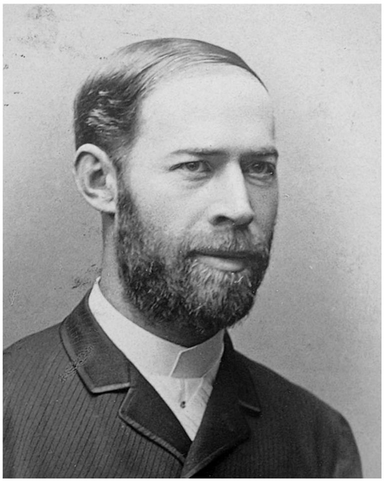

**Fig. 1.** Heinrich Hertz. Source: Wikimedia Commons.

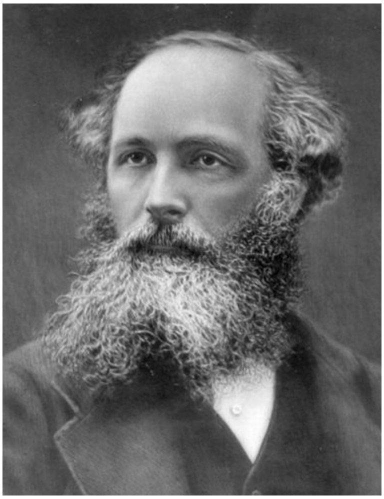

**Fig. 2.** James Clerk Maxwell. Source: Wikimedia Commons.

with an air gap that behaved just like a closed wire when the air broke down at sufficiently high potential. This discovery enabled Hertz to generate waves in air and to investigate their properties.

We will turn in a moment to how Hertz felt as he worked this golden vein of discovery in his laboratory, but first let us jump ahead to see how he presented his work to the scientific world, for here he faced a knotty problem. At that time—the 1880s—there were several competing theories for electromagnetism and only one of them, the British physicist James Clerk Maxwell's (figure 2), required the existence of radiation. But even that one, many leading British physicists of the day felt, did not lead to the possibility of artificial electromagnetic radiation, since most people thought that only processes at the molecular level would do so, and that they would generate only the ultrahigh frequency oscillations that constitute light. This was because no one, even in Britain among Maxwell's followers, had worked out the details of what would later become antenna theory—which in fact became one of Hertz's major theoretical contributions. So Hertz was faced with the very difficult problem of convincing essentially everyone, even the British, that he had actually managed to produce and to control something that none of them (though for different reasons) thought possible.

That conundrum led him into misrepresentation. In the months after his discovery, Hertz was asked by the editor of the premier physics journal of the day to write an account of what he had done. As he thought about how best to present a convincing narrative, Hertz decided to divide the story into three distinct parts, each one being principally concerned with a separate part of the apparatus. In the

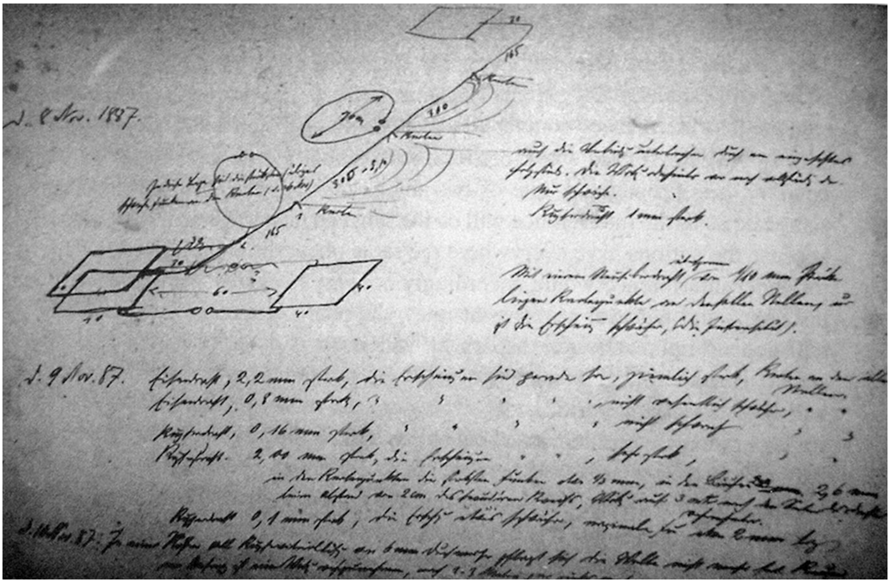

**Fig. 3.** A page from Hertz's laboratory notebook. Source: By permission of H. G. Hertz.

printed trilogy that resulted, Hertz claimed that he was relating the sequence of experiments and thoughts that resulted in his discovery.

