UNCERTAINTY MEDICINE: THE DEVELOPMENT OF RADIATION THERAPY, 1895-1925

A Dissertation

Presented to

The Faculty of the Department of History

University of Houston

In Partial Fulfillment

Of the Requirements for the Degree of

Doctor of Philosophy

By

Jeffrey C. Womack

December, 2016

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ABSTRACT

This dissertation offers an overview of the development of x-ray and radium therapy in both the United States and Great Britain in the period from 1896 to 1925. Specific attention is paid to the early work of pioneering radiation therapists, many of whom had not completed a traditional medical education, and to debates within the field over the safety and efficacy of radiation therapy. The project chronicles early experiments with x-ray and radium treatment, and it situates human experimentation within the ethical paradigm of the period—the so-called "Golden Rule"—as well as examining debates amongst radiation therapists over issues of professional identity. X-ray and radium apparatus receive extensive treatment, as does the changing technology of radium production, the market in radon-infused water, and the debate over dosimetry and radiation exposure. Radiation therapy was an interesting example of a new technology that presented both powerful therapeutic potential and significant risks for patients. Unfortunately, early radiation therapists were often guilty of downplaying both the dangers of exposure and the shortcomings of their equipment. That refusal to recognize danger led to tremendous human suffering for both therapists and patients.

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Beth & Bede,

Joe & Jimmy,

and to Marty.

Thanks.

*"What a goodly company are these our pioneers, who have sacrificed their lives to the Baal-fires of the Roentgen rays! They have joined the band of the immortals…who have lost their lives for the progress of the race. These are not the martyrs, but the gladiators of science. All honour to them and to their survivors, from whose lips we almost seem to hear the proud salutation, '*Moriturі te salutant.*'"*

-W. Deane Butcher (ed.), *Archives of the Roentgen Ray,* 1913

INTRODUCTION – THE PROBLEM OF UNCERTAINTY

The key fact of radiation therapy—the truth overhanging the entire field—was that no one knew how or why it worked. Though sometimes portrayed as merely serendipitous, the discovery of new sources of ionizing radiation in 1895 and 1896 was actually the logical, and probably inevitable, result of ongoing research into cathode rays and other electrical phenomena. This research was made possible by new developments in the technology of vacuum tubes and increasingly powerful electrical equipment. The application of radiation for therapeutic ends also represented a natural progression; individuals working with radiation could not help but notice that it affected their bodies, causing burns and other physiological results. The observation of results, however, did not imply understanding. An active cathode ray tube or a sample of uranium pitchblende would expose a photographic plate. X-ray emitters and radium samples caused skin reactions. To understand *why* these things happened, however, would require observers to develop new knowledge about the world around them.

This study examines the development of radium and x-ray therapy in the thirty-year period following Wilhelm Conrad Röntgen's discovery of x-rays, in 1895. It argues that the best way to understand the behavior of therapists, the progression of the technology, and the development of radiation therapy as a medical profession is to think about how individuals working with radiation responded to uncertainty: uncertainty about the technology and how it worked, uncertainty about the causes of disease, uncertainty about the professional structure of medicine, uncertainty about the availability of materials, and even the day-to-day uncertainty of running a small business. In some cases, the new treatments developed by early radiation therapists represented a triumph over uncertainty, but the high price of those victories was measured in the lives of both therapists and patients. Moreover, much of that suffering was both predictable and unnecessary. People got hurt because many radiation workers became too comfortable with uncertainty, accepting unnecessary risks and allowing optimism to blind them to the evidence of danger.

Historians on X-rays and Radium

In the hundred years following the initial discovery of x-rays, historical treatments of x-rays and radium remained somewhat sparse. Both Röntgen and the Curies attracted a few biographical treatments (especially the latter, given Marie Curie's lasting fame).[1] Marie Curie produced the first of these herself, publishing a biography of her late husband in 1923.[2] The x-ray and radium discovery narratives became a sort of parable in the history of science, told and retold with an emphasis on serendipity—so fortunate that Röntgen noticed the glowing screen!—or scientific grit—the Curies' shack had no heat in the winter!—or as a cautionary tale, as with all of the accounts of the "Radium Girls," whose exposure to radium paint led to gruesome and untimely deaths.[3] The subject of radiation in medicine, specifically, attracted an occasional treatment, mostly from people within the field, such as health physicist Richard Mould. Short retrospectives or founder bios occasionally appeared in *JAMA*, the *BMJ*, or the various radiology journals.[4]

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[1] W. Robert Nitske, *The Life of Wilhelm Conrad Röntgen, Discoverer of the X Ray* (Tucson: University of Arizona Press, 1971); Denis Brian, *The Curies: A Biography of the Most Controversial Family in Science* (Hoboken, N.J: J. Wiley, 2005); Barbara Goldsmith, *Obsessive Genius: The Inner World of Marie Curie (Great Discoveries)* (W. W. Norton & Company, 2011).
[2] Marie Curie, *Pierre Curie* (New York: The Macmillan Co, 1923).
[3] See, for example, Alan Ralph Bleich, *The Story of X-Rays from Röntgen to Isotopes* (New York, NY: Dover Publications, Inc., 1960); Royston M. Roberts and John H. Lienhard, *Serendipity: Accidental Discoveries in Science*, Wiley Science Editions (New York: Wiley, 1989).
[4] Percy Brown, *American Martyrs to Science through the Roentgen Rays* (Springfield, Ill: Thomas, 1936); Émil H. Grubbé, *X-Ray Treatment: Its Origin, Birth, and Early History* (St. Paul: Bruce Publishing, 1949); Russell J. Reynolds, "The Early History of Radiology in Britain," *Clinical Radiology* 12, no. 2 (April 1961): 136–42; E. Posner, "Reception Of Röntgen's Discovery In Britain And U.S.A.," *The British Medical Journal* 4, no. 5731 (November 7, 1970): 357–60; Richard F Mould, *A History of X-Rays and Radium: With a Chapter on Radiation Units, 1895-1937* (Sutton [London, England]: IPC Building & Contract Journals Ltd., 1980); Macklis RM, "Radithor and the Era of Mild Radium Therapy," *JAMA* 264, no. 5 (August 1, 1990): 614–18; more recently, Henry N. Wagner Jr., *A Personal History of Nuclear Medicine* (Springer-Verlag London Limited, 2006).

Beginning with the centennial of Röntgen's discovery, however, radiation technologies, including x-ray emitters and radium products, have received a recent burst of scholarly attention. Joel Howell did an extensive dive into early x-ray records at hospitals in Philadelphia and New York and made x-ray technology central to two chapters of his book, *Technology in the Hospital*. Howell focused on the actual use—or non-use—of x-ray emitters by both hospitals and physicians, showing how they fit into much larger systems, like hospital bureaucracies and codes of propriety.[5] Historians of technology, like Robert Arns and Arne Hessenbruch, have investigated how the operation and development of the devices themselves fit within a broader human context of x-ray workers, patent wars, commercial preferences, and the like.[6] In general, however, historians have focused either on x-rays as a diagnostic tool or on the social and cultural repercussions of x-ray imagery, as suggested by the title of Bettyann Kevles's excellent book, *Naked to the Bone*.[7] Matt Lavine included some material on x-ray therapy in his recent work, but Lavine focused his attention more on the patient experience than on the therapists themselves.[8] When x-ray therapists have come in for scrutiny as a group, it has mostly been with regard to the wave of cancer that wiped out many of the pioneers in the field, as chronicled by both Goldberg and Herzig.[9] Rebecca Herzig also counted the cost of x-ray experimentation

[5] Joel D. Howell, *Technology in the Hospital: Transforming Patient Care in the Early Twentieth Century* (Baltimore, Md: Johns Hopkins University Press, 1995).

[6] Robert G. Arns, "The High-Vacuum X-Ray Tube: Technological Change in Social Context," *Technology and Culture* 38, no. 4 (October 1997): 852–90; Arne Hessenbruch, "Calibration and Work in the X-Ray Economy, 1896-1928," *Social Studies of Science* 30, no. 3 (June 1, 2000): 397–420.

[7] Bettyann Kevles, *Naked to the Bone: Medical Imaging in the Twentieth Century*, The Sloan Technology Series. (New Brunswick, N.J.: Rutgers University Press, 1997).

[8] Matthew Lavine, "The Early Clinical X-Ray in the United States: Patient Experiences and Public Perceptions," *Journal of the History of Medicine and Allied Sciences* 67, no. 4 (2012): 587–625; Matthew Lavine, *The First Atomic Age: Scientists, Radiations, and the American Public, 1895-1945*, Palgrave Studies in the History of Science and Technology (New York, NY: Palgrave Macmillan, 2013).

[9] Rebecca Herzig, "In the Name of Science: Suffering, Sacrifice, and the Formation of American Roentgenology," *American Quarterly* 53, no. 4 (2001): 563–89; Daniel S. Goldberg, "Suffering and Death among Early American Roentgenologists: The Power of Remotely Anatomizing the Living Body in Fin de Siècle America," *Bulletin of the History of Medicine* 85, no. 1 (2011): 1–28.

for patients, and especially for women, many of whom received x-ray exposure for cosmetic treatments, like hair removal.[10]

The risks and costs of radiation exposure have also dominated recent work on radium. Although not directly focused on medical products, both Mullner's *Deadly Glow* and Clark's *Radium Girls* offer useful insights into the industrial production of radium and radium products. As Clark's title suggests, women in particular had an important role in the radium industry, both on the research side and in the industrial production of the substance. Maria Rentetzi has done admirable work to uncover that story, and the research done for this project suggests that there is more to tell.[11] The historian of the Nuclear Regulatory Commission, Sam Walker, explored the development of and controversy around human dosing recommendations in *Permissible Dose*, published in 2000.[12] In 2005, two new historical treatments of radium discussed the development of radium therapy as a cancer treatment. Harvie's *Deadly Sunshine* offered a useful exploration of radium as an industry—mining, production, and so on. Hayter's *Element of Hope* offered a case study of cancer treatment in Canada, which, like the United States, had a productive radium mining operation in the early part of the century.[13] More recently, Angela Creager explored the story of diagnostic radioisotopes in the Cold War, and Luis Campos examined the larger influence of radium and radiation enthusiasm on other branches of science,

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[10] Rebecca Herzig, "Removing Roots: 'North American Hiroshima Maidens' and the X Ray," *Technology and Culture* 40, no. 4 (October 1, 1999): 723–45; Herzig, "In the Name of Science: Suffering, Sacrifice, and the Formation of American Roentgenology."

[11] Maria Rentetzi, "Gender, Politics, and Radioactivity Research in Interwar Vienna: The Case of the Institute for Radium Research," *Isis* 95, no. 3 (2004): 359–93; Maria Rentetzi, *Trafficking Materials and Gendered Experimental Practices: Radium Research in Early 20th Century Vienna*, Gutenberg-E (Columbia University Press, 2007), http://www.gutenberg-e.org/rentetzi/index.html; Maria Rentetzi, "The U.S. Radium Industry: Industrial In-House Research and the Commercialization of Science," *Minerva: A Review of Science, Learning & Policy* 46, no. 4 (October 2008): 437–62.

[12] J. Samuel Walker, *Permissible Dose: A History of Radiation Protection in the Twentieth Century* (Berkeley, Calif.; Washington, D.C.: University of California Press; Nuclear Regulatory Commission, 2000).

[13] David Harvie, *Deadly Sunshine : The History and Fatal Legacy of Radium* (Stroud, Gloucestershire: Tempus, 2005); Charles Hayter, *Element of Hope : Radium and the Response to Cancer in Canada, 1900-1940* (Montreal, QC, CAN: McGill-Queen's University Press, 2005).

like biology and physics, where some researchers looked to radium as a possible answer to questions about the deepest mysteries of the world, such as the origins of life.[14] At the other end of the spectrum, Martin V. Melosi's *Atomic Age America* offers a general overview of the development of nuclear technologies in the United States, built on a discussion of the changing nature of atomic theory and physics.[15]

There remains a gap in the literature, however, specifically around the early development of x-ray emitters and radium salts as therapeutic tools. It is necessarily a topic with a broad scope. Besides the tools themselves, the historical narrative of radiation therapy encompasses a large and diverse group of people. The term "radiation therapist," used throughout this study, encompasses doctors who sought to treat patients with radium salts or x-ray exposure, but the relative lack of regulation in 1896 meant that individuals with incomplete or no medical education could also open clinics devoted to x-ray therapy or sell tonics containing radium, and many of the early radiation therapists were inventors, pharmacists, medical students, entrepreneurs, or outright quacks. The process of trial and error, often on patients, that made possible the development of x-ray and radium therapy also holds an important place in this historical narrative, and the story as a whole raises interesting ethical questions not only for historians, but for the present day practice of medicine.

Ironically, with regard to this project, the lack of discussion around therapy reflects the tremendous value of radiation technologies in *reducing* uncertainty; diagnostic applications have been at the center of much of the recent scholarship. But therapy deserves attention, too. As a case study, the radiation therapy narrative opens a window into the process by which new technologies get transformed into therapeutic tools. That no one understood precisely how

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[14] Angela Creager, *Life Atomic: A History of Radioisotopes in Science and Medicine* (Chicago: University of Chicago Press, 2013); Luis A. Campos, *Radium and the Secret of Life* (Chicago: The University of Chicago Press, 2015).
[15] Martin V. Melosi, *Atomic Age America* (Boston: Pearson, 2013).

radiation worked, or how it would affect human bodies, makes this story exemplary, rather than extraordinary.

Whether attempting to build neuromechanical interfaces for replacement limbs or trying to use genetic manipulation for therapeutic purposes, today's physicians and medical researchers still live with uncertainty and still face many of the same temptations and risks confronted by their predecessors. Ioanna Semendeferi, in her work on ethics education for scientists, argues that history can allow modern-day scientists and doctors to "[put] their feet in the protagonists' shoes," in a way that makes it possible to experience "what it means to be unethical" and to recognize "some underlying commonalities in human behaviors"—i.e., the possibility that modern therapeutic experimenters could replicate the errors of the early x-ray and radium therapists.[16] Semendeferi's point spoke specifically to film, and the power of images and music to convey a powerful impression of the past, but I believe that narratives, like this one, can aim for a similar end. The key, as in this study, is to tell the story not only by addressing the structural factors that motivated experimental work, but to focus on the individuals themselves: men and women like Emil Grubbé, Margaret Sharpe, and Lester Leonard, who lived and died, who treated people, and harmed them, and who left behind accounts of their struggle to understand the new technologies of radiation.

Progress without progress

For physicists and chemists working with x-ray emitters and radium, the two decades that followed the discoveries of Röntgen and Becquerel would prove enormously rewarding, as scientists on both sides of the Atlantic began to answer the questions raised by their observations. In some ways, radiation and radioactivity represented a challenge to basic ideas

[16] Ioanna Semendeferi, "Feelings and Ethics Education: The Film Dear Scientists," *Journal of Microbiology & Biology Education* 15, no. 2 (December 15, 2014): 100–102.

about the world. Radioactivity, for example, fundamentally refuted the notion of atoms as indivisible and unchanging, making real, instead, the old alchemical dream of transmutation.[17] Nevertheless, the work that followed in these fields made steady progress at answering the basic questions raised by the discoveries.

In biology, by contrast, the story proved more complicated. For biologists, x-rays and radioactivity would push some researchers in fruitful new directions, especially with regard to their attempt to understand the origins of life. But the phenomena would also become fodder for the latest instantiation of anti-Darwinism, breathing new life into movements that attempted to describe the study of life in exclusively supernatural terms. Biologists had some sense that the energy output of radiation had interesting effects, but they did not yet have a paradigm for understanding those effects, although Hugo de Vries would posit the eventual way forward—radiation as mutagen, and therefore, as a source of cellular damage—as early as 1904.[18]

For medical users the experimental application of radiation to human bodies only exacerbated the problems created by their lack of a biological paradigm, and the process of knowledge creation seemed almost to go in the wrong direction: as users accrued more experience with x-ray emitters and radioactive substances, they often seemed to know less—a sort of progress without progress. The results of radiation exposure differ greatly when observed across different kinds of tissues, cells, and organisms, in part because the types of radiation generated by a particular source can differ substantially in their characteristics and

[17] Frederick Soddy and Ernest Rutherford, publishing the results of their pioneering experiments with radioactive decay in radium, ultimately settled on describing the process specifically as "transformation." Soddy attributed the linguistic choice specifically to Rutherford, recalling the latter as saying, "For Mike's sake, Soddy, don't call it *transmutation*. They'll have our heads off as alchemists." Campos, *Radium and the Secret of Life*, 13–15.

[18] Ibid., 119. At, fittingly, the dedication of the Department of Genetics of the Carnegie Institution at Cold Spring Harbor, New York, de Vries explicitly suggested that "the rays of Röntgen and Curie" might allow humans to take control of the evolutionary process for specific ends. For an extensive discussion of John Burke's origin-of-life experiments with radium, see Campos, Chapter 2 (pp. 56-99).

effects. Moreover, it soon became clear that radiation exposure can have longer lasting effects, even after the initial symptoms fade. For practitioners experimenting with radiation therapy the basic questions of their field remained unanswered from year to year, even as new uncertainties and variables proliferated in a seemingly endless train. On April 18, 1901, at a meeting of the Roentgen Society of London, therapist Margaret Sharpe summed it up thusly:

> "In November, 1899, I first had the honor of addressing you on the subject of x-ray therapeutics. At that time we were indebted for most of our knowledge of the subject to the labors and writings of our colleagues on the Continent, and our own practical experience was of the smallest. Now things are very different: x-ray departments have sprung up at many of our large hospitals, both general and special, and a few of our skin specialists have introduced the treatment in their consulting-rooms. Cases we have had in plenty . . . but are we still open to the reproach, so abhorrent to the British medical mind, of dealing with an unknown, or, at best, a but little understood force? Are we agreed as to the nature of the force, or the nature of its action, or as to whether it is simple or complex, one or many? . . . I myself have lately arrived at some very definite conclusions on most of these points; how long they will hold out I don't know, perhaps not after tonight."[19]

Of course, physicists, chemists, and biologists also faced frustrations and new problems raised by work in the area of radiation. For medical users, however, the huge gaps in their understanding of the relationship between radiation and living systems created particularly pressing problems. Therapists worked with radiation not in an effort to understand how the world works, but rather in an effort to treat sick patients. Therapeutic users needed to understand ionizing radiation in a way that would allow them to predict its effect on a particular disease, suggest the most effective methodologies for particular circumstances, and make possible the maximum benefit while minimizing the risk of further injury to the patient.

But five years—and ten—passed, and still the key fact of radiation therapy remained: no one knew precisely why, or how, it worked.

The responses of radiation therapists to this uncertainty ran the gamut. Some practitioners attempted to address the problem through measurement and quantification.

[19] Margaret M. Sharpe, "X-Ray Therapeutics," *Archives of the Roentgen Ray* V, no. 4 (May 1901): 85.

Many developed intricate theories to explain the results and phenomena that they had observed while working with x-rays or radium. Some therapists flatly denied elements of the problem, claiming, for example, that x-rays did not actually cause harm to patients. Many ignored elements of the problem, like the fluctuating air pressure of their emitters, for reasons of cost or difficulty. For therapists worried about the possibility of exposing their patients to unknown risks, one of the most common responses to uncertainty was for the therapist to expose him or herself to the treatment—a kind of gentleman's bargain to excuse risk so long as it was equally endured by both parties. Practitioners also tried to push back against uncertainty in the same fashion as their colleagues in other disciplines: by actively sharing knowledge, through mechanisms like societies, conferences, and journal publications.

Radiation therapy as case study

Uncertainty in treatment is an unavoidable problem in medicine. Human bodies are ferociously complex, responsive systems whose functioning relies on mechanisms at every level from the macroscopic and mechanical, such as tendons anchoring muscles to bones, and air moving through passageways into the lungs, to the atomic, where mitochondria harvest energy for cellular activity by shifting electrons and protons in the Krebs citric acid cycle. It naturally follows that influences at any level of the biological apparatus can lead to unpredicted changes, and problems, in other parts of the system. As a result, new therapies almost always imply a degree of uncertainty and risk—a fact repeatedly demonstrated by the recall of new drugs or changes in the recommendations made by professional medical organizations. These problems were not new in 1896. At the November 1, 1900 meeting of the Röntgen Society, then-president John Macintyre pointed out that if therapists could not live with such uncertainties, "we might despair of a complete scientific basis for much therapeutic research. Take any sample drug we are in the habit of administering. How much do we know of how it acts? We know it largely by its results, but whether taken into the alimentary canal or injected into the circulation, it must act in some subtle, but often unknown way."[20]

The core of uncertainty at the heart of medical treatment makes the study of radiation therapy both interesting and useful. The response of users to uncertainty in x-ray therapy suggests useful questions to ask both about other historical episodes, like the deployment of dialysis machines and of retroviral drugs, and about current and future therapeutic technologies, such as stem-cell therapy, that promise major breakthroughs for previously-untreatable diseases, but where the mechanisms of action and the possible consequences or side-effects remain poorly understood. Several other elements make radiation therapy a particularly useful site for historical inquiry. Both its timing and the professional record that developed around radiation make this a particularly interesting moment in the history of medicine and technology. Radiation therapy persists into the present, in some ways relatively unchanged, and its use as an anti-cancer agent, both then and now, dramatically sharpens the ethical questions faced by therapists. Moreover, the risks and problems associated with radiation therapy can be mitigated with better technology, but they cannot be "solved"; x-ray therapists and cancer patients today still contend with some of the same uncertainties that users faced a century ago.

Of course, the value of this case study is also determined by its accessibility. Fortunately, the people involved in that story left behind a rich historical record. The rise of radiation therapy coincided with the rise of the professional press in medicine, and radiation therapists published widely both in general circulation journals, like the *Journal of the American Medical Association* and the *Lancet*, and in specialist publications, like the *American X-Ray Journal* and the *Archives of the Roentgen Ray*. Public interest in x-rays and radium meant that these technologies also

[20] John B. Macintyre, "Presidential Address," *Archives of the Roentgen Ray* V, no. 2 (January 1901): 44.

received wide coverage in the popular press, and reporters found no shortage of radiation therapists willing to give interviews or offer quotes. The development of this new field also led to a veritable flood of books—textbooks and technical monographs, but also memoirs and philosophical treatises—and a sea of pamphlets, brochures, and other advertising materials from companies eager to sell vacuum tubes, film, and radium water to any physician looking to expand into radiation therapy. Early therapists also left behind personal accounts, records, journals, and pictures.

Many of the individuals involved in the development of radiation therapy explicitly wrote or preserved materials with an eye towards history. Therapists like Heber Robarts and Emil Grubbé saw themselves as the vanguard of a technological revolution in medical treatment, and they wanted to make sure that their own contributions received the proper recognition and appreciation. The depth of the written record also reflects the fact that x-ray and radiation therapy developed at a moment of enormous conflict and change both in the medical community and in the larger world.

A Context of Conflicts

Radiation therapy is interesting as a historical topic in part because of the quirk of its particular historical context; the technology spread amidst, and became entangled in, both professional and geopolitical conflicts. Taken together, geopolitical and professional conflict impacted the development of x-ray and radium therapy in profound ways. Moreover, because conflict provided ample fodder for discussion and debate in the pages of the medical press, the context of conflict within which radiation therapy developed created a rich historical record.

On the geopolitical side, the major world powers were, by 1895, actively maneuvering for strategic advantage in ways that anticipated some eventual, apocalyptic showdown, resulting first in a string of military conflicts in the colonized zones of Africa and Asia and eventually in the Great War. By 1900, x-ray emitters had already been tested by army doctors in places like Sudan and Afghanistan, and enthusiastic military support would prove extremely valuable to promoters of x-ray technology. Interestingly, the record suggests that pre-war international tensions did not shut down or inhibit the professional networks that allowed information to flow back and forth between the German, French, and English-speaking radiological communities, but it did lead to a certain amount of posturing, including the eventual decision of British and American x-ray users to do away with the "roentgen" terminology in favor of words derived from "radiation." And although tensions did not prevent scientists and doctors from sharing the results of their experimentation, pre-war strategic posturing had a major impact on the physical availability of radium, with countries moving to control and restrict supplies of both radium-bearing ore and refined radium salts. Government investments in radium materials and x-ray emitters during World War I also played an important role in the post-war development of radiation therapy and radium-based consumer health products.

On the professional side, a variety of internecine battles were resetting the landscape of medicine in fundamental ways. The nineteenth century was a period of intense conflict and transition for doctors and medical researchers on both sides of the Atlantic. Conflict over such basic questions as who could be a doctor, what philosophies of medicine would predominate in the twentieth century, how institutions should function, and the ethics of medicine set medical professionals against one another. For ordinary practitioners—doctors, but also pharmacists, midwives, surgeons, and a plethora of service providers operating under the nebulous label of "therapist," i.e. "electrotherapist"—a debate raged over the interlocking problems of medical theory and professional identity.

Nineteenth century medical orthodoxy

The concept of orthodoxy in medical practice has always existed to some degree. The most famous example comes from the ancient world, in the form of the "Hippocratic Oath," with its principles of ethical medical practice. In nineteenth-century medicine, orthodoxy was essentially defined by consensus. In theory, physicians as a group had settled or would settle on "best" practices (or, at the very least, reject the worst ones). Orthodox doctors specifically described themselves in opposition to "quacks"—practitioners who based their practice on unsafe or unsound theories and principles.

For therapists interested in treating disease with x-rays or radium, this dynamic meant that their adoption of a new technology must coincide with a period of consensus-building, convincing colleagues that radiation should count as an orthodox treatment, rather than as quackery. That necessity helps to explain the striking degree of self-promotion that a reader encounters in the early literature on radiation therapy, especially from x-ray therapists. Rather than treating such claims purely as boasts, the reader should view them as part of an overall bid by radiation therapists to be included in the existing medical orthodoxy, rather than expelled from it.[21]

Tradition-based orthodoxy faced a severe challenge in the nineteenth century, as new discoveries in biology and chemistry overturned some of the most cherished ideas in medicine. By the time Röntgen published his first paper on x-rays, in 1895, the germ theory of disease, spread with a fervor described by historian Nancy Tomes as akin to religion, was displacing

[21] Consensus in nineteenth century medicine had several sources. Historian John Harley Warner found that tradition played an important role in defining the orthodox consensus; adherence to medical ideas passed down from previous centuries—some ideas, like the humoral theory, traced their roots to Greek practice—was both "a leading source of professional pride" among physicians and "as a professional tool to set themselves apart from the variety of alternative practitioners," such as the homeopaths, whose medical ideas had not "received the validation of centuries of trial." John Harley Warner, *The Therapeutic Perspective: Medical Practice, Knowledge,and Identity in America, 1820-1885* (Cambridge, Mass: Harvard University Press, 1986), 162–63.

many of the older theories of contagion.[22] New forms of medical research also challenged the old orthodoxy. The growth of large urban hospitals, in places like London and Paris, and the adoption of new statistical and observational methodologies enabled clinicians to compile data about human biometric norms, the symptoms and progression of specific diseases, and even the comparative value of different treatment regimens.[23] With regard to treatment, the results of these new forms of inquiry were often dismaying; many of the traditional remedies used by doctors showed little or no efficacy under the new forms of analysis, leading, famously, to Oliver Wendell Holmes's exclamation that, as of 1860, "if the whole *materia medica*, *as now used*, could be sunk to the bottom of the sea, it would be all the better for mankind, and all the worse for the fishes."[24]

For adherents of new, radiation-based therapies, the rapid winnowing of the medical arsenal created an opportunity to make their case as legitimate, orthodox medical professionals. Although ionizing radiation had a wide variety of possible uses, advocates and members of the public alike consistently focused their attention on the use of radiation in previously "untreatable" diseases, especially skin infections and cancers. In fact, this particular case highlights the degree to which concerns of legitimacy might trump the concerns of the medical marketplace. Hair removal was one of the most lucrative and popular early uses of x-ray emitters, yet it received relatively little attention in newspaper accounts and journal articles touting the benefits of x-ray therapy. Advocates of x-ray therapy showed noticeably more interest in promoting themselves as medical professionals treating disease than in advertising themselves as specialists in cosmetic procedures.

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[22] Nancy Tomes, *The Gospel of Germs: Men, Women, and the Microbe in American Life* (Cambridge, MA: Harvard University Press, 1999).
[23] Warner, *The Therapeutic Perspective*, 185.
[24] Quotation from "Currents and Counter-currents in Medical Science," an address given before the Massachussetts Medical Society, 1860; quoted in *The American Journal of the Medical Sciences* (J.B. Lippincott, Company, 1860), 467.

As with geopolitical conflict, professional conflict and the changing structure of medicine affected the development of radiation therapy in complicated ways. Initially, many orthodox physicians regarded x-ray technology with skepticism (especially when the new practice threatened to take business), and many continued to regard x-ray therapists as possible quacks or charlatans even after seeing evidence that radiation could treat disease. The professionalism debate also led to deep divisions within the x-ray community, putting practitioners with degrees and formal medical licenses at odds with non-credentialed x-ray users. But advocates of radiation therapy also benefited from some of the changes taking place in the field of medicine; in particular, x-ray therapists found new ethical standards and the changing role of hospitals in the medical system helpful to their cause.

Anglophone medical ethics in the *fin de siècle*

The specialization and professionalization questions, in both Great Britain and the United States, were partly questions of medical ethics. For practitioners in both the United States and the United Kingdom, the concept of "medical ethics" as a code of proper behavior for medical practitioners had its roots in a tradition of oath-taking.[25] The oath adopted by the

[25] The tradition had two distinct strands; one emphasized doctor-patient relations, while the other governed the conduct of physicians as professionals. The latter category encompassed both relationships between members of the community and the duty of physicians to properly represent the profession to the larger society. Any of these elements could be ascendant at a given moment. For example, James VI of Scotland (after 1603, James I, of England) gave primacy to professional conduct when he introduced a loyalty oath known as the *sponsio academica* at Edinburgh University in 1587. In a time of significant upheaval in Britain—including the civil war that lasted from 1642-1651, Cromwell's reign as Lord Protector, and the subsequent tug-of-war over the issue of religion, following the restoration of the monarchy—oaths that aimed to affirm and bind the loyalties of professionals quickly became an important source of both power and resistance in the United Kingdom. Such pledges were not unique to physicians or academics; many professions included a tradition of taking loyalty oaths as a condition of membership, including groups such as military officers and the clergy. In the 1730s, by contrast, the medical faculty at the University of Edinburgh put in place a new *sponsio academica* specifically for medical graduates that emphasized ethical behavior in doctor-patient relations. By pledging graduates to "exercise the Art of Medicine cautiously, purely and honorably, and, as far as I can, to take care faithfully that all [my actions] are conducive to [effecting] health in sick bodies," the Edinburgh emphasized the loyalty of care providers to their patients. Robert Baker, *Before Bioethics: A History of American Medical Ethics from the Colonial Period to the Bioethics Revolution*, 2013, 40–44.

medical faculty of the University of Edinburgh in the 1730s was particularly influential in the English-speaking world. Because of its unique prioritization of fidelity to patients, rather than to the state or a church, Robert Baker described the Edinburgh oath as "the *ur*-text…of physicians' conception of the foundational ethics of what came to be called 'the physician-patient relationship.'" It had a powerful influence on American medicine, in particular, because so many prominent American physicians—men like Benjamin Rush, John Morgan, and William Shippen—traveled to Scotland as part of their medical training.[26]

At another level, however, the Edinburgh oath served as a simple resurrection of older ideas about the interwoven nature of professional and patient loyalties. That relationship characterized the classical Hippocratic Oath. The Hippocratic Oath also seems to have created a professional divide between Hippocratic physicians and those who practiced surgery.[27] Professional divisions between different branches of medicine would remain forceful, particularly in the United Kingdom, where the persistence of the old medical guilds preserved the division between doctors and surgeons well into the nineteenth century, long after professionals in other countries had consolidated the two branches under the collective practice of "medicine."[28]

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[26] Many of these physicians, like Morgan and Shippen, went on to found their own medical colleges in the United States, and they brought home with them not only the text of the Edinburgh oath, but also the underlying ideas about ethical practice that it represented. Ibid., 44–51.

[27] Hippocratic Oath quoted from translation in Paul Carrick, *Medical Ethics in the Ancient World*, Clinical Medical Ethics Series (Washington, D.C: Georgetown University Press, 2001), 83–84. Before they even mentioned patients, adherents of the Hippocratic Oath swore "to hold him who has taught me this art as equal to my parents," caring for the teacher in time of need and providing for him financially, and to pass on "this art—if they desire to learn it—without fee and covenant," but only to deserving students (generally, the oath-taker's own sons and the sons of his teacher) "who have signed the covenant and have taken an oath according to the medical law, but to no one else." As Carrick explains, the apparent prohibition on surgery remains the subject of serious interpretive controversy, in part because we do not fully understand the professional context of Greek medicine.

[28] George Weisz, "Medical Directories and Medical Specialization in France, Britain, and the United States," *Bulletin of the History of Medicine* 71, no. 1 (1997): 23–68; George Weisz, "The Emergence of Medical Specialization in the Nineteenth Century," *Bulletin of the History of Medicine* 77, no. 3 (2003): 536. Weisz argues in the second article that the persistence of a division between surgeons and physicians

Two justifications made right or wrong professional conduct central to the medical ethics debates of the late nineteenth and early twentieth century. First, physicians had an obvious interest in preserving both their reputations and their business, and the unscrupulous physician did a double-harm to his colleagues, usurping their business in the short term with empty promises, rather than with superior skill, and debasing the reputation of the profession as a whole in the long term. Second, right professional behavior served as a kind of *de facto* marker of the trustworthiness and value of a physician's professional advice.[29] As radiation therapists attempted to navigate the ethical challenges of their novel and experimental treatments, the ethical debates over professional behavior would initially loom largest.[30]

Metaphors & Narratives

To understand the professional conflicts within medicine, in particular, it helps to deploy a historical analysis based around metaphor and cognition. In the early days of x-ray therapy medical practitioners discussing and thinking about ionizing radiation had to contend with the fact that x-rays are invisible to the naked eye. Moreover, to repeat an earlier point, no one knew precisely how radiation worked in the body. As a result, discussions of radiation therapy

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specifically contributed to the slow acceptance of specialization in British medicine versus the speed of its adoption in France.

[29] An example of the principle appears in the writings of Renaissance professor of medicine Giovanni Battista Cortesi. Cortesi told the story of a doctor who obtained the patronage of a sick noblewoman both by disparaging the diagnosis and treatment of the physicians previously consulted in her case and by promising a cure to the patient's husband and family members. The doctor's boasts proved false; after a short period of improvement, the woman died. In the end, this unprofessional—and thus, unethical—physician had harmed not only his own reputation, but the reputation of physicians who had behaved appropriately, and, since "letters [had been] sent to distant relatives assuring them that the patient was safe since a famous physician had said so," of the medical profession more broadly. Winfried Schleiner, *Medical Ethics in the Renaissance* (Washington, D.C.: Georgetown University Press, 1995), 14.

[30] In the early years of radiation therapy, American physicians regarded fee splitting—the proper division of patient revenues between primary and consulting physicians—as an ethical issue at least as important as, and probably more contentions than, the proper protocols for conducting patient trials with a novel medical technology. To put it more succinctly, in 1898 the issue of pay for a consulting physician offering x-ray cancer treatment could easily be as or more ethically contentious than the question of whether or not that physician should use the same emitter for an experimental body hair removal treatment. Baker, *Before Bioethics*, 223–25.

inevitably turned into exercises in metaphorical explanation. To give one example, x-ray emitters and vials of radium salts were often visually depicted as miniature suns, with their output of ionizing radiation depicted as the sun's rays. The sun imagery, however, did not simply describe a way of thinking about rays emanating from a source; it also captured a popular notion that x-rays could be used to treat tuberculosis. The basic theory held that radiation, in passing through human tissues, could confer the known antibacterial effects of sunlight to the interior of the lungs, i.e. to places that sunlight could not normally reach.

This example illustrates the way in which a powerful metaphor actually alters its recipients' perception of the world.[31] Sometimes, the metaphors used to conceptualize radiation would prove useful, as when x-ray therapists realized that an x-ray emitter, like a light source, could be filtered to exclude undesirable bandwidths. But other metaphors would alter radiation users' perceptions in harmful ways: thousands of consumers drank Radithor, the radium-laced water, on the theory that a shot of liquid "energy" could reinvigorate a tired body.

The "progress narrative" is another metaphor that could lead radiation therapists astray. In its simplest instantiation the "progress narrative" metaphorically frames a set of historical events as a "story," in the sense normally reserved for works of fiction: a set of characters/protagonists, responding to some problem or source of tension, progress through a series of challenges or difficulties, moving ever closer to a point of resolution that eventually "ends" the story.[32] In writing about or discussing their work, advocates of radiation therapy made it clear that they saw themselves as the characters in a progress narrative. Early x-ray

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[31] George Lakoff and Mark Johnson, *Metaphors We Live by* (Chicago: University of Chicago Press, 1980), 6.

[32] "Progress" is only one of several narrative metaphors that people commonly use to structure their understanding of historical events; tragedy and farce also serve this end for some observers, and the narrative tropes of conspiracy and betrayal have an enduring popularity, particularly amongst amateur historians and those writing for the popular market. So common is the narrative metaphor that individuals often perceive themselves as participants in an *ongoing* historical narrative—a notion of living through history, as it were.

therapists like Emil Grubbé and Heber Robarts were fairly certain that they would eventually be regarded as titans of medicine whose work had helped to banish human suffering, and articles published in the *American X-Ray Journal* and the *Archives of the Roentgen Ray* frequently alluded to the expected accolades of some observer from the future.[33]

In the case of medical technologies, like x-ray emitters and radium salts, progress narratives in particular appeal because many positive developments really have taken place. Penicillin really is a more useful drug than the mercury derivatives of the nineteenth century. Moreover, the progress narrative has obvious appeal for both doctors and patients, who would like to believe that they have the best possible treatment options in history and that those options will only continue to improve. In some ways, the story of x-ray and radium therapy seems to fit into the progress narrative. Radiation offered legitimately exciting technological progress in both the diagnosis and treatment of disease. It made possible the amelioration of previously-untreatable, painful, disfiguring, and deadly cancers and skin diseases.

But the progress narrative also has limitations. In the case of x-ray and radium therapy, the simplicity and appeal of the progress narrative obscured the complexity and the dangers of the technology for both patients and practitioners. The optimism engendered by the progress narrative made it difficult for radiation therapists to see the danger in front of them. In their rush to declare new “cures” or exciting results, advocates tended to ignore the fact that

[33] Those expectations were not unreasonable; the history of nineteenth- and twentieth-century medicine often does read as a set of interlocking progress narratives. One story tracks improvements in the science of medicine, celebrating, for example, the transition from the “humoral” and “miasma” theories of disease to the microorganism-based germ theory. Another narrative centers on drugs and medical technologies, starting with the bloodletting lancet and mercurial purgatives, and progressing through such milestones as the obstetrician’s forceps, Joseph Lister’s use of carbolic acid for antiseptic surgery, the development of x-ray photography, and the discovery of penicillin. A third progress narrative centers on professionalization, celebrating the ascendance of scientifically-educated, state-licensed, card-carrying members of the modern professional societies, such as the American and British Medical Associations, at the expense of dangerous, ill-informed charlatans and quacks. Nevertheless, the author suspects that Grubbé and Robarts would have regarded this present project with skepticism.

radiation, in addition to shrinking tumors, could also sicken patients, give them painful and disfiguring burns, or even kill them.

Consuming Energy

Emil Grubbé and Heber Robarts, along with Sydney Rowland and a few others, are at the heart of the narrative told in Chapter 1, which focuses on the five-year period from January of 1896, when news of Röntgen's discovery broke, to December of 1900, when the American Roentgen Ray Society held its first annual meeting. An incredible flurry of activity took place during this five-year period, which I think of as the "Röntgen Rush," as would-be x-ray therapists rushed to set up clinics and experiment with the new technology. In addition to this story, the chapter also focuses on the early efforts of x-ray enthusiasts to create organizations, specialist journals, and the other scaffolding of a profession in a community where not everyone agreed on what it meant to be a doctor.

Chapter 2 examines the development of x-ray therapy in the period leading up to the First World War. The chapter opens with a deep dive into the actual devices used by early x-ray therapists to power their equipment and produce x-rays, but it also considers the perceptions of users, with an eye towards the problems created by the space between users' understanding and the realities of the equipment. Ultimately, the chapter argues that this disconnect, when combined with the seemingly endless optimism of x-ray enthusiasts, ultimately led to tragedy, in part because users treated uncertainty as an acceptable condition of their work, rather than as a desperate problem in need of an immediate solution.

Chapter 3 shifts the focus of the narrative away from x-rays and over to radium, the other tool of early radiation therapy. The chapter uses the records of two physicians, Robert and Truman Abbe, to sketch out both the dynamics of the early radium industry and the actual process of experimenting with radium treatments. Radium samples, in sharp contrast to x-ray emitters, were produced by mining and refining radioactive ore, and the chapter devotes considerable attention to the dynamics of this market, which became the subject of considerable political and commercial competition in the years prior to World War I. The chapter also considers the process by which radium eventually became a consumer health product, rather than a specialized treatment.

Both Chapter 4 and the conclusion of this project circle back to its starting place, in the person of Heber Robarts. Robarts's personal career intersected with both early x-ray therapy and with radium, which he enthusiastically adopted in the later years of his life. Robarts's biography is by turns glorious and ignominious. His colleagues, at various moments, venerated him as a founder and denigrated him as a quack, and the flavor of his reputation at any particular moment reflected some of the larger conflicts that divided practitioners of radiation therapy. That other practitioners had feelings about Robarts reflected the man's relentless pursuit of publicity, as reflected in his long publication record. As his body gradually succumbed to x-ray induced cancer, some of Robarts's writing took a philosophical turn that suggests an answer to one of the lingering questions at the heart of this study: why did people—both patients and practitioners—accept the risks of radiation therapy in the face of clear evidence that it could harm or kill them?

For both patients and practitioners, the fundamental appeal of radiation therapy seems to have arisen from the association of ionizing radiation with light, and especially with sunlight. From the earliest newspaper headlines of 1896 to the pre-war covers of the *Archives of the Roentgen Ray* and the *American X-Ray Journal* to the 1920s-era pages of Radithor sales brochures, the images and descriptions of ionizing radiation consistently depicted both x-rays and radium in this fashion. The energy of the sun—its warmth and its light—everyone knew, lay at the very foundation of life on Earth. It therefore makes a certain kind of sense that people trusted these new energy sources. We also use the warmth of the sun as a metaphor or shorthand for optimism: a sunny outlook or a sunny disposition or a new day dawning. This connection, too, suits the radiation therapy narrative particularly well, given that advocates of radiation therapy were nothing if not optimistic and enthusiastic.