Until fairly recently, we had only this printed work to go on. But some time ago Hertz's actual laboratory notebook was found in the possession of one of his descendants in Germany (figure 3). That notebook tells a very different story from the public account. It shows unequivocally that Hertz had considerably altered the true course of events in ways that made his path to discovery seem to be much more logical and linear than it was. Hertz had to work very hard to produce electric waves, and there were many more stumbling blocks along the way than he explained.

Were Hertz's actions fraudulent in any truly meaningful sense? Hardly. Hertz did not intend to mislead his readers in order to create in them a false sense of his experimental and logical abilities. That would indeed be fraud. But such was not Hertz's purpose, for he knew very well that his results were difficult to understand. In writing his trilogy, he deliberately chose to lead his readers step by step through unfamiliar territory, to guide them by the hand. This is not fraud; it is good pedagogy. No doubt Hertz might have remarked in a note that he was not literally recounting the paths he had followed, but that would only have raised questions about his results, deflecting attention from them to their production.

Here we have a situation in which the public misrepresentation of the actual course of events served the important purpose of getting results into scientists' hands rapidly and effectively. The tactic certainly did work, for laboratories in

Britain, France, Germany, Italy, and elsewhere rapidly began producing and probing the nature of electromagnetic radiation. Indeed, within scarcely a decade, Guglielmo Marconi in Italy, with the help and advice of the English engineer-scientist John Ambrose Fleming, was transmitting long-wave radiation across dozens of miles. Today, Hertz would perhaps have been more explicit about the actual sequence of events. But he might very well have written his trilogy in very much the same way, just warning the reader that this was not quite how the discovery happened. For that, he would have waited until his Nobel Prize address (unfortunately he died very young, in 1894, and so just missed the first awards, which came in 1901). We will return in a moment to this instance of rapid progression from a fundamental discovery to a new technology, but first let us rejoin Hertz in his laboratory, where we can learn a second lesson.

Fortunately, Hertz kept a diary and wrote his parents frequently about his life and work.[3] In the early winter of 1887, his experiments were going well, and he wrote home that he had had "good luck with my experiments, and though there were some mishaps … I have never before been on such fertile soil, prospects are opening right and left for new, interesting experiments."[4] But a few days later, things had begun to turn sour, because instead of finding proof that electric waves exist, he seemed to be discovering that they do not. And so, he later explained, "disheartened, I gave up experimenting."[5] But he did not stop for long. He returned to the bench, not as enthusiastic as before, but nevertheless determined to go ahead, because he now felt that disproving the existence of something would itself be very important. Not, of course, as exciting as finding something new that no one had ever seen before.

But, as Hertz continued to work, he played around with his device to make new sorts of measurements, trying to be utterly certain about his negative results. And when he did this, he suddenly obtained indications that waves do indeed exist. The old measurements, he decided, had been flawed by disturbing effects. Over the next weeks and months, he successfully tracked electric waves throughout his laboratory; he measured their polarization; he refracted them; in short, he became the first person to produce and to manipulate artificial electromagnetic radiation. He was mining a vein of pure gold and he wrote an arresting letter to his teacher and mentor, the great German polymath Hermann von Helmholtz at the University of Berlin, in the spring. This letter says much about what happens when a truly creative scientist moves beyond the pressures of competition, beyond the immediate cares and concerns of daily affairs, to glimpse something that no human ever had before. He wrote: "I now have the … feeling that I am … on my own ground and territory and almost certainly not competing in an anxious race and that I shall not suddenly read in the literature that someone else had done it all long ago. It is really at this point that the pleasure of research begins, when one is … alone with nature and no longer worries about human opinions, views and demands."[6]

The human world, with its anxiety-producing demands and pressures, disappeared for Hertz as he wandered at will through this unexplored land. Wonder and delight at the discovery of the utterly new have always been a hallmark of the finest creative science and engineering, and they remain so to this day. In 1995, two groups, one at the University of Colorado NIST-JLA laboratory, the other soon after at MIT, also produced an object that had never before existed, called a Bose-Einstein condensate. A graduate student at MIT named Marc-Olivier Mewes reflected on the moment when he first saw the condensate, that it was "one of those rare times in physics when you discover a really new effect. It makes you feel kind of strange," he continued, because "you're seeing something that nobody else has ever seen before."[7] That had been Hertz's own sentiment over a century before.

What lessons might be drawn from this? One is certainly this: however pressing the desire to beat the other person may be—and one would not wish at all to underplay the power of competition—nevertheless, when the truly creative scientist or engineer at last reaches deep into discovery, then the mundane fades into the background and it is the power and wonder of the unknown that carries him or her forward.

There is of course something rather inhuman, or perhaps it would be better to say, unhuman, about this. In most other areas of great creativity, the human world forms an essential part of the enterprise. Literature, after all, concerns human experience; artistic creation may have some of the elements that so gripped Hertz and Mewes, but the world that it produces is, if not unconstrained by nature, nevertheless not so directly entangled in an unknown reality. One does not think of a writer or of an artist as being "alone with nature" in quite the same way. Physically alone he or she may very well be, but the human world can never be altogether far from mind.

In that sense, the scientist really may live in an unhuman world, at least for a time, and this is perhaps one of the things that makes scientific work seem to be so very strange to many people. Even as citizens of technologically advanced countries live in a world increasingly designed by science and engineering, a world that is filled with devices and effects that have never existed before and that in ever more powerful ways mold daily life, belief in the irrational remains as widespread as it has ever been, perhaps even more so in the United States, where millions read their daily astrological charts and believe the earth to have been miraculously created some six thousand years ago. Why is this so? It is not solely because of the sorry state of science education in this country, though that is certainly a factor, and a very difficult one to overcome. The reasons run deeper, and are much more ancient, than mere educational failure.

The universe of magical belief governed the earliest civilizations in Egypt and Mesopotamia, where people believed unhesitatingly that mysterious, conscious forces governed human destiny, forces that required appropriate propitiation to avoid disaster. Yet these antique powers were not unhuman, inimical though they may have been to human destiny, precisely because they could be propitiated; they could be appealed to in ways that were not altogether different from human supplication. The world of modern science since the seventeenth century offers no such hope. We cannot hope to appease the gods because natural law offers no such mechanism. Nature no more listens to human desires than a hurricane attends to a grass hut. Nature is altogether, utterly indifferent to our existence; it is quite literally, and to many, terrifyingly unhuman. Our world is more than ever fabricated out of this apparently indifferent material reality; nature may not care whether humanity exists or not, but people do.

Fabrication is what humans really do best, for if anything is a hallmark of the human species it is the ability to engineer new worlds out of the natural environment, which brings us to another aspect of scientific discovery, its close link to technology. The connection between the two realms is inevitable, profound—and yet strained by a potential tension. Since its true beginnings in the seventeenth century, laboratory science has always used the craftsman's workshop, and eventually large-scale industry, for its instruments. The first air pump was produced by skilled English journeymen, whose names are lost to history; many of Galileo's devices were forged by Tuscan guildsmen; and Hertz's galvanometers and induction coils were built by German instrument makers. In a quite direct sense, laboratory science has always been closely linked to the artisanal world, and, moreover, artisans in the Renaissance and later often thought of themselves as engaged in work that revealed the secrets of nature. However, when we think today of the connection between science and technology, we envision something that does not evoke the guild or the craftsman, but rather applied science, whose usual image is this: a new effect or process is discovered in the laboratory as a result of investigations whose goals had little to do with the world of practical application; afterwards industry capitalizes on the discovery. On this view, science today produces, and industry consumes.

This has never been true, certainly not in such a bald form, and it is not true today. There are instances here and there that do look something like the cliché, but even there the interactions are much more complicated than it might seem. Moreover, there are nearly as many instances in which research driven by essentially practical motivations has led to significant scientific discoveries as the reverse. Let us take a brief look at two examples of past connections between science and technology.