This light, however, did not drive back the darkness; it created uncertainty, rather than dispelling it. At the end of his life, Heber Robarts's optimism was not borne out: the radium Robarts managed to obtain did not cure his cancer. Likewise, this study is not the glorious history that he hoped it would be—the tale of radium and x-ray pioneers conquering all human suffering. And yet, Robarts was also right. Radiation *can* cure cancer, or at least treat it, and radiation therapy *has* improved countless lives in the century since Robarts succumbed to the effects of his own exposure. This, then, is not so much a tale of failure as an examination of the tangled thicket of contradictions and complications within which technological medical progress so often happens. The history of radiation therapy offers a useful reminder that success is often only the endpoint of an otherwise cautionary tale.

CHAPTER 1 – X-RAY THERAPY IN THE RÖNTGEN RUSH, 1895-1900

Wilhelm Conrad Röntgen published his initial report on "a new kind of ray" at the end of December, 1895. A medical student, Émil H. Grubbé, opened Chicago's first x-ray therapy clinic for business in February of 1896. The short space between these two events typified the pell-mell rush to deploy the new technology that took place on both sides of the Atlantic following Röntgen's announcement; it was, in the words of historian Bettyann Holtzmann Kevles, "a craze unlike any that had come before."[34] I have come to think of the five year period from December, 1896, to the American Roentgen Ray Society's first "regular" meeting, held in New York City in December of 1900, as the "Röntgen Rush."[35] The introduction of therapies based on ionizing radiation would create a series of challenges and debates for medical practitioners. The Röntgen Rush is interesting because so many of these questions had their genesis during this comparatively brief window of time. In subsequent chapters, I will delve more deeply into the individual debates over issues like doctor and patient safety, human experimentation, and professionalization in medicine. But in order to properly understand those individual threads, it is helpful to start with a more holistic point of view, and the relatively compact temporal window of the Röntgen Rush offers precisely this opportunity.

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[34] Kevles, *Naked to the Bone: Medical Imaging in the Twentieth Century*, 24.

[35] Periodization is, of course, always a slightly arbitrary exercise, and historians focused on non-therapeutic aspects of the x-ray story have adopted other divisions. Matt Lavine writes about the "gas tube era," which lasted until roughly 1914, when new, filament-heated vacuum tubes replaced the original low-vacuum, cold-cathode Crookes tubes used in early x-ray emitters. Arne Hessenbruch, focusing on diagnostic x-ray use, divides the early story of x-rays between an initial period of experimentation in the 1890s and the subsequent development of roentgenology as a professional discipline in the early 20th century. Although I think that our conclusions are largely compatible, I will argue that the professionalization of the x-ray community began in 1896, almost immediately after the discovery of x-rays. Lavine, "The Early Clinical X-Ray in the United States"; Hessenbruch, "Calibration and Work in the X-Ray Economy, 1896-1928."

Ethics of Uncertainty

The juxtaposition of the Röntgen's publication and the opening of Grubbé's clinic highlights the immediate, oft-overlooked efforts to use x-ray emitters as a therapeutic, rather than simply a diagnostic, technology. While the story of x-ray *diagnosis* fits into a fairly comfortable progress narrative of steady improvement in equipment, patient and practitioner safety, and medical utility, I will argue here that x-ray *therapy* has a more complicated legacy, splicing healing with harm and innovation with risk. X-ray experimentation had a high human cost, both for practitioners and patients: painful and disfiguring burns, amputated limbs, pernicious anemia, deadly cancer. It is tempting to view these as the unintended consequences of a sort of "dark age" of x-ray experimentation, when bad equipment and incomplete knowledge led to tragic outcomes for users of a new technology who did not understand its long-term risks. As I will show, however, the truth is much more complicated. The record left behind by the first wave of x-ray users shows that would-be therapists almost immediately confronted evidence of the dangers of x-ray exposure. Even recognition of the explicit connection between x-ray exposure and cancer did not take long—perhaps five to seven years. But rather than dissuading experimentation, harm, for roentgenologists, provided the very basis of their belief that x-ray emitters had therapeutic applications. The resulting tension fueled constant debate amongst x-ray users over the appropriate use, and limits, of the technology.

X-ray "therapy" amounted to unregulated human experimentation with a technology that users knew could, and did, cause harm. In and of itself, however, this fact does not make the x-ray narrative particularly novel. Historians of medicine have already documented a variety of cases of morally or ethically problematic human experimentation. In general, however, such studies have focused on particular cases, such as the Tuskegee syphilis study or George D.

Barney's tuberculosis experiments.[36] Because it revolves around a new technology, rather than a particular disease, the x-ray narrative offers a different sort of analytical opportunity: the chance to examine ethical and technical decision making in medicine at the level of a larger therapeutic community—one that included not only traditional medical "insiders," but newcomers from both the other sciences and the lay public.

It would take more than three decades to settle some of the ethical debates that arose first around x-ray technology and then around radiation therapy more generally. In the context of nineteenth-century medicine, "medical ethics" was a multifaceted concept that encompassed not only a therapist's behavior with his or her patients, but also one's professional obligations to other medical practitioners and a more abstractly-conceived moral obligation to "the public."[37] The spread of x-ray technology raised questions in all three of these areas. And although x-ray users—even those without professional medical training—generally tried to adhere to the ethical conventions of the day, I will argue that their judgments were often subverted by a combination of factors, ranging from technological enthusiasm, pecuniary interest, and incomplete understandings of experimental methodology, to the technical limitations of early x-ray equipment, particularly with regard to shielding and dosimetry, and the genuine desire of x-ray enthusiasts to alleviate human suffering at all costs.

X-ray practitioners' excitement about the technology, coupled with the relatively slow appearance of harm in the body, meant that numerous patients suffered damage at the hands of their would-be x-ray therapists. These injuries happened even though, as I will show, x-ray

[36] On the Tuskegee experiment, see James H. Jones and Tuskegee Institute, *Bad Blood: The Tuskegee Syphilis Experiment* (New York: Maxwell Macmillan Canada, 1993); for the Barnes and other high-profile cases, see Susan E. Lederer, *Subjected to Science: Human Experimentation in America before the Second World War*, The Henry E. Sigerist Series in the History of Medicine (Baltimore: Johns Hopkins University Press, 1995).

[37] Baker, *Before Bioethics*, 112–13. For an extensive discussion of nineteenth century medical ethics and the historiography on this topic, see my Introduction.

users understood the theoretical dangers of x-ray exposure. In part, the willingness to accept the risks of x-ray exposure lay with patients themselves. Popular ideas about medicine, energy, and human health made x-ray technology appealing and its risks acceptable, despite stories about the hazards of exposure that splashed across national newspapers within months of Röntgen's discovery. Vastly complicating the issue, x-ray therapy could be (and still is) used as a valid treatment option for some diseases, meaning that both patients and therapists attempting to make decisions about the value of x-ray therapy had to navigate an uncertain tangle of complicated and conflicting claims.

In the professional realm, the major ethical debates in nineteenth-century medicine all revolved around the central issues of legitimacy (who could "practice medicine"?) and marketplace competition (how could a doctor advertise, what could he or she promise, etc.). X-ray technology appeared during a period of drastic, worldwide change in professional medicine. In addition to new ideas about the role of technology in medicine, doctors at the end of the nineteenth century had witnessed, and would continue to grapple with, shifts in the importance of licensure and the role of scientific inquiry in medicine, changes in the structure of the profession and the role of specialists, new ideas about the causes of disease, and acrimonious public debates over the ethical obligations of physicians and researchers. So-called "roentgenologists" self-consciously portrayed themselves as medical professionals and constructed a new community and a new medical discipline in the midst of this upheaval. Advocates of x-ray therapy argued, however, that they could base therapy on the standardized operation of a machine rather than on the individual therapist's combination of intricate medical knowledge and skill at interacting with a patient's body. This approach to healing, I will argue, made it possible for individuals to enter the medical profession based on technological expertise, rather than on traditional medical education. As a result, the introduction of x-ray technology undercut the traditional role of physicians' professional organizations in policing therapeutic experimentation and keeping out "quackery"—the designation for unethical treatments or experiments.

It is possible to get at these issues, and the debates they provoked, because early x-ray users left a robust written record of their activities. From the very earliest days, adopters of x-ray technology constructed for themselves diverse, international networks, including journals modeled after other scientific and medical periodicals and organizations of practitioners. "Roentgen Societies" held public forums, shared research, and participated in larger medical and scientific conferences and organizations. Development of the techniques, rules, and limits of x-ray therapy—and the human experimentation used to establish those practices—would happen within this network, creating a lasting paper trail in the form of professional journals, organizational reports, pamphlets, textbooks, and the like.

To get at both the professional and ethical narratives, this chapter focuses particular attention on three influential individuals in what would become the medical field of x-ray therapy. Émil H. Grubbé was the man behind the Chicago x-ray clinic. Grubbé was an interesting character: equal parts scientist, entrepreneur, and would-be physician. Still a medical student in 1896, Grubbé would obtain his M.D. and spend the rest of his life involved in x-ray therapy and research, and he left behind a record of his experiences in a memoir, *X-Ray Treatment: Its Origin, Birth, and Early History*, published in 1949. In London, another medical student, Sydney Rowland, would also play an important role in the early development of the field, producing in April of 1896 the first x-ray specialist journal, *The Archives of Clinical Skiagraphy.* By 1897, the journal had changed its name to the *Archives of the Roentgen Ray* (*ARR*), and it had been joined by the *American X-Ray Journal* (*AXJ*), created by former railroad surgeon Heber Robarts.

The *Archives* and the *X-Ray Journal* both have a steady publication record that stretches back to the earliest days of x-ray technology, and both Rowland and Robarts used their publications as platforms for developing robust organizations of professional x-ray users that have lasted into the twenty-first century. Both journals also benefited from a broad international readership and contributor base. As a result, they give historians a good sense of the debates and evolution of opinion within the x-ray community. The journals also provide, through regular reports on the activities of their associated societies, an excellent window into the process of disciplinary formation in medicine. The fortunes of the journals, however, would contrast somewhat with the fortunes of the two editors, and the disparate fates of Rowland, Robarts, and Grubbé would themselves reveal something about the controversies unfolding in the fledgling field at the end of the rush.

Vacuum tubes: a (brief) history

On August 22, 1879, William Crookes addressed the British Association for the Advancement of Science. Crookes opened his lecture with a brief nod to the work of Michael Faraday. Faraday, in 1816, hypothesized that matter could exist in a fourth state, "as far beyond vaporization as that is above fluidity." Faraday coined the term "radiant matter" to describe this state; modern physicists call it "plasma."[38] Faraday lacked the technological means to test his hypothesis, but he set the path that would eventually lead to the discovery of x-rays. German glassblower (and eventually, physicist) Heinrich Geissler would take the next step, making it technically possible to produce Faraday's "radiant matter." Geissler began his professional life as a traveling artisan. A master glass-blower, Geissler found a steady clientele in Germany's universities by specializing in the creation of blown-glass apparatus for experimental research.

[38] Bence Jones, *The Life and Letters of Faraday* (London: Longmans, Green and Co., 1870), vol. 1, p. 308; William Crookes, "On Radiant Matter," *Journal of the Franklin Institute* 108, no. 5 (November 1879): 305.

Eventually his name would become synonymous with vacuum tubes: the term "Geissler tube" refers to a sealed glass tube, evacuated by a pump to a low internal air pressure, with an electrode at each end, allowing for an electric current to pass through the tube. Neon and mercury-vapor lights are modern forms of the Geissler tube.[39]

Numerous nineteenth-century experimenters, including William Crookes, worked with vacuum tubes, and many produced new iterations of the technology. In a basic Geissler tube, the application of current produces light (its color determined by the composition of gasses in the tube) in the space between the positive and negative electrodes. A small zone of darkness, however, surrounds the negative pole, or "cathode."[40] By using new, more powerful pumps, Crookes dramatically increased the degree of vacuum in his tubes, and he noted that the size of the dark space around the cathode would increase in tandem with the falling pressure inside the glass envelope. Eventually the dark space filled the vacuum tube, and the glass wall opposite the cathode began to glow with an eerie luminescence, its color determined by the material used to produce the bulb—English glass glowed blue, uranium glass, dark green, and German glass (the most commonly used substance) phosphoresced in a shade of "bright apple green." Crookes described his findings in a series of lectures and publications on "Radiant Matter." He correctly deduced that the dark space represented the mean free distance that a particle leaving the cathode could travel before striking another particle; in high-vacuum tubes, the charged particles traversed the full-length of the tube without encountering any other obstacle and struck the glass, causing the phosphorescent effect.[41] Physicists would eventually identify these "cathode rays"—so-named for their point of origin—as streams of electrons.[42]

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[39] T. H. N., "Heinrich Geissler," *Nature* 19, no. 486 (February 20, 1879): 372–372.
[40] "Geissler's Vacuum Tubes.," *New York Times*, March 21, 1880.
[41] Crookes, "On Radiant Matter."
[42] Kevles, *Naked to the Bone: Medical Imaging in the Twentieth Century*, 17.

Crookes publicized his work widely, and other experimenters rushed to replicate his results and investigate this new phenomenon, replacing their “Geissler tubes” with new “Crookes tubes” capable of achieving higher levels of internal vacuum and producing cathode rays. The “Crookes tube” moniker stuck, becoming the default designation for the whole class of energized vacuum tubes, despite the efforts of other innovators to further alter the apparatus and promote their own *noms de tube.* One such experimenter, Philipp Lenard, succeeded in replacing a portion of the vacuum tube’s glass wall with a piece of aluminum foil. Lenard found that this arrangement allowed some of the cathode rays to escape the Crookes tube; he could detect the wayward rays by means of a cardboard screen treated with fluorescent paste, which glowed when placed in front of the aluminum window. Since the glass walls of the apparatus would also phosphoresce, Lenard covered his tube to prevent it from outshining the fluorescent screen. Lenard published his results in October of 1895.

The discovery of x-rays

Wilhelm Conrad Röntgen, then head of the physics department at the University of Würzburg, decided to replicate Lenard’s experimental set-up, including the fluorescent screen and the covered tube. When he initially energized the covered tube, however, Röntgen noticed that the screen, still lying on a chair a few feet away, immediately began to fluoresce. Röntgen expected the screen to fluoresce when he put it close to the emitter, but he did not expect to see it glowing several feet away. Lenard’s experiments had shown that cathode rays could only travel a short distance—about eight centimeters—in regular air. Röntgen’s screen glowed at a distance of 2 meters.[43]

[43] Wilhelm Conrad Röntgen, “On A New Kind of Rays: First Communication (December, 1895),” in *Röntgen Rays: Memoirs by Röntgen, Stokes, and J.J. Thomson, Tr. and Ed. by George F. Barker*, trans. George F. Barker, Harper’s Scientific Memoirs (New York: Harper & Brothers, 1899), 3–13; Kevles, *Naked to the Bone: Medical Imaging in the Twentieth Century*, 16–19.

The magnitude of that difference suggested to Röntgen that the fluorescence resulted not from cathode rays, but from some heretofore unknown force. In the footnotes of his first paper on the subject, Röntgen wrote that, "For brevity's sake I shall use the expression 'rays'; and to distinguish them from others of this name I shall call them 'x-rays.'" Following up on his suspicion that these "x-rays" differed from cathode rays, Röntgen began a series of experiments. Over the subsequent seven weeks, the physicist spent almost every waking hour working with x-rays. The Röntgen family lived in an apartment over the lab, and an oft-repeated legend holds that Frau Röntgen made possible her husband's obsessive hours of observation by slipping quietly in and out with hot meals. Through various experiments, Röntgen found that "all bodies are transparent to this agent, though in very different degrees," as determined primarily by density (although he noted that other, as-yet-unexplained factors also seemed to play a role in the degree of transparency). Unlike cathode rays, x-rays did not deflect in the presence of a magnet; Röntgen regarded this as the acid test which distinguished the new emanation from the cathode rays studied by Lenard and others. Eventually, Röntgen concluded that cathode rays *produced* x-rays when they struck either the glass wall of a normal Crookes tube or Lenard's aluminum window.[44]

Röntgen also tested his x-rays with photographic plates. Because unabsorbed rays expose photographic plates and film, x-ray photographs come out as "shadow pictures," based on the differential absorption of the materials in the photograph. Denser substances, like bone, produce less-exposed negative spaces, while lower-density materials, like soft tissues, look ghostly or invisible on the resulting photo. The most famous of Röntgen's pictures shows the bones of his wife's hand, her wedding ring visible as a dark blob over the fourth finger. Röntgen, like many subsequent x-ray users, found that "the production of [shadow photographs] has a

[44] Röntgen, "On A New Kind of Rays: First Communication (December, 1895)"; Kevles, *Naked to the Bone: Medical Imaging in the Twentieth Century*, 19.

particular charm," noting in his initial report that, in addition to photos of hand bones, "I possess, for instance, photographs of the shadow of the profile of a door which separates the rooms…; the shadow of a covered wire wrapped on a wooden spool; of a set of weights enclosed in a box; of a galvanometer…; of a piece of metal whose lack of homogeneity becomes noticeable by means of the x-rays, etc."[45] In December, Röntgen finally took a break from his work to write up the results and send them off, along with the photo of his wife's hand, to the *Sitzungsberichte der Würzburger Physikalischen-Medicinischen Gesellschaft*—the house journal (*Transactions*) of the Physico-Medical Society of Würzburg—which published the report on December 28, 1895.[46]

Fortuitous circumstances brought Röntgen's discovery to the public with unusual speed. The editor of the Würzburg *Transactions* sent copies of Röntgen's unusual photographs, along with copies of the report, to Franz Exner, a professor of physics in Vienna, who brought them out at a dinner party. One of the guests was E. Lecher, a physicist from Prague whose father was the editor of Vienna's leading daily, *Die Presse*. Lecher passed along the story, and *Die Presse* published the first public account of the discovery on the front page of its Sunday edition on January 5, 1896, although they mistakenly attributed it to Professor "Routgen" (the misnomer would carry into many subsequent accounts). Britain's *Daily Chronicle* and Germany's *Die Frankfurter Zeitung* both picked up the story, as did the *New York Sun* and the *St. Louis Post-Dispatch*.[47] The first account of the new technology in the *New York Tribune* appeared in a short paragraph as part of the world-news roundup; it described a "light, by which a man can be

[45] Röntgen, "On A New Kind of Rays: First Communication (December, 1895)," 11–12.
[46] George F. Barker, *Röntgen Rays: Memoirs by Röntgen, Stokes, and J.J. Thomson, Tr. and Ed. by George F. Barker*, Harper's Scientific Memoirs 3 (New York: Harper & Brothers, 1899), 1, http://hdl.handle.net/2027/pst.000008683182; Kevles, *Naked to the Bone: Medical Imaging in the Twentieth Century*, 20.
[47] Posner, "Reception Of Röntgen's Discovery In Britain And U.S.A.," 357–58; Kevles, *Naked to the Bone: Medical Imaging in the Twentieth Century*, 24–27.

photographed without flesh or muscle, but as he is in his bare bones." The *Times* ran its first full story on the discovery four days later, under the headline, "Hidden Solids Revealed: Prof. Routgen's Experiments with Crookes's Vacuum Tube," and it gives the reader some sense of the immense excitement generated by the discovery, with descriptions of American "men of science…awaiting with the utmost impatience the arrival of European technical journals, which will give them the full particulars of Prof. Routgen's great discovery" and his methods.[48]

X-rays Everywhere

Röntgen mostly used basic, "off-the-shelf" components in his research, so many experimenters already had both the apparatus and the expertise necessary to replicate his discovery just months after its initial publication in December 1895. Émil Grubbé fell into this category. With the help of German glassblower Albert Schmidt, Grubbé had built a small vacuum-tube manufacturing business and experimental laboratory in Chicago.[49] As soon as Grubbé and Schmidt heard about Röntgen's discovery, they began to recreate his work and conduct their own experiments with x-rays in early 1896. As Grubbé later noted, he and many others working with Crookes tubes had almost certainly, if unknowingly, already produced x-rays in the laboratory, so reproducing Röntgen's results was mostly a matter of knowing what to look for.[50] Grubbé and Schmidt's experimental setup consisted of a circular paper hatbox, its

[48] CABLE TO THE TRIBUNE, "CHAMBERLAIN A HERO," *New - York Tribune (1866-1899)*, January 12, 1896; "HIDDEN SOLIDS REVEALED: Prof. Routgen's Experiments with Crookes's Vacuum Tube. TV BULLETS FOUND BY USING LIGHT Opaque Bodies Covered Toy Other Bodies Photographed Views of Profs. O. N. Rood and Hal- I Leek of Columbia," *New York Times*, January 16, 1896.

[49] Grubbé, *X-Ray Treatment: Its Origin, Birth, and Early History*, 25–32. Grubbé's work with vacuum tubes began as the result of an earlier entrepreneurial venture (and possible get-rich-quick scheme), the Moraine Mining Company, which paid stakeholders in platinum. Unable to find buyers for his platinum, Grubbé started the vacuum-tube business as a way to monetize the metal, which was a key component in the tubes.

[50] Ibid.; William Webster, "'Practical X-Ray Work,' a Paper Delivered to the Roentgen Society," *Archives of the Roentgen Ray* II, no. 3 (February 1898): 52; Kevles, *Naked to the Bone: Medical Imaging in the Twentieth Century*, 23–25. The realization by other experimenters that they had already unknowingly replicated Röntgen's achievement led a few to assert *ex post facto* claims to a piece of the x-ray discovery, and debates—some good-natured, others acrimonious—ensued. William Webster, at the January, 1897

bottom converted into a makeshift screen by a coating of barium-platinum-cyanide crystal powder, placed in front of a vacuum tube in a darkened room. When struck by x-ray radiation from the energized tube, the crystal powder fluoresced to produce a real-time x-ray image, visible to an observer peering into the hatbox, of anything placed between the emitter and the screen. Grubbé and Schmidt found that they could sharpen the images by altering the vacuum level in their tubes, with one man operating the emitter and its vacuum pump while the other observed his hand in the fluoroscope and called out feedback. In his memoir, Grubbé vividly described watching the image of his own hand as "the flesh parts gradually became transparent and finally disappeared, leaving only the outline of the bones to cast a shadow in the glow of the crystals."[51]

Similar experiments took place throughout Europe and the United States. On February 9th, 1896, the *Chicago Daily Tribune* reported that America's premier entrepreneurial inventor and tinkerer, Thomas Edison, had flung himself into the study of x-rays, working "without interruption night and day" in a "feverish search for hidden truths."[52] Two days later, the *New York Times* announced that professors at West Point had "succeeded in producing some excellent Roentgen photographs," despite a few false starts blamed on "the meagre details published in regard to the precise method of other experimenters."[53] A dispute broke out in the *Atlanta Constitution* over whether Auburn or the University of Georgia could claim the mantle of

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meeting of London's Roentgen Society, asserted that fellow Englishmen William Crookes and Herbert Jackson had discovered x-rays but failed to understand their results. In an analysis that reflected contemporary British-German rivalries, Webster suggested that Röntgen had "somewhat accidentally…put the finishing touch to the work of two Englishmen," and that "the Union Jack can be placed almost entirely over the discovery." On the other side of the Channel, German physicist Philipp Lenard spent the rest of his life bitterly claiming that he should receive first credit for the work because Röntgen had replicated his experimental set-up.

[51] Grubbé, *X-Ray Treatment: Its Origin, Birth, and Early History*, 39–41.

[52] "LIFE IN THE RAYS.," *Chicago Daily Tribune (1872-1922)*, February 9, 1896.

[53] "THE X RAYS AT WEST POINT: Experiments of Professors of the Military Academy Successful," *New York Times*, February 11, 1896.

"first institution in the south to make successful experiments with the newly-discovered x-ray light."[54]

Röntgen did a public exhibition of x-rays on January 23, 1896, inviting his University of Würzburg colleague, anatomist Albert von Kölliker, to come onstage and have an x-ray photograph made of his hand. To a cheering audience, Kölliker proposed replacing the "x" in "x-rays" with "Röntgen." In the technology's early days, neologisms for the technology had begun to proliferate. Some called the new pictures "skiagraphs" for "shadow photographs." Those who saw x-rays as a new form of cathode ray referred to "cathodographs"; "radiographs," "shadowgraphs," "electro-photographs," "skotographs," and "ultra-actinographs" also had adherents.[55] "Röntgen," however, proved the most durable of the various suggestions, and the term—Anglicized as "roentgen"—came to dominate discussion of the new technology, not only for the x-rays, or "roentgen rays," themselves, but as a descriptor for the study ("roentgenology") and users ("roentgenologist") of x-rays.[56]

Röntgen's demonstration, soon repeated for Kaiser Wilhelm II, augured a trend. In March, Edison announced the creation of a new, much brighter version of the fluorescent screens, using calcium tungstate in place of the barium-platinum-cyanide mixtures; Edison dubbed his device the "fluoroscope." In May, he set up the apparatus to give real-time public demonstrations of x-ray imagery at New York's Electrical Exposition. Sightseers entered a darkened room in groups, formed a line, and placed their hands, one by one, between the fluorescent screen and the emitter. A reporter from the *New York Times* noted that women had an easier time believing that the demonstration was not a hoax, since "most of the women wore

[54] "CLAIM TO BE FIRST," *The Atlanta Constitution (1881-1945)*, February 19, 1896.
[55] Heber Robarts, "Editor's Notes," *The American X-Ray Journal* 1, no. 4 (October 1897): 107.
[56] Bleich, *The Story of X-Rays from Röntgen to Isotopes*, 3–5. Röntgen, for his part, always referred to his discovery as "x-rays," and the "roentgen" terminology eventually fell out of use in the English-speaking world, replaced by "x-ray" and words derived from "radiation," like "radiology/-ist."

rings, and the margin between the bones of the fingers and the rings was too obvious to admit of skepticism."[57]

With the public clamoring to see the new technology in action, journals aimed at scientifically minded audiences soon featured advertisements for x-ray apparatus, including models designed specifically for "lecturers" who would use the machines as part of performance demonstrations.[58] Boston's L. E. Knott Apparatus Company offered a full set-up, including fluoroscope, for around $200 (approximately $5,000 in 2013 dollars)—expensive, but not out of reach for dedicated enthusiasts.[59] Business in the fledgling industry was apparently brisk; by the end of February, would-be experimenters in Philadelphia were confronting a Crookes tube shortage in the city.[60] A professor at Chicago's Armour Institute, frustrated at the high cost of the tubes (which sold for between $6 and $20 apiece), experimented with repurposing incandescent bulbs as makeshift emitters.[61]

The potential to use the new technology for medical diagnosis was obvious to anyone who saw an x-ray image of living bone. The initial wire report that ran in the *New York Tribune* informed readers that, "the results need confirmation, but it seems probable that the report may be of great importance in surgery." Recalling a controversy that many readers would remember—the failure of doctors to find and remove an assassin's bullet from President James Garfield in YEAR—the reporter noted that, "In President Garfield's case it would have been possible by this new method to ascertain by the camera where the bullet was without the use of

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[57] "The New Field Opened by Edison: How the Inventor Solved the Vacuum Problem--Production and Powers of the Fluoroscope," *New - York Tribune (1866-1899)*, March 29, 1896; "FLUOROSCOPE A SUCCESS: MR. EDISON'S INVENTION SHOWN AT THE ELECTRICAL EXPOSITION Visitors Ushered into a Darkened Room, "Where They Placed Their Hands Behind the Screen and Saw Their Bones--One Woman Touched the Wires and Received a Slight Shock--A Talk with the Inventor--Other Attractions of the Show," *New York Times*, May 12, 1896.

[58] Hessenbruch, "Calibration and Work in the X-Ray Economy, 1896-1928," 397–98.

[59] L. E. Knott Apparatus Company, "Apparatus and Methods for Practical Use of X-Rays. 3rd Ed., 1896" (Boston, MA, n.d.), 18.

[60] Kevles, *Naked to the Bone: Medical Imaging in the Twentieth Century*, 23.

[61] "HE CHEAPENS THE BULBS.," *Chicago Daily Tribune (1872-1922)*, February 19, 1896.

the probe or the inductive balance."[62] The first *New York Times* article on the subject claimed, incorrectly, that "Prof. Routgen [sic] has already used his process to detect the exact location of bullets in gunshot wounds," before opining that x-rays would bring about "a transformation of modern surgery by enabling the surgeon to detect the presence of foreign bodies of whatever kind in any part of the human body."[63]

X-rays and human bodies

Interestingly, x-ray users began to suspect that the technology had *therapeutic* applications almost as quickly as they recognized its diagnostic potential. To understand why, it helps to return to the Chicago roentgenologist, Émil Grubbé. In the course of his experiments with the hatbox fluoroscope, Grubbé's left hand, subjected to long x-ray exposures while he tested and tweaked apparatus, became inflamed and painful. It swelled "to twice its normal size" and looked "as though it had been scalded"; hair on the skin fell out, and "blebs and blisters" developed.[64] Other experimenters suffered similar problems. In his search for a brighter fluorescent material to use in his fluoroscope, Thomas Edison had primarily relied on his assistant, Clarence Dally, to conduct tests by creating different screens, holding up his hand between each screen and an energized emitter, and judging the clarity and brightness of the resulting image. Dally, like Grubbé, described hands "badly burned" or "scalded," swollen to the point that he could no longer work, and constantly painful.[65]

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[62] CABLE TO THE TRIBUNE, "CHAMBERLAIN A HERO"; Kevles, *Naked to the Bone: Medical Imaging in the Twentieth Century*, 9–13. See Kevles for an excellent discussion of the Garfield case and medical technology at the end of the nineteenth century.
[63] "HIDDEN SOLIDS REVEALED."
[64] Grubbé, *X-Ray Treatment: Its Origin, Birth, and Early History*, 44–45.
[65] "C. M. Dally Dies A Martyr to Science: Was Burned While Experimenting with X-Rays," *New York Times (1857-1922)*, October 4, 1904.

"New and Bewildering Problems": X-ray Dermatitis

Both Grubbé and Dally were experiencing x-ray "burns." X-rays, like other forms of ionizing radiation, damage living tissue, and the damage manifests in both short- and long-term ways. Though they are significantly higher energy on the electromagnetic spectrum, x-rays initially affect human skin in a fashion similar to ultraviolet radiation, causing a sunburn-like reddening of the skin; the medical term for this condition is "erythema." Erythema will fade if the x-ray exposures cease, but long or repeated low-level exposures can cause the affected skin to develop a lasting bronzed or tanned quality. When more significant damage occurs, the affected skin becomes painfully inflamed and may blister—a condition known as "dermatitis." Hair on the affected area falls out, the skin can take on a rough, fissured appearance, and persistent ulcers develop. In extreme cases, a patient exposed to high levels of x-ray radiation will develop radiation sickness, including anemia, from the destruction of blood-cell producing bone marrow, nausea, and general weakness. These effects appear within days or weeks of a damaging x-ray exposure, and most of the symptoms will heal or disappear with enough time. But x-ray exposure can also cause longer-term harm, permanently damaging skin and other tissues. Sometimes the short-term effects simply never go away. Ulcers refuse to heal, or they reappear, and changes in the color and texture of skin can persist long after the other visible effects disappear. Often, the worst damage remains invisible: mutations and tissue damage that lead to aggressive cancers in the affected regions.[66]

Most early emitters had no shielding; anyone working with such tubes would be exposed to x-ray radiation. In an early photo of x-ray work printed in the *Archives of Clinical Skiagraphy*, the glass-bulb vacuum tube hangs in clear view, just a few inches over the patient's

[66] William James Storey Bythell and A. E. Barclay, *X-Ray Diagnosis and Treatment: A Textbook for General Practitioners and Students* (London: H. Frowde, 1912), 117–19; Bleich, *The Story of X-Rays from Röntgen to Isotopes*, 150.

leg, and the doctor's hand appears right beside it, manipulating the apparatus. In his advice to readers, x-ray advocate Sydney Rowland offered guidelines on matters such as the proper color of an active tube—"one-half is fluorescing a yellowish green and the other half remains comparatively dark"—that made clear his expectation that users would work in close proximity to live emitters. Moreover, the vagaries of operating these devices meant that early x-ray photos often involved lengthy exposure times, ranging from ten minutes to an hour or more.[67]

Under these conditions, erythema and dermatitis, like that experienced by Grubbé and Dally, inevitably developed into a near-universal experience amongst early roentgenologists, as they arranged subjects, manipulated apparatus, and performed other tasks without either turning off their devices or using protective shielding, and reports of the problem began to trickle into various publications beginning as early as the summer of 1896, just months after Röntgen first published on the subject of x-rays.[68] And, to be clear, public reports of possible x-ray harm went well beyond a few stories of redness or minor skin damage. The *British Medical Journal*'s (*BMJ*) 15 August 1896 issue, in its digest of the international medical literature, reported a case from Germany of "a young man, aged 17, in whom repeated experiments were carried out for the purpose of investigating the new photography." After developing "a slight diffuse redness," a patch of epidermis "as large as a plate" sloughed off the subject's back, leaving behind a patch of exposed dermis with "hæmorrhages and exudation [sic]."[69]

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[67] For a reader with even an inkling of the dangers, reading the captions on early x-ray photographs can be an wrenching experience. The first two plates published in the *Archives of Clinical Skiagraphy*, for instance, show an x-ray image of the torso and head of a three-month old child that, according to the caption, represented a 14-minute exposure. Sydney Rowland, "Introduction," *Archives of Clinical Skiagraphy* I, no. 1 (April 1896): 11; Sydney Rowland, "Description of Plates & Answers to Correspondents," *Archives of Clinical Skiagraphy* I, no. 1 (April 1896): 23, Plates I & II.

[68] Herzig, "In the Name of Science: Suffering, Sacrifice, and the Formation of American Roentgenology," 566.

[69] "An Epitome Of Current Medical Literature," *The British Medical Journal* 2, no. 1859 (August 15, 1896): 25–28.

In October, the journal *Nature* ran a letter to the editor from "S. J. R.," one of the aforementioned x-ray "lecturers." The writer worked at the "X-rays Syndicate, Indian Exhibition," in Earl's Court, London, where he spent several hours each day conducting demonstrations with a fluorescent screen. Concerning "the effect of repeated exposure of the hands to the x-rays," S. J. R. wrote that the effect, "though perhaps interesting from a medical and scientific point of view, has been most unpleasant and inconvenient to myself." The writer went on to describe symptoms "similar to an acute sunburn," including blisters, massive swelling, the loss of his nails and several layers of skin, and constant pain.[70]

Another medical case, written up by Henry C. Drury under the heading "Dermatitis Caused by Roentgen *X* Rays," appeared in the November 7, 1896 issue of the *BMJ*; an update appeared a year later, in the June 26, 1897, issue of *The Lancet*. The patient, a 35-year-old male, came to his physician, "Dr. Apostoli," with a possible kidney stone. The physician hoped to produce an x-ray photograph of the stone (if it existed), and the patient sat for two x-ray photography sessions, on May 22nd and 28th. The first exposure lasted between forty minutes and an hour; the second session lasted about ninety minutes. After both sessions, the patient reported feeling nausea, and following the second exposure, "an erythema appeared…followed by vesication and an abundant serous discharge." Eventually "the whole of the affected surface sloughed," and the unfortunate subject reported "a severe burning pain." The affected area took approximately a year to heal.[71]

Editor W. S. Hedley obliquely raised the subject of dermatitis in the July, 1897, inaugural issue of the *Archives of the Roentgen Ray*, when his state-of-the-field essay described "an effect

[70] S.J.R., "Letters to the Editor: Some Effects of the X-Rays on the Hands" 54, no. 1409 (October 29, 1896): 621.
[71] Henry C. Drury, "Dermatitis Caused By Roentgen X Rays," *The British Medical Journal* 2, no. 1871 (November 7, 1896): 1377–78; "A Case of Severe Dermatitis Following Two Exposures to the X Rays.," *The Lancet* 149, no. 3852 (June 26, 1897): 1759.

(so far as is yet known, usually an injurious one) on the living organism" produced by x-ray exposure.[72] Stories of x-ray harm became a virtual touchstone for the journal. The second, November, issue included a transcript of remarks by Roentgen Society President Silvanus Thompson. Noting that "exposure to [x-rays] frequently resulted in severe local inflammation of the skin," accompanied in some cases by hair loss, and acknowledging the reports of some roentgenologists that subjects had suffered harm "even deep-seated within the tissues" or "on the side of the body furthest from the Cookes's tube," Thompson opined that x-rays "brought new and bewildering problems" to medicine.[73] In February, the *ARR* carried a transcript of William Webster's remarks to the Roentgen Society, which described Webster's personal experience of dermatitis: "in October 1896, while heating the tube, my right hand was attacked by the rays similarly to sunburn, and afterwards, by constant exposure, the nails of the thumb and second finger died, being replaced by new growths."[74]

X-ray Therapy: Harm as Hope

Given the ubiquity and severity of the dermatitis experience, it seems reasonable to propose that roentgenologists would have begun to regard their equipment with some measure of caution. In reality, the experience of dermatitis had the opposite effect, as shown by Grubbé's case. Seeking relief from the pain in his hand, the lecturer-student consulted with colleagues in the Hahnemann Medical College faculty lounge. Grubbé's instructors and colleagues offered a few suggestions to address the symptoms in his hand, but several of them, upon learning that it resulted from exposure to x-rays, also made another suggestion: "any physical agent capable of doing so much damage to normal or healthy cells and tissues might offer possibilities, if used as

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[72] W. S. Hedley, "Roentgen Rays, A Survey, Present and Retrospective," *Archives of the Roentgen Ray* II, no. 2 (July 1897): 12.
[73] "The Roentgen Society (11/5/1897 Meeting)," *Archives of the Roentgen Ray* II, no. 2 (November 1897): 28.
[74] Webster, "'Practical X-Ray Work,' a Paper Delivered to the Roentgen Society," 53.

a therapeutic measure, in the treatment of pathologic conditions for which irritative, blistering, or even destructive effects might be desirable."[75]

In such statements lurked several schools of nineteenth century medical thought. Doctors in this period made use of a variety of chemical purgatives, blistering agents, diuretics, and known poisons as therapeutic interventions, although physicians explained their use of mercury compounds, mustard plasters, and other chemicals with a wide variety of medical theories. In "heroic medicine," which included both the Galenic tradition of imbalanced body humors and Benjamin Rush's theory that all fevers derived from a single root phenomenon related to over-excitement in the body, heroic treatments helped with the evacuation of presumed excess or noxious substances from the body.[76] The heroism came from the patient, who would have to endure the tribulations treatment—the diarrhea, vomiting, blisters, and the like—in order to regain health. Heroic medicine had already begun to disappear by 1896, but their particular affiliation with the Hahnemann would have associated Grubbé's teachers with the competing theoretical approach of homeopathy, which employed harm-causing chemicals under the "like cures like" philosophy of homeopathy (although homeopaths generally prescribed extremely diluted preparations of such agents).

Interest in the destructive effects of x-rays also hints at an important shift taking place in American medicine at this time. By 1896, growing acceptance of germ theory and new ideas about infection was causing physicians to rethink both their purpose and their strategies of treatment.[77] Though it posited a completely different cause for disease and infection, germ theory did not obviate all of the old tools of heroic treatment. In particular, caustic or

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[75] Grubbé, *X-Ray Treatment: Its Origin, Birth, and Early History*, 44–45.
[76] Baker, *Before Bioethics*, 78–82; for a more in-depth discussion of Rush's theories, see also Lester S. King, *Transformations in American Medicine: From Benjamin Rush to William Osler* (Baltimore: Johns Hopkins University Press, 1991), 49–55; see also Warner, *The Therapeutic Perspective*.
[77] For a full discussion of germ theory, see Tomes, *The Gospel of Germs*.

destructive remedies seemed to hold out new possibilities to physicians; diseases caused by microorganisms ("germs") might respond to treatments aimed at killing or weakening those same germs. Joseph Lister offered a version of this principle in his famous paper, "On the Antiseptic Principle in the Practice of Surgery," when he credited Pasteur with demonstrating that infections resulted not from exposure to the atmosphere, but rather from "minute organisms suspended in it." For Lister, this revelation led to the idea that infection might be avoided or treated "by applying as a dressing some material capable of destroying the life of the floating particles," such as carbolic acid – what became known as antisepsis or antiseptic treatment.[78]

Grubbé's account does not make clear which medical theories his colleagues relied on in evaluating the possible therapeutic value of x-ray exposure. No matter which approach one used, however, the end result was that ethical, well-intentioned people could reasonably conclude that dermatitis signaled, under the right conditions, a possible benefit, rather than a drawback or hazard, of x-ray technology. That starting place made it almost inevitable that experimentation with x-rays would continue, with resulting harm to patients, since damage served as a legitimate proof of therapeutic potential.

Inspired by some combination of the harm-to-heal hypothesis, two of the Hahnemann physicians, in February of 1896, sent patients to Grubbé for experimental treatment with x-rays. One of the patients, "R.Lee" suffered with late stage, terminal breast cancer. The other patient, "A. Carr" suffered from *lupus vulgaris* spread across one side of his face. Carr's case, in particular, exemplified the mix of new and old ideas about treatment. He reported having endured multiple "escharotic" and "heroic" interventions, including carbolic, acetic, and chromic acid treatments, several applications of silver nitrate, curettage (attempting to scrape off the

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[78] Joseph Lister, "ON THE ANTISEPTIC PRINCIPLE IN THE PRACTICE OF SURGERY.," *The Lancet* 90, no. 2299 (September 21, 1867): 353–56.

diseased tissue), and "actual cautery once." This last procedure had left "scarred areas and thick band-like contractures" which "interfered with control of the lips in talking and caused considerable disfigurement."[79] Carr's case also suggests how heroic medicine had created a dynamic that could work against the interest of experimental patient-subjects: confronted with the heroic alternatives, experimental x-ray treatment with resulting dermatitis looked like a relatively small price to pay for possible healing.

Grubbé invited colleagues to send their patients to his "laboratory"—actually a workspace carved out of the factory building that Grubbé used for his assaying, refining, and vacuum-tube manufacturing business. Amidst the "dust and noxious gases" Grubbé set up "an improvised operating table" for a patient to lie on, "made as comfortable as could be with the aid of all the paper, cloth and leather items available in the laboratory." For treatment, Grubbé used a basic Crookes tube; since he could not precisely direct the x-rays on a particular treatment area, the would-be x-ray therapist settled for proximity, suspending the emitter a few inches from the diseased area. Having personally experienced the painful symptoms of x-ray dermatitis, Grubbé used "a number of pieces of sheet lead, taken from the inside of Chinese tea chests," to drape "the healthy parts adjacent to the diseased area" in an effort to protect them. Then he powered up the apparatus, filling the space with "the sputtering produced by the contact pieces of the mechanical interrupter" and generating an "apple-green glow or light in that half of the Crookes tube which was nearest the [patient]."