## Marconi's and Fleming's Wireless Demonstration

The first takes us back to electromagnetic waves. By the early 1900s, Marconi and Fleming (figure 4) were working to produce a useable system that could transmit signals without wires. Here they faced a number of problems. First of all, though the idea of wireless communication arose quite rapidly after Hertz's discovery, two issues cropped up nearly at once. The first was distance: it seemed to everyone who

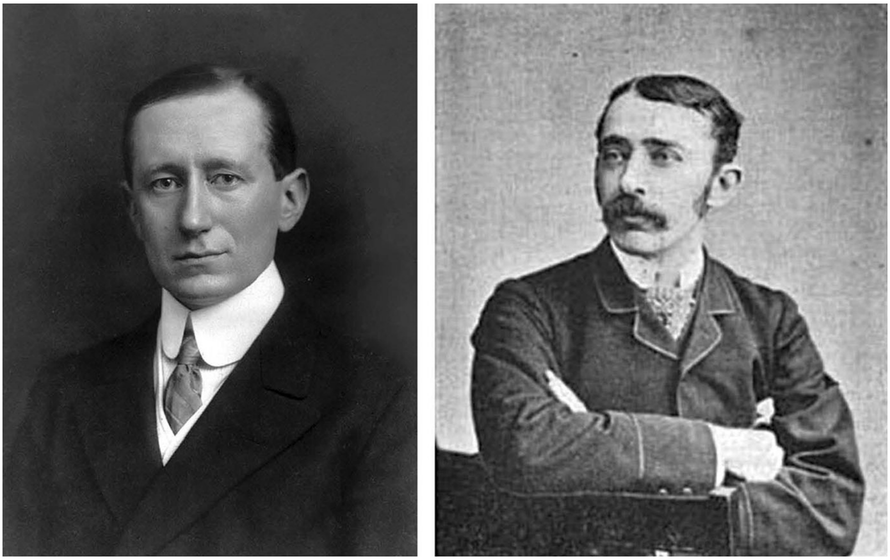

**Fig. 4.** Guglielmo Marconi (left) and John Ambrose Fleming. Source: Marconi (left) from Library of Congress; Fleming (right) from *Electrical World* **16** (1890), 467.

knew anything that reaching farther than line-of-sight to the horizon would be no more possible than seeing around the curvature of the earth. This seemed to limit wireless usefulness to ship-to-shore or ship-to-ship communication, where each could see the other. In other words, wireless might at best replace the old system of signaling by line-of-sight semaphore stations. But even this was problematic. Semaphore signaling had been used by militaries since the days of Napoleon, and it worked extremely well, albeit with the critical disadvantage for navies that it worked easily only over land. Still, where they could be used, semaphore signals did not interfere with one another; you just had to look in the right direction. Here, wireless was at a considerable disadvantage, because devices based on Hertz's original oscillator were heavily damped. As a result they had what would later be called a very wide bandwidth—which meant, though this was not well-understood at the time, that you could not separate one signal from another.

The Hertz oscillator, so useful for scientific discovery, was clearly useless for practical communication. Yet all but one of the competing systems in the early 1900s were based directly on Hertz's device. The sole exception was Marconi's. He had developed a method to narrow the bandwidth, in effect to tune the oscillator. This meant that messages sent at different frequencies did not swamp one another, and (of equal importance at the time) the ability to choose a specific frequency held out the hope of ensuring military secrecy and of communicating at sea. Only those who knew the right frequency could hear the message (at least until simple methods of receiver tuning were developed).

Marconi and Fleming held a public demonstration in London to exhibit the virtues of their system. It nearly failed because of one of the earliest instances of industrial sabotage. One of their rivals, knowing the time and location of the demonstration, swamped the delicate tuning of the Marconi system by transmitting a wide-bandwidth signal sent from a typical Hertzian oscillator. The transmission ceased just moments before the true message was sent from another Marconi device, which prevented public embarrassment, but only because the saboteur's timing was off. One might say, how unfair. But fairness was hardly the issue. The failed sabotage actually demonstrated something quite important. Namely, that Marconi's new system was a good one only in a world where Marconi-like systems excluded all others. If the world also had noisy, wide-bandwidth radiators, then Marconi's device would be utterly useless. What eventually happened was that the noisy devices were everywhere legislated out of existence, and only narrow-bandwidth oscillators were permitted. Since even in wartime the benefits of wireless communication were great for each side, no one thereafter attempted to block transmissions by swamping *all* the airwaves.

Here, we see just how complex the path from discovery to technology can be, even when it seems that the original discovery was quite complete, as Hertz's did indeed seem to be at the time. First of all, the very nature of Hertz's device blocked its exploitation for over a decade. But second, the eventual success that Marconi achieved was not simply fixing a problem with the original discovery. Not at all. Marconi had literally to forge an entirely new technological world in which only devices of his particular kind were allowed to exist. Far from just developing a new technology to fill an uncertain market niche, Marconi had to persuade governments, and through them industry, to build an exclusive realm uniquely controlled and defined by Marconi-like devices. Since in the first decade or so only Marconi knew how to build such things, his company had an extraordinary monopoly, one that only the First World War and rapidly growing competition eventually destroyed.

Even in this case, where it seemed that we had a nearly pure instance of the application of a new scientific discovery to industry, the situation was vastly more complicated, involving as it did major departures from the original science in direct connection with economics, society, and government. Let us turn next to a different example, from much earlier in the nineteenth century, where the situation seems rather the reverse, one in which new science emerged from new industry, to see what lessons we can find there.

## Fraunhofer's Glass

In the years after the final defeat of Napoleon in 1815, Britain thoroughly secured its dominance over the rapidly evolving industrial world. Its factories produced textiles, munitions, and soon other products, some of which had never existed before. British imperial dominance ensured markets for its products, with the notable exception of the former American colonies, which, in New England, were themselves rapidly industrializing, while the American South was mired in the horrors of slavery, with the inevitable consequence that the South remained a producer principally of raw products for the New England and British mills. Germany was not yet a unified country, consisting of three hundred different principalities linked to one another in a common customs union, prefiguring the European Common Market. Industrial plants were beginning to develop in some of these principalities, which however faced the competitive colossus of Great Britain.

In circumstances like these, the best tactics for success are not to challenge the colossus on well-established grounds unless one has truly significant competitive advantages in quality or price. Since labor in Germany was if anything more costly than in Britain, price competition seemed unlikely to succeed. Quality was another matter. Here a man named Joseph Fraunhofer decided to challenge Britain in an area of comparatively minor economic, but great strategic and scientific importance: the manufacture of the finest optical glass, in which Britain had for a century been predominant.

Glassmaking had always been much more of an art than a science; it depended on the skilled craftsmen's knowledge of the right temperatures, the right times, and ways to mix, what proportions to add in and when, and the appropriate cooling procedure. The chemical properties of glass mixes were almost entirely mysterious. Large-scale industrial production depended to a high degree on methods for controlling and replicating craft knowledge, but even so, in Britain glass foundries were beginning to look more like factories than like sites of traditional craftwork.

The young Joseph Fraunhofer (figure 5), who had the support of the Bavarian government through personal connections, decided to challenge England. To do so, he produced a unique combination of craftwork, industrial production, and scientific acumen. Fraunhofer took advantage of existing craft skills in Bavaria by building his foundry in a secularized monastery, which had been expropriated under Napoleon. The generous size of the location, the monastic tradition of dedicated craft labor, and the very layout of the monastery, which was congenial to the keeping of trade secrets, suited Fraunhofer's plans perfectly. The foundry he constructed bore little resemblance to contemporary British ones: it was much smaller, more conducive to craftsman-like control of the processes.

In this unusual environment, Fraunhofer and his workers rapidly developed new methods for producing extremely high-quality optical glass. But there was a problem: how could you convince buyers that this new glass was vastly better than any other—especially English—glass? There was no accurate, easily repeatable way to test optical quality at the time. The usual method was to examine the refraction of the glass, but one long-standing problem here was that all glass is dispersive, that is it produces a rainbow of colors from white light. Consequently, in order to specify quality, you had to decide just what part of the spectrum to measure, and this meant very high inaccuracies.