For both Lee and Carr, Grubbé settled on a treatment regimen of daily, hour-long exposures, to continue "until cumulative effects, as would be shown in the development of dermatitis, made their appearance." Unfortunately, both patients died within the month—Lee of "systemic carcinosis" and Carr of a bad fall, which fractured his skull—but both patients,

[79] Grubbé, *X-Ray Treatment: Its Origin, Birth, and Early History*, 46–53.

according to Grubbé, reported that x-ray treatment decreased their pain in the treatment areas. It is impossible to know for sure whether or not Lee and Carr benefited from radiation therapy or from a placebo effect. In the case of Lee, in particular, it is entirely possible that long x-ray exposures could have shrunk her tumors, but it is equally possible that the radiation exposure worsened her overall health. Lee's physician sent her to Grubbé in part because he regarded the case as "absolutely hopeless"; Lee was "losing weight," with detectable cancer throughout her body, "considerable swelling" and pain in the left arm, and a complexion turned "a muddy grey." Nevertheless, Grubbé found the results of his therapeutic experiment more than satisfactory. In February of 1896, he opened an x-ray therapy practice "near the college of clinic," with an entrepreneurial eye toward "the source of the supply of patients."[80]

According to Grubbé's account, R. Lee began receiving x-ray therapy on January 29, 1896, just a month after news of Röntgen's work appeared in the papers; based on this assertion, Grubbé explicitly claimed priority as the first person to experiment with x-ray therapy. As a source, Grubbé's memoir remains somewhat problematic. His description of the events on January 29th, for example, begins with a lengthy internal monologue that includes such statements as, "Up to the present time the x-ray has been used only to *find* disease…but now it might also offer possibilities in the *cure* of disease" (italics in original).[81] It would have made little sense for Grubbé to have actually held or expressed such sentiments in January of 1896; x-rays were certainly not in widespread use for finding disease a mere month after the public debut of the technology. Moreover, despite his later assertions of priority, Grubbé did not publish any accounts of the cases until after he received his medical degree, in 1898.[82] Even if

[80] Ibid., 51–55.
[81] Ibid., 49.
[82] According to Grubbé, he could not "make a clinical report or write a paper giving the details, because [he] had not yet graduated as a physician and consequently did not have access to the medical journals." He may have underestimated the possibility of access, for reasons discussed below. Ibid., 55.

correct, however, the priority claim never amounted to much; neither Grubbé nor anyone else received anything like the status accorded either to Röntgen or to other pioneers of treatment technology, like Robert Jarvik, because so many individuals undertook and published similar experiments in x-ray therapy at more or less the same moment as Grubbé.

In fact, the real value of Grubbé's story lies not, as he believed, in its exceptional character; rather, the Grubbé narrative stands in as a sort of parable, typifying the early world of experimental x-ray therapy in almost every element, from the experience of dermatitis to the selection of test subjects and ailments, the economic interest and experimental treatment methodology of the researcher, and even Grubbé's quasi-professional status in the medical profession. From a historical point of view, the story also raises interesting questions about the ethical dilemmas and judgments of practitioners, like Grubbé, engaged in what amounted to unregulated human experimentation. These questions would eventually be taken up by practitioners themselves, many of whom, unlike Émil Grubbé, saw themselves as members of the professional medical community.

Therapeutic Experimentation Around the World

Grubbé probably overestimated the novelty of his early x-ray therapy partly because he remained cut off from the larger community of roentgen enthusiasts. The confluence of medical theories of therapeutic harm with the ubiquity of the dermatitis experience amongst roentgenologists led to widespread interest in the therapeutic possibilities of x-rays. In February of 1896, just as Émil Grubbé was setting up his first x-ray clinic, a story ran on the front page of the *Chicago Tribune* proclaiming "Life in the Rays: Roentgen Process Revives Mouse Supposed to be Dead." The story described the report of a Columbia professor that x-ray exposure had revived a drowned mouse.[83] Although the *Tribune* debunked the story a day later, its reporter found several physicians who noted the germicidal power of sunlight and suggested that x-rays might prove useful as a sort of sunlight that could pass through the body.[84]

By 1897, x-ray practitioners in Italy, France, Vienna, and New York had already published on their attempts to retard the growth of tuberculosis, ameliorate cancers of the mouth and stomach, remove unwanted body hair, and treat "a patient suffering from hectic, with congestion, inflammation, and suppuration of the lungs or pleura of uncertain origin." Although the particulars of apparatus varied, the basic treatment remained the same from researcher to researcher: long exposures, with the vacuum-tube emitter placed near the afflicted area of the body; the "hectic" patient, for instance, "was daily submitted for fifty minutes to the action of the rays."[85] In essence, anyone with enough technical knowledge to acquire and operate the equipment could replicate the technique of x-ray therapy simply by asking the patient to describe the complaint and repositioning the vacuum tube. This simplicity of method, in combination with the ready availability of apparatus, distinguished the x-ray emitter from other new medical technologies of its time and enabled x-ray therapy to spread quickly relative to other medical innovations. It also democratized the therapeutic process, enabling individuals without traditional medical training to experiment with treating patients—a fact which would have significant long-term implications for the professionalization of roentgenology as a specialist medical discipline.

Inevitably, given the many different types of users conducting their own experiments with x-ray therapy, reports of the results of x-ray treatment varied wildly, giving rise to a durable controversy amongst roentgenologists over the efficacy and value of x-ray treatment and

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[83] "LIFE IN THE RAYS."
[84] "NO LIFE FOR THE DEAD.," *Chicago Daily Tribune (1872-1922)*, February 10, 1896.
[85] Hedley, "Roentgen Rays, A Survey, Present and Retrospective," 12.

therapeutic experimentation. One side advised caution and tended to regard reports of therapeutic success with skepticism. Summing up the state of the field in July of 1897, W. S. Hedley warned readers that "Roentgen ray therapeutics rest as yet on the slenderest possible foundation." Hedley argued that it boded ill for the possibilities of x-ray therapy that, in spite of the relative ease of assembling emitters and the large number of reported experiments, no one had yet definitively established the value of x-ray therapy.[86] That same month, the *British Medical Journal* ran a pair of short stories on the experimental disappointments of x-rays. "'X' Rays and Their Physiological Effects" opened with a lament that "the further investigation is carried into the vital effects of the Roentgen rays, the more obscure and less defined these effects seem to be," and concluded that, "at present, the whole position might be described in the words of a well-known physiologist as consisting of 'guesses and gaps.'" An article by Sydney Rowland on "'X' Rays and Tuberculosis" in the July 31, 1897, issue of the *BMJ* informed readers of experimental work by Paris therapist "M. Gariel," who "subjected five patients to the action of the rays" in hopes of treating pulmonary tuberculosis. Commenting on the failure of the treatment, Rowland noted that "there are already several cases recorded in which it has been sought to determine a bactericidal action of the x-rays, and in none of them has positive effect been observed."[87]

On the other side of the debate, x-ray optimists and boosters claimed not only that x-rays could provide relief from a variety of diseases and forms of human suffering, but that x-ray users had already seen the results of the new technology at work. The *Journal of the American Medical Association* reported in its January 23, 1897 issue that experiments had "already shown" that x-ray illumination could be used to bypass cataracts, making it possible to "imprint

[86] Ibid.
[87] "'X' Rays And Their Physiological Effects," *The British Medical Journal* 2, no. 1909 (July 31, 1897): 298; Sydney Rowland, "'X' Rays And Tuberculosis," *The British Medical Journal* 2, no. 1909 (July 31, 1897): 298–99.

visual images upon the retinae of persons a healthy nervous optic apparatus.” On the subject of tuberculosis, the January, 1898, issue of *The American X-Ray Journal* ran a short story on experiments using guinea pigs infected with tuberculosis. Claiming to have successfully treated the animals using daily, hour-long x-ray exposures, the authors concluded argued that x-ray therapy should immediately be deployed in “human beings, especially children, with superficial tuberculosis, limited to the pleura, or with tuberculous ganglia in the mesentery.” In November, the *Archives of the Roentgen Ray* reprinted a pair of papers from the German *Fortschritte der Röntgenstrahlen* (*Advances in X-rays*) on the successful treatment of lupus vulgaris and eczema by repeated x-ray exposure.[88]

This particular set of claims reflected the variation in both the promises made by x-ray advocates and their relative success or failure. Claims that x-ray exposure could mitigate or cure blindness never came to fruition, but the basic idea of using radiation to bypass dysfunctional eyes remained popular, such that when doctors began to experiment with radium, some of the first experiments involved strapping radium sources to the heads of the blind in hopes of restoring sight.[89] The tuberculosis claim, meanwhile, provoked a long back-and-forth on the subject. Tuberculosis treatments had a particularly fraught history owing to the tuberculin debacle, and other researchers immediately undertook their own experiments with tuberculosis. The matter came to a head at the 1898 Congress of Tuberculosis, in Paris, when

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[88] “The Employment of X Rays for the Relief of Some Forms of Blindness,” *The Journal of the American Medical Association* XXVIII, no. 4 (January 23, 1897): 180; “Experimental Tuberculosis Attenuated by the Roentgen Ray (Reported from Article First Appearing in the Bulletin Medical),” *The American X-Ray Journal* 2, no. 1 (January 1898): 158; “The Treatment of Lupus (Reprint of Paper by Albers-Schönberg, First Printed in Fortschritte Der Röntgenstrahlen, II.20),” *Archives of the Roentgen Ray* III, no. 2 (November 1898): 55–56; “The Treatment of Eczema and Lupus with X Rays (Reprint of Research First Appearing in Fortschritte Der Röntgenstrahlen, II.16),” *Archives of the Roentgen Ray* III, no. 2 (November 1898): 56.

[89] “Sight Restored by Radium Rays: Blind Child Sees Again After Treatment,” *Los Angeles Herald*, August 24, 1903; for a more complete discussion of this story, see Chapter 2 and Lavine, *The First Atomic Age*.

several researchers reported results that thoroughly debunked the original finding.[90] The use of x-rays to treat skin diseases had by far the most success of the three. X-ray therapy would, in the short term, prove successful as a treatment for several types of skin conditions and infections, an apparent miracle for patients that had, as previously discussed, faced therapeutic options that amounted to torture while offering little hope for success. In reality, such therapy had devastating long-term consequences, as shall be discussed below, but in the early years, lupus would serve as the story on which x-ray boosters could hang their hopes—a real and relatively painless cure for a disease that had caused widespread suffering and resisted previous efforts at treatment.

X-ray Promises

Tuberculosis and lupus vulgaris represented the two extreme possibilities for x-ray therapy—debunked junk science versus revolutionary treatment—but most of the frustration and controversy in the fledgling field resulted from claims more in the mold of the neither-proven-nor-refuted theory that x-rays could treat blindness. The key, often, was that proponents of x-ray therapy framed their claims in a way that strongly promised results without committing to a falsifiable outcome. One strategy involved layering the claim with caveats or framing it against other, more grandiose promises. The *JAMA* story on using x-rays as a workaround for cataracts offers a good example of the genre. The anonymous author opened by warning readers against newspaper stories promising “that the blind might be made to see by means of the x-rays.” Only after warning that “it was not expected by intelligent minds that

[90] “An Epitome of Current Medical Literature: Congress of Tuberculosis (Paris, 1898),” *British Medical Journal* 2, no. 1964 (August 20, 1898): E29–32; the rapid and vociferous debunking of the x-ray tuberculosis treatment was almost certainly fueled by the fresh memory of the tuberculin debacle. X-ray practitioners were certainly aware of the problem, as will be discussed later in this chapter. See “Redemption, Danger, and Risk: The History of Anti-Bacterial Chemotherapy and the Transformation of Tuberculin,” by Christoph Gradmann, in Thomas Schlich and Ulrich Tröhler, eds., *The Risks of Medical Innovation: Risk Perception and Assessment in Historical Context*, Routledge Studies in the Social History of Medicine 21 (London ; New York: Routledge, 2006).

vision of any kind could be restored when atrophy or degeneration or other form of destruction of optic nerves" had caused the condition did the author suggest that x-rays might make it possible for blind persons to read documents and see specially treated images. The caveat—x-rays cannot cure *all* blindness—served to make the central claim sound more reasonable, and explained away what otherwise seemed to be therapeutic failures.[91]

The *JAMA* article also demonstrated another trope common to claims about x-rays: the vague allusion to successful experiments conducted elsewhere. With regard for using x-rays to bypass cataracts, the author assured his audience that "experiments have already shown the idea to be theoretically correct and practically feasible," though no citations were provided. Writing just a few months later, in May of 1897, it seems entirely possible that Heber Robarts had this very claim in mind when he mentioned to readers that "the application of the x-rays has been used with success in certain cases of the blind." A third version of the story turned up a few months later in London, where Silvanus Thompson, speaking to an audience, retraced the steps again. He first noted that attempts to treat blindness with x-rays had "been investigated already in a number of cases, but hitherto with no real success." Thompson repeated again the caveat that, in regard to the hope of therapeutic intervention, "where blindness is due to destruction of the optic nerve such hope is utterly in vain." But then he made the turn, suggesting that, despite the fact that "almost every case examined has given negative results," Thompson held out hope, having heard recently about an experiment in which "an intelligent man who had become blind from paralysis" could "distinguish the form and position of a Crookes's tube, and could even distinguish letters cut in stencils placed over the tube."[92]

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[91] "The Employment of X Rays for the Relief of Some Forms of Blindness."
[92] Ibid.; Heber Robarts, ed., "Announcement," *The American X-Ray Journal* 1, no. 1 (May 1897): 1–2; "The Roentgen Society (11/5/1897 Meeting)," 28.

With each retelling, the story about using x-rays to bypass cataracts and blindness picked up momentum. Many other promises and claims about x-ray therapy gained credence in a similar fashion; indeed, the volume and variety of claims made x-ray advocates sound as if they had lifted language from the advertisements for tonics and nostrums that dotted the pages of contemporary newspapers and medical journals. For both good and ill, however, x-ray therapy had two major advantages that separated it from other types of dubious "miracle" treatments. First, a bit of "raying" (in the words of some therapists) might or might not heal gout or treat depression, but it clearly did *something*. Anyone with a fluoroscope or a photographic plate could produce images of living bone with reasonable ease, and besides erythema and dermatitis, x-rays would reliably cause hair to fall out.[93] Secondly, an international, professionalized community of practitioners collectively backed x-ray therapy; the fact that roentgenologists had organized themselves similarly to other professional medical and scientific communities increased the legitimacy of the discipline as a whole. In this area, roentgenologists benefited from the efforts of other medical societies, such as the American Medical Association, which had devoted considerable effort to policing their own ranks and raising the status of the medical profession by driving out illegitimate practitioners. But although roentgenological associations adopted the same professional structures as other groups of doctors, roentgenological societies diverged from other medical associations on the question of who should count as a therapist.

Organizing, Proselytizing, and Professionalizing

New technology cannot effect transformation on its own; tools only matter insofar as people use them. For many doctors and surgeons, the first exposure to x-ray technology and its

[93] This last fact made hair removal one of the most popular and durable types of x-ray treatment, with tragic results for many patients. See Herzig, "Removing Roots."

possibilities came through the medical press. At first, medical publications like the *British Medical Journal* and the *Lancet* ran general-interest stories on the discovery of x-rays with more or less the same content and tone as those appearing in the non-medical press. In both the United Kingdom and America, however, x-ray enthusiasts successfully took control of the medical coverage of the new technology and turned it to their own ends. Working both through the traditional medical press and through their own, specialist organs, advocates like Sydney Rowland and Heber Robarts proselytized on behalf of the new technology, encouraging doctors to use x-ray emitters in their practice, disseminating basic technical knowledge to x-ray adopters, and attacking the technology's critics. Both men also used the medical press as a vehicle for organizing x-ray users to create a new kind of professional, international medical community, bound together by interest and expertise in technology, rather than in the human body. And as the hazards of x-ray exposure grew increasingly apparent, the organizations and their attendant journals would provide the forum for vigorous debates over the ethics of human experimentation.

X-rays and Medical Journalism

On January 11, 1896, the *Lancet* became the first of the London medical press to pick up the x-ray story, running a short, tongue-in-cheek piece, "The Searchlight of Photography." A week later the *British Medical Journal* (*BMJ*) published a report from physicist Arthur Schuster, to whom Röntgen had sent a copy of his original paper. The two journals had an ongoing rivalry, and *BMJ* editor Ernest Abraham Hart saw expanded x-ray coverage as an opportunity to generate publicity. Hart recruited his nephew, Sydney Rowland, as the journal's "Special Commissioner for Investigation of the Applications of the New Photography to Medicine and Surgery." In 1895, Rowland had completed his medical studies at Cambridge and secured an appointment at St. Bartholomew's Hospital, which housed the U.K.'s oldest electrotherapy department. Rowland was, according to friends, a natural tinkerer, "an extraordinarily good mechanic, and never happier than when devising technical means for attacking some problem or making the apparatus to be used in the investigation."[94] He took to x-ray technology easily, and in February, he began publishing a regular column, "Report on the Application of the New Photography to Medicine and Surgery," that ran in the *BMJ* through June of 1897. Rowland constructed his own emitter, and the journal began regularly reproducing x-ray photographs, both from Rowland and from other contributors, in glossy spreads.[95] Rowland, however, had much larger ambitions: he saw x-ray technology as the foundation of a new field of medical practice, in need of its own print forum.

A key change in the nineteenth-century medical landscape had been the rapid expansion of the medical press: in the United Kingdom, for example, the number of medical journals tripled between 1800 and 1840. By the end of the century, the United States, though still regarded as lagging behind the centers of Europe, would boast some two hundred seventy-five medical journals, with a combined annual circulation of well over 16 million. Weekly publications, like the *BMJ* and *Lancet*, aimed for large circulation numbers by appealing to general medical audiences, but many of the new publications served more specialized functions. Adherents of alternative schools of medical thought had their own journals, as did the practitioners of particular regions and, in a development that would prove significant for roentgenologists, an increasing number of medical specialist societies. The growth of the medical press resulted from a variety of factors. Some, like improvements in mass printing and postal service, were exogenous to medicine. Within the profession, however, the rising

[94] "The War. (Sydney D. Rowland Obit.)," *British Medical Journal* 1, no. 2933 (March 17, 1917): 374–76.
[95] "The Searchlight of Photography," *The Lancet*, Originally published as Volume 1, Issue 3776, 147, no. 3776 (January 11, 1896): 109–12; Arthur Schuster, "On the New Kind of Radiation," *British Medical Journal* 1, no. 1829 (January 18, 1896): 172–73; P. W. J. (Peter W. J.) Bartrip, *Mirror of Medicine : A History of the British Medical Journal* (Oxford: Clarendon Press, 1990), 136–41.

significance of “scientific” medicine—broadly speaking, the production of new medical knowledge through clinical research and other forms of evidence-based inquiry—proved decisive, because it drastically increased the importance of publication.[96]

Scientific medicine placed an emphasis on publication, both to share results and to establish a researcher’s priority to a specific discovery. Professional societies of learning—for medicine, but also for physics, astronomy, chemistry, and so on—proliferated in Europe and the Americas throughout the nineteenth century, and many of these groups published a house journal, often designated as the *Transactions of*, the *Proceedings of*, or the *Reports of* the particular group, as a benefit of membership. In addition to spreading local professional news, these journals gave members of a society a friendly outlet for publication of their results and discoveries. Wilhelm Conrad Röntgen’s original announcement of x-rays, for example, ran in the *Transactions of the Physico-Medical Society of Würzburg*, in a special issue published exclusively for Röntgen’s x-ray paper (he had not yet actually presented a paper to the society). Society journals also provided a sort of wire service to keep members abreast of new developments in their fields.

In his role as the *BMJ*’s x-ray correspondent, Sydney Rowland traveled the United Kingdom, meeting with doctors and physicists and establishing a network of correspondents. Envisioning the creation of a distinct society of x-ray users, Rowland launched a journal billing itself as the *Archives of Clinical Skiagraphy* (*ACS*) in April of 1896, proclaiming that, “although Prof. Röntgen’s discovery is only a thing of yesterday, it has already taken its place among the approved and accepted aids to diagnosis.” The *ACS* embodied an interesting mix of medical and technical professionalism and amateurism. Rowland explicitly designed the *Archives* to resemble

[96] Bartrip, *Mirror of Medicine*, 6–12; “U.S. Medical Journal Circulation Data (Untitled Blurb),” *The American X-Ray Journal* 2, no. 3 (March 1898): 222; Weisz, “The Emergence of Medical Specialization in the Nineteenth Century.”

other professional medical journals, and he expressed his hope in the Preface that the publication would "take a permanent place in medical literature" as a "storehouse of skiagraphic clinical records" for practicing physicians to draw on.[97] The journal opened with an introductory essay that explained Röntgen's experiments and the basics of x-ray technology, and it included a correspondence section, where Rowland answered technical questions in along the lines of "How do I know when my focus tube is working correctly?" and "What material should I use in my fluoroscope?"[98]

The heart of the publication, however, was the section of actual x-ray images, printed on heavy, glossy stock, and protected with interleaved tissue paper, with captions and explanations in the section entitled "Description of Plates." Rowland produced many of the journal's x-ray photographs, or "skiagrams," while working at St. Bartholomew's Hospital, but the publication also contained x-ray images from doctors in Glasgow, Edinburgh, Cardiff, and several major hospitals in the London area. In a clear nod to the medical journal approach, many of the descriptions included case narratives, often partially attributed to the physician who either treated the case or produced the x-ray photograph. In the description of Plate VII, labeled "Hypertrophic Osteo-Sclerosis of Fibula," the opening remarks were attributed to "F. C. Abbott, F.R.C.S., Resident Assistant Surgeon to St. Thomas's Hospital":

> Male, aged seventeen. Admitted to St. Thomas's Hospital under the care of Mr. Clutton. For five months past the right leg has ached after walking and playing football. There is no history of injury. He is not clear when he first noticed the tumour, but it had been getting larger for the last few days before admission to the hospital.[99]

Despite the trappings of a professional medical journal, however, the actual medical value of the *ACS* images is less clear. The aforementioned case, for example, contained a "Note

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[97] Sydney Rowland, "Preface," *Archives of Clinical Skiagraphy* I, no. 1 (April 1896): 3–4. "Skiagraph" was a word coined to translate roughly to "shadow image."
[98] Rowland, "Description of Plates & Answers to Correspondents," 23.
[99] Ibid., 17.

on the Skiagram," from editor Sydney Rowland, which allowed that the x-ray, taken to confirm an initial diagnosis of sarcoma in the bone, had failed to clear up the question "due to our then want of knowledge of the appearance of a sarcoma in a shadow picture." The final diagnosis of "a general fusiform enlargement of the fibula with sclerosis of the bone" arose from surgery, not x-ray photography, and the story of Plate VII was actually a story of the diagnostic failure of the new technology.[100]

Of course, Plate VII and its caption could still have proven useful to physicians trying to diagnostically interpret their own x-ray images of similar bone growths; failure does not automatically obviate the value of a case study. But many of the images and "cases" presented in the *ACS* seem to have been created less for diagnosis than for curiosity. Rowland promised in the preface to show "the most striking applications of the New Photography to the needs of Medicine and Surgery," and his readers probably would have agreed that the *Archives* delivered. An image from Glasgow showed John Macintyre's attempt to produce photographs of the living human heart. Many of the plates focused on anomaly: the deformed hands and feet of a patient under the care of Charing Cross Hospital's Richard Barwell, who sought to produce "for publication a series of skiagrams of foot deformities," a photo of a stillborn, partial double-fetus, from "Prof. Hughes, of University College, Cardiff," who promised to give "a fuller description of this [specimen] before one of the learned societies next winter," a "Skiagram of Six Toes in Each Foot" (although in that case a surgeon reported using the photo "to operate with much greater precision than if he had not had it to guide him"). The journal concluded with a feature section on "Skiagraphy in Zoology" that showed x-ray photos of a lobster, two crabs, and a frog leg.

[100] Ibid., 17–18.

Clearly, the *ACS* walked on both sides of the line between medical professionalism and technological sensationalism.[101]

The *Archives of Clinical Skiagraphy* put an interesting admixture of elements on display. It sought the label of professional medical journal, but its editor had not yet fully entered the profession. It brimmed with technological enthusiasm, but many of the case studies illustrated the limits of that technology. Some of the photos had clear utility for practicing physicians, but others were clearly aimed more at curiosity seekers than medical professionals. In many ways, this stew of the professional and amateur, the medical and sensational, the disparity between expectations and outcomes, and the world where a medical student could become a medical authority through technological expertise, captured the disruptive dynamic of x-ray technology for the medical profession.

Sydney Rowland's belief in the future of x-rays was mirrored on the other side of the Atlantic, where *The American X-Ray Journal* (*AXJ*) launched in May of 1897, promising its readers a monthly survey of "practical x-ray work and allied arts and sciences." Unlike Sydney Rowland and Émil Grubbé, editor Heber Robarts was an experienced physician, having graduated from Missouri Medical College in 1880. Robarts shared with his two colleagues a lifelong enthusiasm and affinity for tinkering and new technology. When the x-ray news broke, Robarts held the position of surgeon of the Great Northern Railroad, in Montana. Robarts immediately left his job with the railroad, returned to St. Louis, and opened an x-ray office in the spring of 1896.[102] *The American X-Ray Journal* struck a more populist and commercial tone than its British counterpart,

[101] Rowland, "Preface," 3; Rowland, "Description of Plates & Answers to Correspondents," 20, 29, Plates XI–XII, XV–XVI.
[102] Percy Brown, "Heber Robarts (1852-1922)," *American Journal of Roentgenology* 165 (1995): 473–76.

with jokes, poetry,[103] and human-interest stories on subjects like "The Banana Cure" and discrimination against women in life insurance contract rates. Nevertheless, Robarts defined the journal's fundamental mission in terms similar to the *Archives.* Citing his "seventeen years of medical and surgical experience," the editor promised "to give to readers and thinkers a faithful resume of all x-ray work done in any portion of the globe . . . imparting x-ray information more appropriate for those who read and think."[104]

The *Archives of Clinical Skiagraphy* and *The American X-Ray Journal* would help nourish an ongoing conversation amongst x-ray users on issues, including eventual debates about the hazards of x-ray exposure, dosimetry, and the ethics of experimental treatments. The publications would also aid in the construction of an international, professional x-ray community. When another issue of Rowland's journal appeared, in July of 1897, the editor had adopted the "roentgen" terminology, rebranding his journal as the *Archives of the Roentgen Ray* (*ARR*). Rowland explained to readers that, "the more comprehensive title indicates the wider field that it is intended to cover," and he marked the connection by listing the first *ARR* issue as "Volume 2" of the series. The new incarnation promised to run on a quarterly basis and serve "not merely as a journal of the new photography, but to some extent as the exponent of an important discovery."[105] Rowland had expanded the *Archives'* reach around the United Kingdom and across the Atlantic, with a nine-member editorial committee that included William J. Morton, of New York, and William White, of Philadelphia.

Together, the *Archives of the Roentgen Ray* and the *American X-Ray Journal* provided a centralized forum the professional discussion of x-rays in the Anglophone world. The two

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[103] "The say microbes are in a kiss,"/ Quoth he—their lips had barely parted. / "I am a homeopath," the miss / Returned in tone not quite faint hearted; / "In 'like cures like' I put my trust." / Whereat their lips again concussed. "Similia Similibus," *The American X-Ray Journal* 1, no. 1 (May 1897): 22.
[104] Robarts, "Announcement."
[105] Sydney Rowland and W. S. Hedley, "Editors' Note," *Archives of the Roentgen Ray* II, no. 1 (July 1897): 3; "The Roentgen Society (6/3/1897 Meeting)," *Archives of the Roentgen Ray* II, no. 1 (July 1897): 5.

journals would operate in dialogue with one another, cross-posting articles, sharing contributors, and publishing comments on one another's work, particularly with regard to controversial issues, like the question of whether or not x-ray exposure could cause harm. The journals had ample material to work with. Researchers around the world—including, of course, Röntgen himself—continued to study and publish on the subject of x-rays at a prodigious rate. For x-ray users attempting to keep abreast of all this new work, the problem of staying current went beyond the usual issues of language skills and journal access. In a reflection of the interdisciplinary interest in the technology, x-ray publications appeared in an unusually wide array of places: scientific journals and society transactions, but also medical journals and publications, and even magazines aimed at amateur photographers or popular audiences. With a mixture of reviews and state-of-the-field essays, the *Archives* and the *American X-Ray Journal* attempted to keep readers informed of these developments. The footnotes from the first state-of-the-field review essay in the *Archives* give the reader a sense of the variety of experimental interest in x-rays, with the cited publications ranging from the *Journal of the Royal Academy of Sciences* and the German *Zeitschrift für Elektrochemie*, to the *Lancet* and the French *Revue d'Électrothérapie*, to the American *Electrical World*.[106]

The author of that state-of-the-field essay was William Snowdon Hedley. Hedley had joined Rowland as the co-editor of the *Archives*, and his influence in the publication would soon exceed that of the journal's founder. Hedley, like Heber Robarts, already had a career as a physician, having served first as a surgeon in the Army Medical Service, and then in civilian practice.[107] By 1895, Hedley was the head of London Hospital's electrotherapy department. Because electrotherapy and roentgenology made use of several of the same pieces of apparatus

[106] Hedley, "Roentgen Rays, A Survey, Present and Retrospective."
[107] "THE SERVICES.," *The Lancet*, Originally published as Volume 2, Issue 2991, 116, no. 2991 (December 25, 1880): 1038.

and required many of the same technical interests and skills, practitioners often crossed back and forth between the two fields, and many of the pioneering roentgenologists would have prior experience in electrotherapy. As a result, roentgenology, as a field, would rise on the foundation laid by electrotherapy.

Specialists or Quacks?

Often, new medical tools or technologies are adopted by doctors as ancillary technical skills. The introduction of forceps, for example, significantly altered obstetric practice, but no one speaks of "forcepology," even for doctors who specialized in their use.[108] In the case of roentgenology, however, a new medical technology became the basis of a new medical specialty, in part because it built on the foundation laid by another medical technology. Electrotherapy, which arose following the nineteenth-century development of reliable power sources and new, safer methods of delivering electric stimulation, gave roentgenologists a model, if not a wholly successful one, of how a new, technologically-based specialty might gain acceptance amongst orthodox, or "regular," medicine.

The trend towards specialization tends to look inevitable from the vantage point of the present, but the growing importance of specialists in medical communities usually prompted conflict, rather than equanimity.[109] The acceptance and professionalization of medical specialists

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[108] For an extensive discussion of the adoption of forceps and their role in changing the practices of childbirth in the United States, see Judith W. Leavitt, *Brought to Bed : Childbearing in America, 1750-1950* (Cary, NC, USA: Oxford University Press, Incorporated, 1988).

[109] To quote Weisz, the literature on specialization requires "a bibliography rather than a footnote." As a starting place, see George Rosen, "Changing Attitudes of the Medical Profession to Specialization," *Bulletin of the History of Medicine* 12 (1942): 343; Rosemary Stevens, *Medical Practice in Modern England: The Impact of Specialization and State Medicine* (New Haven, CT: Yale University Press, 1966); Ornella Moscucci, *The Science of Woman : Gynaecology and Gender in England, 1800-1929* (New York: Cambridge University Press, 1990); Roger Cooter, *Surgery and Society in Peace and War : Orthopaedics and the Organization of Modern Medicine, 1880-1948* (Houndmills, Basingstoke, Hampshire: Macmillan Press, in association with the Centre for the History of Science Technology and Medicine, University of Manchester, 1993); Bruce Fye, *American Cardiology : The History of a Specialty and Its College* (Baltimore: Johns Hopkins University Press, 1996); Weisz, "Medical Directories and Medical Specialization in France, Britain, and the United States"; Weisz, "The Emergence of Medical Specialization in the Nineteenth

was actually a rather novel nineteenth-century development. Previously, orthodox physicians and surgeons successfully resisted specialization, ostracizing specialist practitioners and branding as charlatans and hucksters those who professed "secret" knowledge of the body or a singular focus for therapeutic interventions, such as bonesetters and opthalmologists.[110]

The shift toward acceptance of specialists as orthodox medical practitioners reflected several nineteenth century developments. Partly, specialization reflected a reorientation of medicine that embraced scientific inquiry and advanced technology as important elements of modern medicine. In 1795, a physician could reasonably invoke Galen as a source of medical authority, and the doctor's own five senses remained the gold standard for assessing disease. By 1895, Röntgen's rays represented only the latest in a string of new diagnostic aids, including the sphygmomanometer and thermometer for pulse and temperature, the ophthalmoscope and stethoscope for eyes, heart, and lungs, and even the microscope, for observing a tiny, but growing number of identified pathogens.[111] Moreover, these new approaches to medical research discarded older, whole-body approaches to disease, such as humoral theory, in favor of dividing the body into discrete systems with particular functions and diseases—a shift that obviously favored the growth of specialists.[112]

Century"; George Weisz, *Divide and Conquer: A Comparative History of Medical Specialization* (New York: Oxford University Press, 2006).

[110] Opthalmology (or "oculists"), in particular, featured a disquieting abundance of itinerant practitioners—described in one screed as "irresponsible destroyers and murderers of eyes"—who took sufferers' money in return for what amounted to, at best, incredibly dangerous attempts at kitchen-table cataract-removal surgery. Such "specialists" gave the whole idea of specialization a bad name and led orthodox physicians to prevail upon the state to enter the marketplace in an effort to provide consumer protections. Rosen, "Changing Attitudes of the Medical Profession to Specialization."

[111] For a deep discussion of this seismic shift in medicine, see Warner, *The Therapeutic Perspective*. In Warner's words, "The proposition that experimental science should govern therapeutic practice had fundamental implications for professional identity; the doctor-patient relationship; the professional meanings of the hospital, medical education, and codes of ethics; therapeutic practice; and the very definition of professional orthodoxy" (pg. 258).

[112] Weisz, "The Emergence of Medical Specialization in the Nineteenth Century," 538–39.

Specialization also reflected the growing importance of "administrative rationality" in the nineteenth century at every level, from the individual hospital to the emerging nation-state.[113] The influence of administrative rationality can be seen in how specialists began to configure themselves as communities of professionals, rather than as singular experts, by creating journals and professional organizations. In France, for example, the growing acceptance of specialization directly coincided with the founding in 1839 of *L'Esculape: Journal des spécialités médico-chirurgicales* and the *Revue des spécialités et innovations médicales et chirurgicales*. In the United States and Great Britain, however, orthodox practitioners held out far longer against specialization than their peers in Europe, and the discovery of x-rays, in 1895, took place at precisely the moment when advocates and opponents of specialization were sharpening the knives for some of their bitterest battles.[114]

The conflict over specialization put radiation therapists in a strange position. On the one hand, x-ray therapists, in particular, explicitly described themselves in a way that seemed to fit with the "specialist" paradigm, deriving their legitimacy from the specialized technical knowledge required to construct and operate x-ray emitters. When applied to the treatment of patients, however, actual x-ray therapy looked very different from other forms of specialist practice, both in justification and in fact. Insofar as they accepted the logic of specialization, orthodox physicians in both the United Kingdom and the United States saw the division through explicitly biological terms: specialists could focus on particular body parts or systems—i.e. "ear,

[113] Ibid. Throughout the century, the number of doctors interested in "scientific" medicine grew steadily. Physician-researchers narrowed their practice for the same reasons as other types of experimenters: to enable mastery of the existing knowledge base and focus research. As Weisz points out, "only specialization . . . allowed for the rigorous empirical observation of *many* cases that had become necessary in academic medicine"; generalists simply saw too many different types of maladies to conduct meaningful research. The trend towards specialized labor, knowledge, and expertise, Weisz points out, holds not only in medicine across nations, but more generally across Europe and North America.

[114] Rosen, "Changing Attitudes of the Medical Profession to Specialization"; Stevens, *Medical Practice in Modern England: The Impact of Specialization and State Medicine*; Weisz, "The Emergence of Medical Specialization in the Nineteenth Century."

nose, and throat" doctor—or on particular manifestations of disease. Roentgenologists, by contrast, focused on a particular technology. For a therapist like Emil Grubbé, a tuberculosis infection of the skin and breast cancer had the same solution, even though the two cases involved different diseases, in different organs, located in different parts of the body.

Given their technological focus, electrotherapy offered a clear example of a medical specialty based on equipment and technical knowledge, rather than medical knowledge. There was a significant overlap between electrotherapy and radiation therapy in the early days, in part because many x-ray therapists started out as electrotherapists. But the practice of radiation therapy dramatically diverged from electrotherapy over time, in part because manufacturers simplified x-ray apparatus, and in part because the orthodox medical community came to regard most forms of electrotherapy as ineffective, at best, and quackery, at worst—precisely the sort of outcome that advocates of radiation therapy sought to avoid.

Technology and Medical Specialization

New technologies can create a material basis for the process of specialization. Sometimes, as with the laryngo- and opthalmoscope, new technologies legitimized existing fields of specialist study or treatment.[115] In the case of electrotherapy, however, practitioners specialized in the technology itself. Electrotherapists needed at least basic mechanical and electrical skills to maintain and operate their equipment, and would-be researchers, like the "father" of the field, Parisian electrotherapist Guillaume Benjamin Amand Duchenne, often had to develop their own apparatus.[116] Changes in the role, administrative structure, and overall

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[115] By making it possible to study the eye, for example, the opthalmoscope helped to finally elevate the fortunes of eye specialists beyond their traditional status as disreputable quacks. Rosen, "Changing Attitudes of the Medical Profession to Specialization"; Stanley Joel Reiser, "Medicine and the Reign of Technology" (Cambridge University Press, 1978).

[116] Duchenne's great contribution was to develop the technique of delivering electrical stimulation to the muscles with moistened electrodes on the skin, rather than by inserting needles that could damage surrounding tissue and cause infections. Duchenne communicated his findings to the Paris Academy of

influence of hospitals in the medical system also specifically aided the growth of technological specializations, like electrotherapy, because hospitals began to reconfigure themselves away from charity care and into centers of medical technology and research. Hospitals were often among the earliest purchasers of new technology—Guy's Hospital in London, for example, established an "electrical room" as early as 1836.[117]

Roentgenologists would follow electrotherapists in all three particulars, emphasizing scientific inquiry, defining themselves based on the mastery of a particular technology, and, in many cases, like Sydney Rowland at St. Bart's, working with equipment and space furnished by hospitals. X-ray users, however, would rapidly surpass their predecessors, creating a much larger and more influential disciplinary niche in the United States and the United Kingdom. Although hospitals like Guy's might employ doctors in an electrotherapy department, the persistent resistance to specialization in the Anglophone medical community meant that British and American physicians came to and practiced electrotherapy as a sort of hobby or secondary interest. X-ray advocates, by contrast, would succeed early on in organizing themselves into formal medical and scientific societies, building the first powerful community of technical specialists in Anglophone medicine, and presenting themselves as roentgenologists—x-ray practitioners—rather than as doctors with a hobby. They succeeded in this project, and particularly in establishing specialist diagnostic positions. Moreover, because x-ray technology

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Sciences in 1847, before going on to map out the different muscle stimulation points on the body, discovering in the process that certain specific points on each muscle—eventually recognized as the entrance points of the motor neurons—generated much stronger responses. Duchenne published his findings in *De l'electrisation localisée*, in 1855. Hector A. (Hector Alfred) Colwell, *An Essay on the History of Electrotherapy and Diagnosis* (London, W. Heinemann, ltd., 1922), 96–97.

[117] Colwell, *An Essay on the History of Electrotherapy and Diagnosis*; Howell, *Technology in the Hospital*; Charles E. Rosenberg, *The Care of Strangers: The Rise of America's Hospital System* (New York: Basic Books, 1987); Rosemary Stevens, *In Sickness and in Wealth: American Hospitals in the Twentieth Century* (New York: Basic Books, 1989). Interestingly, as Joel Howell has shown, the adoption of technology did not always translate into actual use the use of new equipment. Actually using an electrical room required not only individuals with technical training, but bureaucratic changes—the creation of billing systems, the upgrade of ancillary facilities, and so on—that would foster use. Guy's electrotherapy room seems to have gone in and out of use in bursts throughout the century.

both required less in the way of doctor-patient interaction than electrotherapy and showed a more immediate therapeutic result, roentgenology would, at least for a time, begin to redefine the boundaries of orthodox medical authority, opening the field to individuals who derived their expertise from technical mastery, rather than from traditional medical education.

Organizing and Professionalizing

In their journals, Sydney Rowland and Heber Robarts presented x-ray users as practitioners of a specialist discipline, using terms like "skiagraphy" and turns of phrase, like "this new Art" and "the new science," to emphasize that x-ray users were practicing a discipline, rather than simply taking fancy pictures. Both editors also used their publications as platforms to construct new professional communities around x-ray technology. In the *Archives of Clinical Skiagraphy*, Sydney Rowland included an invitation for readers to join a "Skiagraphic Society," created "by some of the leading men interested in the study of the X rays [sic], both in their medical and general scientific application," that would meet in London.[118] When the rebranded *Archives of the Roentgen Ray* appeared the following year, it ran as the formal organ of that group, which now called itself the Roentgen Society of London. The Society held its first General Meeting on June 3, 1897. "Laws were passed, Members enrolled, and Officers elected," according to the official proceedings, published in the *ARR*. The Roentgen Society counted numerous members of the Royal College of Surgeons amongst its initial membership, and the society elected as its President Silvanus P. Thompson, a well-known physics professor and Fellow of the Royal Society, the United Kingdom's premier scientific association.[119] The Roentgen Society's founding members organized the group to resemble the other professional

[118] Rowland, "Description of Plates & Answers to Correspondents," 37.