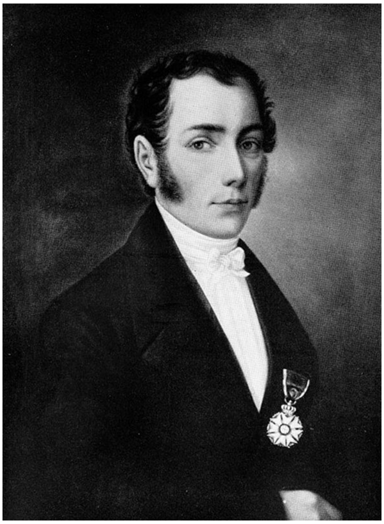

**Fig. 5.** Joseph Fraunhofer. Source: Wikimedia Commons.

It is exactly here that scientific discovery merged with new technology. In looking hard for ways to market his glass by examining its refraction at a very high level of detail, Fraunhofer discovered what were soon thereafter named the "Fraunhofer lines" in the solar spectrum. These were to become major sources of scientific interest during succeeding decades as physicists linked them to atomic processes. But this was not what Fraunhofer used them for, though he was pleased eventually to be accorded the status of a research scientist as well as a producer of very good glass. His goal had been to find ways to market the new glass, and the lines were exactly what he needed. What he did was to use the lines as markers in the spectrum, and to measure the refraction of the glass at each of them. That way he had a precise method for specifying the quality of his glass—and a way to force his English competitors to compete on his own grounds, for now they had to be able to produce and to measure the spectral lines just as Fraunhofer had. That alone was not an altogether easy thing to do until Fraunhofer himself invented the diffraction grating. Before then, anyone wanting to compete with Fraunhofer had to master the comparatively intricate technique that he himself described in print. If you could not do it properly, then you could not compete. Much like Marconi many decades later, Fraunhofer created his own technical world by forcing everyone else to use the very procedures he had himself invented in order to compete.

And compete they did, or at least they tried. English scientists visited Fraunhofer's factory, as much of it as he allowed them to see. They took back to England specimens of his optical glass, and then they tried to reverse-engineer it by chemically analyzing its composition. The finest English chemist of the day, who was soon to make fundamental discoveries in electromagnetism, Michael

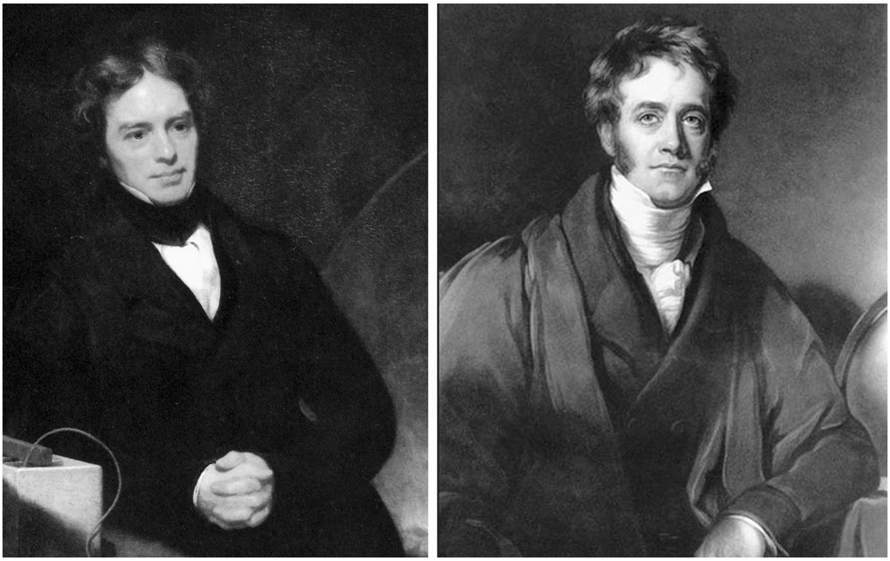

**Fig. 6.** Michael Faraday (left) and John Herschel. Source: Faraday (left) by Thomas Phillips from Wikimedia Commons; Herschel (right) by Alfred Edward Chalon from Wikimedia Commons.

Faraday, tried hard to understand Fraunhofer's glass, and he was advised by England's greatest optical scientist, John Herschel (figure 6). Unfortunately for the English, they failed utterly. Fraunhofer's clever use of craftsmen's knowledge, which he refused to divulge, preserved his control of optical glass, which remained for a century and a half a German specialty and near monopoly.

The discovery of the spectral lines, then, originated in Fraunhofer's desire to compete in a market controlled by the English. Yet as Fraunhofer continued his work, he became profoundly involved in new scientific research that did not have clear industrial application; he was among the first to probe the implications of the new wave theory of light for instruments, for this was how he came to invent the diffraction grating. In Fraunhofer's world, the links between new science and new technology were so tight that it is probably pointless to distinguish between the two areas.

We have looked at several past technologies and sciences, at their mutual connections, and at some of the motivations and work that went into producing them. We will close with a different kind of story, one of science and moral virtue. This tale takes us back to Germany in the third quarter of the nineteenth century, shortly after the region's many principalities had been unified. Berlin had become the directing center of the new country, its university the most powerful and productive one of all. German science had by this time begun to surpass its British and French competitors at nearly every level; German training methods, based on the apprenticeship of the graduate student to a doctoral supervisor, had become an engine producing highly competitive, intensely committed researchers. In fact, by the end of the century this German system became the model for university training in the United States and remains with us to this day.

## Helmholtz's Cosmopolitanism

The new rector of the University of Berlin was Hermann von Helmholtz (figure 7), under whom the young Hertz would shortly apprentice. Helmholtz was one of the discoverers of the principle of energy conservation; he had created the first mathematics for fluid vorticity, had perfected the trichromatic theory of color vision, and had completely renovated the study of sound. At just about the time that he became rector at Berlin, he had also developed an entirely new form of electrodynamics, which he taught to Hertz. Any one of these achievements would likely have won him a Nobel Prize today, though, like Hertz, he died several years before the prize was created.

Decidedly Germanic in many ways, Helmholtz was thoroughly cosmopolitan when it came to science. He was an internationalist and, more than that, had a profound belief in the virtues of free investigation, unconstrained by ideologies or religious beliefs. Several years before his rectorship, Helmholtz had translated into German an extraordinary textbook of the day, the *Treatise on Natural Philosophy*, which had been co-authored by one of the most famous Scottish scientists of the time, his close friend William Thomson, who became Lord Kelvin, and Peter

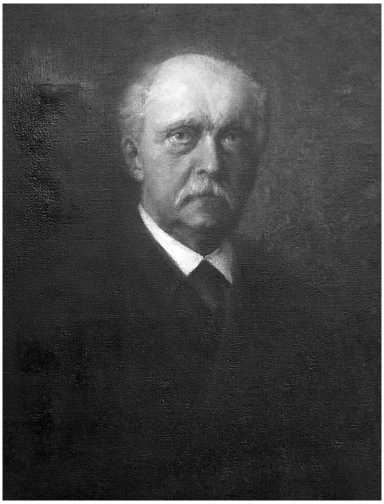

**Fig. 7.** Portrait of Hermann von Helmholtz by Hans Schadow. Source: Wikimedia Commons.

Guthrie Tait. This admiration for a foreign product did not sit well with a number of Helmholtz's German colleagues, who were increasingly steeped in the poisonous atmosphere of xenophobia that would eventually send Germany, and very nearly the world, to utter destruction.

Among these colleagues was a scientist by the name of Friedrich Zöllner, who had not long before invented the first photometer that could be used to produce reliable values for stellar magnitudes. Zöllner accused Helmholtz of propagating un-Germanic science because of his dealings with and friendship for the British, in particular Thomson, whom Zöllner thought to be addicted to the crudest of materialistic beliefs, whereas he, Zöllner (and he was hardly alone in this), was certain that the world was guided by a uniquely Germanic spirit. "Judging from what [Zöllner] aims at as his ultimate object," Helmholtz scornfully remarked,

> it comes to the same thing as [the philosopher Arthur] Schopenhauer's Metaphysics. The stars are to "love and hate one another, feel pleasure and displease, and to try to move in a way corresponding to their feelings." Indeed, in blurred imitation of the principle of Least Action, Schopenhauer's Pessimism, which declares the world to be indeed the best of possible worlds, but worse than none at all, is formulated as an ostensibly generally applicable principle of the smallest amount of discomfort, and this is proclaimed as the highest law of the world, living as well as lifeless.[8]