[119] Rowland and Hedley, "Editors' Note," 3; "The Roentgen Society (6/3/1897 Meeting)," 5. Given its circumstances and continuing connection to Rowland's journal, it seems likely that the Roentgen Society was a rebranded and substantially expanded successor to the "Skiagraphic Society" announced in the original issue of the *ACS*.

and technical organizations to which they belonged. Prospective members had to “have shown some scientific interest in the Roentgen rays” and to pass a general ballot in order to join. Scientific inquiry, in this case, encompassed both medicine and physics, and society president Silvanus Thompson emphasized in his opening remarks that the group’s interests fell across interdisciplinary lines. Whether “devoted purely to medicine, to physics, [or] to photography,” the society, according to Thompson, “would be glad to welcome any combination of workers.”[120]

The Roentgen Society adopted an explicitly public orientation, resolving at its first meeting to hold an evening exhibition in London, of the sort that had proven popular elsewhere. The November, 1897, issue of the *ARR* described the event. Noting “the great and growing interest that centers round the Roentgen ray,” editors Rowland and Hedley wrote that the exhibition attracted so many lords, ladies, and other members of the London elite that it “proved to be not only a meeting of the Society, but a ‘society’ meeting in a sense that may have surprised some of its original promoters.” Following a short speech by Silvanus Thompson, patrons wandered amongst “smaller hall lantern demonstrations” of X-ray photography, dark rooms showing real-time X-ray images using fluorescent screens, “short lectures and demonstrations,” and a number of “the more prominent equipment manufacturers,” displaying “an endless variety” of “coils, focus tubes, batteries, contact breakers, fluorescent and intensifying screens, photographic films.” Members evinced particular pride over “one of Mr. Nikola Tesla’s new electric oscillators, which had been specially sent over from New York” for the display, and a demonstration where “many may have seen for the first time a living heart in action.”[121]

[120] “The Roentgen Society (6/3/1897 Meeting),” 5–6.
[121] Sydney Rowland and W. S. Hedley, “Editorial,” *Archives of the Roentgen Ray* II, no. 2 (November 1897): 21–22.

Heber Robarts knew about the society in London. His journal reprinted the London group's reports from the *Archives of the Roentgen Ray*, and the group elected Robarts to membership at its meeting on April 5, 1898. The American decided to use his *American X-Ray Journal* as a platform for creating a similar organization of roentgenologists in the United States. In March, 1900, he hosted "a meeting of a number of workers with the x-rays" for the purpose of establishing the "Roentgen Society of the United States" (eventually renamed to its current designation, the "American Roentgen Ray Society"). The society adopted the *The American X-Ray Journal* as its official organ, and the editor—who also accepted the title of President of the Society—devoted a few pages of each subsequent issue to promoting the organization.[122] Like its predecessor in London, the Roentgen Society of the United States organized an exhibition of "the finest collection of x-ray appliances yet brought together anywhere in the world." In this case, the show took place alongside the group's first official conference, set in New York City on December 13th and 14th. Besides the trade show, the assembled roentgenologists heard twenty-five papers and a speech from Heber Robarts and undertook "the necessary business connected with completing the permanent organization of the society," such as appointing committees and discussing the location of the next meeting.[123]

Inclusion as Disruption

The American society emphasized the medical applications of x-ray technology as a primary objective, but the American roentgenologists, like their British counterparts, welcomed non-medical x-ray enthusiasts, including "technical electricians, chemists, teachers of chemistry

[122] J. Rudis Jicinsky, "Organization of the Roentgen Society of the United States," *The American X-Ray Journal* 6, no. 3 (March 1900): 723–24.
[123] "The First Annual Meeting of the Roentgen Society," *The American X-Ray Journal* 7, no. 6 (December 1900): 805–6.

and physics, [and] specialists and experts in electro-technique" into their ranks.[124] The inclusion of non-medical users in the ranks of roentgenologists would have significant ramifications for the development of x-ray therapy. In its initial instantiation, the spread of x-ray technology would disrupt the traditional lines of authority in orthodox medicine by legitimizing therapists based on technical expertise, rather than medical education and credentials. Sometimes this disruption happened within existing professional societies and organizations, like hospitals, which initially accepted non-traditional practitioners based on their expertise with x-ray equipment. The more significant challenge, however, came from the roentgenological associations and journals, which created an alternative space, primarily inhabited by advocates and enthusiasts of the new technology, for adjudicating disputes over issues like therapeutic efficacy and patient safety that would otherwise have been handled within the orthodox medical community.

Initially, roentgenologists with actual medical training and credentials assumed that, in the words of one editorial,

> "The physicist and the medical man meet here on common ground; they are, in many respects, useful to each other. They claim, as their field of work, all that appertains to the Roentgen ray. But they fully recognize the fact that the purely medical aspect of any question must always remain the province of a purely medical society. The dividing line is obvious, and common sense must draw it."[125]

In reality, of course, deference to the dividing line proved neither obvious nor necessary to the many roentgenologists who came to the societies and journals from non-medical backgrounds. Many of these practitioners felt that their technical skill alone was sufficient for them to operate as "x-ray therapists" (a not-insubstantial number preemptively granted themselves the title of "doctor"), and they both participated actively in debates over medical questions and, like Emil

[124] J. Rudis Jicinsky, "The Roentgen Society of the United States: Constitution," *The American X-Ray Journal* 6, no. 3 (March 1900): 724.
[125] Thomas Moore and Ernest Payne, "Editorial" II, no. 3 (February 1898): 39–40.

Grubbé, undertook their own therapeutic experiments on human subjects. X-ray therapists recognized that this arrangement created problems, but having welcomed all comers into their ranks, roentgenologists would find it difficult to police the field.

Technology in the medical marketplace

The tensions that would grow between different groups of x-ray therapists resulted from the fact that all members of the community operated in a shared commercial space—what historians have termed the "medical marketplace." At the most basic level, the "marketplace" metaphor emphasizes that "medicine was a service that was purchased in a competitive arena," with different practitioners and different treatment options vying for a potential patient's patronage. By choosing amongst remedies and physicians, patients affected not only the direction of their own care, but the standards of care accepted by their societies.[126]

New medical technologies often destabilize medical marketplaces, as a divide opens between adopters of the technology and those who stick with therapeutic methodologies that preceded the new technology.[127] In the case of radiation therapy, practitioners actively sought to harness patient excitement about new technology to x-ray therapists' advantage, advertising it, for example, as a painless alternative to surgery. In so doing, roentgenologists often

[126] The "marketplace" is a metaphor; Mark Jenner and Patrick Wallis trace the initial appearance of the "medical marketplace" concept to work on the history of medicine in early modern England. The "marketplace" is a way to describe an environment of patient choice; in the metaphor patients looking to purchase "healing" could choose from amongst a wide variety of different practitioners and methodologies—physicians, but also apothecaries, home remedies, and so on. Mark S. R. Jenner and Patrick Wallis, eds., *Medicine and the Market in England and Its Colonies, c.1450-c.1850* (Basingstoke [England] ; New York: Palgrave Macmillan, 2007), 1–3.

[127] Judith Leavitt, in her book *Brought to Bed: Childbearing in America, 1750-1950*, found an example of this process at work when she looked at the adoption of anesthetic technology in the nineteenth century. When ether and chloroform technology appeared in the middle of the century, the imprecision of the delivery methods and dosing standards meant that patients faced a not-insignificant risk of complications while anesthetized, and many physicians hesitated to use the new chemicals simply to mask pain, which they also used to mark a woman's progress in labor. Some physicians even actively campaigned against the use of ether or chloroform during delivery. Middle- and upper-class women who could afford anesthetic, however, clamored for the new treatment, often based exclusively on the enthusiastic testimony of friends or relatives. As a result, the use of anesthetic technology rapidly grew and spread, despite continuing misgivings and debate within the medical community. Leavitt, *Brought to Bed*, 117–19.

portrayed themselves as patient advocates, secure in their belief that patients, when given “the last word in the dispute, surgical versus radio-therapeutic treatment,” would always prefer treatment by an x-ray therapist to “mutilation” by a traditional surgeon.[128] As this statement suggests, the marketplace was a front-of-mind concern, not only for x-ray therapists, but for all types of medical practitioners.[129] It also created an obvious incentive for conflict, since participants in a marketplace had every reason to want to exclude or discredit their competitors.

The medical marketplace had always included practitioners with either no medical education or training that reflected alternative or specialist medical philosophies, but British and American physicians in the orthodox establishment actively attacked these heterodox healers, denying their status as doctors. Competitors like the homeopaths and Thompsonians made some inroads, particularly in terms of temporarily rolling back licensure laws in the United States in the mid-19th century, but the gains proved short-lived. The charge of quackery or fraud carried considerable weight on both sides of the Atlantic, and by the end of the century, the “regular” medical degree was once again ascendant. Ultimately, alternative practitioners (with a few key exceptions, like the osteopaths) would fail to win acceptance in the orthodox medical community and find themselves on the losing side when licensure laws made a reappearance in the late nineteenth and early twentieth century. Publication of the Flexner Report, in 1910, signaled the ascendance of the modern medical school, with its emphasis on orthodox medical education, laboratory science, interconnections between medical schools and hospitals, and biological science as the basis for medical knowledge. Ultimately, a medical

[128] W. Deane Butcher, “The Intensive X-Ray Treatment of Myoma,” *Archives of the Roentgen Ray* XVIII, no. 1 (June 1913): 3.

[129] Historian James Schafer recently elucidated this point in his study of physicians in Philadelphia: private practice dominated the medical landscape at the beginning of the twentieth century, and we cannot understand private practice without thinking of doctors as small business operators competing in a service industry. James A. Schafer, *The Business of Private Medical Practice: Doctors, Specialization, and Urban Change in Philadelphia, 1900-1940*, Critical Issues in Health and Medicine (New Brunswick, New Jersey: Rutgers University Press, 2014).

degree from such an institution would represent the sole viable route to obtaining a license to practice medicine. In the very midst of this transition, however, non-degreed roentgenologists succeeded, at least briefly, in parlaying their expertise with the new x-ray equipment into a seat at the orthodox medical table, taking positions in hospitals, opening clinics, joining medical societies, and presenting in the print and public forums of orthodox medicine.

Both Sydney Rowland and Émil Grubbé had medical training, but not a degree, when they entered private medical practice. Orthodox physicians, in their journals and professional organizations, would initially accept roentgenologists into their ranks, and some x-ray therapists, like Grubbé, went on to obtain credentials through licensed medical schools. But other roentgenologists argued that x-ray therapists should derive their professional standing from technical expertise and education in subjects like physics, rather than from traditional courses of medical study. Sydney Rowland fell into this camp. The first issue of the *Archives of the Roentgen Ray* listed "B.A., Camb.," after the editor's name. Rowland had gone on through his medical coursework at Cambridge before moving to London to complete his studies at St. Bartholomew's, where he got involved with x-ray technology. Rowland delayed his qualifying exams while working for the *BMJ* as its x-ray correspondent. According to Rowland's obituary writer, when the roentgenologist finally returned to Cambridge to complete the requirements for his medical degree, he found "by experiment that the examiners for the Cambridge M.B. would not accept knowledge of physiology and general science in lieu of that of midwifery," and Rowland never received a medical degree. Nevertheless, Rowland's x-ray work earned him an invitation to join the Royal College of Surgeons, in 1897, and he opened a private x-ray practice.[130]

[130] "The War. (Sydney D. Rowland Obit.)."

Rowland's membership in the Royal College of Surgeons represented more than just an upgrade in prestige. Organizations served a critical role in defining and policing ethical behavior for medical practitioners, whether formally, through rules and grievance procedures, or informally through the shared opinions, accolades, and opprobrium of fellow society members. On both sides of the Atlantic, ethical guidelines were a live, hotly debated issue in 1896, both at the national level, in groups like the American Medical Association, and in local societies. Had roentgenologists as a whole finished formal medical education, like Émil Grubbé, or remained within the confines of older medical communities, like Sydney Rowland, the debate over the efficacy, necessity, and ethics of x-ray therapy and human experimentation would have happened within a very different community and might have taken on a very different character. But just as Rowland moved x-rays out of the confines of existing medical journals and into their own, specialist publication, so did roentgenological societies seek to establish themselves outside the boundaries of existing medical communities.

Roentgenologists Unto Themselves

When Heber Robarts and J. Rudis-Jicinsky[131] began to canvas fellow physicians about possibly forming a Roentgen Society in the United States, "ex-Presidents of the American Medical Association and others therein interested" advised the roentgenologists to establish their society as a specialist group within the AMA. Robarts and Rudis-Jicinsky, however, decided to charter the Roentgen Society as an independent organization, answerable to none but its own members and open to all types of x-ray enthusiasts. The decision to go it alone created

[131] Rudis-Jicinsky was a Czech immigrant with ties to Chicago's Czechoslovakian expat community. Although he is listed in the Illinois State Board of Health's Official Register of Physicians as "John," Rudis-Jicinisky published exclusively under the first initial, and it seems likely that "John" was either an anglicized version of a Czech name or a convenient designation adopted post-immigration. Western Surgical Association, *Transactions of the Western Surgical Association. ...: Annual Meeting ...* (The Association, 1910), 362; Joseph Jahelka, "The Role of Chicago Czechs in the Struggle for Czechoslovak Independence," *Journal of the Illinois State Historical Society (1908-1984)* 31, no. 4 (December 1, 1938): 391.

some controversy. Many roentgenologists (including Robarts) had medical licenses and belonged to the umbrella organization. Many of these AMA members found the separate structure unnecessarily burdensome, if for no other reason than that they had to attend a second conference. Some doctors also framed their objections within the larger movement toward medical licensure and orthodoxy, arguing that roentgenologists' legitimacy depended on being "accepted by and made contributory to [American doctors'] great associations." Joining the American Medical Association, however, would have put roentgenologists in a difficult position, given the number and prominence of users in their midst who, possessing neither an M.D. nor a license to practice medicine, could not join the AMA.[132]

Forced to navigate the sensibilities of these two camps within the Roentgen Society, Heber Robarts gave a speech that carefully danced around the sources and nature of medical legitimacy. Opening with a discussion of the AMA membership controversy, Robarts offered "three overshadowing reasons, any one of which was enough," for roentgenologists not to organize themselves as a specialist society within American orthodox medicine's governing body:

1) X-ray equipment "required technical knowledge to properly operate, the discussion of which would be impracticable in another medical body."
2) Where x-rays offered new treatment possibilities for a disease, physicians trained in older therapeutic methods might "hamper" x-ray discussions with "unnecessary prejudices."
3) Roentgenologists required "the assistance of physicists in our meetings, which could not be, as members of the American Medical Association."[133]

[132] Heber Robarts, "President's Address," *The American X-Ray Journal* 7, no. 6 (December 1900): 806–7.
[133] Ibid., 807.

Though he phrased the initial reason in terms of practicality, the first and second explanations made clear that Robarts saw x-ray users as professionally ascendant over medical practitioners whose misguided "prejudice" led them to reject "the greatest and most certain diagnostic assistant ever offered the profession." Robarts would hammer this point throughout his speech, heaping scorn on the "'owls of the old ways' who know nothing of the technique of good raying," and leveling accusations of slander against "some surgeons [who], having acquired their alleged x-ray knowledge by instinct, have cut elsewhere than upon the bullet, and then written learnedly of the 'uncertainties of the x-rays.'"[134] With such claims, Robarts took direct aim at his medical peers, explicitly deploying the same legitimacy-by-technology logic that other doctors had wielded in, for example, using the introduction of forceps and ether to help displace midwives.

Meanwhile, by portraying non-physician x-ray users as expert consultants—degreed professionals (physicists) invited to "assist" medical roentgenologists—Robarts waved off other medical professionals' concerns about the interdisciplinary nature of the Roentgen Society. On its face, the claim seemed reasonable; physicists had produced and would continue to produce much of the research and many of the technical improvements that made x-ray therapy possible. But the mention of physicists elided the more controversial issue: the Roentgen Society welcomed members who portrayed themselves as doctors or therapists but who could not join the AMA because they had not completed a medical education or received a license.

Having dismissed the issue of AMA membership, Robarts appeared to switch topics, offering his listeners a brief history of Wilhelm Conrad Röntgen's career, the discovery of x-rays, and the early developments in medical imagery and therapy. The story was an unabashed progress narrative, with a series of increasingly grandiose claims piling upon one another by the

[134] Ibid., 807–8.

end of the tale: “The x-rays clear up all doubt,” “it contemplates the cure of those diseases known by the profession as practically incurable,” “with the x rays failures are not known.” To the audience, the speech must have felt like a fist-shaking celebration. Robarts, however, was winding his listeners up in order to create a friendly room. He had not lost sight of the divisive question of legitimacy, and when the turn came, it arrived with almost comical suddenness. “After all this glorious record sufficient for a hundred years,” the president suggested, “we see the necessity for standardizing some method for accurate diagnosis and therapy…we realize this must be done before we can be said to have a science acceptable to the thinking people and especially to the courts.”[135]

Developing Rules and Standards for X-Ray Practice

The practice of medicine always involves a relatively high degree of uncertainty. Symptoms can result from a wide variety of possible causes, ranging from pathogens in the body to allergies and diet to mental disorders. The same treatment that ends one patient’s symptoms may cause no change in another and provoke a life-threatening allergic reaction in a third. And, as Robarts’s mention of “the courts” suggests, distrustful patients can cause real problems for doctors in the form of malpractice claims. Under these conditions, the actions of a few bad or incompetent medical actors can reflect poorly on the profession as a whole, diminishing the authority or legitimacy of all doctors in the eyes of their customers. Doctors had a term, “quackery,” to describe fraudulent or ignorant medical practice. It was intended as an appellation of shame and often applied with relish in public forums, such as the letters sections of medical journals.[136] Quacks represented both an ethical and a marketplace challenge to regular medical practitioners. On an ethical level, quackery could harm patients, whether

[135] Ibid.
[136] See, for example, “QUACKERY.,” *The Lancet*, Originally published as Volume 2, Issue 929, 36, no. 929 (June 19, 1841): 464; VERITAS, “Quackery in the Medical Profession.,” *The Boston Medical and Surgical Journal (1828-1851)*, December 19, 1849.

physically or financially. In the marketplace, quacks could compete against other doctors not only on price, but on the strength of unsubstantiated and even fraudulent claims. As a group, regular physicians employed a variety of strategies to fight quacks and elevate the status of orthodox medicine. In the United States, the American Medical Association embraced both an enhanced standard of medical education and a renewal of licensure laws in the mid-19th century as part of its push to discredit and displace unscrupulous or incompetent therapists. At its most basic level, AMA membership represented a seal of authenticity for a doctor after the organization was founded in 184X.[137]

Legitimacy Through Standardization

Roentgenologists, having inserted themselves squarely into the therapy business, had the same concerns about quackery as other medical professionals. Robarts envisioned the Roentgen Society performing the same gatekeeping function as the AMA; it would have a record of "every accredited x-ray therapeutist and diagnostician in the States, and also know those who are in competent [sic]." The Roentgen Society, could not, however, rely on a medical school credential or state license to weed out practitioners for precisely the same reason that they could not join the AMA: too many of their peers fell on the wrong side of the line. And although the society's charter welcomed enthusiasts of all stripes, many physician-roentgenologists took a dim view of x-ray therapy as practiced by members from non-medical backgrounds. Cleveland surgeon N. Stone Scott spoke for many of his peers when he lamented in the *The American X-Ray Journal* that:

> In the beginning of the x-ray work, many physicists, because they were in a position to manipulate a Crookes tube, undertook to pass judgment upon medical subjects. Much wild semi-medical talk and writing was indulged in, and not a little harm done, so that

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[137] For a more complete discussion of regular physicians' battles against nineteenth and twentieth century quackery, see Eric Boyle, *Healing Society : Disease, Medicine, and History : Quack Medicine : A History of Combating Health Fraud in Twentieth-Century America* (Westport, CT, USA: ABC-CLIO, 2013).

> the profession was not slow to see that medical men must equip themselves to retain control of medical subjects.[138]

Heber Robarts expressed similar views in his speech to the Roentgen Society, revealing to his listeners that, "so far as my conceptions go I cannot conceive of a first-class x-ray diagnostician who does not possess a fair knowledge of anatomy and pathology, on the one side, and technical training of the use of the apparatus and of the skiagraphic reading on the other." But whereas Scott's complaint provided the basis for his claim that "medical men" should wrest control of x-ray medicine from other roentgenologists, Robarts framed his beliefs about the importance of a medical education as a misguided prejudice that he had overcome. Having personally witnessed that "accurate work may be done by some who do not possess the first essentials," Robarts shared with his audience that he had adopted a new position, that "acting with a standardized tube and from a standardized method, any operator with a fair amount of judgment may become an x-ray therapeutist [sic]."[139]

Robarts pitched standardization as the method by which the Roentgen Society could police the field. He proposed to the audience of gathered roentgenologists that "we standardize diagnosis first and then fix an approved therapy for the x-rays," creating "a way or fixed rule . . . which shall be approved by this society." Legitimacy, in this model, would result from the application of a pre-approved methodology, rather than from an M.D. The argument proved convincing; the society's members agreed that "to regulate details and determine standards essential to accurate x-ray work was the imperative need of the hour," and they appointed a committee to address the issue.[140] For reasons both technical and social, standardization would prove more difficult and divisive than anyone expected, and the issue would bedevil roentgenologists around the world for the next two decades. But in elucidating the idea that

[138] N. Stone Scott, "X-Ray Injuries," *The American X-Ray Journal* 1, no. 3 (August 1897): 57–66.
[139] Robarts, "President's Address."
[140] Ibid., 808–10; "The First Annual Meeting of the Roentgen Society."

individuals with relatively little formal medical training could be trained to provide x-ray therapy using standardized equipment, Robarts had unwittingly described a new type of medical professional: the technician. Eventually, x-ray users without medical degrees would find themselves shunted into this new role, much to the detriment of their professional standing vis-à-vis physicians.

The Golden Rule

Interestingly, none of the accounts of x-ray therapy discussed here stand at odds with the basic principles of ethical human experimentation accepted by the majority of doctors in this period. Professional medical associations in the United States had, during this period, effectively adopted a *laissez-faire* approach to therapeutic experimentation wherein each doctor was expected to make his or her own moral judgments about treatment. In 1894, would-be reformers tried to explicitly write these principles into the American Medical Association's code of medical ethics, and by 1903 they succeeded in doing so, with the overwhelming support of the profession.[141]

The lack of controversy reflected a general consensus on the basic tenets of doctor-patient relations. That consensus began with two concepts: the virtuosity of the physician's character and intent, assumed to be the foundation of a proper physician-patient relationship, and the ethic of reciprocity, often referred to as the "Golden Rule," which would serve as both a personal rule of measure and a line of defense against patient complaints. Together, these concepts emphasized that ethical behavior was the product of good character, rather than good codes. Ethical decision-making fundamentally lay in the personal judgement of the physician, rather than in the application of pre-existing professional rules or philosophical precepts.

[141] For a full discussion of the evolution of codes of medical ethics in the United States, see Baker, *Before Bioethics*.

The first of the two pillars of nineteenth-century medical ethics, virtuosity of character, was enshrined in the Edinburgh oath, with its language about practicing medicine "purely and honorably"; the language also signaled the aspiration of physicians to gentlemanly status.[142] The quest for gentlemanly status through professionalization put physicians alongside other highly-educated service workers, like barristers, and it also became a feature of professional classes in the United States. In 1854, representatives from Ohio and Massachusetts tried to amend the American Medical Association's code of medical ethics to allow physicians to claim patents on new medical devices or formulas for medicine, arguing out that patents would allow medical professionals to properly benefit from their innovations. The national organization rebuffed the effort in the harshest possible terms, threatening to expel the offending members of the state medical societies; gentlemen professionals could not abide the sorts of grubby fellows who would try to profit by restricting their colleagues' access to new techniques or medicines.[143] Of course, the same gentlemanly ideals that decried commercialism also stood at odds with any kind of enforcement of ethical codes, leading to a situation that satisfied no one.[144]

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[142] In part, this signaling had to do with the marketplace: "being a gentleman," according to Baker, "or at least being perceived as one, was a prerequisite for practicing medicine among the status-conscious middling and upper classes" in the United Kingdom. Ibid., 44–54.

[143] Ibid., 182–83; for an extensive discussion of gentlemanly status and its role in professional identity in the United States, see Samuel Haber, *The Quest for Authority and Honor in the American Professions* (Chicago: University of Chicago Press, 1991).

[144] The AMA *Code of Medical Ethics* adopted in 1847 spelled this divide out explicitly in its first article, which described professional and moral obligations of the physician as "the more deep and enduring, because there is no tribunal other than his own conscience, to adjudge penalties for carelessness or neglect." As the subsequent flap over patents demonstrated, however, AMA members faced with a situation where their colleagues' consciences ran at odds with the official AMA position could find it in themselves to take enforcement beyond the pangs of conscience. Those actions, however, engendered their own backlash, and a growing number of physicians adopted the position articulated by Lewis Pilcher, in 1883: despite the language about having no tribunals, attempts to enforce the AMA code had created "a multitude of star-chambers all over the land, in which men have assumed the right to sit in judgment" over fellow physicians. As a result, Pilcher asserted that physicians should reject "the present code of the American Medical Association, or any like set of definite ethical rules," returning to the original notion that physicians, as gentlemen, could be trusted to consult their own consciences in matters of ethics. Lewis Pilcher, "Codes of Medical Ethics," in *An Ethical Symposium: Being a Series of Papers Concerning Medical Ethics and Etiquette from Liberal Standpoint*, ed. Albert C. Post, et al. (New York: G. P. Putnam, 1883), 42–43 & 52–53; quoted in Baker, *Before Bioethics*, 145 & 210–11.

This question of conscience produced a feud amongst American physicians that only deepened over the remainder of the nineteenth century. On one side, the "Old Coders" continued to support the AMA's statement of 1847; on the other side stood the "No Coders," who rejected not just the 1847 document, but *any* codification of medical ethics. In between these two extremes, a variety of "New Coders" sought to revise the 1847 document in ways that might address complaints from both sides, with the predictable result that they pleased no one. In 1896, when the first x-ray therapy clinics opened for business, the battle was raging; Old Coders continued to hold the line, but only by driving out their colleagues, who created a variety of competing professional organizations. By 1903—the same year that Marie Curie, Pierre Curie, and Henri Becquerel would share the Nobel Prize in Physics—enough of the old guard had retired or died for the No Coders to more or less win the war. The AMA would replace its "Code of Medical Ethics" with a new document, the "Principles of Medical Ethics," whose preamble expressly declared it "a suggestive and advisory document" containing neither rules nor penalties.[145]

Baker describes the ethical framework of the No Coders as "laissez-faire ethical libertarianism," and given the experimental nature of early radiation therapy, it should come as no surprise that this approach predominated amongst x-ray and radium therapists on both sides of the Atlantic. In the case of x-ray therapy, many practitioners came from outside the ranks of traditional physicians; they had no oaths to uphold, in the case of British physicians, and no standing to lose at AMA conventions, for the Americans. Even those x-ray therapists who did have medical degrees tended to be younger, which generally put them on the side of the libertarians. In the case of radium therapists, the difficulty of acquiring radium meant that physicians were wealthy, well-connected, or operating under the imprimatur of a powerful

[145] Baker, *Before Bioethics*, 207–19.

institution, such as the various national institutes organized by various European governments. All three circumstances offered individual experimenters a certain amount of protection against charges of quackery or unethical behavior.

"*Laissez-faire*" should not, however, be seen as code for "no" ethics. Instead, advocates of laissez-faire argued that ethical behavior resulted from more general principles of society. Basically, a gentleman should know how to behave himself, and if a physician behaved unethically towards patients, the problem lay not in a lack of formal rules but rather in the flawed character of the individual (which no rules could correct). In particular, advocates of laissez-faire ethics often cited the "Golden Rule"—do unto others as you would have them do unto you—as a universal concept that would produce ethical behavior.[146]

Radiation therapists were not the only ones engaged in medical experimentation with human subjects, and three episodes of "Golden Rule" self-experimentation stand out. In 1892, as part of a feud with Robert Koch over the cause of cholera, Max von Pettenkofer ingested a broth contaminated with cholera bacteria.[147] In 1916, Joseph Goldberger, in an effort to show that pellagra resulted from a vitamin deficiency rather than from an infectious agent, submitted a group of volunteers to muscular injections of blood from patients showing symptoms of the disease. The volunteer test subjects, which included Goldberger and his wife, also ate dough

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[146] In reference to medical experimentation, the classic Golden Rule formulation was laid out by the British medical ethicist Michael Ryan, who wrote in 1831 that there should be no difference in what "the practitioner does or advises to be done, for the good of his patient, and what he would do in his own case, or in the case of those dearest to him." Ryan had no illusion about the dangers of medical experimentation, noting that such experiments had led to fatalities. Nevertheless he argued that so long as physicians accepted "the most dangerous experiments upon themselves," therapeutic experiments performed under the auspices of the Golden Rule are "not blamable, for they are necessary" in the pursuit of better treatments. Howard A. Brody, Zahra Meghani, and Kimberly Greenwald, eds., *Michael Ryan's Writings on Medical Ethics* (Springer Science & Business Media, 2009); quoted in Baker, *Before Bioethics*, 73–74.

[147] For a full recounting of the feud and its resolution, see Alfredo Morabia, *Enigmas of Health and Disease : How Epidemiology Helps Unravel Scientific Mysteries* (New York, US: Columbia University Press, 2014), 47–62.

that contained skin, urine, nasal, and fecal samples from pellagra patients.[148] In both cases the subjects sought to influence public health debates over the disease in question. Of course, each man also believed that he was proving that the disease in question was not caused solely by exposure to an infectious agent; in that sense, neither man believed he was taking an actual risk.[149]

In Walter Reed's yellow fever research, by contrast, the experimental investigators correctly believed that they were working with the actual cause of a deadly disease. In an effort to prove out the suspicion that mosquitoes could transmit the disease, the researchers would expose subjects to infected mosquitoes. In accordance with the Golden Rule, several of the researchers also subjected themselves to bites from infected mosquitoes, and in both communications exchanged before the experiment and subsequent accounts given to interviewers, participants in the yellow fever project repeatedly framed that choice in the language of honor and duty. When one of the researchers, Jesse Lazear, contracted yellow fever and died, his compatriots hailed Lazear as a hero and a "martyr."[150]

Both x-ray and radium therapists embraced the Golden Rule approach to ethics. In some cases, that ethical choice was very explicit and premeditated. As with the Lazear case, however,

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148 Stephen J. Mooney, Justin Knox, and Alfredo Morabia, "The Thompson-McFadden Commission and Joseph Goldberger: Contrasting 2 Historical Investigations of Pellagra in Cotton Mill Villages in South Carolina," *American Journal of Epidemiology* 180, no. 3 (August 1, 2014): 237.

149 An extensive discussion of both cases is available in Morabia, *Enigmas of Health and Disease*. In Goldberger's case, pellagra is caused by a vitamin deficiency, rather than an infectious agent—a piece of information that Goldberger had already tested using prisoners, in an experiment that would today be considered flatly unethical on multiple grounds. Goldberger did not opt to participate in that experiment. In von Pettenkofer's case, the German was actually trying to recover his reputation after the city of Hamburg, following his advice not to filter its water, suffered some 8,606 cholera deaths; the neighboring community of Altona, which used sand filtration, had no fatalities. Von Pettenkofer did not suffer a fatal case of cholera, but it did not matter; his claim had been decisively proven incorrect by the Hamburg outbreak. Von Pettenkofer committed suicide in 1901.

150 Interestingly, Lazear probably contracted the disease from an incidental bite, rather than an intentional one. Still, working in close proximity patients and mosquitoes made the distinction mostly meaningless, and Reed himself was explicitly told by superiors not to visit the site, for that reason. For extensive discussion of the yellow fever experiments, see Lederer, *Subjected to Science*; Baker, *Before Bioethics*.

intentionality was not actually a necessity. In the early years, most x-ray emitters lacked even the most basic shielding. As a result, even therapists who did not set out to use themselves as guinea pigs still ended up as de facto subjects in a test of the long-term effects of ionizing radiation on the human body. As the dire consequences of that exposure became increasingly clear over the years, radiation therapists adopted the same religious language used by Dr. Lazear's colleagues, describing death by radiation-induced cancer as "martyrdom" in the cause of science.

Advocates of *laissez faire* did not presume that doctors should experiment on patients without considering the consequences; rather, they emphasized that decisions about therapeutic experimentation were a tricky business, bounded by complicated, idiosyncratic factors, such as the particulars of a given case, the overall health, age, and status of the patient, and the doctor's own comfort or discomfort with a particular type of therapy. To navigate this morass of factors, most medical organizations suggested that doctors abide by the "golden rule" when contemplating experimental therapies: a doctor should not use a treatment on a patient without first trying it out on the physician's own body, family members, or friends.[151] In theory, the golden rule had a self-limiting quality, since doctors presumably would want to avoid unnecessarily harming themselves or their loved ones. Émil Grubbé's initial experiments with breast cancer and lupus vulgaris clearly met the golden rule test; he did not offer to induce dermatitis in his test subjects without first having done it to himself. In the case of x-rays, however, the golden rule would lose its efficacy, because roentgenologists proved extremely resistant to acknowledging the depth of harm that x-rays had wrought in their own bodies.

[151] For a good example of this principle, see the discussion of the Walter Reed team's experiments with yellow fever, which included several researchers submitting to infection, one of whom died as a result: Lederer, *Subjected to Science*; Baker, *Before Bioethics*.

Dermatitis Denial

Recognizing the ubiquity of the burn experience, the Roentgen Society of London appointed a special committee to investigate the problem of dermatitis in April of 1898. The language of the committee appointment, however, suggests a problematic twist in roentgenologists' understanding of the risks of exposure: the committee would "collect information on the subject of the *alleged* injurious effects of the Roentgen rays," with an eye towards distinguishing between harm caused by "the x-rays themselves," and harm caused by some other factor, such as "leakage discharge of current," or "varying electrostatic charges on the surface of the tubes," or even "some other hitherto unrecognized kind of radiation emitted simultaneously with the x-rays." The instructions carry a subtle, yet undeniable subtext, questioning even the existence of harm and emphasizing the possibility that ancillary equipment, rather than the x-rays themselves, cause burns.[152]

The dermatitis discussion in the *Archives of the Roentgen Ray* had always included distinctive notes of skepticism about the safety or danger of x-ray exposure. In a discussion of "injurious effects" included in his 1897 state-of-the-field essay, *ARR* editor W.S. Hedley distinguished between "the Roentgen ray" and "some part of the process connected with its production," arguing that the cause of harm remained unknown and that even the *existence* of x-ray dermatitis "cannot be considered definitely settled."[153] William Webster, the roentgenologist who had reported losing the nails on his right hand, opined that that "the rays only affect unhealthy skins." After the initial burn, Webster had equipped his instrument with a metal guard, but he made no connection between this shielding and the lack of subsequent

[152] "X Ray Traumatism," *Archives of the Roentgen Ray* II, no. 4 (May 1898): 61. Emphasis mine.
[153] Hedley, "Roentgen Rays, A Survey, Present and Retrospective," 12.

harm, basing his conclusion instead on the observation that his right hand had previously suffered "with gouty eczema," whereas his left hand had healthy skin.[154]

Such a seemingly obvious oversight was, if not the rule, then at least a common occurrence in the community most excited about x-rays. The *Archives of the Roentgen Ray* devoted almost all of its February, 1899 issue to dermatitis, and British dermatologist David Walsh explicitly brought up roentgenological denialism with cautionary tale in the February, 1899 issue of the *ARR*:

> "Soon after the Roentgen rays came into general use, as everyone knows, cases of resulting injury to the skin began to be reported. This effect was usually spoken of as X-ray dermatitis, but the present writer…ventured to suggest the use of the non-committal name of 'focus-tube dermatitis…' Only a week or two since, a Roentgen ray worker, in the course of a conversation, was hinting his doubts as to the occurrence of the condition, when I pointed it out to him on his own hands—that is to say, his fingers were red and leathery, and his nails brittle and broken, and wanting the sharp groove that defines the lower border of a healthy nail."[155]

The same *ARR* issue framed the dermatitis debate with the work of two Philadelphia roentgenologists: Elihu Thomson and Charles Lester Leonard, both of whom had designed experiments specifically to understand and obviate the problem of x-ray dermatitis. Thomson and Leonard both showed that sufficient metal shielding, placed between the emitter and the subject's skin, would reduce the incidence of dermatitis, but the two came to very different conclusions about why the shielding worked. Thomson (more or less correctly) concluded "that burns are produced by those rays of the X ray order which are most readily absorbed by the flesh—viz., from tubes of low vacuum," or "soft" rays, which metal shielding could filter out. Leonard, by contrast, argued that "the devitalization and destruction of tissue are due to the static charges" associated with emitters and that metal shielding served "to keep off electrostatic effects." Both Leonard and Thomson had recognized what Webster had not, that

[154] Webster, "'Practical X-Ray Work,' a Paper Delivered to the Roentgen Society," 53.
[155] David Walsh, "Focus-Tube Dermatitis," *Archives of the Roentgen Ray* III, no. 3 (February 1899): 69–70.

metal shielding could offer some protection from x-rays, but the *ARR* discussion of the Leonard-Thompson debate kept alive the underlying, problematic denial of the dangers of x-rays as such. In a turn that would eventually prove darkly ironic, the research summary presented in the *ARR* informed readers that, although Thomson's work seemed "the most complete," the author nevertheless had thrown in his lot with Leonard. The article concludes with an optimistic pronouncement that new, more powerful emitters would make possible shorter exposures that would limit problems to only those patients "specially susceptible to the electrical forces which cause injury."[156]

Skin Reaction as Dosimetry

In addition to enthusiasm and denial, the dermatitis discussion was clouded by a lack of information, and especially by the practical problem of measurement. X-ray radiation remained a poorly understood phenomenon in the years following its discovery; x-rays were relatively easy to produce but very difficult to study. In the first issue of the *Archives of the Roentgen Ray*, Silvanus Thompson lamented that, despite much research in the nineteen months since Roentgen's discovery of x-rays, "we seem to be very little nearer to a true understanding of their real nature." In his first speech as president of the Roentgen Society, Thompson called for investigation of a long list of questions, including determining whether the rays represented another wave-form, like light, or something altogether different, such as "flights of atoms or subatoms."[157]

The progress that Thompson looked for was slow in coming, especially in the area of measurement. The x-ray category encompasses a whole spectrum of electromagnetic radiation,

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[156] E. Payne, "Notes on the Effect of X Rays," *Archives of the Roentgen Ray* III, no. 3 (February 1899): 68–69.

[157] Silvanus P. Thompson, "On the Nature of Roentgen's Rays," *Archives of the Roentgen Ray* II, no. 1 (July 1897): 3; "The Roentgen Society (6/3/1897 Meeting)," 6. Thompson's comment was prescient; X-rays fall on the electromagnetic spectrum, but some other forms of ionizing radiation, such as alpha and beta radiation, result from the flight of sub-atomic particles.

and the exact mixture of rays produced by any particular device varied wildly based on factors ranging from the design of the emitter to the length of its use. Moreover, roentgenologists could not measure the rate at which human bodies would absorb that energy content—what a modern doctor would describe as the "dose" of radiation. Accurate dosimetry requires one to consider not only the absolute quantity of energy reaching a human body, but specific qualities of that radiation, such as its penetrative power. Further exacerbating the problem, no one in this period knew exactly *why* or *how* x-rays affected living tissue.

Lacking some method for "measuring the quality and quantity of x-rays generated in a given period of time," Émil Grubbé had, in his initial therapeutic experiments, resolved to give his patients daily x-ray therapy "until cumulative effects, as would be shown in the development of dermatitis, made their appearance."[158] This approach brought together the basic problem of measurement with the universal experience of roentgenologists: being burned by their emitters. It should therefore come as no surprise that many researchers hit on the same strategy as Grubbé, making reddened or inflamed skin the initial benchmark standard for radiation dosimetry.

This mode of thinking first appears in the *Archives of the Roentgen Ray* in the November, 1898 issue, in a digest of Dr. Albers-Schönberg's experiments with treating lupus, originally published in *Fortschritte der Röntgenstrahlen*. After arranging a mask of cardboard, covered with tinfoil, to isolate the treatment area, Albers-Schönberg described giving his patients daily half-hour x-ray exposures until "the redness begins to show itself." Albers-Schönberg specifically warned listeners interested in adopting this method of therapy to "keep a close watch on the effects produced during the first applications, to avoid, if possible, a general

[158] Grubbé, *X-Ray Treatment: Its Origin, Birth, and Early History*, 54.

dermatitis which hinders the cure."[159] That differentiation between erythema (reddening of the skin) and dermatitis soon became the standard in x-ray dosimetry. Roentgenologists began to describe their therapeutic activities in terms of the "erythema dose": the amount of radiation exposure necessary to produce reddening of the skin.[160]

X-rays at the End of the Rush

The picture painted by the November, 1898, and February, 1899 issues of the *ARR* captures the state of x-ray technology and roentgenology at the end of the century. In the initial Roentgen Rush, x-ray enthusiasts from a variety of backgrounds—physicists and physicians, but also medical students, professional and amateur inventors, photographers, and public performers—used x-rays for purposes ranging from research and medical diagnosis to hair removal and public entertainment. Calling themselves by a variety of names, including "skiagraphers," "radiographers," and eventually "roentgenologists," users of the technology from across the disciplinary spectrum had begun to coalesce into a professionalized, international community, forming societies and creating their own journals in London, Germany, and the United States. Fired by their enthusiasm for the new technology, roentgenologists celebrated the therapeutic possibilities inherent in the x-ray burns appearing on their own and their patients' bodies even as they maintained a healthy skepticism about the "real" cause of dermatitis and made a virtue of the rays' vice, adopting erythema as a useful standard of dosimetry.

Both the American and British Roentgen Societies and their journals would prove durable and influential. The societies would last into the present, in their modern incarnations as the American Roentgen Ray Society and the British Institute of Radiology, respectively.

[159] "The Treatment of Lupus (Reprint of Paper by Albers-Schönberg, First Printed in Fortschritte Der Röntgenstrahlen, II.20)."
[160] Bleich, *The Story of X-Rays from Röntgen to Isotopes*, 30.

Founders Sydney Rowland and Heber Robarts, however, would not entirely share in that success. Editing the *Archives of the Roentgen Ray* raised Rowland's stature as a roentgenologist, but it did not pay his bills. Moreover, his editorial skills apparently left something to be desired; an obituary writer later described Rowland as "singularly devoid of any literary faculty." When the *Archives of the Roentgen Ray* began a new volume in August, 1898, Rowland's name had disappeared from the cover. Rowland's editor-uncle, Ernest Hart, died that same year, costing Rowland his job at the *BMJ*, whose British Medical Association overseers vociferously objected to Rowland's ongoing requests for equipment funding (a furor ensued when the British Medical Association, which owned the journal, received a bill for £98, or twice the correspondent's annual salary, in 1896). His private x-ray practice never took off; in the words of an observer, "it was impossible for one of his nature and unbusinesslike habits to remain at his rooms and await the advent of patients." At the end of 1898, Rowland joined the Lister Institute as an assistant bacteriologist, permanently abandoning the field of roentgenology.[161]

For Heber Robarts, the first meeting of the Roentgen Society of the United States would prove to be the high water mark of his career as a roentgenologist. As the third chapter will discuss, the very interdisciplinary tensions that Robarts had addressed in his speech would soon fracture the Roentgen Society, and the erstwhile founder would lose control of both *The American X-Ray Journal* and the organization. Unlike Rowland, however, Robarts had not lost interest in the possibility of using ionizing radiation for therapeutic ends. A new development in the field had caught his eye: an announcement from Europe that Pierre and Marie Curie had found a new element, which they called "radium," that constantly emitted energy with

[161] At the Lister, Rowland would find his calling in the budding field of epidemiology. He would go on to an interesting and successful career as a frontline epidemiologist, working at outbreak sites around the world to study, and attempt to prevent, the transmission of diseases like plague and typhoid fever. Rowland died in the midst of such work, contracting meningococcus while attempting, as an army medical officer, to help contain an outbreak of the disease in France, in 1917. "The War. (Sydney D. Rowland Obit.)."

properties similar to those of x-rays. Robarts would soon remake himself as a pioneer in this new field. Unfortunately, he would also carry the legacy of his time as an x-ray pioneer in his own body—the fatal legacy of long exposure to radiation.[162]

[162] Brown, "Heber Robarts (1852-1922)."

CHAPTER 2 – THERAPY OF UNCERTAINTY: X-RAYS FROM 1900-1914

In 1900, roentgenologists evinced boundless enthusiasm about the therapeutic possibilities of x-rays. By 1912, when English physicians W. J. S. Bythell and A. E. Barclay published a textbook on *X-Ray Diagnosis and Treatment*, the excitement had curdled. Despite equal billing with diagnosis in the textbook's title, therapy took up only a single chapter at the end of Bythell and Barclay's manual, and the authors offered a decidedly mixed assessment of x-ray treatment, informing readers that although "x-rays, like all new remedies that have given striking results, have been tried in almost every ailment under the sun…in many instances they have been found wanting." Even when such therapy offered a benefit, "x-ray treatment," Bythell and Barclay opined, "may be well described as 'skating on thin ice,' and in this case it is usually the object of the operator to keep as close to the danger zone as he can."[163]

The universal experience and acceptance of low-level burns that characterized the early days of roentgenology had, by 1912, left a brutal legacy. Many x-ray workers suffered with persistent, painful symptoms of x-ray exposure in their hands—ulcers that would not heal, burning sensations, fingers so swollen that they could no longer manipulate apparatus—and a grim long-term prognosis. Dermatitis typically gave way to aggressive cancers that could be alleviated only by amputation; in most cases, the cancer would eventually recur, leading to progressively more tissue and limb removal, in a process sometimes described as "dying by inches." As x-ray induced cancers and related maladies decimated the ranks of roentgenologists, the "gloved or amputated hand," according to Kevles, "became the emblem of x-ray workers," and they began to speak of a shared, inevitable "martyrdom."[164]

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[163] Bythell and Barclay, *X-Ray Diagnosis and Treatment*, 117.

[164] Kevles, *Naked to the Bone: Medical Imaging in the Twentieth Century*, 47–48; Herzig, "In the Name of Science: Suffering, Sacrifice, and the Formation of American Roentgenology"; Goldberg, "Suffering and Death among Early American Roentgenologists: The Power of Remotely Anatomizing the Living Body in Fin de Siècle America."

This chapter will explore the development of x-ray therapy between the turn of the century and the start of the First World War. During this period, the introduction of new technologies, methodologies, and personalities dramatically expanded the scope of radiation therapy beyond its original borders as an x-ray treatment, but I will here focus on the winding narrative arc suggested by Bythell and Barclay, focusing on the particular challenges of x-ray therapy, before opening a discussion of the other major radiation treatment, radium, in Chapter 3. Together, these two technologies represented the most important technological pathways for radiation therapy, and these chapters analyze both x-ray and radium treatment, using the concept of a technological frame to explore two particular questions: what exactly were therapists attempting to do, and what limitations constrained therapeutic decision-making? These questions are broad enough to apply not only to radiation therapies, but to other stories of medical innovation. Nevertheless, when answered with specificity, they yield useful, particular analytical insights.

Seeing the artifacts

In thinking about the rise and spread of radiation therapy, it is important not to lose sight of the artifacts themselves. Stanley Joel Reiser describes medical technologies as having "two essential dimensions: form and purpose."[165] We often think of form as driven by purpose, for example, "what form of vacuum tube will best achieve my purpose of generating x-rays?" In reality, however, the two dimensions exist in a reciprocal relationship. X-ray therapy offers an example of the reverse case; only after they observed the effects of x-rays on their own bodies did x-ray users turn the existing emitter form to a new, therapeutic purpose.

[165] Stanley Joel Reiser, *Technological Medicine: The Changing World of Doctors and Patients* (Cambridge: Cambridge University Press, 2009), 187.

X-ray emitters and radium salts were physical things in the world, and the specific properties of their forms played a crucial role in determining the development of radiation therapy. In the case of x-ray emitters, the particular properties of the devices, and especially the difficulty of controlling variables in the emitters' operation, would hamper the efficacy of treatment and make it much more dangerous for patients, even as the relative availability of parts and ease of operation made it possible for individuals without medical degrees to offer x-ray "treatment." In the case of radium, the tremendous scarcity of the substance would dramatically affect both access to radium therapy and its public perception.

In these two chapters, I argue that x-ray and radium therapy developed idiosyncratically in response to specific constraints and technical barriers: the limitations of apparatus and the problem of dosimetry, in the case of x-rays; the problem of scarcity, for radium. Despite these differences, however, I believe that the two technologies must be considered alongside one another because x-ray treatment, radium therapy, and other radiation technologies developed according to a shared logic and a common ancestry. The shared logic assumed that "energy," writ large, provided the animating force of human bodies, and that radiation therapy, in its various guises, produced healing by introducing this positive force into a faltering biological system. The shared ancestry resulted from both the temporal proximity of various discoveries—the aforementioned discovery of radioactivity and x-rays coincided with other "energy" breakthroughs, in areas such as phototherapy and physics—and the professional overlap within the field itself, as the same small community of doctors—men like Deane Butcher and American x-ray pioneer Heber Robarts—conducted therapeutic experiments with electricity, x-rays, and radium, publishing their results in many of the same outlets and carrying on professional discussions within a shared community. Answering the second of my two analytical questions, both the logic and the ancestry would prove to be key limitations in the development of radiation therapies, particularly from the standpoint of patient safety.

Unfortunately, the two technologies also shared a dark legacy, for both doctors and patients. The stories examined in this chapter ended in a variety of unhappy circumstances, including disfigurement, cancer, anemia, and death. As the professional record makes clear, therapists working with radiation understood the risks of the technology, at least in a general way. But I will argue that although they approached the hazards of radiation therapy in ways that would have read as "ethical" amongst contemporaries, radiological practitioners' therapeutic judgment was fatally compromised by factors ranging from irrational technological exuberance to mundane pecuniary interest. Experimental subjects, patients, and practitioners suffered harm as a result.

Inevitably, the creation of new medical technologies and the evolution of a new medical discipline had important impacts on the professional structure of medicine in both the United States and Great Britain. In order to give this subject its due, I will discuss the professionalization of radiology and the relationship between radiation therapists and other medical professionals at length in the fourth chapter. But although I have chosen to shift the professionalization narrative to its own space, this chapter places the professionals themselves at the heart of the story. The stories of individual therapists, like Lester Leonard, Robert Abbe, Heber Robarts, and Émil Grubbé, put a comprehensible, human face on technical subjects like dosimetry and dermatitis. Radiological practitioners left a public written record through journal publications, textbooks, pamphlets, and public talks. Those professional forums, where would-be radiation healers and pioneers aired their hopes, their frustrations, and occasionally their despair, provide the foundation for my analysis, which focuses not only on the artifacts and practices themselves, but on the technological and therapeutic frame that produced them.

Framing X-ray Therapy in 1900

The start of World War I offers a convenient cutoff point for discussing the development of x-ray therapy in part because it roughly coincides with the arrival of a critical new technology: the electron, or "hot cathode," vacuum tube. The initial patents for such devices were issued between 1911 and 1913 to inventors from both the United States and Europe, and they achieved rapid market penetration in part because of an x-ray boom brought on by the war (discussed in Chapter 4).[166] The formative work in x-ray therapy, however, did not wait for the introduction of hot cathode tubes. Instead, the first x-ray therapists developed their treatments using "cold cathode" or "gas" tubes that had serious limitations, particularly in terms of the operator's ability to control the x-ray output of the vacuum tube. These limitations, and practitioners' strategies for dealing with the problems, had a significant impact on the development of x-ray therapy, including methodologies, the diseases selected for treatment, and practitioners' understanding and assessment of therapeutic risks.

Obviously, therapists did not "choose," in the sense of selecting amongst competing technologies, to use cold cathode tubes; doctors and technicians worked with the tools at hand. And as a technological narrative, the cold cathode period feels somewhat inert. The inherent limitations of cold cathode vacuum tubes made them a technological dead end, inferior in every way to the thermionic ("hot cathode") emitters developed just prior to World War I. From an analytical perspective, however, the cold cathode period offers an illuminating—and sometimes, disquieting—case-study of medical innovators attempting to proceed in the face of imperfect knowledge and sharp technological limitations.

X-ray users faced three problems experienced, to varying degrees, by many medical researchers. The first problem was intellectual; although x-ray therapists could see clear, visible

[166] Arns, "The High-Vacuum X-Ray Tube."

results of x-ray exposure, they did not understand *why* the therapeutic intervention might produce those results. No one understood precisely how radiation interacted with complex biological systems or why it might produce a particular result in a particular case. The other two problems were technological. The cold cathode emitters employed by early x-ray therapists were fundamentally imprecise, in a way that could be mitigated, but not overcome. The output of any particular x-ray tube changed constantly, in ways that directly impacted the results of therapeutic exposure. The second shortcoming exacerbated the first: x-ray users did not have the technical capacity to measure or quantify the output of those constantly fluctuating emitters. As a result, x-ray therapists did not—could not—know precisely what they were doing to patients.

As a way of encapsulating the state of the x-ray therapy in field, I will start with a paper that appeared in February of 1900, in the *Archives of the Roentgen Ray*'s first issue of the new century. In the opening editorial, editors Thomas Moore and Ernest Payne reminded readers of "the threefold nature of the work of the Roentgen Society," which included not only x-ray photography, but "all the discoveries, inventions, and practical appliances connected therewith," and "the effects of exposure to x-rays, whether of a beneficial or injurious character." The journal's first article of the new century, they noted, offered readers insight in these latter two ends. It was the text of an "admirable" paper on "The X-Ray Treatment of Skin Diseases," given at a Roentgen Society meeting by a London electrotherapist who specialized in the treatment of women, Margaret M. Sharpe. Sharpe's paper offers an overview of the state of the field at the turn of the century. Her talk ranged widely, covering her personal experiences with x-ray therapy, several of the major debates and developments within the field, and a discussion of the advantages and drawbacks of different types of electrical apparatus.[167]

The breadth of Sharpe's paper makes it an excellent vehicle for discussing what Wiebe Bijker describes as the "technological frame" of the cold cathode x-ray community. The term describes the shared understanding of an artifact constructed by the members of a particular community (or, in this case, two overlapping communities: the cold cathode users of the Roentgen Society, in general, and therapists like Margaret Sharpe, in particular) as they interact with the artifact and one another. Technological frames have a reciprocal quality. Even as the community develops a particular understanding of a technology, that evolving technological frame begins to exert a powerful influence; in Bijker's words, it "structures the interactions among the members" of the group, "shapes their thinking and acting," and serves to "guide future practice."[168]

Gathering together the observations from Sharpe's paper, and particularly from her state-of-the-field discussions, reveals several aspects of the technological frame of cold cathode users in 1900. X-ray users understood that the output and internal condition of their emitters changed over time, and they understood that the particular mix x-rays put out by a vacuum tube had some relationship to both the electrical current passing through the tube and the degree of vacuum inside the glass envelope. Operators generally accepted that higher-vacuum tubes put out more penetrating x-rays and required higher operating voltages, but the precise relationship between these variables remained hazy, at best. In fact, an important element of the cold

[167] Thomas Moore and Ernest Payne, "Editorials.," *Archives of the Roentgen Ray* IV, no. 3 (February 1900): 51.

[168] Wiebe E. Bijker, "How Is Technology made?—That Is the Question!," *Cambridge Journal of Economics* 34, no. 1 (January 1, 2010): 69. This state-of-the-field article, published in 2010, provides a useful starting place for finding and working through the deeper literature on this subject, much of which was written by Bijker himself. The Social Construction of Technology (SCOT) approach, and its influence on this project, is discussed in more depth in the Introduction.

cathode technological frame was the emphasis on continuing experimentation: x-ray therapists they saw themselves as experimental investigators, rather than simply as treatment providers. As highlighted by Bijker's explicit linking of his technological frame with Thomas Kuhn's concept of the paradigm, however, a technological frame can also cause obstruction.

To understand the obstructions in cold cathode users' technological frame, we must return to the three problems outlined above: gaps in users' understanding of radiation and biology, the problem of measurement, and the fundamental flaws in cold cathode emitters. Each of these factors reflected a kind of uncertainty. Indeed, this chapter will argue that uncertainty was the common theme, and core problem, of the frame through which x-ray therapists understood their work. And although uncertainty is an inevitable force in the adoption of any new technology or novel therapy, cold cathode users sometimes yielded too easily to the uncertainties that they faced, accepting uncertainty as an acceptable cost of business and papering it over with endless technological optimism.

Gas and gaps: early x-ray equipment

As the starting point of this analysis, the technological issues loom largest, in part because the flaws in x-ray therapists' tools set the fundamental parameters of the other discussions. The obstruction of flawed tools hampered operators' ability to quantify their treatments, confused efforts to parse out the variables that determined therapeutic outcomes in x-ray treatment, and even thwarted the ability of therapists to accurately explain and discuss their efforts with fellow professionals. X-ray users understood and acknowledged some of the limitations in their tools, and innovators strove mightily to overcome the problems with a variety of technological and methodological workarounds. Language about the "miracle" of x-rays notwithstanding, therapists viewed and represented themselves as researchers and scientists, rather than faith healers or magicians, and they correctly saw these issues as direct challenges to their legitimacy and professional identity. But x-ray operators responded to technical limitations in problematic ways. Proponents of x-ray therapy downplayed, and even denied, problems that they encountered, and therapists consistently overestimated their ability to overcome technical shortcomings with some mixture of experience, skill, and personal ingenuity.

Margaret Sharpe discussed the ins and outs of apparatus throughout her talk. Like many early x-ray therapists, Sharpe had a background in electrotherapy, so when she first heard about x-ray therapy, through "an article by Dr. Kümmell of Hamburg in a French scientific journal, the *Annals of Electrobiology*," Sharpe could easily join the revolution. She was comfortable working with the necessary electrical equipment, already had some of the components for an x-ray set-up on hand, and had a relationship with local suppliers who could provide her with the rest of the requisite apparatus. Apparently Sharpe was a reliable customer; Isenthal, Potzler and Co. allowed her to use their equipment and laboratory space for her first two patient trials, presumably to try out the emitter before purchasing it. Sharpe described her initial x-ray set-up as "a 20-inch coil…worked at 20 volts, with an average current of 5 amperes," combined with "a mercury interrupter worked at 6 volts, giving 250 interruptions per minute." The emitter consisted of "a double bulb—one from the Voltohm Electric Company in Munich; diameter of bulb, about 5 inches; vacuum low," its anticathode positioned "at a distance of 5 inches from the patient's nose." After those first two patients, Sharpe purchased "a high vacuum tube," which she installed in her home-office. To power the emitter, she already had "a Wimshurst machine" which "has twelve 2-foot plates, and gives a 10-inch spark under favorable conditions."[169]

[169] Margaret M. Sharpe, "The X-Ray Treatment of Skin Diseases," *Archives of the Roentgen Ray* IV, no. 3 (February 1900): 52–54. The "favorable conditions" probably refer primarily to the humidity level; as humidity rises, Wimshurst machines lose efficiency and eventually stop working altogether.

To understand the meaning of this array of numbers and terminology and the problems confronted by x-ray therapists like Margaret Sharpe, it helps to distinguish broadly amongst the types of equipment and understand the process of x-ray production in cold cathode tubes. For the sake of analytical simplicity, I will divide the devices described in Sharpe's paper into three categories. The first category encompasses the electrical equipment—the components visible in the background of the image—which had to reliably provide a high voltage current to the emitter's anode and cathode in order to produce an electron-accelerating electrical field. Second are the emitters themselves: the vacuum tubes and their interior components. The third category is a catch-all for ancillary equipment, including shielding, measurement devices, and therapeutic paraphernalia, like the fluoroscope in the foreground of the image. Eventually, the equipment in this third category would become significantly more standardized, but as Sharpe's article suggests, in the early days it often fell to operators to produce such equipment on their own. As we shall see, this reliance of therapists on homemade equipment and personal tinkering would become an important part of the technological frame that helped structure their therapeutic choices.

In order to get an x-ray vacuum tube to work, the operator first had to solve the problem of electricity; most of the equipment described by Margaret Sharpe falls into this category. Although electrification was already proceeding in many cities by 1900, x-ray technicians often still had to produce their own electricity, usually by employing an electrostatic generator, like Sharpe's Wimshurst machine. Wimshurst devices (also referred to as "influence machines") were impressive looking devices with large, counter-rotating wheels containing a series of metal plates (mentioned in Sharpe's description). An influence machine had a pair of electrodes, often placed on movable arms, with a gap between them, sometimes referenced by operators as the "spark gap." The operator spins the wheels using a mechanical crank, a foot pedal, or even a small engine, and the device builds up a charge between the two electrodes. When a large enough imbalance accumulates between the two electrodes, a spark jumps between them, briefly creating a complete electric circuit, and rebalancing the charges.

When Sharpe described her Wimshurst machine as producing a "10-inch spark," she referred to the distance between the two electrodes. The amount of voltage required to produce a spark between two electrodes rises along with the distance between them, in accordance with Paschen's law. The movable arms of the Wimshurst machine's electrodes thus functioned as a crude voltage regulator, with the machine sparking at higher voltages when the operator moved the electrodes farther apart. Sharpe described just such a case towards the end of her article, noting that her latest set of equipment, acquired only days before the talk, allowed an operator to "graduate the current from a 5-inch to 10-inch spark."[170] The description of spark length, in general, functioned amongst x-ray operators as a crude proxy for the voltages produced by their machines and used in emitters, with longer sparks equating to higher voltages.

Operators with access to the gradually expanding electric grid did not need electrostatic machines to generate a current, but they still had to overcome the problem of supplying their vacuum tubes with a voltage much higher than that provided by a household outlet. To solve this problem, x-ray therapists relied on a type of transformer known as an "induction coil," which consisted of two sets of insulated wire wrapped around a common iron core. Running a current through the primary wire sets up a magnetic field; when this field collapses, it sends a high-voltage pulse through the secondary wire. To cause the magnetic field to collapse, the operator had to interrupt the flow of electricity through the primary—thus Margaret Sharpe's reference to her "mercury interrupter," which probably consisted of a needle repeatedly dipped

[170] Ibid., 59.

into a cup of mercury. When the needle lifted out of the cup, it broke the circuit, collapsing the magnetic field and producing a high-voltage spark. Induction coils, like Wimshurst machines, used paired electrodes on moveable arms as a crude measure of the voltage produced.

Collision course

Having solved the problem of electricity, the operator could focus his or her attention on the actual x-ray emitting vacuum tube. A cold cathode x-ray emitter, in its most basic form, consists of a sealed glass bulb containing a positive and negative electrode—the anode and cathode, respectively. To work, cold cathode emitters relied on the fact that their tubes, though evacuated to a very low internal pressure, still contained residual gas (the name "vacuum" tube, in this case, is something of a misnomer). A collection of gas atoms naturally includes a few particles that have a positive or negative charge, including "free" electrons (electrons not bound to an atom) and also a few positively charged gas ions (atoms or molecules with more protons than electrons). Introducing a high-voltage charge between the two electrodes produces an electric field, drawing the positively charged ions to the anode and the negatively charged free electrons towards the cathode.

As the charged particles in a cold cathode tube accelerate towards their respective destinations, they collide with other atoms and molecules in the tube. Three kinds of collisions matter for the functioning of the tube. X-rays are produced when free electrons, accelerated away from the cathode and towards the anode by the electrical field in the tube, slam into a molecule with enough energy to knock an electron out of place in its atomic shell. The other two types of collisions are chain reactions that provide a source of free electrons to sustain the process. Free electrons result when a positively charged ion strikes the metal cathode in a cold cathode tube with enough force to knock an electron loose; the newly-freed electron flies away, accelerating in the direction of the anode. The collisions also produce substantial heat energy, and electrons will escape from a sufficiently hot surface, in a process known as "thermionic emission" (in this sense, the term "cold cathode" is a misnomer; the cathode actually gets very hot during operation). When a charged particle traveling through the vacuum tube strikes another gas molecule in the tube with enough force to knock an electron out of its atomic shell, it creates a free electron and a gas ion, both of which begin to accelerate towards their respective electrodes. This last event, by increasing the number of charged particles in the tube, is the chain reaction that sustains the process of x-ray production. This is why older cathode x-ray tubes are sometimes described as "gas tubes"; without gas in the tube to facilitate those electron-freeing collisions, the emitter would cease to work.

As this description suggests, the functionality of a cold cathode x-ray emitter depended on the interplay of a variety of factors, any of which could interrupt the chain of events that ended with an x-ray photon leaving the tube. X-ray therapists essentially found themselves confronting a fiendishly complex multi-variable problem, and x-ray emitters, as a result, developed a reputation for finicky, or failed, operation.[171] The problem, and the strategies of various users for solving it, became a major influence in the technological frame of cold cathode users. Margaret Sharpe made the question a central point in her talk, asking her audience, "How are we to reconcile the different and exceedingly opposing actions of the x-rays which have come under my observation?" and suggesting a whole series of possible variables, beginning with "the distance of the tube from the skin, the length of the sitting, the strength of the current, and the vacuum of the tube."[172]

All four of Sharpe's variables were fairly straightforward, and they all fit within the general category of what Robert Arns calls *macroscopic observables*, elements of an operating

[171] Robert Arns coined the term "temperamental tube" to describe the cold cathode devices, and he unearthed a variety of amusing descriptions from contemporary medical journals, including "globes of glass surrounded by zones of profanity." Arns, "The High-Vacuum X-Ray Tube," 859.
[172] Sharpe, "The X-Ray Treatment of Skin Diseases," 56–58.

emitter that a therapist could comprehend and measure with their own five senses. By 1900, many x-ray users—and certainly Sharpe and Harris's colleagues in London—had at least some knowledge of J. J. Thomson's work with cathode rays, and relatively sophisticated users, like those in the Roentgen Society, would have generally understood that the production of x-rays had something to do with interactions at the microscopic level, i.e. atoms, molecules, and subatomic particles. Arns argues, however, that in the technological frame of cold cathode users, the macroscopic variables essentially crowded out the microscopic concepts.[173]

The crowding out process resulted from the fact that macroscopic observables had more day-to-day utility for cold cathode users than microscopic concepts. All of the observables on Sharpe's list—"the distance of the tube from the skin, the length of the sitting, the strength of the current, and the vacuum of the tube"—represented factors over which a therapist could exercise some control. Sometimes that involved adjusting some element of the physical setup, like the length of an exposure or the distance between the patient and the emitter. In other cases, as when Sharpe replaced the Wimshurst machine with an induction coil, or a high-vacuum tube with a lower-vacuum one, the adjustment involved swapping out pieces of equipment. As Arns makes clear, the reliance on macroscopic variables did not represent a simplistic approach. If anything, it meant committing to a life spent constantly slaving over and tinkering with equipment in an attempt to achieve an elusive state of alignment. But macroscopic observables did at least make the challenges comprehensible; they represented solvable problems. Unfortunately, a technological frame organized around these concepts could also obscure important information about how x-ray tubes actually worked.[174]

[173] Arns, "The High-Vacuum X-Ray Tube," 866–67.
[174] Ibid.

Harris and the Three Tubes

John Delpratt Harris illustrated some of the problems with a paper, given at the Roentgen Society in May of 1900 and published in the August issue of the *ARR*, entitled "On a Form of Focus-Tube Which is Self-Heating." Harris (in professional contexts, he generally reduced the first name to its initial) had been made surgeon of the Royal Devon and Exeter Hospital in 1881. At some point he began to develop an interest in electrotherapy, probably as a side practice or hobby; friends described Harris in the classic gentleman-scientist mold: "an interesting companion, being well informed on many subjects," Harris dabbled in a variety of fields, including astronomy (he had inherited "a well-equipped observatory") and music. Electrotherapy, however, seems to have particularly captured Harris's attention. He would eventually switch professional tracks, returning to school to earn an M.D., in 1903, and leaving his surgical post to organize and oversee the creation of an electrical department at his hospital in 1904. His experiments with electrotherapy meant that Harris, much like Emil Grubbé, had most of the equipment already to hand when he heard about Röntgen's new rays.[175]

In his paper to the Roentgen Society, Harris opened with a brief reminiscence on his own early experiences with x-ray tubes:

> "I was in time to use the first bulbous, pear-shaped tube, which gave off x-rays of a brilliant, luminous character, but from so large a surface that extremely misty outlines were the result. The glass became intensely heated almost immediately, and whereas at first the bulbous portion of the tube was brightly illuminated with a greenish-yellow light, there shortly appeared dark spots on the glass, which obtained the name 'fatigue spots.' We were instructed to turn of the current when they appeared, for a minute or more, and then turn on the current again, and so on. If the glass were at once touched with the hand, it was found that it was intensely hot, almost to melting-point."[176]

[175] "J. DELPRATT HARRIS, M.D," *British Medical Journal* 1, no. 3259 (June 16, 1923): 1040.
[176] Delpratt Harris, "On a Form of Focus-Tube Which Is Self-Heating," *Archives of the Roentgen Ray* V, no. 1 (August 1900): 9–11.

When a fast-moving free electron strikes an atom, the collision can cause one of the atom's electrons to briefly shift to a higher-energy state. When the electron falls back to its lower-energy state, it emits a photon. Only a small fraction of the photons produced in this fashion count as x-rays; most fall lower on the electromagnetic spectrum, producing the light and heat that account for Harris's description of "greenish-yellow light" and the intensely hot glass. Crookes tubes, like Harris's "pear-shaped tube," had the anode offset from the primary vacuum chamber, so fast-moving electrons would fly past the anode and strike the glass wall opposite the cathode, turning the glass into the primary "target" for electron collisions.

As suggested by Harris's account, however, the heating of the glass sharply limited the life and performance of such emitters. To fix the problem, a King's College professor named Herbert Jackson began to create emitters with the anode in the center of the vacuum tube, forming a straight-line path with the cathode. In addition to altering the internal geometry of the emitter, Jackson also redesigned the electrodes themselves; in his new tubes, the cathode had a concave shape, intended to focus the stream of free electrons more precisely onto the repositioned anode, which would now function as the principle target for photon-, and thus x-ray-, generating collisions. The redesigned anode featured a flat plate made of heat-resistant metal and set at an angle to "reflect" the x-rays out of the tube. With "the anticathode taking the heat rays and becoming red-hot instead of the glass," Harris pointed out, the new design allowed for much longer exposures, and Jackson's "focus tube" became the standard starting point for x-ray vacuum tube design.[177]

[177] Although this chapter discusses x-ray tube design extensively, it is important to keep in mind that, in the words of Robert Arns, "x-ray tubes were produced by a large number of small firms, a skilled glassblower being the primary requirement for entry into the business, and there was very little standardization, either among manufacturers or among the properties of the tubes made by an individual manufacturer." Tube manufacture, in other words, remained to a large extent artisanal: even when a firm sold "standard" tubes, each was still produced individually, by hand, with the inevitable result that tubes

Here, however, emitter designers encountered the hard technical limitations that would bedevil x-ray users. As Harris explained to his audience, the "fly in the ointment" of focus tubes was that "the brilliancy of the tube was not equal at all times. It seemed as though the resistance inside the tube was increased by use, and we spoke of the tube as a 'hard' or 'hardened' tube."[178] Margaret Sharpe described a similar experience: "I used a high vacuum tube with my influence machine [until] it got so high that the current refused to go through."[179] By 1900, cold cathode users understood that tube "hardening" had something to do with the process of adsorption, wherein the gas in the vacuum tube gradually bonded with the glass walls and other surfaces of the vacuum tube. Recall that the gas in a cold cathode vacuum tube functioned as a sort of fuel for the ongoing chain reaction that produced x-rays—the chain of events began when ionized gas molecules crashed into the cathode. Adsorption effectively reduced the amount of fuel in the vacuum tube. When the amount of gas in the bulb fell below a certain level, the whole process came grinding to a halt. Users would sometimes describe the emitter flickering, like a light bulb, before it stopped working entirely.

Adsorption, like glass temperature, could function as a macroscopic variable; Harris described it to his audience by saying that "the molecular bombardment had imprisoned some of the air molecules in the glass, *which accounted for its frosted appearance*." (Note, however, that the emphasis falls on the visual—the frosted glass, rather than on the actual air pressure within the vacuum tube. As we shall see, this difference mattered a lot.) Adsorption occurs as the result of many factors, but the ionization of gas in the tubes during their operation dramatically sped up the process. Adsorbed gas could also, however, be released from the internal surfaces of an x-ray tube, particularly by heat. As Harris explained, "we took a glass

of the "same" design always had idiosyncrasies. This fact would have important ramifications, as discussed below. Ibid., 9; Arns, "The High-Vacuum X-Ray Tube," 861.

[178] Harris, "On a Form of Focus-Tube Which Is Self-Heating," 9.

[179] Sharpe, "The X-Ray Treatment of Skin Diseases," 58.

spirit-lamp, and, having lighted it, waved it below the focus-tube, and shortly the platinum anticathode glowed and the brilliancy of the tube was restored. It was explained by saying that the heat had reduced the vacuum and restored the original condition of the tube." Harris's description makes clear that the spirit lamp technique involved working with an energized emitter; in order to judge when the lamp had worked, the operator visually observed "the brilliancy of the tube"—a reference to the aforementioned discharge glow generated as a side effect of the ionization of gas molecules in an active emitter.[180]

Harris had previously ordered a special, "extra-sized tube," and he noted in working with the oversize emitter that "it was not enough to simply warm the glass of the bulb. The tube became bright sooner if the flame were made to impinge as near the cathode as possible." Concluding that "there was an advantage in making the cathode hot," Harris proceeded to order a series of tubes designed to insulate the cathode, preserving the heat generated by the gas ion bombardment and keeping the metal at a high temperature. "In the happiest specimens" of the resulting emitters, according to Harris, the tube "regulates itself, and may be kept at work without stopping, with scarcely any flicker, for half an hour or more."[181]

Harris had probably stumbled onto a crude version of the technology that would ultimately displace cold cathode emitters: the aptly named "hot cathode" emitter. As previously discussed, "cold" cathodes actually get quite hot during operation, as positively-charged gas ions crash into the surface of cathode. That heat helps to sustain the emitter's performance, since a sufficiently hot surface will emit free electrons. Hot cathode emitters bypass the ion collision step, heating the cathode instead with an electric filament. As a result, an efficient hot cathode emitter can operate independent of the gas pressure in a tube; it does not require bombardment of the cathode by ionized gas molecules to start or sustain the reaction. Since

[180] Harris, "On a Form of Focus-Tube Which Is Self-Heating," 9–10. Emphasis mine.
[181] Ibid., 10.

Harris's design did not include a heating filament, it still relied on the residual gas in a tube to start the reaction and heat the cathode and had nowhere near the level of efficiency of a true hot cathode emitter. Of the half dozen such vacuum tubes Harris commissioned, only one or two could apparently maintain high enough cathode temperatures to function without the occasional support of external heat from a spirit lamp. Nevertheless, the Harris design represented a significant potential upgrade in reliability, able to operate longer and at higher levels of vacuum.[182] Moreover, the Harris design was innovative; he devised and publicized his "self-heating" or "self-regulating" tube four years before Arthur Wehnelt would describe the cathode-heating phenomenon and eleven years before Julius Edgar Lilienfeld would apply for the first patent on a hot cathode x-ray emitter, using a modified version of Wehnelt's design.[183]

Explaining Harris's paper, and his self-heating tube, in a way that emphasizes the heating of the cathode and thermionic emission as the source of Harris's success implicitly relies on a modern technological frame for x-ray emitter. With its focus on the macroscopic observable factor of heat, however, Harris's technological frame led him to tell a slightly different story based on the two macroscopic observables that he could readily discern: the temperature of the vacuum tube's glass envelope and the "brilliancy" of the "greenish-yellow light" that it gave off during operation, which Harris associated with the quality of its x-rays. So powerful were these associations that they ultimately took on an additional significance for Harris, who, at the end of his talk, offered a new theory, that heating the glass walls of the emitter both "acts in assisting [x-rays] to pass through the otherwise partially opaque [to x-rays] glass." Thus, in Harris's technological frame, the story of the self-heating tube read like a popular children's tale. The first vacuum tubes had "a very satisfactory brilliance," but their glass

[182] Ibid. We do not have to trust Harris on this assessment; he allowed other Roentgen Society members to try out the tubes in advance of the meeting, and their favorable reports appeared in the comments.
[183] Arns, "The High-Vacuum X-Ray Tube," 862–63.

walls got too hot—"intensely hot, almost to the melting point"—and had to be turned off. With Jackson's focus tube, the glass wall "would be quite cool," allowing for extended operation, but "the brilliancy of the tube was not equal at all times" and required the heating intervention of the spirit lamp. Harris's tube, with its glass heated "not intensely, but very perceptibly," combined the "brilliance" of the early emitter with the extended operation of Jackson's focus tube; it got the tube temperature just right.[184]

Uncertainty Built In: The Vacuum Problem

In fairness to Harris, he ascribed the value of glass temperature to its relationship with the vacuum level in a vacuum tube, theorizing that "the heat-waves helping to repel the viscous gas molecules from the surface of the glass, as well as from the electrodes," preventing adsorption and thus keeping the vacuum of the tube from creeping too high. Harris got to this explanation in a way that made sense; heating the glass of a tube with a spirit lamp really did liberate some adsorbed gas and reduce the vacuum inside the tube. His statements about vacuum levels, however, elide a crucial fact: Harris never actually measured the vacuum inside the tubes. Instead, he *theorized* about the conditions inside the glass envelope based on his technological frame, starting from a belief that, he acknowledged, actually had not been settled—while Harris ascribed to the vacuum theory, he noted that "many explanations have been given to account for these phenomena."[185]

Margaret Sharpe made the controversy over vacuum much more explicit. Although Sharpe included vacuum amongst her four macroscopic observable factors and made reference to it several times, as when she explained the failure of a treatment by saying that "I may have been using a tube with too high a vacuum," Sharpe also had doubts about whether the air

[184] Harris, "On a Form of Focus-Tube Which Is Self-Heating."
[185] Ibid., 9.

pressure inside the emitter bulb mattered at all. Describing the work of another therapist, Dr. Albers-Schonberg, who had seen dermatitis develop in many of his patients, Sharpe informed her audience that "I am inclined to think that the dermatitis has more to do with the condition of the current than with the condition of the vacuum of the tube," based on personal experience: "I used a high vacuum tube with my influence machine till it got so high that the current refused to go through; now I am using a very low one, but I do not see any difference between the action of a tube excited by a coil…and a tube excited by a Wimshurst…"

The confusion about vacuum matters for an analysis of x-ray therapy because it gets at a recurrent theme of the cold cathode technological frame: users like Sharpe and Harris often substituted theorizing for measurement and observation when it came to determining what was going on with their equipment. Often, these theories relied on a sort of half-truth version of the story—think of Harris, using the knowledge that heating glass could release adsorbed gas to conclude that keeping the glass warm would repel gas molecules before they could adhere in the first place. Had Sharpe, Harris, or other cold cathode users actually measured or experimented with conditions in their tubes, they would have found that the vacuum story was far more complicated, and problematic, than they realized.

In the May, 1900 issue of the *ARR*, James H. Gardiner published a paper describing the results of just such an experiment, undertaken by Gardiner in hopes of developing a standardized system for measuring and quantifying the output of x-ray emitters. Gardiner created a testing rig intended to eliminate the external variables that might alter emitter output, only to discover that

> "the resistance of the tube, as measured by an alternative spark-gap, varied in a most unaccountable manner, which, of course, meant that the rays given out by the tube varied in a corresponding degree. Generally, using a tube that had not been excited for some days, for the first twenty or thirty seconds the resistance was high; then there was a rapid fall, which was followed by a slow and permanent rise. The only way I found of keeping the resistance uniform during an experiment was by connecting the

> tube to a chamber of considerable capacity, and keeping it attached to the exhaust apparatus, so that air might occasionally be let in or removed."[186]

In a sense, Gardiner had shown what users like Sharpe and Harris suspected: that their tubes stopped working because the vacuum level in the tube had gotten too high. But his actual observation, that the resistance of the tube began to change within less than a minute of operation (first dropping precipitously and then rising steadily through the remainder of the operating period), revealed some of the glaring flaws in Sharpe's and Harris's understanding of their equipment. To understand why, it helps to start with the process at work in the tube, so we must return to those ionized gas molecules—the fuel for the chain reaction in a cold cathode tube. The ionization of gas molecules in a vacuum tube could result from either an electron collision or a sufficiently powerful electromagnetic field. The two effects essentially substitute for one another; as the number of collisions increases, the voltage required to sustain the chain reaction in the tube decreases, and vice versa. When Gardiner energized a tube, the metal cathode and anode grew red hot within seconds. That heat released some of the top layer of adsorbed gas from the interior surfaces of the tube, raising its internal air pressure and driving down the electrical resistance of the tube. After that initial internal puff, however, the adsorption of ionized gas molecules onto the tube's internal surfaces began again. The pressure in the glass bulb began to drop, creating the "slow and permanent rise" in electrical resistance described in Gardiner's paper. Eventually, the voltage requirement exceeded what the equipment could deliver, and the tube began to flicker or stopped working altogether—the phenomenon described by both Harris and Sharpe.

Gardiner's account of his experiments makes clear that even though he knew that x-ray tubes theoretically changed their characteristics over time, the actual speed of the change—

[186] J.H. Gardiner, "Measurements of the Absorbability of Roentgen Rays," *Archives of the Roentgen Ray* IV, no. 4 (May 1900): 94.

beginning within twenty or thirty seconds of use—and the degree of variation came as a nasty shock. As we have seen, cold cathode practitioners knew that tubes' resistance changed over time, and they theorized that the change had something to do with rising vacuum. How, then, should we read Gardiner's description, that the voltage on the tube "varied in a most unaccountable manner"? Why could Gardiner not predict or account for the variation? The answer has to do with perception and measurement. Although cold cathode practitioners had a vague sense that the resistance value of a tube moved upward with use, in practice they thought of particular vacuum tubes as having particular qualities, including a relative level of vacuum (usually expressed as the aforementioned "hardness"). In spite of the fact that their technological frame contained the concepts necessary to arrive at a different, more accurate model of tube performance, x-ray users had developed a technological frame where the starting value of a tube essentially remained stable until it stopped working.

This simpler, less-accurate mental model prevailed in part because quirks of the equipment meant that the changes in a tube remained functionally invisible to x-ray operators. As previously discussed, influence machines and other electrostatic generators function by building up a charge imbalance between two electrodes; when the potential difference exceeds the breakdown voltage of the air between the electrodes, a spark jumps across the gap. In an x-ray set-up, the x-ray vacuum tube replaces the electrodes in the circuit. Charges would accumulate in the cathode and anode. The resulting electromagnetic field would ionize the gas in the tube, kicking off the series of chain-reaction collisions that would both produce x-rays and equalize the charges between the electrodes. Then, provided that the influence machine's wheels kept spinning, the process would begin again. The electrical systems cobbled together by users like Sharpe, Harris, and Gardiner had little or no capacity for metering or regulating output; so long as the electrical system energizing the vacuum tube had spare capacity, it would continue to deliver ever-higher voltage to the x-ray tube. This effectively meant that changes in x-ray tube hardness happened without the operator's intervention. Margaret Sharpe experienced this effect, based on her aforementioned comment that "I used a high vacuum tube with my influence machine [until] it got so high that the current refused to go through."[187]

Influence machines had an upper voltage limit, determined by variables like the size of the machine and the speed of its wheels. Beneath that ceiling, however, charge accumulation was primarily a function of time. Gardiner's experiments revealed that as soon as a therapist activated a cold cathode x-ray emitter, the degree of vacuum, and thus the voltage requirement of the tube, began to change. The output of the influence machine would follow suit, and the tube would "soften" or "harden," all without any intervention from the human operator. Induction coils worked in a slightly different fashion, but the key facts were the same: until it hit a hardware limit, the charge on the tube changed automatically to match the vacuum level inside, until such point as the electrical system could not supply sufficient voltage, and the emitter stopped working. Since both influence machines and induction coils produced a non-continuous flow of electricity, all x-ray tubes flickered to some degree, and a careful analysis could theoretically have revealed that the elapsed break between moments of x-ray production would have increased as the vacuum level in the x-ray tube rose. In practice, however, the whole cycle of accumulating charge followed by discharge within the tube happened within fractions of a second—too fast for easy observation. Moreover, charge accumulated exponentially in the electrodes, so the elapsed break between discharges did not grow in a linear fashion with the tube's voltage requirement.

For cold cathode operators, the issue of vacuum lay at the nexus of two kinds of failure. On the one hand, the equipment itself had a fundamental flaw: the working conditions of a cold

[187] Sharpe, "The X-Ray Treatment of Skin Diseases," 58.

cathode x-ray emitter began to change almost immediately, like a stringed instrument beginning to go out of tune the moment the musician starts to play. The second failure, however, came from the fact that users' technological frame was built with equipment that made it difficult to see the movement in tube vacuum levels. As a result, users' day-to-day experience elided what they knew, or at least suspected, about the behavior of vacuum tubes. Of course, Gardiner's and Harris's articles also make clear that technological fixes existed for both of these problems. Those fixes, however, would remain largely ignored by cold cathode users like Sharpe and Harris, owing to another important aspect of their technological frame: the emphasis on using cold cathode emitters as therapeutic tools.