So much for Zöllner's metaphysically based science. But that was not the only target of Helmholtz's disdain. From the point of view of someone like Zöllner, no one should pursue science except according to Germanic ideological principles. He was a vocal and influential enemy of academic freedom, on whose remarks the Nazis would draw decades later. This disgusted and dismayed Helmholtz. He replied in his inaugural address as rector at Berlin on that very topic. Helmholtz praised the great freedom of the German university, where "the most extreme consequences of materialistic metaphysics, the boldest speculations upon the basis of Darwin's theory of evolution, may be taught with as little restraint as the most extreme" pursuit of religious belief. Where, he continued, though "it is forbidden to suspect motives or indulge in abuse of the personal qualities of our opponents, nevertheless there is no obstacle to the discussion of a scientific question in a scientific spirit."[9]

The free pursuit of scientific research was for Helmholtz a model for intellectual freedom, and a model as well for a tolerant and a moral society. Germany abandoned that in the 1930s, when biology was deformed by racism and when it became expedient to think that there was such a thing as "German physics." The statue honouring Heinrich Hertz at the University of Karlsruhe, where he had first produced electric waves, was thrown out because he had Jewish ancestry, while Hertz's own assistant at Bonn, Philip Lenard, who had won the Nobel Prize in 1905 for his work on cathode rays, denounced his mentor and extolled the virtues of a purified Germanic physics.

The United States welcomed many of those who fled Nazi Germany, though it was not welcoming enough. Science and scholarship in the United States achieved their heights as a direct result of this forced European flight, heights that might never have otherwise been reached. In the decades since World War II, science and technology in this country have benefited in extraordinary ways because so many from around the world have come to study and often to stay here. Let us hope that the present climate of wariness, fear, intolerance and, not least, Congressional disdain for scientific and humanistic truth dissipates and that the freedom which Helmholtz so admired will in time be embraced even by those who would silence their opponents.

## References

[1] Each of the examples considered here derives from the following publications, where full details can be found. On the ozone layer: Jed Z. Buchwald and George Smith, "*The Ozone Layer* (review)," *American Scientist* **89** (2001), 546–49. On Hertz: Jed Z. Buchwald, *The Creation of Scientific Effects: Heinrich Hertz and Electric Waves* (Chicago: University of Chicago Press, 1994) and Manuel G. Doncel, "On the Process of Hertz's Conversion to Hertzian Waves," *Archive for History of Exact Sciences* **43** (1991), 1–27. On Fraunhofer: Myles W. Jackson, *Spectrum of Belief: Joseph von Fraunhofer and the Craft of Precision Optics* (Cambridge, MA: MIT Press, 2000). On Marconi and Fleming: Sungook Hong, *Wireless: From Marconi's Black-Box to the Audion* (Cambridge, MA: MIT Press, 2001). On Helmholtz: Jed Z. Buchwald, "Helmholtz's Electrodynamics in Context: Object States, Laboratory Practice and Anti–Idealism," in *Hermann von Helmholtz and the Foundations of Nineteenth-Century Science*, ed. David Cahan (Berkeley: University of California Press, 1993), 334–73.

[2] To wit, the egregious, apocalyptic *Left Behind* series by the Christian dispensationalist and John Birch Society member Tim LaHaye, co-authored with Jerry B. Jenkins.

[3] Johanna Hertz, ed., *Heinrich Hertz: Memoirs, Letters, Diaries*, 2nd enl. ed., trans. L. Brinner, M. Hertz, and C. Susskind (San Francisco: San Francisco Press, 1977).

[4] Ibid., 237.

[5] Heinrich Hertz, *Electric Waves, being Researches on the Propagation of Electric Action with Finite Velocity through Space*, trans. D. E. Jones (1893; New York: Dover, 1963), 8.

[6] Hertz, *Memoirs* (ref. 3), 255.

[7] Quoted in Jed Z. Buchwald, "Reflections on Hertz and the Hertzian Dipole," in *Heinrich Hertz: Classical Physicist, Modern Philosopher*, ed. D. Baird, R. I. Hughes, and Alfred Nordmann (Dordrecht: Springer, 1998), 269–280, on 269.

[8] H. Helmholtz, "Helmholtz on the Use and Abuse of the Deductive Method in Physical Science," trans. Crum Brown*, Nature* **11** (1874), 149–51.

[9] Quoted in Buchwald, "Helmholtz's Electrodynamics" (ref. 1), 372.

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