Therapy: The Uncertain Frame

In discussing technological frames, Bijker emphasizes the capaciousness and methodological flexibility of his approach; expanding or shrinking the scope of the frame, by including or excluding groups of users, for example, or by thinking more broadly about the different systems that incorporate an artifact, can open the way to new analytical insights. As discussed in Chapter 1, the early x-ray community incorporated an extremely broad spectrum of practitioners, ranging from academic physicists, like Roentgen Society President Silvanus Thompson, to medical users, to commercial inventors and manufacturers, like Thomas Edison, and even public performers. Members of all of these varied groups participated in the vigorous early debates around x-ray technology. To understand the development of both the technology and practice of x-ray therapy, however, we must tighten the technological frame around users like John Delpratt Harris, Margaret Sharpe, and James Gardiner—users who came to cold cathode x-ray emitters specifically as therapists. The technological frame of these users emphasized the intertwined relationship between the equipment and their needs, skills, and knowledge as medical practitioners, and the frame had unique characteristics that sometimes put therapists at odds with other kinds of cold cathode users. In a sense, therapists actually cared less about x-rays than other users; Sharpe and Harris saw tinkering with emitter design and electrical hardware as the means to reach particular therapeutic goals, rather than as an end in itself or a means to answer "scientific" questions about how the world works. But therapists also saw the technology, and their mastery of it, as central to their professional identity: Sharpe considered herself not simply a therapist, but rather an *electro*therapist.

Like all technological frames, the therapeutic frame arises from the shared concerns, knowledge, and practice of a particular group of users and had its own particular logic. Therapeutic users had very different equipment needs than other members of the cold cathode community, such as emitters that could operate for long periods of time. Unlike, for example, their physicist colleagues, therapists like Margaret Sharpe usually came to x-ray technology as small business owners or service providers. They had to think about the operating characteristics of an emitter in terms of the day-to-day realities of a practice, accounting for factors like the relative ease of use and the ongoing costs of operation. At a time when much remained yet unknown about x-rays themselves, therapists needed to think about how this new energy might interact with living bodies and biological systems. More than other types of x-ray users, therapists had to grapple with the thorny problem of measurement and the potential dangers of radiation exposure, which they witnessed to a much greater degree than other x-ray users. Working in the therapeutic frame, in short, meant living with uncertainty. Therapists faced that challenge with optimism, but sometimes they also made problematic tradeoffs, both in the design of their emitters and in their willingness to treat any condition with intensive radiation exposure.

Vacuum in the Therapeutic Frame

The basic flaw of cold cathode x-ray tubes—the continuous fluctuation in a tube's degree of vacuum and the long-term rise in resistance caused by adsorption—offers an example of how users working in the therapeutic frame diverged from their colleagues. The vacuum issue impacted all cold cathode x-ray users, but it caused significantly more problems for x-ray therapists than for any other group. As discussed in Chapter 1, x-ray photography in the early days often required patients to endure long sittings, ranging from ten minutes to half an hour, in order to produce clear, useful images. But although a few diagnostic imaging applications, such as using fluoroscope screens during surgery, would continue to involve long exposures, x-ray photographers as a group moved to shorten exposure times as much as possible. At the same Roentgen Society meeting where Harris introduced his new tube, for example, Chisholm Williams had given a short talk on a new type of film that he could "strongly recommend" to his colleagues based in part on "the practical advantage . . . that the time of exposure is of little moment," a claim that he backed up by passing around (and printing) side-by-side x-ray images of his hand, one produced in twenty minutes, and the other in twenty seconds.[188] By contrast, therapeutic users had settled on an average of around fifteen minutes' running-time for treatments, based on the survey of reports compiled by Margaret Sharpe, and a therapist often expected to conduct several of these long sessions each day.[189]

As a result of this split, x-ray therapists increasingly followed a different technological path than their peers in the cold cathode community, as the combination of long run-times with the phenomenon of rising, vacuum-driven electrical resistance pushed x-ray therapists into a kind of electrical arms race. In his July, 1900 Presidential Address, Wilson Noble surveyed the

[188] Chisholm Williams, "A New X-Ray Film--'Cristoid.,'" *Archives of the Roentgen Ray* V, no. 1 (August 1900): 12.
[189] Sharpe, "The X-Ray Treatment of Skin Diseases."

casualties of that contest, noting the “doubtful” value of pushing the boundaries “with the coils we are at present using, which for the most part are made for working under different conditions.” He cited, as an example, the popular Wehnelt electrolytic breaks, which functioned using a platinum anode in an acidic solution. To get enough production out of their Wehnelt breaks, therapists began experimenting first with lengthening the platinum anode, and then with adding “a number of short platinums in parallel”; Noble reported that, although the resulting device “overcomes the difficulty to some extent, it does not wholly remedy it.” Sharpe had a blunter assessment: “At first I used a Wehnelt, which worked very well, but had two disadvantages: it was very noisy, which distressed the patient, and the platinum disintegrated at the rate of three-sixths an hour, which distressed me.” Moreover, modifying the electrical equipment to raise output simply pushed the problem down the line, since “no tube,” according to Noble, would “stand it except for a short time.”[190]

As these comments suggest, equipment frustrations played an important role in the therapeutic frame, directly impacting x-ray practice. Unfortunately, it was often a perverse role. John Delpratt Harris ran directly into the limitations of the therapeutic frame when he came to the Roentgen Society to proselytize on behalf of his self-heating tube design. Harris’s talk had all the hallmarks of a good pitch. It began with the origin story, discussed previously, before moving to the sad testimonial of a friend who “had to deplore the loss of his favorite tube, melted by the spirit lamp.” Harris helpfully pointed out that, “should [self-heating] tubes come into common use, such catastrophes as above related would seldom, if ever, occur,” before issuing a direct appeal: “My object in bringing these few notes of experiments to the notice of the

[190] Wilson Noble, “Presidential Address,” *Archives of the Roentgen Ray* V, no. 1 (August 1900): 25; Sharpe, “The X-Ray Treatment of Skin Diseases,” 59.

members of the Roentgen Society is that those having time and leisure may try these tubes in their laboratories, and help to bring them to their best perfection."[191]

The audience, however, greeted Harris's presentation with skepticism. In the subsequent question-and-answer section, William Noble reported that he had experimented with one of Harris's tubes, and although Noble reported "that the tube was a very good one," in light of the emitter's long run-times, "he feared that the glass near the cathode would soon deteriorate and puncture." Noble's comment prompted another audience member to weigh in, mentioning "some experiments he had made some years since on the same lines," and reporting that "he always found the glass liable to crack and become punctured just where it joins the globe behind the cathode." Harris responded "that the tube shown had seen a great deal of service during nine months" and emphasized that "so far the glass showed no deterioration and no puncture had taken place," but his pleas fell on deaf ears. The tube design won no converts.[192]

To understand why, it helps to pay close attention to what Harris actually asked of his audience: not that they purchase his tubes, but rather that they adopt and attempt to improve upon his design. Cold cathode vacuum tube production was a quasi-artisanal process. Although some firms sold "standard" vacuum tubes through distributors or the mail, the actual production of emitters still relied on the skilled work of a single glassblower, producing bulbs one at a time. X-ray users purchasing their equipment in person expected to double as vacuum tube designers, giving the glassblower specific design requests and specifications.[193] As a result, even tubes produced by a single glassblower varied substantially from one another; by Harris's

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[191] Harris, "On a Form of Focus-Tube Which Is Self-Heating." The solution to Harris's problem, eventually hit upon by Wehnelt and other tube designers, was to heat the cathode directly, with an electric filament, rather than trying to design the glass envelope in such a way as to reflect heat back onto the cathode.
[192] Ibid., 11.
[193] Arns, "The High-Vacuum X-Ray Tube," 867.

own admission, “whilst all” of the half-dozen tubes he had ordered “have the property of being self-heating, all have not been equally good in generating sufficient heat to be self-regulating.” In fact, only a few performed as hoped, running for a half hour or more without requiring intervention or adjustment.[194]

Under such circumstances, it should come as little surprise that other cold cathode users did not rush to try their hand at implementing Harris’s tube design. Other therapists saw little reason to risk their money on testing a design that might not work and might break even if it did work. Moreover, insofar as Harris had identified a clear problem, a solution already existed, in the form of the spirit lamp. To move beyond the lamp and work out how heat affected cathode performance required a certain amount of theorizing about the behavior of subatomic particles in an energized vacuum tube—put crudely, thermionic emission was a “physics problem.” Physicists belonged to the cold cathode user base and shared some of the problematic assumptions of its frame; as Robert Arns noted, even the physicist who designed one of the first true thermionic emitters initially described his device using the assumptions and language of the cold cathode technological frame and may not have initially understood what he had achieved. Nevertheless, while there is no guarantee that physics training would have caused Harris or other therapists to tease out the actual mechanisms at work in his self-heating tube, the fact that the group most interested in longer-running tubes lacked that knowledge as a part of their technological frame certainly made it less likely that Harris or his colleagues would recognize what he had created. It would not be the only instance of therapists passing up a solution to the problem of changing vacuum levels.[195]

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[194] Harris, “On a Form of Focus-Tube Which Is Self-Heating.”
[195] Arns, “The High-Vacuum X-Ray Tube.”

Hardness and the Human Body

As we have seen, adsorption-driven changes in vacuum levels affected the operation of x-ray emitters, driving up the electrical resistance of a tube until it ceased to function. Changing vacuum and rising electrical resistance also changed the nature of radiation produced by an x-ray emitter. For therapists, in particular, these changes critically impacted both the therapeutic efficacy of an x-ray treatment and the relative risk of the procedure for patients.

The term "x-ray" encompasses a band of radiation at the upper end of the electromagnetic spectrum. All of the energy in this band can cause both short and long-term damage to human bodies, but the particular biological effect of x-ray exposure varies significantly based on the energy level of the particular ray. X-ray practitioners understood that their emitters produced a spectrum of different rays with different properties. J. H. Gardiner succinctly summarized the state of knowledge in the field in the introductory portion of his talk to the Roentgen Society:

> "That Roentgen rays, produced by different tubes or by the same tube under varied conditions of excitation, differed considerably in their properties was recognized in the very early days of the discovery… One of the most important differences in the properties of Roentgen rays is that of their power of penetrating various substances—their absorbability. These variations are so striking when using a luminescent screen that some time ago it was proposed that Roentgen's symbol X should be mad to stand for rays of one degree of absorbability, and that others should be called X_1, X_2, X_3, and so on… It has now become customary to describe a tube which gives rays that have but little penetrating power, and are easily absorbed by the medium they encounter, as a soft tube; and a tube giving rays of great penetrating power, which are comparatively slightly absorbed, as a hard tube. Between these two extremes, of rays almost wholly absorbed by a few millimeters of soft tissue and other that will penetrate several inches of iron, it is possible to produce rays of any intermediate degree of absorbability."[196]

As Gardiner's description suggests, early x-ray therapists understood the "hardness" of an x-ray tube or its radiation (practitioners used the terms more or less interchangeably, i.e. a "hard" tube produces "hard" x-rays) in terms of its correlation with "penetration" and

[196] Gardiner, "Measurements of the Absorbability of Roentgen Rays," 90.

“absorbability.” Bullets offer an apt comparison for understanding x-ray penetration. Like bullets fired from a slingshot versus bullets fired from a rifle, rays at the lower end of the x-ray spectrum have considerably less penetrative power than those from the upper reaches of the band, with the ease of penetration determined by a combination of the density and depth of the material.[197] “Hard” x-ray photons come from the upper end of the x-ray spectrum. They are the most energetic photons, and, as a result, have the highest penetration value. The “hardness” terminology has persisted, with x-rays at the upper and lower ends of the spectrum still described as “hard” and “soft,” respectively. From a therapeutic perspective, the relative hardness of an x-ray determines its usefulness for targeting a treatment to a particular part of the body. One cannot, for example, treat internal tumors with soft x-rays, because the patient’s skin will absorb the rays before they reach the tumor.

The concept of “absorbability” can likewise be thought of using a bullet metaphor. Bullets and other projectiles cause damage to living tissue through the transfer of kinetic energy. In the case of x-rays, rather than simply stopping the photon, the target actually absorbs it. The result, however, is similar, in the sense of producing a harmful energy transfer. All ionizing radiation (including the emissions of radium, discussed in the next chapter) confers therapeutic benefit by this mechanism: energy absorbed from radiation does damage to living cells. Cellular damage can take many forms, but the most important harm occurs in the cellular DNA—the molecular blueprints that control the cell’s activities. In some instances, that harm kills the cell outright. In other cases, the cell survives but suffers impairment, particularly with regard to its ability to copy its DNA and successfully complete the process of cellular reproduction.

[197] The analogy is imperfect; in reality, x-ray penetration works in fractional terms, hazily describing the percentage of photons that pass through a given target, somewhat like taking a survey of the results of multiple bullets on separate trajectories, and then attempting to describe the overall results.

Because it does the most important harm through DNA damage, radiation disproportionately affects cells with high rates of division and replacement: tumors and cancer cells, but also skin and hair, the tissues that manufacture red and white blood cells, the cells that line the inner surfaces of the digestive tract, the mucus membranes, and the reproductive cells. This differential susceptibility to harm makes “absorbability” a tricky concept, rooted not only in the qualities of a particular category of radiation, but in the fact that some tissues appear to absorb radiation more readily than others, showing the effects of exposure more quickly or more dramatically. Since x-ray radiation does its work at the point of absorption, targeting is critically important for successful x-ray therapy. X-rays that do not reach the diseased area are worse than useless; they do actual harm to the unintended absorbing tissues. But, as Gardiner noted, early emitters had

> “no sharp line of demarcation between the production of one kind of rays and another; and although under given conditions a tube will give out a sheaf of rays in which those of one degree of absorbability preponderate, yet at the same moment of time a certain proportion of rays of *all* degrees of absorbability are produced.”[198]

Understanding Hardness and Vacuum

Because, as previously discussed, x-rays result from a quasi-random process of collision, it follows that all x-ray emitters produce a broad spectrum of photons at different energy levels—even in modern equipment, only a fraction of the radiation produced by an emitter falls into the x-ray band. Nevertheless, users can exert significant control over the spectrum produced by an emitter. To do so requires the user to keep a specific voltage on the emitter and, in the case of a cold cathode tube, to control the vacuum level in the bulb. Recall that the photons streaming from a cold cathode bulb are the product of a collision between a free electron, knocked loose from the cathode and accelerated by the electromagnetic field in the

[198] Gardiner, “Measurements of the Absorbability of Roentgen Rays,” 90. Emphasis mine.

vacuum tube, and an atomic target. The energy level of the photon produced relates directly to the energy of the collision between the free electron and its atomic target; higher energy collisions produce higher-energy photons. A cold cathode tube contains gas molecules, some of which inevitably get in the path of the free electrons accelerating through the tube. The resulting collision might create an additional charged particle, sustaining the reaction in the tube, but it also slows down the free electron, reducing its kinetic energy, and reducing the potential energy of the photon produced in the next collision. As a result, adding gas to a cold cathode tube—say, by using a spirit lamp to heat the glass and release some adsorbed gas from the internal surfaces of the bulb—"softens" the tube's output. The emitter begins to produce "softer" x-rays, i.e. a mix containing, on average, lower-spectrum x-ray photons.

Conversely, as adsorption reduces the number of gas molecules in a vacuum tube, free electrons can move down the length of the tube without impediment, arriving at the target with the maximum possible kinetic energy, and producing more hard x-rays. Moreover, the acceleration of free electrons in a vacuum tube is a function of the strength of the electromagnetic field created between the tube's anode and cathode. As previously discussed, reducing the amount of gas in a cold cathode tube raised the tube's resistance, forcing the attached electrical system to operate at ever-higher voltages to sustain the tube's operation. The process of tube hardening thus reinforced itself, as falling gas pressure and rising voltage both contributed to hardening the x-ray emitter and its output.

Cold cathode users in the therapeutic frame had some sense of these facts, but the connections between them remained somewhat unclear, and the very fact of fluctuation made it difficult for x-ray therapists in 1900 to tease out the relationships or make sense of the evidence they collected in the course of their work. Margaret Sharpe offered several examples of the confusing outcomes associated with x-ray therapy. In one instance, Sharpe sought to experiment with the particulars of her equipment set-up using a patient suffering from psoriasis. Since the patient had affected skin all over her body—some parts showed "isolated patches," while other areas contained "no normal skin at all"—Sharpe could try out slightly different treatments on different parts of the patient. She "began on a small patch," using a vacuum tube energized by an influence machine, before, "by way of experiment," switching to a power supply with an induction coil, tied to a household alternating current, to treat another area. Sharpe reported to her audience that the second set-up produced an effect "nothing like so good" as the first, leading Sharpe to speculate that "I may have been using a tube with too high a vacuum." This explanation, however, did not satisfy the therapist. In fact, later in her talk, she came to a wholly different conclusion, telling her audience that:

> "our theory that a soft tube is better than a hard one, because it has not got so much penetration, [must] be given up, and we must seek some other standard of usefulness…I used a high vacuum tube with my influence machine until it got so high that the current refused to go through; now I am using a very low one, but I do not see any difference in the action. There is, however, a great difference between the action of a tube excited by a coil…and a tube excited by a Wimshurst..."[199]

In these observations, Sharpe essentially sought to isolate the variables, current versus vacuum, based on a mix of her personal experiences with treatment and the reports of other x-ray workers. Her theories about tube performance, however, reflected an incomplete understanding of the precise mechanisms at work. To a reader armed with a better understanding of x-ray phenomena and cold cathode tubes, this account reads very differently. As we have seen, current and vacuum were intertwined as variables. Sharpe's Wimshurst machine, according to her description, could produce a maximum spark of ten inches (and that, only under "favorable" conditions). Her induction coil, hooked up to the alternating current in her home, easily doubled that length—it had a significantly higher output ceiling than the induction machine. As Sharpe elsewhere noted, because induction machines often lacked

[199] Sharpe, "The X-Ray Treatment of Skin Diseases," 55–56.

sufficient output to energize a tube for long stretches of time, they were regarded in most circles as acceptable for x-ray photography but unsuitable for therapeutic use.[200]

As Gardiner reported in his paper to the Roentgen Society, after less than a minute of operation the vacuum level of a cold cathode tube and its operating voltage first drops and then begins a steady rise; the two variables move in tandem. As the rising vacuum drives up the electrical resistance within the tube, the voltage output of the influence machine or induction coil rises to compensate, until the electrical system can no longer deliver enough voltage to maintain the process, and the emitter stops working. Margaret Sharpe was correct that the tube energized by an induction coil would produce a different type of x-ray than a tube energized by a Wimshurst machine, but she apparently failed to understand that insofar as the difference reflected the much higher voltage output of the coil, it also reflected a higher vacuum level within the tube.

Of course, to accept this explanation, we have to reckon with Sharpe's assertion that tubes of differing vacuum levels perform similarly. On closer inspection, however, Sharpe's description of the vacuum levels in her tubes begins to look shaky. Like most x-ray therapists, Margaret Sharpe worked with vacuum tubes that came from the manufacturer as sealed units; she did not make use of a vacuum pump. When Sharpe described the vacuum level in her tubes, she was relying on a combination of some value assigned at or before activation and then using the ongoing function of the tube as a proxy measurement of electrical resistance—thus her description of the vacuum in a tube as "so high that the current refused to go through." The problem, of course, is that the same tube, when hooked to Sharpe's induction coil, would have immediately begun to work again; it would look like a "low vacuum" tube, even though the electrical resistance would remain the same. It therefore seems likely that Margaret Sharpe's

[200] Ibid., 53–54.

description of the internal vacuum level of her cold cathode tubes bordered on meaningless, reflecting neither the actual degree of exhaustion in the tube nor the actual working voltage on the emitter.[201]

Discarding Sharpe's assessment of the working conditions in her tubes resolves the discrepancy in her analysis. When Sharpe suggested that the variation in effectiveness between her two psoriasis treatments reflected a variation in the vacuum of the tubes, she was correct. But she was *also* correct when she subsequently concluded that the variation resulted from replacing the Wimshurst machine with the induction coil, because the additional voltage supplied by the coil allowed the tube to operate at a higher degree of internal exhaustion. This also suggests a possible explanation for the coil's unsatisfactory performance. The higher-power emitter produced harder x-rays, with a higher penetration value, so less of the radiation got absorbed by the afflicted skin. Instead, the rays either passed through the body entirely or were absorbed by other tissues.

Accepting Uncertainty

In both this example and the self-heating tube article, both Margaret Sharpe and John Delpratt Harris relied on theorizing, rather than measurement, when it came to determining the internal vacuum level of their cold cathode x-ray tubes. Moreover, neither therapist had a means for controlling the vacuum or the electrical resistance, which fluctuated over the course of a therapeutic session (although Harris's self-heating tube made some headway on the problem). As we know from James Gardiner's paper, however, Sharpe and Harris could have addressed both problems. For voltage measurement and regulation, Gardiner used spark gaps, both in parallel and in circuit with the cold cathode vacuum tube. These consisted, in his description, of "two highly-polished balls ½ inch in diameter," held apart at a fixed distance by

[201] Sharpe, "The X-Ray Treatment of Skin Diseases."

adjustable armatures. Though crude, Gardiner's methodology worked, providing both a standard for measurement (inches of gap) and some capacity for voltage regulation. It allowed him to show, for example, that tube hardness depended in part on voltage, since he could take a "soft" tube and "harden" it by lengthening the gap that preceded the tube in circuit. Meanwhile, to regulate the vacuum in his tube, Gardiner connected it "to a chamber of considerable capacity, and [kept] it attached to the exhaust apparatus, so that air might occasionally be let in or removed." This equipment configuration also made it possible for Gardiner to monitor the pressure in his vacuum tube.[202]

Gardiner's strategies for measuring and controlling vacuum and voltage on his x-ray tube were not particularly novel. All x-ray therapists would have understood at least the basic concept of the spark gap; it was a common feature on both electrostatic machines and induction coils, used to measure and regulate the devices' output. In fact, we know that Sharpe's x-ray set-up actually included a spark gap, but set in parallel, rather than in series, with the emitter. And although they had fallen out of favor by 1900, the original x-ray set-ups often made use of a vacuum pump. As discussed in Chapter 1, Emil Grubbé described a vacuum pump as part of the assembly for his original x-ray apparatus, with Grubbé and his collaborator, Albert Schmidt, actually using the pump to control the output of their emitter.[203]

It would be a mistake, then, to treat the lack of measurement and control mechanisms in Sharpe's and Harris's x-ray emitters as the unavoidable product of technical limitations. Rather, therapists like Sharpe and Harris *accepted* uncertainty: the imprecision of their information and control directly resulted from choices made within the therapeutic frame—choices that felt entirely reasonable, but led to problems. From the time Emil Grubbé opened

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[202] Gardiner, "Measurements of the Absorbability of Roentgen Rays," 94.
[203] Grubbé, *X-Ray Treatment: Its Origin, Birth, and Early History*; Gardiner, "Measurements of the Absorbability of Roentgen Rays."

his first clinic in Chicago, x-ray therapy was dominated by the traditional business model of customers paying for treatment from a therapist in a private practice. For therapists working as small business owners, it made good sense to adopt the logic articulated by Sharpe, that a therapist "cannot lay too much stress on the importance of simplicity," because "the less there is to get out of order, the more chance there is of a fairly peaceful life for the electro-therapeutist."[204] Her pronouncement was mirrored in advertisements of x-ray equipment, where suppliers increasingly sold complete apparatus kits, often contained in cabinetry as a single unit, and touted simplicity and ease of use, rather than complexity and customization. In a typical example, Boston's L. E. Knott Apparatus Co. took out a full page ad in the *American X-Ray Journal* celebrating new coils that, by drawing from an electric light socket, eliminated components for "easy and reliable" operation.[205] In the therapeutic frame, introducing extra spark gaps and vacuum pumps had no appeal; it represented additional complexity and additional expense for users who had no desire for either.

Skin Deep

Thus far, this analysis has focused on the technical realities of radiation and x-ray emitter equipment, as it existed in 1900. The opening of this chapter proposed analyzing the development of x-ray therapy using the technological frame approach in conjunction with two analytical questions: what exactly were x-ray therapists attempting to do, and what limitations constrained therapeutic decision-making? The technical analysis has focused on the question of limitations—fluctuating vacuum levels, equipment without tools for measurement or control, punctured and melted glass, harried therapists trying to run a practice. Implicitly, this analysis has answered the other question in business terms; x-ray therapists like Margaret Sharpe sought

[204] Sharpe, "The X-Ray Treatment of Skin Diseases," 59.
[205] "New Coil for X-Ray Work," *The American X-Ray Journal* 6, no. 3 (March 1900): 746.

to run a successful practice, where patients would pay for x-ray treatment. But when x-ray therapists spoke to one another about what they were attempting to do, they described it not in business terms, but in terms of treating disease and improving life for patients. To get at a deeper answer to the question of what therapists were trying to do therefore requires thinking about the specific qualities of the diseases and conditions that therapists sought to treat. A deeper analysis must also grapple with what early x-ray therapists understood or misunderstood about the causes of disease, the effects of x-ray exposure, and the processes at work in the human body. Finally, any analysis of therapy must pay attention to methodology. The outcomes of therapeutic experimentation hinged not only on mechanical choices, like what type of emitter to use or how long to expose the patient to radiation, but on how practitioners measured the output of their tubes and how they defined and assessed success or failure in treatment.

Speaking in December of 1900 to the assembled members of the newly-formed Roentgen Society of the United States, President Heber Robarts claimed that x-ray therapy "contemplates the cure of those diseases known by the profession as practically incurable." In this category, he included two skin conditions, non-syphilitic eczema and lupus (a broad term that referred to several diseases of skin, discussed below, rather than to the autoimmune disorder), that had "baffled the world" prior to the advent of x-ray therapy. "With x-rays," he proudly proclaimed, "failures are not known and the cures are effected without giving pain or leaving scars."[206] Robarts, as was his wont, overstated the case a bit, but he had a point. X-ray therapists experimented with treating almost every disease imaginable, but skin diseases and conditions received far and away the most attention in the early days of x-ray therapy, often with striking results. Margaret Sharpe, like Robarts, mentioned both eczema and lupus in her paper to the Roentgen Society of London. Like Robarts, she reported using x-rays to improve the

[206] Robarts, "President's Address," 808.

appearance of scar tissue, and she also added psoriasis and hypertrichosis (unwanted body hair) to the list.[207]

As Sharpe reminded her audience: "the discovery, so to speak, of x-ray therapeutics [was] due to certain accidents, in the shape of severe dermatitis, that befell both experimenters and experimentees in the early days of skiagraphy."[208] The clear reaction of human skin to x-ray exposure made it a logical focus for therapeutic experimentation. Skin diseases also offered other advantages for experimental therapy. In terms of assessing the success or failure of treatment, skin diseases have the tremendous benefit of being visible to the naked eye. One can easily tell whether or not a hypertrichosis treatment has succeeded based on whether or not the hair has fallen out in the treatment area. Skin is also easy to work with; a therapist could see the treatment area and place the x-ray emitter within a few inches of it. Margaret Sharpe suggested that her audience, when embarking on treatment with a new patient, "begin with the tube at 8 inches . . . then gradually lessen [the distance] to 4 inches" once the therapist had ascertained that the subject showed no unusual proclivity to x-ray dermatitis. Moreover, many skin diseases *did* respond to x-ray treatment, sometimes with almost magical results: "The next case I operated upon with my influence machine was an eczema . . . It simply faded away, getting paler and paler each time. There was no inflammation or soreness. At the present time (it was done in April) you could not tell which hand it had been on."[209]

To the analytical question—"What exactly were x-ray therapists attempting to do?"—an interesting answer begins to emerge: they saw x-rays as a sort of energy-based skin cleanser. The skin conditions treated by Margaret Sharpe and other x-ray therapists shared the critical quality of location, but they had very little else in common, either in cause or presentation. The

[207] Sharpe, "The X-Ray Treatment of Skin Diseases."
[208] Ibid., 58; see also Grubbé, *X-Ray Treatment: Its Origin, Birth, and Early History*. The relationship between accidental dermatitis and x-ray therapy, is discussed extensively in Chapter 1.
[209] Sharpe, "The X-Ray Treatment of Skin Diseases," 55 & 58.

term “lupus,” as used by Sharpe, Robarts, and other x-ray therapists, usually referred to a patch of skin with a steadily-expanding ulcer or lesions; it could have a variety of causes, including tuberculosis infection and cancer. Psoriasis—usually presenting as patches of red, scaly skin—results from an autoimmune disorder. “Hypertrichosis” describes a wide variety of complaints related to unwanted body hair, with causes ranging from genetics to chemical exposure.

In treating a tuberculosis infection of the skin, a present-day physician would attempt to remove the offending pathogen, perhaps with the use of antibiotics. The same doctor would almost certainly not respond to a complaint about body hair with antibiotics. Instead, he or she would think about patients’ afflictions as individualized problems with particular solutions. X-ray therapists, by contrast, viewed skin afflictions as a quasi-universal category and x-ray exposure as a similarly broad solution. Put more simply, the purpose of x-ray therapy was to remove things from the skin, and any unwanted skin condition—ulcers and infections, but also hair, pigments, and scars—offered a possible treatment opportunity. Interestingly, x-ray therapists did not deny that skin conditions resulted from a variety of possible causative agents, including bacterial infections, cancer, and simple human variation; no x-ray therapists seriously contended that lupus and hypertrichosis had the same root cause. Instead, therapists sought to develop theoretical models that could explain the success of x-rays in treating multiple types of conditions either by attributing the therapeutic action to some quality of energy exposure or by suggesting that x-rays could somehow provoke the body into its own therapeutic response.

X-ray therapists’ focus on skin would define many aspects of early treatment practice, even after they began to move to treating non-skin conditions, like uterine fibroids and tumors. The early emphasis on treating skin conditions also highlighted some of the major shortcomings in the x-ray therapy frame. It eliminated the distinction between disease and cosmetic treatment, since the same emitter could remove hair or shrink an ulcer, and opened wide the door for therapeutic experimentation, since a therapist could take, for example, success in the treatment of lupus as a reasonable basis for expecting success in the treatment of eczema. Moreover, it revealed the limits of what therapists knew, or did not know, about biology, radiation, and human health.

Mysteries and Contradictions in the Therapeutic Frame

Margaret Sharpe, in her summary of the x-ray field, identified four pathways by which x-rays might produce a positive therapeutic result in patients' skin. First, she suggested that x-rays could produce a "revitalizing effect" in conditions, such as scar tissue, where skin had suffered prior damage. Second, they seemed to have an "antiphlogostic" effect in cases of psoriasis. The word seems to have an anti-inflammatory connotation, but Sharpe probably chose it in an effort to distinguish the result in psoriasis, where she reduced red lesions on a patient's skin, from the third possible pathway that she identified: the "inflammatory," as seen in cases of dermatitis. Many therapists argued that provoking inflammation could lead to healing, through some mechanism such as stimulating the body to attack a parasite or promoting better blood flow to a diseased area. The fourth pathway involved the "bactericidal" effect of x-ray exposure—the idea that it could kill pathogens. Finally, Sharpe mentioned the possibility of an "analgesic action" associated with x-rays, which therapists had reported in cases ranging from breast cancer to neuralgia.[210]

The particular truth of any of these claims is difficult to tease out. The mechanisms of therapeutic action are, at best, complicated, and terms like "antiphlogostic" and "revitalizing" leave considerable room for interpretation. Moreover, many of these claims ran counter to one

[210] Regarding pain relief, Sharpe professed to have "had no experience with it" herself, but the claim appeared in many other places, including Heber Robarts's Presidential address to the first meeting of the American X-Ray Society. Emil Grubbé, as discussed in Chapter 1, claimed to have provided pain relief for both of his first two patients, one suffering from lupus on his face, the other from terminal breast cancer. Ibid., 56; Robarts, "President's Address," 808; Grubbé, *X-Ray Treatment: Its Origin, Birth, and Early History*.

another. Using inflammation as a treatment pathway, for example, seemed to contradict not only the "antiphlogostic" effect, but also the "revitalizing" action posited by Sharpe, since, as she pointed out, dermatitis could "go on to necrosis." The record makes clear that therapists recognized and struggled with these contradictions and shortcomings. Sharpe took direct aim at the contradiction between pro-inflammation theories and her own claims, arguing to her audience that, where treatment appeared to reduce inflammation, what actually happened involved "a healing or revitalizing effect on the inflamed tissues, [enabling] them to combat the disease of which they are the outward and visible signs." Sharpe categorically rejected inflammation as a therapeutic tool, telling her colleagues that, by causing dermatitis, "you defeat your own object."[211]

More often, though, therapists opted to simply embrace the uncertainty with optimism. Her certainty on inflammation notwithstanding, Sharpe concluded that "my own small but varied experience inclines me to think that the key to the action is still missing," and hedged with her audience, stating that her theories about the biological action of x-rays represented "only my personal opinion; I offer it for criticism," before eventually concluding that the knowledge lay "outside my province as an electro-therapeutist." Sometimes the degree of disconnect took on an almost comical quality. Sharpe's survey of the field included the story of "Dr. Hahn, of Hamburg," who she described as "the first to operate on chronic eczema" by exposing the afflicted skin to x-rays. When Hahn decided to experiment with eczema treatment, he began from a place of almost total ignorance—he professed to have no theory to explain either the cause of the disease or the value of the treatment. In Sharpe's telling, Hahn saw this ignorance as no impediment to good therapy. He theorized that if eczema resulted from "a parasitic affection, the bactericidal action of the [x-]rays…would effect a cure; if, on the other

[211] Sharpe, "The X-Ray Treatment of Skin Diseases," 56.

hand, it were a case of disturbed nutrition of the skin, he hoped that the inflammatory reaction which the process of 'raying' set up might, by the alteration produced in the circulation of the diseased parts, eventually effect a cure." The treatment failed as an explanatory experiment, as "the result did not help [Hahn] to a decision as to the nature of the disease or the nature of the cure." Still, according to Sharpe, "the disease disappeared rapidly, viz., after ten to sixteen applications in all three cases," thus reaffirming the logic that undergirded such leaps into the unknown and recurred throughout the x-ray literature as the rationale for *every* therapeutic experiment with x-rays: "However little we may know about their nature and method of working, we can no longer doubt that we possess in the so-called x-rays a very valuable therapeutic agent."[212]

Dosimetry: measuring the problem, or the problem of measurement

Therapists' uncertainty over how x-rays worked was exacerbated by the difficulty of knowing precisely what type of radiation exposure they were giving to patients. As discussed in Chapter 1, early would-be therapists like Émil Grubbé had no way to measure dosimetry, or the amount of radiation being absorbed by the afflicted tissues, beyond looking at a patient's skin for evidence of erythema or dermatitis. This approach, however, had obvious problems. As discussed below, the concept of dosimetry-by-erythema raised patient safety concerns. It also, however, had severe limitations from a pure functionality standpoint. Patients had different skin types and tones, and reactions to x-ray exposure differed wildly—according to Sharpe, "no two patients ever behave alike . . . [so] you have, so to speak, to take the measure of sensitiveness of each one." As a result, definitions of the erythema standard took on a Talmudic quality. One early publication defined the "Threshold Erythema Dose," as "that quantity of radiation which, when delivered at a single sitting, will produce in 80% of all cases tested, a faint reddening or

[212] Ibid., 56–58.

bronzing of the skin, in from two to four weeks after irradiation, and in the remaining 20% will produce no visible effect." Putting these factors together made measuring dosage through skin reactions an imprecise exercise, to say the least. It also made real-time measurement impossible, since erythema took time to develop.[213]

In response, operators developed a variety of strategies for gauging the output of their tubes in real time. Chapter 1 recounted Emil Grubbé's description of watching his own hand in a fluoroscope. Grubbé noted that as the degree of vacuum in an x-ray tube rose, the image of his hand in a fluoroscope screen changed: "the flesh parts gradually became transparent and finally disappeared, leaving only the outline of the bones to cast a shadow in the glow of the crystals." By 1900, according to James Gardiner, it had become "the usual practice to denote the kind of rays produced with a given tube descriptively by saying that the flesh of the hand is opaque or transparent, or that the bones are transparent, etc." Visual inspection of the energized tube offered another means of assessing its operation. Electron-gas molecule collisions produce a soft glow, so, as suggested by Harris's previously-discussed description of "fatigue spots" and "brilliance," an experienced x-ray therapist could tell something about the operating conditions of the tube by observing the changing quantity and quality of the light it produced.[214]

Both of these approaches were versions of the "macroscopic observable" approach previously discussed, and both had problems. Although therapists did not fully appreciate the danger at the time, both methodologies exposed the operator to x-ray radiation. That fact would come to matter very much. More obviously, as Gardiner pointed out, "such descriptions, although giving certain general information of the kind of rays produced, lack the exactness desirable" for clinical work. To overcome this difficulty, Gardiner developed an improved version

[213] Ibid., 58; Mould, *A History of X-Rays and Radium*, 13.
[214] Grubbé, *X-Ray Treatment: Its Origin, Birth, and Early History*, 39–41; Gardiner, "Measurements of the Absorbability of Roentgen Rays," 90; Harris, "On a Form of Focus-Tube Which Is Self-Heating."

of a methodology first described by Röntgen. The method used an aluminum shield punched with a series of holes. Röntgen covered the holes with increasingly thick sheets of aluminum foil; when placed between an energized emitter and a fluoroscope, the holes formed a visual scale depicting the relative penetration value of the radiation coming from an energized tube—the higher the penetration value of the radiation, the more dots of fluorescence would appear on the screen. Gardiner developed several improvements for the apparatus, such as replacing the static scale with a sliding strip indicator. He also applied the methodology to photographic plates, thus creating a reproducible, shareable record. Unfortunately, as previously discussed, Gardiner's experiments also unexpectedly revealed the enormous degree of constant fluctuation in the output of cold cathode emitters. And although Gardiner managed to stabilize his tube by hooking it to a vacuum pump, for therapists who chose to continue working with sealed bulbs the additional precision offered by Gardiner's and Röntgen's methods existed only in the moment, given the constant flux of the vacuum within the tube.[215]

The Constant: Uncertainty

Thus far, this chapter has argued that every aspect of x-ray therapy at the beginning of the century was characterized by a striking admixture of uncertainty and optimism. Key elements of the apparatus fluctuated, and in ways that users could not easily monitor. Radiation remained difficult to measure, leading many operators to rely on imprecise and potentially misleading macroscopic observables. And where possible technological fixes appeared, the cost, complexity, and difficulty of working with emitters made therapists leery of upgrades. X-ray therapists did not understand the therapeutic action of their equipment, and insofar as they theorized on the subject, the various theories ran in direct opposition to one another. Against

[215] Harris, "On a Form of Focus-Tube Which Is Self-Heating"; Gardiner, "Measurements of the Absorbability of Roentgen Rays," 90.

these uncertainties, however, x-ray therapists maintained a rock-solid faith in the value of x-ray radiation for treating every conceivable kind of skin complaint. And although x-ray therapists would expend much energy in pursuing and improving their art, this set of facts—the uncertainty, the optimism, the limitations of apparatus and measurement—would prove strangely durable in the fourteen years prior to World War 1.

Still mysterious, still contradictory

Perhaps the most obvious example of this theme appeared in a piece exploring the mechanisms by which radiation might impact the body. Writing thirteen years after Margaret Sharpe, *ARR* editor Deane Butcher nevertheless echoed Sharpe's central theme of uncertainty when he wrote that, "We have already at our disposal a vast amount of material accumulated during many years of empirical treatment, but are data, both physiological and pathological, are so bewildering that it is almost hopeless to attempt to marshal them in any due sequence or order." Butcher described x-ray therapy as a mass of contradictions—"an agent which appears to blow both hot and cold." Beginning with the frustrating acknowledgement that "the x-rays are said to cure cancer, but they also cause cancer," Butcher recited a litany of apparent contradictions, culminating in his statement that x-rays of exactly the same quality in slightly different doses will cause irritation with increase of physiological activity . . . or total destruction of tissue and death." In some ways, x-ray theory had moved on from Sharpe's therapeutic pathways; the "antiphlogostic" and irritation discussion had given way to the "Mechanical theory" and the "Cannibal theory," both of which claimed to explain the action of x-rays through (vaguely) cellular and molecular pathways. But Sharpe's "revitalization effect" would have been right at home with Butcher's "Radio-vaccination theory," described as a "vitalistic" approach that had "gradually grown up to account for a number of phenomena otherwise inexplicable," including "the fact that a number of skin diseases of totally different origin and nature seemed to improve exceedingly under small doses of x-rays." The therapeutic frame had not changed much between 1900 and 1913; it was still fundamentally built on optimism and uncertainty.[216]

Vacuum and Apparatus

In the February, 1903 edition of the *Archives of the Roentgen Ray*, the journal's editors described an exciting new piece of x-ray equipment provided to them by Isenthal and Co.—the same equipment manufacturer that provided Margaret Sharpe with her equipment. The new emitter had special side chambers with alternate circuits. One of these heated up an unnamed substance in the side chamber, releasing it into the tube as additional gas. The other alternate circuit performed the opposite function, drawing gas from the main bulb into a side chamber. With these two features, the editors concluded, "it becomes possible to maintain the vacuum approximately constant during prolonged treatment," thus addressing one of the major, persistent flaws in cold cathode tubes.[217]

Or so it seemed at first blush. In reality, the Isenthal tube offered only the grossest measure of control. Referring to an included diagram, the article explained that a practitioner would find it "necessary, by means of a wooden stick, to lift wire *W* and allow a few sparks to jump to *B*, in order to lower the vacuum; on the other hand, in the case of the tube being too low, wire *H* is lifted up for a short while, and the surplus of gas occluded in the metallic coating of the bulb *E.*" Moreover, the pictured emitter still did not include any sort of system for monitoring the actual pressure inside the glass envelope. A therapist willing to master the finer points of the stick method still had to rely on observable factors to determine the vacuum in the tube.[218]

[216] W. Deane Butcher, "The Rationale of Roentgen- and Radium-Therapy," *Archives of the Roentgen Ray* XVIII, no. 1 (June 1913): 16–24.
[217] "New Apparatus," *Archives of the Roentgen Ray* VII, no. 5 (February 1903): 80–82.
[218] Ibid.

The basic concept of "regenerating" a tube by equipping it in this fashion would be iterated and refined. By putting the secondary chamber on a parallel spark path, the process could be made somewhat automatic, insofar as it would automatically trigger when the vacuum and resistance level of the tube rose high enough that it stopped working. But As Robert Arns pointed out in his article on the development of hot cathode tubes, regeneration technology only served to mitigate the problems of cold cathode tubes. It never provided a fine level of control, and eventually a tube under heavy load would still reach a level of exhaustion that caused it to stop working. There remained no substitute for keeping the emitter bulb hooked up to a large air chamber and a vacuum pump—and no appetite, in therapeutic circles, for the additional cost and complication of such an arrangement.[219]

Measurement: Going Ostentatiously Nowhere

This same dynamic—innovation that failed to actually solve the problem of uncertainty—played out even more dramatically in response to the issue of dosimetry. The March, 1903 issue of the *ARR* reported to its readers the development of "Two Gauges for Showing the 'Hardness' of X-Ray Tubes," including one developed by "M. L. Benoist, of Paris, based on the results of his investigations on the absorption of Roentgen rays." As the editors noted, by 1903 numerous individual roentgenologists had, like John Gardiner, developed devices and scales for measuring x-ray penetration. The early systems mostly relied on looking at a scale using a fluoroscope, but systems that employed small, radiation-sensitive film or chemical badges to estimate the amount of radiation reaching a patient's skin gradually gained in popularity. The February, 1904 issue of the *ARR* described such a system, Guido Holzknecht's "chromo-radiometer," which used potassium bromide salts that took on a blue tint when

[219] Arns, "The High-Vacuum X-Ray Tube," 861–63. Arns's article includes a delightful illustration of a parallel-spark regenerating tube, which looks like a light bulb designed by Dr. Frankenstein.

exposed to x-ray radiation. Another popular version, the Kienböck Quantimeter Scale, consisted of a silver bromide film strip, which blackened when exposed to x-rays. A roentgenologist put the strip on a patient's skin, then compare the relative blackening of the developed film strip to a visual scale, its levels denoted by a number and the letter "X": 1 X, 6 X, 10 X, and so on. Another version, the "Sabouraud-Noiré pastille" took advantage of platinobarium cyanide, which changes color with exposure; roentgenologists using this method referred to a "pastille dose" or "Sabouraud pastille."[220]

As the latter two examples suggest, part of the appeal of developing a measurement standard derived from the naming rights. Measurement offered lucrative business opportunities; for Kienböck, Sabouraud, and other purveyors of measurement systems, acceptance of a particular standard could lead to sales of name-branded film strips, testing kits, color charts, and the like. Predictably, a format war ensued, as x-ray entrepreneurs flooded the pages of the *ARR* and other professional journals with new units and measuring devices. A typical example appeared in the December, 1913, issue: a passionate essay by the intriguingly-initialed "W. E. Schall" on behalf of the "Walter" as a standardized unit of dosimetry. In addition to promising that international adoption of the Walter would "greatly simplify both the prescription of the dose in therapeutics and the length of the exposure in radiography," Schall put in a plug for "a slide-rule which has been specially constructed" for use in converting Walters to focus distances.[221] Readers unimpressed by the Walter might instead adopt the

[220] "Two Gauges for Showing the 'Hardness' of X-Ray Tubes (from Fortschritte Auf Dem Gebietefder Röntgenstrahlen, Vi. 2.)," *Archives of the Roentgen Ray* VII, no. 6 (March 1903): 103; "Exact Measurement with Radio-Therapeutics," *Archives of the Roentgen Ray and Allied Phenomena* VIII, no. 9 (February 1904): 151; Mould, *A History of X-Rays and Radium*, 17–20.
[221] Although it is amusing to speculate that "Walter" was the first name of "W. E. Schall," it seems equally likely that the name was chosen for its association with several prominent Walters in the x-ray community. The first of the systems described in the aforementioned "Two Gauges" article was attributed to "Dr. B. Walter," and the Benoist scale and units were sometimes referred to by the co-naming convention of "Walter-Benoist" in the literature. Schall was primarily an equipment manufacturer; examples of the slide-rule appeared in the Schall & Son catalog, and may have been based on a prior

"mega-mega-ion," promoted by Dr. B. Szilard, whose "Iontoquantimeter" would replace imprecise color- or shade-based dose charts with "the motion of a needle on a scale."[222]

Such methodologies seemed to substitute reassuring technological precision for the old skin-based dosimetry. But in a 1913 "Report on Roentgenometry" given to the Fourth International Congress of Physiotherapy in Berlin, Heinz Bauer surveyed the situation with dismay, opening his professional rebuke with a quote from Lord Kelvin: "If you cannot measure it, cannot express it numerically, your knowledge is but poor and deficient." Opining that the market in dosimetry devices and standards had gotten completely out of hand, Bauer concluded that, "Of methods of measurement there is no lack. Indeed, there are too many; for every Roentgonologist devises a new one. But the very number of the methods is the best evidence that *the* method is still awanting [sic]—the method which can be easily reproduced" and understood by any person working with ionizing radiation.[223]

This lack of information also meant that roentgenologists could not share their work with any kind of precision. Palpable frustration with the situation permeated the pages of the *ARR*. In the January, 1914 issue, for example, editor W. Deane Butcher described the development of "a universal standard of X-ray measurement" as "an apparently hopeless task." Noting that "the question of measurement has been so frequently ventilated in these columns that it is unnecessary to dwell further on the desiderata," Butcher compared the quest for accurate dosimetry to the search for "perpetual motion, [and] the North-West passage."[224] A

German model. The Oak Ridge Associated Universities Health Physics Historical Instrumentation Collection contains a Schall slide-rule, which can be seen online at https://www.orau.org/ptp/collection/sliderules/Schall%20Slide%20Rule%20X-ray.htm. W. E. Schall, "X-Ray Units and Measures," *Archives of the Roentgen Ray* XVIII, no. 7 (1913): 273–76; "Two Gauges for Showing the 'Hardness' of X-Ray Tubes (from Fortschritte Auf Dem Gebietefder Röntgenstrahlen, Vi. 2.)"; J. E. Burns, "Radiographic Exposure Slide Rules," *British Journal of Radiology* 72, no. 853 (1999): 48–54.

[222] B. Szilard, "On the Absolute Measurement of the Biological Action of the X Rays and Gamma Rays," *Archives of the Roentgen Ray* XIX, no. 1 (1914).

[223] Heinz Bauer, "Report on Roentgenometry," *Archives of the Roentgen Ray* XVIII, no. 6 (1913): 220.

[224] W. Deane Butcher, "Editorial," *Archives of the Roentgen Ray* XVIII, no. 8 (1914): 285–86.

piece in the *ARR* describing the research of “Professor Gauss,” in Germany, exemplifies the information problem. Gauss found it “quite impossible to estimate the amount of Roentgen energy” used in his treatments; his results could “only be reproduced with certainty when using Gauss’s exact technique” and apparatus. The *ARR* editor settled on describing the dose as either “300 X,” citing Gauss’s own use of the “Kienböck’s photographic method,” or enough radiation “to ionize thirty Sabouraud pastilles consecutively to the standard color.”[225]

A supplementary comparative chart of measurements at the end of the article attempted to help readers translate these numbers in terms of the “Unit H” of Holzknecht’s Chromo-Radiometer, the “tint” units of Bordier’s Chromo-Radiometer, the Bordier-Galimard Units, the Mache units of Guilleminot’s Fluorometric Quantimeter, and the “kaloms” of Schwarz’s Fallungs-Radiometer. Sometimes the numbers matched well; sometimes they did not. The “10 X” measurement from the article translated into “5 H,” “Tint 1,” “3 to 4 I,” “625 M,” and “3.5 kaloms,” respectively. The very necessity of such a chart demonstrates the extent of the measurement mess. And in their efforts to bring some uniformity to the chaos, members of the roentgenology research community found themselves continually turning back to their original point of reference. At the bottom of the *ARR* chart, a line informs the reader that “5H, or Tint B, or 10 X is the Erythema Dose, the Villard unit.”[226]

The trappings of X-ray dosimetry had grown more technical, but the basic concept of using skin reaction to measure treatment remained as central to x-ray dosimetry in 1912 as it had in 1897. The *British Medical Journal*’s August 31, 1912 issue included an article on “The Treatment of Hypertrichosis” (unwanted body hair). The author, Arthur Ernest Rayner, made reference to both the Benoist and Sabouraud measurement systems, but he ultimately explained the progression of his technique in terms of his patients’ skin reactions. Following

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[225] Butcher, “The Intensive X-Ray Treatment of Myoma,” 2–4.
[226] Ibid., 5.

treatment, the first patient had a "very smart" or "very marked erythema," which led Dr. Rayner to add more shielding over his patients' faces. The results in the second patient showed improvement, with only "some moderate erythema," and in the third patient, a "middle-aged woman" who hoped to get rid of "a full, short beard," Rayner managed to produce hair-loss in the treated areas with only "some faint temporary erythema."[227]

Besides demonstrating the profession's continued reliance on erythema as a way of measuring and communicating dosage, the quality of Rayner's descriptions, with their reliance on categorizing reactions as "smart," or "moderate," or "faint," suggests the continuing imprecision of this approach to dosimetry. When Robert Knox tackled the problem of dosimetry in his 1915 textbook, *Radiography: X-ray Therapeutics and Radium Therapy*, he concluded that there was only one way for a practitioner to calibrate a new cold cathode emitter with respect to the vast array of standards on the market. "Take a new x-ray bulb," he advised, "and give an erythema dose." It turned out, however, that Knox had buried the lede. In 1913, William David Coolidge had filed a patent for a new kind of x-ray emitter that used thermionic emission and did not depend on ionized gas to function. Having expounded at length on the various methods for calibrating troublesome cold cathode tubes with a variety of measurement devices, Knox concluded his section on x-ray measurement with a simple recommendation: "The ingenious method used for the control of the Coolidge tube appears to be the perfect one for the estimation and control of the hardness of the x-ray . . . This not only dispenses with other more tedious methods of estimation but enables the operator to reproduce at any time the particular

[227] Arthur Ernest Rayner, "The Treatment Of Hypertrichosis With X Rays," *The British Medical Journal* 2, no. 2696 (1912): 480–81.

ray he may require." Knox's words might as well have been carved as the inscription on the cold cathode emitter's headstone.[228]

The Trouble with X-rays

In a strange echo of the debates that took place in 1900 over whether x-rays could simultaneously treat inflammation and cause healing through dermatitis, x-ray therapists at the end of the cold cathode period simultaneously relied on erythema as a standard of measurement and decried it as a sign of malpractice. Some practitioners had worried about skin damage as early as 1900. Margaret Sharpe, for example, always used "a mask covered with tinfoil" and "sheets of lead with holes cut in them" to protect the skin around the treatment area. Sharpe advised extending the covering across large swaths of the body to guard against accidental exposure, and she implored her audience, "at the first sign of reaction" on the patient's skin, to "stop the treatment for the time." Many therapists, however, regarded this approach as excessively cautious, and a few even argued that inflammation could promote healing. By 1913, however, Sharpe's view prevailed decisively enough for *ARR* editor W. Deane Butcher to proclaim it "little less than criminal" for therapists to treat patients, as Rayner had done, "without full and adequate skin protection" against erythema and dermatitis.[229]

As the rest of the myoma article demonstrated, however, Butcher's concern for patients literally went only skin-deep. In his attempts to treat myoma, Gauss had used multiple short exposures and metal filters to "harden" the x-ray beam, filtering out the softer x-rays in order to prevent erythema and dermatitis in his patients. Gauss developed these techniques specifically to enable a treatment regimen of "enormous doses," measuring as high as "300 X," or thirty times the erythema dose. Painfully absent from this discussion was whether or not doses that

[228] Robert Knox, *Radiography: X-Ray Therapeutics and Radium Therapy* (A. & C. Black, Limited, 1915), 55–61; Arns, "The High-Vacuum X-Ray Tube."
[229] Sharpe, "The X-Ray Treatment of Skin Diseases," 58; Butcher, "The Intensive X-Ray Treatment of Myoma," 5.

would have destroyed a patient's skin might have some detrimental effect *inside* the body. Butcher must have at least contemplated the issue; in the article, he mentioned that Gauss's emphasis on ever-higher doses aroused "not a little criticism, and even opposition" in the German radiological community.[230]

X-ray martyrdom

The myoma episode suggests that although x-ray therapists in 1912 had adopted a much more cautious approach to skin exposure, the technological frame of x-ray therapy still included some of the same problematic elements that had existed in 1900, including a dangerously high tolerance of risk and a tendency towards therapeutic optimism in the face of uncertainty. Moreover, although both Rayner and Gauss recognized the importance of not burning their patients' skin, neither Rayner, Gauss, nor Butcher seems to evince any sensitivity to the long-term effects of x-ray exposure, even though evidence of those dangers had exploded in the x-ray community.

A celebration in the September, 1913 issue of the *ARR* of the first-ever appearance of radiology as a sub-section at the International Congress of Medicine was marred by the following note: "Dr. Lester Leonard was unable to be present, although he contributed an exhaustive report on the 'Radiography of the Stomach and Intestines.' It is a touching testimony of devotion to our art that, in spite of much suffering due to X-ray dermatitis, he delayed the necessary operation for some weeks in order to finish this report."[231] Leonard died that same month, at the age of fifty-one. He had lived for a decade with x-ray induced cancer, suffering through a progressive series of amputations that began with his finger and ended with the

[230] Butcher, "The Intensive X-Ray Treatment of Myoma," 1–4.
[231] W. Deane Butcher, "The International Medical Congress," *Archives of the Roentgen Ray* XVIII, no. 4 (1913): 159.

removal of his entire arm below the shoulder.[232] His obituary, printed in the November, 1913 issue of the *Archives of the Roentgen Ray* (*ARR*), gives some sense of roentgenologists' grim acceptance of the unfolding disaster:

> "We have lost a good friend, a keen radiologist, and a charming personality in Dr. Lester Leonard of Philadelphia, who has just died after years of suffering, a victim to the x-rays… The votaries of medicine have ever been prodigal of life and health in the pursuit of their calling, and it says much for the courage and enterprise of our students that they were not deterred by the unknown terrors of the x-rays. But our losses in dead and wounded are assuming serious proportions. The x-rays are not yet twenty years old, and yet the number of our Veterans is already sadly depleted, and from the roll no name will be more missed than that of Lester Leonard.
>
> What a goodly company are these our pioneers, who have sacrificed their lives to the Baal-fires of the Roentgen rays! They have joined the band of the immortals—the warriors, the seamen, and the airmen who have lost their lives for the progress of the race. These are not the martyrs, but the gladiators of science. All honour to them and to their survivors, from whose lips we almost seem to hear the proud salutation, "*Morituri te salutant."*[233]

Leonard's story was not unique or surprising. Nine years earlier, the similar circumstances and death of Thomas Edison's primary assistant on the fluoroscope project, Clarence Dally, had made news across the United States.[234] In the preface to the second edition of his *Medical Electricity and Röntgen Rays*, Sinclair Tousey described the situation as a cautionary tale, telling readers that "the regrettable death of many of our associates in röntgenology should cause universal observance of the simple precautions which are necessary to ensure the safety of the operator and patient," and lamenting that "too often, however, this warning is disregarded."[235] Hector Colwell and Sidney Russ devoted an entire chapter of their book on *Radium, X-rays and the Living Cell* to "The Production of Malignant Disease" by

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[232] W. Deane Butcher, "Charles Lester Leonard (Obit.)," *Archives of the Roentgen Ray* XVIII, no. 6 (November 1913): 206–7; Herzig, "In the Name of Science: Suffering, Sacrifice, and the Formation of American Roentgenology," 575.

[233] W. Deane Butcher, "Editorial," *Archives of the Roentgen Ray* XVIII, no. 6 (November 1913): 205–6.

[234] "C. M. Dally Dies A Martyr to Science: Was Burned While Experimenting with X-Rays."

[235] Sinclair Tousey, *Medical Electricity Röntgen Rays and Radium: With a Practical Chapter on Phototherapy*, 2d ed., thoroughly revised and greatly enlarged (Philadelphia: Saunders, 1915), 9.

radiation. Noting the plight of their colleagues, Colwell and Russ opened with the "melancholy fact that x-rays, when applied for prolonged periods to the human body, may set up a condition of malignancy." The authors noted that "the early radiologists have suffered especially in this manner, subjected as they were to fractional doses of the rays they were administering," with cancerous lesions appearing most frequently "on the left hand of the operator"—in other words, the hand used by so many therapists to assess the hardness of their tubes at the beginning of a therapy session.[236] Ultimately, the fatal effects of x-ray exposure would wipe out virtually an entire generation of therapists. At the end of his memoir on the early practice of x-ray therapy, Emil Grubbé included a grisly chapter on "The Effects of X-rays on the Author's Body" in which he mused on being, by 1949, probably "the only living [x-ray therapist] who dates his work back to the fall of 1895." Grubbé had outlasted his colleagues by pursuing an extremely aggressive strategy of having "the offending parts cut out, burned out or amputated," but he still reported that "I have not had a comfortable or painless hour, night or day, during all these years" since the appearance of dermatitis in his hands.[237]

What about the patients?

In October of 1913, just a few months after the myoma article, the *Archives* reported a case of "Fatal Leucopenia Following X-ray Treatment." The author, E. P. Cumberbatch, described a female patient, thirty-two years old, who came to the doctor with symptoms of leukemia. Doctors treated the patient by x-rays "directed on the spleen and on the red bone-marrow," beginning with an intense, sixteen-day regimen of "nearly ½ pastille dose per diem." The patient's leucocyte counts diminished by two-thirds, and she began to rapidly lose weight, at which point her physicians reduced the regimen to "1 pastille dose each six days" for the

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[236] Hector A. Colwell and S. Russ, *Radium, X Rays and the Living Cell; with Physical Introduction* (London: Bell, 1915), 283.
[237] Grubbé, *X-Ray Treatment: Its Origin, Birth, and Early History*, 88.

remainder of the 111-day treatment period. Afterwards, “the patient felt so well that she went back to her work.” Within two months, however, the symptoms of leukemia returned, and the patient received another “pastille dose of x-rays…given to the spleen.” When she returned, three weeks later, a new set of symptoms had appeared; the patient now had “a profound anæmia of the pernicious type,” brought on by radiation exposure. She died shortly thereafter. The case, Cumberbatch wrote, starkly illustrated “the risk that may accompany the treatment of leucæmia by X rays” [sic] at high dose levels.[238]

As a group, x-ray therapists consistently downplayed or ignored the hazards of this new technology. Roentgenologists measured their efforts in a dosimetry scheme premised on a baseline assumption of harm to patients that offered neither trustworthiness nor precision, and they evinced a suicidally high tolerance for the short- and long-term risks of x-ray exposure, writing off death as a necessary sacrifice for their art. Under such circumstances it should come as little surprise that patients ended up suffering harm. As Matt Lavine has shown, however, many patients had heard of and even feared x-ray burns. A lingering question thus remains: why did patients continue to accept treatment from the men with the missing hands? The question seems especially pertinent when, as in the case of Rayner’s patients, the therapy served a purely cosmetic function for which non-harmful alternatives existed.

In part, the answer may lie in timing; roentgenology appeared at a moment of massive change in Western orthodox medical practice that included an emphasis on technology in treatment. Roentgenologists aggressively promoted themselves in this rapidly changing medical marketplace as an alternative to other, more traditional therapies. As discussed in Chapter 1, many of those therapies involved an expectation of unpleasantness. The traditional treatments for skin diseases included acid, curettage, and cauterization; such treatments often entailed pain

[238] E. P. Cumberbatch, “Fatal Leucopenia Following X-Ray Treatment,” *Archives of the Roentgen Ray* XVIII, no. 5 (1913): 187–89.

and permanent scarring. So great was the expectation of awfulness, according to Margaret Sharpe, that she regularly struggled with "overcoming the patients' skepticism: they cannot believe that anything is going on when there is nothing to be seen or felt." Other therapists echoed this complaint, and a few actually justified producing mild erythema in patients on the basis that it demonstrated the value of the therapy. When *ARR* editor W. Deane Butcher boasted that patients given "the last word in the dispute, surgical versus radio-therapeutic treatment," would always prefer "the x-ray couch" to "mutilation" upon "the operating table," he probably spoke a good deal of truth. X-ray therapists promised a treatment that, though "long and tedious," did "not make invalids of patients . . . saves them from disfigurement," and required neither "detention in a sick-room" nor "a long convalescence while the wound heals."[239]

Often, patients also had no options. Many of the conditions classified by x-ray therapists as "lupus" or "rodent ulcer" were actually cancerous growths for which existing treatments offered little or no hope. "Does a skin specialist ever pass a working day," asked Sharpe, "without seeing in his consulting-room someone whose life is more or less spoilt by the burden of some chronic skin disease that does not endanger life, but which the most skillful treatment can only temporarily relieve?" Object lessons in human suffering appeared in every issue of the journal: the "woman with an exuberant growth on the end of her nose, making it about the size of a billiard-ball . . . the surface was roughly granular, intensely red, bleeding easily, and exuding a sanious fetid discharge" (Sharpe wrote that "only the reflection that no nose at all was better than such a nose emboldened me to undertake the treatment"); the man with a tumor at the base of his tongue, suffering pain "of a constant smarting, shooting, aching character; he could not swallow food, and could only manage to swallow fluids after spraying his throat with a

[239] Sharpe, "The X-Ray Treatment of Skin Diseases," 58; Butcher, "The Intensive X-Ray Treatment of Myoma," 3.

solution of eucaine," and was steadily "losing weight and strength"; even Cumberbatch's patient was already living with the prospect of a near and miserable death before she ever lay on the x-ray couch.[240]

Here, though, x-ray therapists' optimism in the face of uncertainty had a terrible cost for patients. Numerous accounts of dermatitis emphasized that it was often long-lasting and painful. In particularly bad cases patients developed weeping ulcers that took months to heal. Women who received x-ray hair removal treatments like those described by Rayner often suffered long-term sequelae, such as scarring and ulcers on the treated areas.[241] Moreover, in 1911 Colwell and Russ concluded their chapter on x-ray induced cancer with an ominous note that, in addition to a high incidence of cancer amongst x-ray practitioners, "there appears to be a number of cases in which the irradiation of lupus has resulted in the subsequent appearance of a carcinoma."[242] That the ruinous disease would appear first in the cases that had previously served as the *cause célèbre* for x-ray therapy must have seemed particularly cruel not only to the patients, but to the practitioners who had unwittingly, if recklessly, sacrificed their lives on the altar of x-ray therapy.

Or perhaps not. The technological frame of x-ray therapy, as this chapter has shown, was built on optimism, rather than tragedy or acceptance, and x-ray users, for good or ill, seemed almost pathologically unable to admit defeat, even when it had already overtaken them. Thus it should perhaps come as no surprise that x-ray users turned to another form of radiation to save them from the damage wrought in their bodies by x-rays. In a darkly ironic turn, Edinburgh physician and surgeon Dawson Turner reported, in 1914, that "Sir J. Mackenzie

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240 Sharpe, "X-Ray Therapeutics"; G. M. Lowe, "On X-Ray Work in Private Practice," *Archives of the Roentgen Ray* VII, no. 5 (February 1903): 75–76; Cumberbatch, "Fatal Leucopenia Following X-Ray Treatment."
241 For an extensive discussion of x-ray hair removal, see Herzig, "Removing Roots."
242 Colwell and Russ, *Radium, X Rays and the Living Cell; with Physical Introduction*, 286.

Davidson treated some patches of x-ray dermatitis on his own hands with a 29-milligramme tube of radium for ten minutes, with success." According to Turner, Mackenzie had also achieved "remarkable improvement by treating in the same way the extensive lesions on the hands of an x-ray operator." Finally, Turner informed his readers that "Dr. Deane Butcher has found that the application of radium has a soothing effect upon x-ray burns, something like that of cocaine."[243]

[243] Dawson Turner, *Radium, Its Physics and Therapeutics*, 2d ed., enl (London: Baillière, Tindall and Cox, 1914), 138–39.

CHAPTER 3 – RADIUM THERAPY

The September, 1904 issue of the *Archives of the Roentgen Ray* opened with an editorial declaration: "The catachrestic use of the term 'radiation' to denote any ray-like projection, whether visible or invisible, corpuscular or ethereal, has been sanctioned by Science. Not least amongst these radiations, both on account of their theoretical interest and therapeutical importance, are those spontaneously emitted by radio-active substances, like thorium and radium, or artificially produced by the passage of an electric current through the rarefied atmosphere of a focus-tube." One year earlier, the journal had updated its name to *The Archives of the Roentgen Ray and Allied Phenomena* and debuted a new cover, with an illustration of a labeled energy source in each corner. The illustrations of "Phototherapy" and "Electrotherapy" were fairly recognizable as a sun and a ball shooting out lighting bolts, respectively. "Radiotherapy" and "Thermotherapy," by contrast, seemed to have confounded the artist, who settled for generic quarter circles shooting out squiggled lines. The accompanying editorial explained that the name and image update "had been rendered a necessity" by "the fact that recently rapid strides have been made in the application of a variety of rays to medicine and surgery," especially with regard to the "dread disease," cancer, but the editorial never actually mentioned radium by name. By September of 1904, the reorientation suggested by the new cover imagery had come to fruition. In addition to running several stories about radium therapy, the *ARR* had massively expanded its editorial staff and review board, including two of the most prominent names in radium therapy: Dawson Turner, of Edinburgh, and New York surgeon Robert Abbe.[244]

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[244] J. Hall-Edwards, "Editorial: Addition of Sub-Title to the 'Archives,'" *Archives of the Roentgen Ray and Allied Phenomena* VIII, no. 6 (November 1903): 95–96; J. Hall-Edwards, Clarence A. Wright, and Henry G. Piffard, eds., "Editorial," *Archives of the Roentgen Ray and Allied Phenomena* IX, no. 4 (September 1904): 71–72.

Radiation therapy is the rare story that fits into a neat temporal box—in this case, the discovery and spread of the technology coincided almost perfectly with the opening of a new century. The technology and practice of radiation therapy followed two distinct, but closely interrelated pathways: one based on using x-ray emitters, discussed at length in the previous two chapters, and the other based on using newly-discovered radioactive materials. This chapter will follow that second technological pathway.

The previous two chapters argued that x-ray therapy was primarily constrained by uncertainty: untrustworthy equipment, the lack of accurate, shared tools and systems of measurement, users' lack of knowledge, or even agreed-upon theories, of how and why radiation might affect the body. The previous analysis explored the uncertainty built into the technological frame of x-ray therapists—what caused it, how practitioners responded to it, and how it impacted the development of x-ray treatment. From an access standpoint, however, x-ray therapy had a distinctly populist quality. The barriers to entering the new field remained relatively low; it essentially required only a relatively modest investment in equipment and a willingness to tinker. As a result, practitioners without formal medical training came to play an important role in the x-ray therapy community, to the degree that, for example, the American Roentgen Ray Society opted not to ally itself with the American Medical Association, whose rules for membership would have excluded many x-ray "doctors." Moreover, x-ray users experimented exuberantly, and even recklessly, with their emitters, pointing them at every sort of ailment and condition.

Radium therapy, by contrast, was constrained by *scarcity*, both real and perceived. Prior to World War 1, radium existed only in small quantities, and only a relatively small number of users, based on some combination of money and connections, had access to what many saw as a precious, almost magical resource. The perception of scarcity determined what users did with radium—no one suggested, for example, experimenting with radium as a hair-removal treatment—but also how they thought about and described its therapeutic qualities. In a sharp divergence from the x-ray story, scarcity also drove governments to play a significant, active role in the early development of radium therapy, competing with one another for access to supplies of the material and providing support for much of the early radium research. Its association with nebulous conceptions of *vital energy* (a subset of the more general theories of vitalism popular at the time) would also play an important role in the development of radium as a therapeutic tool, providing a ready-made, if hazy, rationale for its operation that sparked interest and enthusiasm in both patients and practitioners.

Perception and Thing

The above ideas suggest that an analysis of radium therapy has to confront both the facts of the material's existence as a thing in the world and the perceptions attached to it. Sometimes these two elements moved in tandem; sometimes they did not. In the case of scarcity, for example, it is broadly true that users could not obtain, say, a gram of pure radium metal in 1910 at any price—in that sense, radium remained extremely scarce. But insofar as "radium" stood in for "radioactive material," buyers had a wealth of options, many of them explicitly marketed as "radium." Inevitably, of course, the advertising for such products emphasized and reinforced the perception of scarcity. In such instances, the link between radium, the element, and "radium," the concept, grows increasingly tenuous, with the concept ascendant.

Maria Rentetzi grappled with this bifurcation as part of her history of the Institute for Radium Research, in Vienna. Drawing on the work of anthropologist Igor Kopytoff, Rentetzi started with the concept of biography, approaching radium, the thing, with "questions similar to those one asks about people." Such questions might include subjects like the thing's origins, its “lifespan,” the physical locations to which it moves, and the functions it performs, but a questioner must also think about perception, for example, what are “the biographical possibilities inherent in its ‘status’ and in the period and culture, and how are these possibilities realized?” and “what do people consider to be an ideal career for such things?” To deepen this latter side of the analysis, Rentetzi turned to the semiotic approach of philosopher Roland Barthes, concluding that, “radium was transformed to a myth—maybe the most powerful one in the early twentieth century—that is, a system of communication, a mode of signification.”[245]

“Radium” was both a thing in the world and a carrier of the meanings attached to it. Barthes explained his concept using the example of bunch of roses, given to a beloved as a sign of passion. Both the flowers (the signifier) and the emotion (the signified) pre-existed the sign, the “‘passionified’ roses.” Nevertheless, the roses, once “weighted with passion,” resist deconstruction. The participants in the exchange will struggle to disentangle the physical object from the message; they no longer “see” the roses merely as a collection of physical properties or in terms of those particular flowers’ own biography. Similarly, any analysis of radium as a therapeutic tool must reckon with the myth of radium—the ideas that animated practitioners, patients, and purveyors when they referenced the substance. To describe these ideas as part of a mythology is not, in this analysis, intended to suggest a lack of truth; the basic elements of the radium mythology were, I will argue, always based on the actual conditions of the radium market and the actual properties of the substance. But the central conceits of the radium mythology remained influential over time in part because of their flexibility. It was true that radium was, by reasonable and popularly-accepted measures, both “scarce” and “expensive” in

[245] Rentetzi, *Trafficking Materials and Gendered Experimental Practices: Radium Research in Early 20th Century Vienna*, para. Introduction, 15; Igor Kopytoff, “The Cultural Biography of Things: Commoditization as Process,” in *The Social Life of Things: Commodities in Cultural Perspective* (Cambridge University Press, 1988), 66–67; Roland Barthes, “Myth Today,” in *Mythologies*, trans. Annette Lavers (New York: Hill and Wang, 1984), 2–3, http://faculty.georgetown.edu/irvinem/theory/Barthes-Mythologies-MythToday.pdf. For additional discussion of my approach to the analysis of material objects, see Introduction.

1902, 1910, and 1920, but those words had radically different meaning at the two ends of that temporal period.

As this example suggests, the analysis in this chapter will encompass a longer timespan than the narrative presented in the previous two chapters. Two key plotlines in the radium story—one following the production of radium, the other focusing on its deployment in doctors' offices—actually straddle the First World War. In the service of those entwined narratives, and the better to do justice to my argument about the long and interesting relationship between radium mythology and radium reality, this chapter will approximately span the first two decades of radium therapy, from its discovery, in1898, to the first therapeutic trials, in 1903, to the high-profile health disasters of the 1920s.

Becquerel, the Curies, and radioactivity

In 1896, French physicist Henri Becquerel found that uranium salts, like x-ray emitters, produced "rays" that could pass through low-density substances and darken a photographic plate. Though weaker than an x-ray emitter, uranium emitted energy spontaneously, on a continuous basis. In 1897, Marie Skłodowska Curie decided to follow up on Becquerel's "uranic rays." Curie was uniquely positioned for this research, owing to the groundbreaking work of her husband, Pierre, and his brother, Jacques, on the piezoelectric effect. By combining a precision piezoelectric instrument with an electrometer, the brothers created a system for measuring very small electrostatic effects with a great deal of precision.[246] With such apparatus ready to hand, Marie could accurately measure the energy output of various compounds containing uranium. One such material, uranium pitchblende, emitted significantly higher levels of energy

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[246] Philippe Molinié and Soraya Boudia, "Mastering Picocoulombs in the 1890s: The Curies' Quartz–electrometer Instrumentation, and How It Shaped Early Radioactivity History," *Journal of Electrostatics*, 11th International Conference on Electrostatics 11th International Conference on Electrostatics, 67, no. 2–3 (May 2009): 524–30. According to Molinié and Boudia, the Curies' instrument, properly used, had almost the same degree of precision as most of the modern, digital instruments used for measuring charges.

than could be accounted for by the ore's known uranium content. Hypothesizing that the ore must contain trace quantities of other, as-yet-unknown elements, Marie began attempting to isolate the source of that extra energy. In a later account of the research, she described the excitement at that moment of realization, noting that "such an interesting vista of possible original discovery was unfolded that my husband, M. Curie, gave up what work he had in hand to take part in my labors. We put forth our united efforts to the task of isolating some new radioactive substances and pursuing the study of them."[247]

It was a grueling process—"long and difficult and costly." Radium is a decay (or "daughter") product in the chain that begins, in nature, with uranium; radium, in turn, decays to form radon gas. The most common isotope of radium, ^{226}Ra, has a half-life of 1,601 years, meaning that half of the radium atoms in any particular sample will decay during that period. Though relatively long in terms of human lifespans, radium's life amounts to little more than a blip in geological time, so natural radium never has time to accumulate in large quantities. Instead, it is found in trace amounts—a fraction of a gram per ton—in uranium-bearing ores like the Curies' pitchblende. To separate radium from the pitchblende required a long chain of chemical treatments, starting with repeatedly boiling and washing the ore, using concentrated soda and acid preparations, and ending with a painstaking process of fractional crystallization that allowed the Curies to gradually produce salts containing ever-higher concentrations of radium chloride. The Curies identified both polonium and radium in 1898, but it took them until March of 1902 to isolate and purify enough radium chloride, one-tenth of a gram, to have its existence "confirmed" by the calculation of its atomic weight with a spectroscope. The Curies

[247] Madame Sklodowska-Curie, "The Radio-Active Elements," *The Independent ... Devoted to the Consideration of Politics, Social and Economic Tendencies, History, Literature, and the Arts (1848-1921)*, June 25, 1903.

also coined a term, “radioactive,” to describe elements that spontaneously produced radiation.[248]

When the Curies began to release small samples of radium and polonium salts to other researchers—an act of generosity that also created additional publicity for their discoveries—the vials emitted a faint, luminescent glow that never failed to excite a crowd. A story in the *Boston Globe* captured the amazement created by this new “light without heat”; according to the reporter, when a scientist at the Smithsonian opened a radium sample, “the room was filled with a clear, greenish glow, bringing out in relief the features of everybody present. The light was cold and harmless, and the substance could be picked up with impunity.”[249] All of the press, along with the more than 30 papers published by the Curies between 1898 and 1902, made Marie and Pierre into scientific celebrities.

In 1903, the couple shared the Nobel Prize in Physics with Henri Becquerel. Unfortunately, Marie and Pierre did not have long to enjoy their shared celebrity; in 1906, Pierre was killed in a street accident. Marie felt a lasting grief over the loss, but she continued her work, becoming the first person to win two Nobels when she received the prize in chemistry, in 1911. Marie's fame would ultimately eclipse that of her husband, and the iconic image of radium, used in publications, pamphlets, and advertisements around the world, would show Marie, in her blue dress, holding a glowing tube. Curie remained a worldwide celebrity throughout her life and even after her death, and she received a variety of honors, including, in 1995, re-interment in the Paris Panthéon—an honor reserved for those designated by the French Parliament as "National Heroes."

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[248] Ibid.; Harvie, *Deadly Sunshine*, 37–45; see also Ross Mullner, *Deadly Glow : The Radium Dial Worker Tragedy* (Washington, DC: American Public Health Association, 1999).

[249] “LIGHT WITHOUT HEAT.: New Substance, Radium, Discovered by Mme Currie in Paris Gives Out Light Without Burning.,” *Boston Daily Globe (1872-1922)*, May 5, 1901. In reality, of course, radium cannot be handled “with impunity”; it is extremely dangerous, as discussed below.

Radium and X-rays

In a technical sense, radium and x-ray treatments involve different types of radiation. Whereas x-rays, as discussed in Chapter 2, fall on the electromagnetic spectrum, radium and radon produce alpha, beta, and gamma radiation through the dissolution of their atomic structures, in a process known as radioactive decay. Owing, however, to the circumstances of their discovery, x-rays and radium remained closely linked as therapeutic interventions in the period prior to the First World War. The Curies announced the discovery of radium in 1898, at the height of the Röntgen Rush, with the result that radium got swept into the x-ray frenzy. Much of the initial interest in the new element explicitly focused on using it as an alternative to x-ray emitters. Stories with headlines like "Radium Better Than The X Rays," promised that radium emanations, possessing "all the qualities of the Röntgen rays," meant that "the wonderful results of the x-rays . . . can be duplicated by a method much cheaper."[250]

Initially, the Curies' lab served as the principle source of radium in the world, and because it took them several years to isolate enough radium salt for experimental purposes, we cannot speak of "radium therapy" until at least 1902. Still, when doctors finally did get their hands on the stuff, the theory of early radium therapy looked similar to that of x-ray therapy, as discussed in Chapter 1: simple exposure of the treatment area to radiation, in this case through proximity to a radioactive sample. As a result, practitioners often viewed the two forms of treatment as complementary or interchangeable. Given this overlap, it should come as little surprise that many early radium users had a connection to x-ray therapy. Despite their names, both the *Archives of the Roentgen Ray* and the *American X-Ray Journal* expanded to include not only radium therapy, but a variety of other treatments based on introducing energy to the

[250] "Radium Better Than the X Rays: A Discovery of Much Practical and Theoretical Importance," *New - York Tribune (1866-1899)*, December 18, 1899; "A New Chemical Element," *New - York Tribune (1866-1899)*, December 17, 1899. Such claims, repeated in many publications, were incorrect in almost every particular; the cost issue, in particular, will be discussed below.

human body, with articles on electrotherapy, phototherapy, and a smattering of similar subjects. Although many x-ray practitioners still referred to themselves as “roentgenologists,” a new, more capacious term, “radiologist,” began entering the lexicon, particularly in the Anglophone world, where growing anti-German sentiment in the run-up to World War 1 made the Germanic “roentgen” appellation increasingly unpopular.

As discussed in Chapter 1, x-ray users began to experiment with using emitters for therapy almost immediately, and reports of miraculous medical results—some true, others fanciful—became a newspaper staple in 1896. As a result, speculation about radium and other radioactive substances, like polonium and uranium, initially focused on the possibility of using such material as a one-to-one replacement for x-ray emitters. In an early *New York Times* story on radium, for example, the writer suggested that radium might provide “a more convenient and economical agent for surgical exploration” than x-rays, when used for photographs and fluoroscopic examination. A subsequent article expanded on the claim, promising that radium “is apparently easy to prepare and has none of the bad effects, such as burning and blistering, which generally accompany x-ray work.”[251] Newspapers all over the United States picked up and reprinted such claims, which apparently originated in the work of a University of Pennsylvania physicist, George F. Barker.[252]

Read in relation to the actual physical qualities of radium and its eventual medical uses, these stories contained little in the way of useful insight. The particular mix of radiation types produced by radium actually make it a poor choice for x-ray-style imaging applications, and the claims about the safety of radium would prove entirely false; its capacity to harm would, in fact, prove crucial to its therapeutic applications. But the early reports do begin to illustrate the

[251] “A New Chemical Element”; “Philadelphia’s Contribution to Science.,” *The Washington Post (1877-1922)*, December 21, 1899; “Radium Better Than the X Rays.”
[252] “BIG CLAIMS FOR RADIUM: Newly Discovered Chemical Element in a X Ray Rote,” *The Hartford Courant (1887-1922)*, January 8, 1900; “Philadelphia’s Contribution to Science.”

specific role of perception in the radium story. In the case of x-rays, the tool and its perception developed in tandem, as discussed in Chapter 2 under the rubric of the "technological frame." In the case of radium, by contrast, the initial perceptions of radium, its qualities, and its proper uses as a therapeutic tool developed well before any actual radium became available; almost three years would pass before the developing mythology could be put to the test with actual radium samples. That gap, however, did not make perception insignificant. Quite the opposite: the basic perception of radium as a substitute for x-rays would help set the terms for the first experiments with radium therapy. Like x-ray users before them, radium therapists would look to radium primarily as a treatment for skin conditions, and especially for cancers and stubborn infections, and for superficial tumors.

Radium: Fame and Mythmaking

On February 20, 1902, a short blurb in the *New York Times* informed readers that William J. Hammer had brought to the New York Academy of Medicine "tubes that contained small particles of the much-talked of radium." The newspaper reported that Hammer's samples, "obtained in Paris from M. Curie, contained a specimen pure enough to be 8,000 times as powerful as uranium." Hammer claimed that one of his tubes contained, according to the Curies, "the only particle of pure radium in existence, and that he would not sell it for $20,000." The tubes "glowed visibly in the dark," according to the report, and Hammer reported that Pierre Curie "would not like to be in the same room with a kilogram of the substance," because it "would probably burn the skin off of his body and the eyes out of his head."[253]

Hammer had a gift for both the theatrical and the self-promotional, and fin de siècle America had enormous appetite for precisely what he was offering: scientific wonders, both as

[253] "Costly Particles of Radium. Exhibited to Physicians at a Meeting of the Academy of Medicine.," *New York Times*, February 20, 1903.

spectacle and as the promise of miracles just around the corner in the new century. Although Hammer's presentation took place at a gathering of physicians, it comfortably fit Fred Nadis's vision of the "wonder show": "a performance genre that [offers] a populist vision of science and technology," portraying the process of technological innovation and scientific inquiry in a way that both "fulfills public desires and conjures new fears." Hammer actually had long experience with the theatrical approach to technology. While working as an electrical engineer at Edison's Menlo Park, Hammer had assembled several remarkable displays for public consumption, including a New Year's Eve "Electric Diablerie," wherein Hammer transformed his home into a sort of electric funhouse presided over by an automaton Jupiter (wielding his lightning bolt), and, in 1888, a miniature Niagara with colored lights and an electric rainbow that could fade in and out of its mists.[254]

X-ray performers offering everything from photography demonstrations to Röntgen séances had already blazed a trail on the wonder-show circuit; now the promise of radium would pull in audiences for lectures and lantern shows. Hammer executed a masterful version of what today might be termed a rollout or media blitz. He gave interviews to any reporter who asked and entered into high-profile collaborations with several physicians, offering to loan out a few of his nine vials of radium to test the possibilities of using radium to treat tuberculosis, cure cancer, and restore sight to a little girl made blind by spinal meningitis.[255] Hammer also published one of his longer lectures as a book: *Radium, and other radio-active substances; polonium, actinium, and thorium. With a consideration of phosphorescent and fluorescent*

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[254] Fred Nadis, *Wonder Shows : Performing Science, Magic, and Religion in America* (New Brunswick, NJ, USA: Rutgers University Press, 2005), xiv, 48-49 & 57. Based on his "Electric Diablerie," Hammer missed, by an unfortunate half-century, his actual calling as a designer of rides for Disneyland, *et al*.

[255] "RADIUM FOR TUBERCULOSIS: Can Be Used Internally. It Is Said--May Cure Cancer and Tumor," *New - York Tribune (1900-1910)*, November 29, 1903; "RADIUM AS A CANCER CURE.: Astonishing Results Obtained in the Case of a Patient Whose Condition Apparently Was Hopeless.," *New York Times (1857-1922)*, December 30, 1903; "Radium Removes Total Blindness.," *Los Angeles Times (1886-1922)*, August 24, 1903.

substances, the properties and applications of selenium, and the treatment of disease by the ultra-violet light. Wielding such a title, yet clocking in at 72 pages, with illustrations, Hammer's volume exemplified both its author's race to monetize his moment and his success at hitting the technical-yet-popular sweet spot. The book's *New York Times* reviewer, while lamenting that "the author did not deem it worth the time and trouble of revision," leading to "much material which has no proper place in a book," had to allow that, "for one who wishes to keep abreast of current thought, investigation, and experimentation . . . it is convenient and useful."[256] Hammer's efforts made him, according to radium historian Luis Campos, "undoubtedly the most famous of the scores of radium experts" that eventually worked the wonder-show circuit.[257]

Scarcity & Power

William Hammer, thanks in part to his flair for public showmanship, played an important role in crafting the perception and mythology of radium. In both the newspaper coverage of his exploits and in the claims made in Hammer's book, the elements that would define the public perception of radium—its mythology—began to take shape. That mythology rested on three basic claims: first, that radium was phenomenally scarce, second, that it was extraordinarily powerful, and third, that it, as a natural source of energy, was similar to the sun. Each of these claims had a particular basis both in language and experience. Together, they created a perception of radium that would influence its therapeutic use for the better part of two decades.

William Hammer's aforementioned claim, that he possessed a tube with "the only particle of pure radium in existence, and that he would not sell it for $20,000," captures the

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[256] "The Radio-Active Substances.: RADIUM AND OTHER RADIO-ACTIVE SUBSTANCES. With a Consideration of Phosphorescent and Fluorescent Substances, the Properties and Applications of Silenium, and the Treatment of Disease by the Ultra-Violet Light. Pp. 72. Illustrated. New York: B. Van Nostrand & Co. $1.," *New York Times (1857-1922)*, August 15, 1903, sec. The New York Times SATURDAY REVIEW OF BOOKS AND ART.
[257] Campos, *Radium and the Secret of Life*, 40.

essential elements of radium's *scarcity* mythology. First Hammer described the physical rarity of the substance, with his claim that only one "particle" even existed. Second, he emphasized its monetary value; $20,000 equates to approximately half a million dollars in 2016 terms. Finally, and just as importantly, Hammer concluded with a simpler assessment of scarcity: the only particle of pure radium in existence was unobtainable, because Hammer would not part with it. Versions of these three statements—simply put, "the world contains very little radium," "radium is incredibly valuable/costly," and "radium is unobtainable"—would become the stations of the radium cross, appearing over and over again in every sort of forum, from books and newspaper stories to public lectures and Congressional testimony to advertisements for products containing the radium that people supposedly could not obtain.[258]

The statement attributed to Pierre Curie—that simply being in the same room with a kilogram of radium "would probably burn the skin off of his body and the eyes out of his head"—exemplified the mythology of radium as *extraordinarily powerful*. "Power" is one of those words with an extraordinarily broad set of applications, making "power," by its very nature, a difficult thing to quantify. Radium boosters, nevertheless, were eager to try, and they consistently did so in two ways, both on display in Hammer's talk. First, they explained the radioactivity of radium by referencing uranium, i.e. "a specimen pure enough to be 8,000 times *as powerful* as uranium."[259] This standard of measurement inevitably resulted in arbitrarily and absurdly high numbers, often accompanied by the word "power"; early advertisements for radium would include price lists differentiating between weak, "240 times power" radium, the average "8,000 times power" samples, and rare, expensive, "300,000 times power" samples.[260] Lecturers and newspapers speculated about the uses of "pure" radium, which they described as

[258] "COSTLY PARTICLES OF RADIUM."
[259] Ibid. Emphasis mine.
[260] "Eimer & Amend," n.d.

possessing “a million times greater radioactivity than uranium.”[261] Setting aside concerns of technical precision—i.e., “what isotope mix of uranium?”—such descriptions offered a reasonably accurate depiction of radium radioactivity levels, but in a way that mythologized power, stripping it of its original context. What, after all, does it mean to have the “power” of uranium, in real-world terms? For the Curies in their original research, “power” measured the output, in picocoulombs, of a particular sample in the chamber of an electrometer.[262] Unsurprisingly, that explanation did not make its way into the subsequent discourse.

Instead, explanations of the “power” of radium relied on extraordinary descriptions of what radium could *do*. Sometimes, as in Hammer’s description of burning an observer’s eyes out of his head, the claims described an amazing or terrifying outcome. Hammer opted for a little of both in his lecture to the American Institute of Electrical Engineers and the American Electrochemical Society (the same lecture reprinted as *Radium and other radioactive substances…*), informing his audience at one point that “at the present moment the clothes of every person in this room and all the walls of the room are radioactive by reason of the presence of the nine preparations of radium which I have here this evening.”[263] A story in the *Manchester Guardian* explained to readers that one gram of radium had sufficient power “to raise 500 tons a mile high,” and that “an ounce would therefore suffice to drive a 50-horse power motor-car at the rate of 30 miles an hour around the world.”[264] In other examples, the description relied on a comparison. A piece in the *Wall Street Journal*, for example informed

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[261] A. CORRESPONDENT, “The Commercial Possibilities of Radium: Commercial and Financial Notes Mail News Outward in Ward,” *The Manchester Guardian (1901-1959)*, September 8, 1903.
[262] Molinié and Boudia, “Mastering Picocoulombs in the 1890s.”
[263] William Joseph Hammer, *Radium, and Other Radio-Active Substances; Polonium, Actinium, and Thorium. With a Consideration of Phosphorescent and Fluorescent Substances, the Properties and Applications of Selenium, and the Treatment of Disease by the Ultra-Violet Light* (New York, London: D. Van Nostrand company; C. Lockwood & son, 1903), 24.
[264] CORRESPONDENT, “The Commercial Possibilities of Radium.” This quote, attributed in some places to Rutherford, was picked up and repeated *ad nauseum* in the English-language press on both side of the Atlantic.

readers that “one pound of radium” could do “1,152,000 times the work of one pound of coal” and that “four pounds of radium” would be sufficient such that “a fast ocean liner could cross the Atlantic.”[265] Always, the emphasis in these stories lay on what radium *could* do, or what it *might* do, given sufficient quantities. When news stories described what radium actually *did*, in laboratory conditions, the results fell markedly short: radium glowed, but only enough to be seen in a dark room. Radium gave off enough heat to “melt half its own weight of water once an hour.”[266] These were fascinating and exciting qualities; they overturned old ideas about, for example, the stability of the atomic structure. But the mythology around radium was not built on melting small quantities of water. It was built on flinging immense weights into the air and burning people’s faces off with something a million times more potent than anything the world had yet seen.

The Myth of Helios

Claims about the power of radium described the extraordinary things it could do. The third element of the radium mythology—that radium was a natural energy source, specifically akin to the sun—described where that power came from. The route to this claim was particularly interesting, based in part on a series of chemical coincidences connecting stellar mechanics and radioactivity. In *fin de siècle* Britain and America, principles borrowed from thermodynamics, especially the ideas of entropy and the conservation of energy, had become entwined with questions about the origin of life and the mechanisms of reproduction for multicellular organisms, most famously as the result of new theories of vitalism being developed by Hans Driech based on his experiments with sea urchin embyos.[267] Luis Campos, in his study of

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[265] “Possibilities of Radium.,” *Wall Street Journal (1889-1922)*, August 29, 1903.
[266] “The Mystery of Radium,” *The Sun (1837-1990)*, June 6, 1903.
[267] E. G. Spaulding, “Driesch’s Theory of Vitalism,” *The Philosophical Review* 15, no. 5 (1906): 518–27; Edward T. Smith, “The Vitalism of Hans Driesch,” *Thomist : A Speculative Quarterly Review* 18 (January 1, 1955): 186–227.

the historical use of radium in origin-of-life biological research, found that radium got swept into this debate. With its constant, measurable energy output in the form of both heat and light, radium seemed to suggest "ways out of the pessimistic fin de siècle thermodynamic narratives of the end of the world." By 1905, John Butler Burke, working at the Cavendish laboratory, would claim that radium submerged in a tube of beef bouillon could produce simple, somewhat cell-like growths that, though not alive in the classic sense, might represent some intermediate state between inert matter and living cells. Burke christened his particles "radiobes."[268]

The existence of a source that spontaneously produced heat and light had an obvious corollary in the sun. By 1903, however, the sun's ability to generate energy was actually very much in question. In the 1890s, a feud had developed in Anglophone scientific circles over the age of the Earth and the sun. Lord Kelvin led a faction arguing that the sun's light and heat resulted solely from the gravitational collapse of stellar material—that the sun was essentially a collection of material which, though raised to a white-hot state, was steadily cooling, like a dying ember. Based on his calculations of the cooling rate of such a body, Kelvin concluded that the sun could not have existed for longer than 100 million years. Geologists and advocates of Darwinian evolution took the other side of the debate, arguing that the Earth had supported life for at least 500 million years.[269]

The two sides remained deadlocked in part because the nature and mechanism of the sun itself remained largely mysterious in 1903. One of those mysteries related to solar composition; spectroscopic analysis conducted by several scientists, beginning in 1868, had shown that the sun's corona contained large amounts of a previously unknown element,

[268] Campos, *Radium and the Secret of Life*, 31, 56–57. Chapter 2 of the book offers a full and interesting discussion of the Burke affair and its long-term impact on origin-of-life research.
[269] "The Age of the Earth as an Abode Fitted for Life," *Science* 9, no. 228 (1899): 665–74; "Probable Age of the Sun.," *Los Angeles Times (1886-1922)*, October 18, 1903; Paul G. Seybold, "Better Mousetraps, Expert Advice, and the Lessons of History," *Journal of Chemical Education* 71, no. 5 (May 1994): 392; Joe D. Burchfield, *Lord Kelvin and the Age of the Earth* (Chicago, IL, USA: University of Chicago Press, 1990).

dubbed “helium” in a reference to the Greek designation for the Sun, *helios*. As astronomers worked out a methodology for applying the same analysis to distant stars, they quickly found that helium represented a major component of stars all over the galaxy, and that particularly “in the hottest stars we know of, the atmospheres of those stars consist almost entirely of hydrogen and helium.”[270] But although helium appeared to exist in abundance throughout the galaxy, researchers on Earth found it impossible to locate a sample of the element until 1895—coincidentally, the year that Wilhelm Conrad Röntgen discovered x-rays—when William Ramsay found helium amongst the gases produced in a reaction between sulfuric acid and cleveite.[271] Ramsay’s discovery kicked off a burst of experimental activity, as other researchers rushed to replicate his results and determine the various chemical properties of the new element.

The discovery of terrestrial helium and the cataloguing of its properties did not solve the mystery of the sun’s light and heat. Helium is an inert noble gas; it does not fuel combustion or otherwise interact with hydrogen, so the mere existence of helium in large quantities in the sun and other stars did not seem to explain anything about their workings. It certainly did not contradict the aforementioned theory, defended by Lord Kelvin, describing the sun as an intensely-hot, but steadily-cooling body, its radiation the leftover and fading evidence of the intense crunch of stellar gravity. Today, we know that both the sun’s radiation and its helium result from the same process: the fusion of hydrogen atoms in the stellar core (here, Kelvin deserves some credit, since fusion does indeed result from the intense pressure of gravity). On Earth, however, helium arises from a different process: radioactive decay.

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[270] J. Norman Lockyer, “The Story of Helium (Part 2 of 2),” *Nature* 52, no. 1372 (February 13, 1896): 346; see also J. Norman Lockyer, “The Story of Helium (Part 1 of 2),” *Nature* 53, no. 1371 (February 6, 1896): 319–22.

[271] “THE DISCOVERY OF HELIUM,” *The Lancet*, Originally published as Volume 1, Issue 3735, 145, no. 3735 (March 30, 1895): 822–23; H. N. Stokes, “Helium and Argon,” *Science* 2, no. 43 (1895): 533–39.

Radium atoms can decay in several ways, but the most common pathway is alpha decay, wherein the atomic nucleus of a radium atom emits an "alpha particle," or helium nucleus—basically, two protons and two neutrons, bound together to form a new, separate atomic nucleus. In essence, radioactive decay divides one atom of the element radium, with its 88 protons, into two new atoms: a two-proton helium atom, and an 86-proton radon atom. The decay process releases energy, mostly in the form of the kinetic energy of the helium nucleus, as it flies away from the parent isotope; through various processes, this energy output results in the light and heat associated with radium. Interestingly, this makes alpha decay a sort of reverse analogue to the process of energy production taking place in a star: like the sun, radium produces light, heat, and helium, but whereas the sun yields those products when two hydrogen atoms fuse together in the stellar core, the products of radium result from the division of one atomic nucleus into two smaller constituents.

In 1902, Ernest Rutherford and Frederick Soddy worked out that radioactivity in thorium resulted from the "transformation" of the radioactive material into "a chemically inert gas" and "a gaseous product which in the radioactive state constitutes the emanation."[272] By 1903, Soddy had joined Sir William Ramsay's laboratory, in Cambridge, and had also obtained some radium from a London dealer. The pair soon determined that radium passed through a process similar to what Soddy and Rutherford had observed in thorium: radium produced a radioactive "heavy gas" (radon) and an inert gas, which they identified as helium, in addition to a steady output of energy.[273] The next intuitive leap was obvious, and a variety of observers immediately made it:

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[272] E. Rutherford and Frederick Soddy, "LXXXIV.—The Radioactivity of Thorium Compounds. II. The Cause and Nature of Radioactivity," *Journal of the Chemical Society, Transactions* 81, no. 0 (January 1, 1902): 849.

[273] Sir William Ramsay and Frederick Soddy, "Experiments in Radio-Activity, and the Production of Helium from Radium," n.d.; CABLE TO THE CHICAGO TRIBUNE, "RADIUM TURNS INTO HELIUM.: Latest Discovery with Mysterious New Element Baffles All the Laws of Chemistry. BECOMES ANOTHER METAL Sir William Ramsay Gives World of Science the Result of His Recent Experiments. How Discovery Was Made. Changes

in spontaneously producing both energy and helium, radium offered a possible solution to the riddle of the sun, and a way around Kelvin's objections about the age of the Earth. George Darwin, son of the famous naturalist and a successful astronomer and mathematician in his own right, commented to a reporter that "knowing, as we do now, that an atom of matter is capable of containing an enormous store of energy in itself, I think we have no right to assume that the sun is incapable of liberating atomic energy to a degree at least comparable with that which it would do if made of radium." Darwin explicitly expressed skepticism that the sun was "actually to any large extent made of radium," but others showed less circumspection.[274] Headlines like "Radium in the Heavens," appeared in newspapers; in a typically breathless claim, the *Chicago Daily Tribune* informed readers that "transmutation of radium into helium identifies both with the sun and the essence of that luminary with probably a single origin of all life."[275]

The myth of radium as *helios*—the energy- and life-giving stuff of the sun—was born, and it would prove incredibly durable. For the select few individuals actually working with radium chloride the faint, luminescent glow emitted by the salts had always been the most striking of the phenomena associated with radium. In truth, radium emits primarily in the form of alpha particles, which consist of two protons and two neutrons; the "glow" of radium samples actually results from the stimulation of phosphorescent material or of nitrogen molecules in the surrounding air. Nevertheless, the association of radiation with light would play a powerful role in its perception as a salubrious force. Recognition of the healing powers of light had generally grown throughout the nineteenth century. Florence Nightingale, in 1856, publicly advocated

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into Helium. One Metal Becomes Another. May Solve World's Riddle. Millions of Years to Dissolve. Worth $3,750,000 an Ounce.," *Chicago Daily Tribune (1872-1922)*, November 27, 1903.

[274] "Probable Age of the Sun."; Kelvin, "A Kelvin Letter: Radium and the Age of the Earth," *The Manchester Guardian (1901-1959)*, January 2, 1908. As the second document shows, Kelvin himself never gave in on the issue of the Earth's age, dismissing radium as not generating enough heat to substantially alter the situation.

[275] "Helium and Radium.," *Chicago Daily Tribune (1872-1922)*, November 28, 1903.

orienting hospital wards to allow sunlight into the wards, and spa-type "thermal clinics" in Europe advertised sunlight therapy to health travelers. After Downes and Blunt demonstrated in 1877 that sunlight had antibacterial properties, a host of experiments followed, culminating in the awarding of the 1903 Nobel Prize in Medicine to Niels Ryberg Finsen for his research into using filtered sunlight to treat lupus vulgaris.[276] The prize in Physics, of course, went to Marie and Pierre Curie and Henri Becquerel, for "their joint researches on the radiation phenomena."

As discussed in Chapter 1, proponents of x-ray therapy had immediately seized on light therapy as a possible model for the medical uses of x-rays. In February of 1896, for example, a reporter for the *Chicago Tribune* quoted several physicians who noted the germicidal power of sunlight and suggested that x-rays might prove useful as a sort of sunlight that could pass through the body, treating tuberculosis *in situ*, within the lungs.[277] In the fall of 1897, British physicist Silvanus Thompson expressed to the gathered crowd at the Roentgen Society's public exhibition in London his the hope that if "the Roentgen rays which penetrate tissues opaque to light should have any direct effect in nerve stimulation, then the door would be opened to the hope that by their aid sight might be, in some cases, artificially given to the blind."[278] The cover illustration of *The American X-Ray Journal*, which appeared in 1897, showed an angel hovering in space, holding a vacuum tube over the globe, where it casts a cone of illumination over North America.

Radium, with its aforementioned tendency to phosphorescence, fit even more readily into this narrative. In 1903 a series of doctors claimed to have finally made progress on Thompson's dream, treating blindness in New Jersey, Philadelphia, and Russia not with x-rays,

[276] For a brief summary of the subject, see Rik Roelandts, "The History of Phototherapy: Something New under the Sun?," *Journal of the American Academy of Dermatology* 46, no. 6 (June 2002): 926–30.
[277] "NO LIFE FOR THE DEAD."
[278] "The Roentgen Society (11/5/1897 Meeting)," 28.

but with containers of radium salts strapped to patients' heads.[279] From early on, advocates and advertisers in the radium marketplace enthusiastically adopted visual metaphors of light for imagery of the new radiation. When companies in both America and Europe began to manufacture products with radium, they plastered imagery of candles, suns, stars, and glowing vials all over promotional materials, and radium distributors often carried business cards with glowing spots of paint. Of the three parts of the radium mythos, the mythology of helios—radium as sun—would prove the most powerful, in terms of capturing consumers' attention and shaping their understanding of radium as a therapeutic product.

Robert and Truman Abbe: The Realities of Radium

On February 2, 1903, just as William Hammer was embarking on his first radium tour, New York surgeon Robert Abbe sent an inquiry to the city's premier chemical supply house, Eimer & Amend, regarding the "element of Radium." Two days later, the distributor responded that "we have received a cable reply from our friends in Paris as to the price of same, which will be about $13,250.00 per gramme. However, it will take a little time before even the smallest quantity can be delivered as the only parties that are making the element are Mr. & Mme. Curie."[280]

Abbe was a New York surgeon and a fellow of the College of Physicians of Philadelphia. Born in New York City on April 13, 1851, Abbe had attended Columbia University, earning his M.D. in 1874. Abbe's abilities and interests ranged widely; his skills as a sketch-artist, painter, and photographer would eventually serve him well in documenting his radium research. By 1903, Abbe had developed a prominent practice in New York City, where he attended at St.

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[279] Lavine, *The First Atomic Age*, 35–36.
[280] "Eimer & Amend Correspondence to Robert Abbe," 1903, n. dated 4 Feb. 1903, Earliest Sale of Radium in America - with Subsequent Correspondence (scrapbook), Digitized by the Historical Medical Library of The College of Physicians of Philadelphia, http://www.cppdigitallibrary.org/admin/items/show/4344. The quoted price translates to roughly $350,000 in present-day terms, which actually would have been a bargain in light of a subsequent rise in the price.

Luke's, the Babies' Hospital, the Roosevelt, and the Cancer Hospital. Abbe specialized both in treating carcinoma—basically, cancer that occurs on surface tissues, like skin, gums, and the linings of the gastrointestinal tract—and sarcomas arising from cancerous bone and cartilage tissue (most commonly, in Abbe's case, in the patient's jaw). Prior to the advent of radiation therapy, treatment usually involved surgical removal. Since surgery to remove cancerous tissue often left patients badly disfigured, Abbe also specialized in various types of plastic surgery, including skin grafts and reconstructive procedures. Abbe pioneered or improved on several important surgical techniques; the "Abbe flap" technique, used to repair damaged lips and reconstruct the mouth, usually following excision of a squamous cell carcinoma, still bears his name. Abbe also saw patients with a variety of other disfiguring skin diseases, and especially tuberculosis infections, popularly referred to as "lupus." When it came to experimenting with new surgical and therapeutic techniques, Abbe described himself as a ready innovator. As an example, he related an anecdote about a patient "who had had both his hands blown off by dynamite" and came to inquire about having replacement hands grafted onto his arms. According to Abbe, "I told him it was absurd; the thing could not be done and hand not been done. However, it stimulated me to make a lot of experiments on cats and dogs and sheep, and I learned many things that almost forced me to try to do it, but I never did."[281]

Robert Abbe came from a family of scientists; the most famous was Robert's brother, Cleveland Abbe, the meteorologist. By 1903, Robert had been joined by his nephew, Truman Abbe, who had received his M.D. from Columbia in 1899 and completed a post-graduate stint in Europe. Both Abbes kept casebooks, correspondence, research notes, and other ephemera

[281] Robert Abbe, "Radium and Radioactivity - Address Delivered before the Yale Medical Alumni Association, March 9, 1904, 8:15 P.m." (Yale Medical Journal, June 1904), Earliest Sale of Radium in America - with Subsequent Correspondence (scrapbook), Historical Medical Library of the College of Physicians of Philadelphia; *In Memory of Robert Abbe* (Portland, ME: Mosher Press, 1928); Charles L. Gibson, "ROBERT ABBE 1851–1928," *Annals of Surgery* 88, no. 4 (October 1928): 794–97.

related to their early radium experiments. Some of these eventually went into a scrapbook that made its way to the College of Physicians of Philadelphia.[282]

Radium for Sale

The original offer of radium at $13,250 per gram actually proved too optimistic; Eimer & Amend eventually reported back that pure radium metal could not be gotten at any price—indeed, that it did not exist, according to the Curies. In March, however, Robert Abbe received good news from his suppliers, in a note dated March 20, 1903: "we have received from the agents of Mr. & Mme. Curie, the Radium which you ordered the 7th ult. We have been able to procure the highest concentration made at present i.e. of an activity of 300,000 x. In fact we were fortunate enough to get 5/100 grm." The salt cost $160, or approximately $4400, in present-day terms. It came in a stoppered glass tube, and it glowed in the dark.[283]

For the Curies, selling radium salts offered a valuable source of research funding. In an interview given by Marie Curie in 1903, the scientist pointed out that radium extraction required such large quantities of ore and so many steps that even cheap processes rapidly grew expensive. In addition to transporting tailings from the mines around St. Joachimsthal to France, the separation process required chemicals for mixing with the pitchblende, factory floor space and large tubs, and even fuel to boil the mixtures. In the early days, the Curies relied on support and donations from a variety of sources. The Austrian government donated the first ton of ore (which they saw, at the time, as a useless waste product). The Society of Encouragement for National Industry, where Marie Curie had actually worked when she initially moved to Paris, the

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282 *In Memory of Robert Abbe*; Gibson, "ROBERT ABBE 1851–1928"; Nathan Reingold, "A Good Place to Study Astronomy," *Quarterly Journal of Current Acquisitions* 20, no. 4 (1963): 211–17; Robert Abbe and Truman Abbe, "Earliest Sale of Radium in America - with Subsequent Correspondence (Scrapbook)" n.d., Historical Medical Library of the College of Physicians of Philadelphia. The story of the scrapbook is one of those long, strange tales that terminates in a happy archival accident, when the author spotted an out-of-place box in the course of some volunteer work.

283 "Eimer & Amend Correspondence to Robert Abbe," n. dated 20 March, 28 March, and 25 April 1903.

Academy of Sciences of Paris, and an "anonymous benefactor" had "furnished us the means of chemically preparing a certain quantity of the product," and the Central Society for Chemical Products had given the couple floor space in its factory, as well as performing "preliminary chemical treatment of the mineral" for free. By selling some of the products of their lab, the Curies could finally support their own work.[284]

In April, Eimer & Amend put out a sales circular addressed "to the scientists of the United States and Canada." The circular offered "a new element 'RADIUM,'" and it dutifully repeated—and embellished—the key points of the radium mythology. Radium was scarce, produced only by the processing "of several *hundred* tons of Uranium residues." It was "extraordinarily active . . . the strongest which has yet been placed on the market," capable of amazing and terrifying feats; as the circular warned, purchasers "must not hold strong RADIUM salts for any length of tie in the hand, nor close to the body during experiments, because on account of its extremely high activity, it will act immediately upon the skin, the muscles and even the brain." And the salt was an independent energy source, capable of producing "a <u>Phosphorescence</u> of its own, i.e. it has the faculty of being incandescent <u>without</u> previous <u>exposure to light</u>." The circular offered a variety of salts differentiated, as previously discussed, by a comparison with uranium samples, from "240 times activity" to the "300,000 times" salt offered to Robert Abbe.[285]

The correspondence in the Abbe scrapbook does not make entirely clear whether Eimer & Amend had gotten into radium sales prior to Abbe's original request. Robert Abbe made his personal inquiries about the purchase of radium just weeks before William Hammer gave his talk to the New York Academy of Medicine, so the distributors may have simply perceived a new opportunity and jumped on it. Despite the claims made in the sales correspondence, the Curies'

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[284] Sklodowska-Curie, "The Radio-Active Elements."
[285] "Eimer & Amend Correspondence to Robert Abbe." Underlining in original.

lab was not the only source of radioactive salts or radium in the world. In her interview, Marie Curie mentioned the work of “M. Marckwald,” in Berlin, who claimed to have found a much easier method of precipitating radioactive material out of pitchblende. The Curies published details of their process and allowed outside observers into their lab, making it possible for virtually any researcher with access to uranium-bearing ore to begin processing it for radium and polonium. Historian Maria Rentetzi concluded that American buyers actually got most of their radium from Austrian and German suppliers, both of whom worked from the same St. Joachimsthal pitchblende as the Curies. German chemist Friedrich Giesel, in particular, would begin processing ore in 1901, and he would become one of the major commercial producers of radium salts. Frederick Soddy and William Ramsay made the helium discovery using radium bromide that had come from Giesel’s labs, sold by Isenthal’s, in London. Soddy would later recall his utter surprise at seeing a sign advertising “pure radium compounds” at eight shillings per milligram, having believed that radium could only be obtained directly from the Curies. The Austrian and German producers sold salts at a variety of activity levels. Lower-activity salts underwent a relatively low level of enrichment and had correspondingly low levels of radium content, but they were still noticeably radioactive, and could be produced cheaply. Eimer & Amend’s lowest-purity, “240 times activity” salts went for just $5 per gram.[286]

Golden Rule Experimentation

Although physicians had some expectation that radium exposure might have effects similar to those of x-rays, no one knew exactly how radium would impact the human body. Several individuals working with radioactive materials had suffered and reported radiation burns. Henri Becquerel suffered a burn after carrying a vial of radioactive salt in his vest pocket.

[286] Sklodowska-Curie, “The Radio-Active Elements”; Rentetzi, “The U.S. Radium Industry,” 438–39; Campos, *Radium and the Secret of Life*, 14; “Eimer & Amend Correspondence to Robert Abbe.”

Pierre Curie, after he heard from Becquerel, intentionally conducted an exposure test on his own arm, using a sample of radium salt from his lab. Both men reported that the symptoms took approximately fifteen days to appear. The exposed skin turned red and developed an ulcer that took several months to heal.[287]

As discussed in Chapter 1, medical professionals in 1903 subscribed to the "golden rule" for experimenting on human patients: a physician should not try out a treatment on a patient that he would not first use on his own body, family members, or friends. In essence, the golden rule asked physicians to bear the same risks to which they subjected their patients, like the steward sampling the wine in front of the monarch to prove that it was not poisoned. The golden rule also encouraged physicians to develop a personal familiarity with an experimental treatment before attempting to administer it to a patient, presumably with the practical result that he or she could identify obvious problems and work out improvements before being exposed to liability.[288]

Robert Abbe's radium arrived in New York in July. The surgeon, however, liked to travel in the summer; a note sent by Eimer & Amend on July 7 apologized for the delay in delivery—"although we tried our best to obtain it at an earlier date, we were unable on account of Prof. Curie's experiments on the calorimetric value of Radium, for which he needed, for the time being, all the Radium obtainable"—and mentioned that the courier had "found you out of town." A hand-scrawled note at the top of the page indicates that Abbe responded three days later, on July 10. In an interesting twist, however, Abbe apparently decided to make some other tests with the samples before trying them out on human subjects.[289]

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[287] Harvie, *Deadly Sunshine*, 50.
[288] Baker, *Before Bioethics*.
[289] "Eimer & Amend Correspondence to Robert Abbe," n. dated 7 July 1903.

In a lecture given a year later to the Yale Medical Alumni Association, Abbe told his audience that French researchers had claimed that "living skin is burned as by x-rays," that "seeds exposed to radium refuse to grow," and that "small mice and young guinea pigs are killed by a few hours' exposure to the rays." These remarks probably referred to a talk given on February 16, 1903, at the Paris Academy of Sciences, by Dr. Roux, of the Pasteur Institute, which appeared both in the press and in the Eimer & Amend sales materials for radium. Despite the fact that he had sought after radium and paid a handsome sum for his own samples of high-activity salt, Abbe reported to his audience that he felt "much skepticism until I had made a large number of experiments myself." One of the first involved exposing rape seeds to the radium samples for different lengths of time. Each sample group had 100 seeds, and Abbe carefully noted how many of the seeds sprouted plants and how high each tendril grew, pulling and photographing the plants against a piece of graph paper. He apparently also conducted experiments with "trout and pollywogs, less than an inch long, in finger bowls with radium tube immersed," "caged ants with their larvae exposed," and "young mice put in a glass jar, in which strong radium is suspended." Abbe eventually included the results of the rape seed experiment in his first journal article on radium, published in the *Medical Record* in 1904.[290]

The evidence presented below suggests that Robert Abbe probably began experimenting with lower-activity radium salts as early as September, but he only left case notes dealing with the "strongest radium"—the 300,000x sample specially attributed to the Curies' own lab (often referred to by Robert as the "Curie radium"). By November, Robert Abbe was ready to begin testing this sample on human subjects. In accordance with the golden rule, he made himself into the first test subject. Abbe's notes on the self-experiment record the first

[290] Abbe, "Radium and Radioactivity - Address Delivered before the Yale Medical Alumni Association, March 9, 1904, 8:15 P.m."; Robert Abbe, "The Subtle Power of Radium (off-Print Included in 'Earliest Sale of Radium in America - with Subsequent Correspondence' scrapbook)," *Medical Record*, August 27, 1904.

experiment on November 3, 1903. Abbe covered the skin of his forearm with "four thicknesses of lead foil," cut "a very narrow slit" at three locations, and bound the tube of radium to his arm for half an hour in the first spot, an hour in the second, and two hours in the third. The test produced results, but not particularly dramatic ones; it took twenty four days before the "first sign of pink" appeared, under the site of the two-hour exposures, although the line subsequently darkened, widened, and produced a raised area on the skin. This relatively benign result apparently convinced Abbe to move on to a more dramatic experiment on December 8. He put the glass tube containing the radium into a lead case with a large hole cut in one side and bound the case to his forearm for five hours, essentially putting his unshielded skin as close to the radium salts as possible without actually removing the grains from their glass tube. "Immediate hyperaemia about exposed part," he wrote, underlining the word "immediate." By December 11, the exposed area had grown "slightly tender," turned "mottled yellow and pink," and Abbe felt "some discomfort" at the site. He had managed to give himself the sort of radium "burn" described by both Pierre Curie and Henri Becquerel.[291]

Robert's nephew, Truman Abbe, also tested out the radium samples on his own body. His notes go back to September 27, when Truman tried out the "German radium" on his left leg, "unfiltered, unscreened," for seven and a half hours. The "German radium" probably referred to a batch of the cheaper, lower-purity Giesel salts; Abbe's notebook records the activity level as "2000," less than a hundredth of output of the material obtained from the Curies. The initial test produced very little result, but Truman kept at it, giving himself long exposures throughout the month of October, until finally, on November 8, he recorded a "slight reddening of the skin." On

[291] Robert Abbe, "1st Radium Experiments - Self (Handwritten Notes, Cut and Pasted into Scrapbook)," December 3, 1903, Earliest Sale of Radium in America - with Subsequent Correspondence (scrapbook), Historical Medical Library of the College of Physicians of Philadelphia; Robert Abbe, "2nd Radium Exp.," December 7, 1903, Earliest Sale of Radium in America - with Subsequent Correspondence (scrapbook), Historical Medical Library of the College of Physicians of Philadelphia.

November 13, he conducted his first experiments with the high-purity samples, starting by holding the radium to the tip of his tongue for five minutes; according to his notes, it produced a tingling sensation that lasted for ten minutes. That same day, he began a version of his uncle's experiments, exposing his arm in three places, for differing lengths of time. Like Robert, Truman succeeded in giving himself a radium burn. More interestingly, Truman haphazardly returned to his case notes throughout the rest of his life, adding addendums as late as 1952 ("scars of two hour and three hour exposures may be visible in the summer tan but were not evident during winter").[292]

The Abbes began experimenting on patients with radium salts just as soon as they began experimenting on themselves. In fact, Truman's case notes record his first application of radium to a patient as happening on the same day, September 27, 1903, that he began conducting radium tests on himself. In fact, the case notes, when combined with the notes on "self-experiment" entries, give a much better sense of how the Abbes thought about the "golden rule" of experimentation. According to the patient case notes, "Mrs. G." came to the Abbes on a referral; she had a mass, "microscopically determined to be carcinoma," filling her uterus and advancing downward, past the cervix. The patient was suffering pain and a variety of unpleasant symptoms. On September 27th, "a sealed glass bulb of German radio active Barium bromide (labeled 'Activity 1,900')" arrived, "loaned by the Smithsonian Institution of Washington, D.C." In an attempt to shrink the mass, or at least to arrest its development, Truman inserted the bulb, "without protection or filter" as close to the cancer as possible, and left it "held in place by packing for several hours each day" throughout most of the month of October. Based on the timing and correlation with his other logs, it seems fairly clear that

[292] Truman Abbe, "Experimental Work on Self," 1952 1903, Box: "Abbe, Robert and Truman Abbe. Scrapbook on radium, 1900s-1950s"; Folder: "Experimental work on self and other cases," Historical Medical Library of the College of Physicians of Philadelphia.

afterwards Truman removed the radium sample from the patient, then affixed it to the "outer part [of his own] left thigh," probably before going to bed ("overnight exposure" appears elsewhere in the self-treatment logs).[293]

There is a kind of frank intimacy in this episode. Truman's logs leave relatively few details about "Mrs. G.," beyond her race ("white") and her age (59), but the details in the notes make clear both that her doctors regarded Mrs. G.'s case as hopeless and that she experienced considerable suffering. Under these circumstances, Truman's administration of radium looks like what might today be described as "compassionate use"—an experimental therapy given in desperation, as a last-ditch measure to a patient who has no other options. Professional medical ethics required Truman not to experiment on his patient without accepting that same risk for himself, and so he gave the single available sample to his patient, each day, and then put it next to his own skin, each night. In the end, Mrs. G. seemed to derive some benefit from the exposure. The mass shrank and even receded a bit; by the end of October, Mrs. G. reported that her pain had abated to the point that she no longer had to take morphine. On November 24, Truman wrote "no more bleeding, general condition apparently better, skin color more normal, appetite better." The respite, however, proved temporary. On December 19, Truman added a new note: "Radium stopped on account of vaginal irritation and onset of uremic symptoms." She died three weeks later, of uremia.[294]

Developing Radium Therapy

Mrs. G.'s case exemplifies many of the central elements of early radium therapy. As with Mrs. G., the experimental treatment in every case consisted of placing a small container of radioactive material, sometimes described as a "bulb" or "tube," in close proximity to the tumor

[293] Truman Abbe, "Early Radium Cases," 1952 1903, Box: "Abbe, Robert and Truman Abbe. Scrapbook on radium, 1900s-1950s"; Folder: "Experimental work on self and other cases," Historical Medical Library of the College of Physicians of Philadelphia; Abbe, "Experimental Work on Self."
[294] Abbe, "Early Radium Cases."

and leaving it in place for a period ranging from a few minutes to fifteen hours. “Tube,” in this case, referred not to a vacuum tube, as in x-ray therapy, but to a “small thin glass tube, about one inch long, and scarcely larger than a match stick.” Some patients received daily treatment; others came in a few times a week or just a few times a month. For skin treatment, “radium tubes were laid on the disease”; for internal uses, the doctors used packing. The physicality of this process sometimes led to problems. At one point in the case notes for Mrs. G., Truman noted that the radium tube had worked loose and fallen out. In Truman’s third case, a patient with a fast-growing cancer in her lower jaw, the doctor sent the patient home with instructions to keep one of the lower-grade, “240 activity” tubes tucked into her lip. On February 10th, 1904, he wrote, “Radium tube broken two days ago. Noted gritty feeling and bitter taste but has kept on using the tube full of saliva ever since.” Because of the tremendous monetary value of high-purity radium salts, broken tubes actually posed a serious problem for doctors. After he broke a tube in March of 1906, Robert Abbe published a short article in the *Medical Record* describing his experience and offering the clever suggestion of using x-ray plates and fluorescent material to plot the locations of the missing grains of salt.[295]

The physical nature of radium treatment also encouraged experimentation. In 1904, shortly after he began to treat patients with what he called the “Curie” or “French” radium—the “300,000 times activity” salts obtained from the Curies’ lab—Robert Abbe received “a lad, aged 17,” suffering from a rapidly-growing tumor in his jaw. In two months, the soft mass had “replaced the substance of the bone, except its lower border, from the middle line toward the left for an inch and a half”; Abbe classified it, based on a biopsy, as a “giant cell sarcoma.” At first, Abbe treated the mass by using “two grains of French radium (300,000) in its glass tube laid

[295] Robert Abbe, “Radium in Surgery.” June 1906, 1, Earliest Sale of Radium in America - with Subsequent Correspondence (scrapbook), Historical Medical Library of the College of Physicians of Philadelphia; Abbe, “Early Radium Cases”; Robert Abbe, “Explosion of a Radium Tube,” *Medical Record (Reprint)*, April 21, 1906.

upon the part inside the teeth, with a bit of lead plate to protect the tongue," with good results. When Abbe tried to treat the outer portion of the tumor, however, he found that placing the tube of radium between the healthy lip and the cancerous jaw immediately produced a painful radiation burn in the healthy tissue of the patient's lip. Rather than attempting to craft a complicated shielding arrangement, Abbe hit on a new idea:

> "I then determined to plunge it directly into the tumor . . . I ventured to pierce the tumor by a fine knife placed in front of the teeth and thrust vertically to the lower bony shell. The tumor was uniformly soft, so that the knife penetrated almost by its own weight, and when it was withdrawn blood flowed freely, but was checked when the radium tube was pushed into the knife channel. The tube was half filled by the radium, which occupied nearly an inch in in length, and was therefore all buried in the tumor. I left the radium in situ three hours. On its removal a piece of gauze, with powdered boric acid rubbed into it, was laid over the spot, and held there for ten minutes."[296]

The results achieved were near-miraculous. Traditional treatment of the condition would have involved "the mutilating operation of excision of at least the half lower jaw." Under radium treatment, the tumor first shrank, then "developed ossific points throughout, and finally became solid with new bone." Robert Abbe saw this episode as one of his finest radium moments. He recounted it in every radium paper he published for the next decade, usually with an update on the patient. In 1909, for example, he reported that the ossified section "each year has seen a continued reduction in the bone, so that now the jaw seems normal except that it is slightly thicker than on the opposite side." Such stories seemed to fit into the mythology of radium's extraordinary power. As the case of Mrs. G. suggested, however, the reality of radium treatment was more complicated.[297]

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[296] Abbe, "The Subtle Power of Radium (off-Print Included in 'Earliest Sale of Radium in America - with Subsequent Correspondence' scrapbook)," 9–11.

[297] Ibid., 9; Robert Abbe, "The Specific Action of Radium as a Unique Force in Therapeutics (Reprint)," *Medical Record*, October 12, 1907, 9; Robert Abbe, "Radium as a Specific in Giant Cell Sarcoma," *Medical Record (Reprint)*, January 1, 1910, 2–3. Over the years, Abbe became ever more enthusiastic, and detailed, in describing how "at each treatment the knife cut tougher tissue," and how he knew ossification had begun because he could feel "gritty points as the knife cut it." Most of these articles appeared first as papers given at local medical societies, and the presence of illustrative hand gestures is strongly suspected, though unproven.

As the two cases already discussed suggest, most of the patients treated by the Abbes with radium suffered from cancer. Truman Abbe's file contains notes on eight other experimental cases. Of those, he explicitly designated seven as cancerous, and the other case, designated as "lupus vulgaris," probably also described a cancerous growth. One patient had cervical cancer, five (plus the "lupus" case) had growths on the jaw, lip, or face, and one had rectal cancer. By June of 1906, when Robert Abbe gave a paper reporting on his radium experiments at the June meeting of the American Medical Association, most of the 125 cases he described involved a condition that would fit under the broad umbrella of "cancerous," although Abbe himself defined "cancer" more narrowly (for example, differentiating it from "sarcoma"). Abbe emphasized to his audience that he had by far the greatest success in treating "the new growths, lupus, epithelioma, cancer and sarcoma," which "represent in their incipiency a large overgrowth of cells normal to the part invaded or grafted on it." Even when Abbe discussed using radium to treat non-cancerous conditions, like warts and moles, he still described it in terms of similarity with cancer, i.e. "the ordinary wart, small or large, is but an overgrowth of cells." These descriptions, seen in relation to the therapeutic discussions analyzed in Chapter 2, also suggest the degree to which Robert Abbe still viewed radium treatment in terms of x-ray therapy. Abbe made that comparison explicit for the audience at his AMA panel. Of the six conclusions offered at the end of the talk, the first three related to x-rays: "1. Radium action resembles that of Roentgen rays. 2. It differs specifically and will cure some cases promptly which resist the latter. 3. It is applicable to the interior cavities of nose and mouth, inaccessible to the Roentgen ray."[298]

As shown by the case of Mrs. G., however, the effectiveness of radium in cancer cases had very sharp limits. Radium could shrink tumors, but not much else. In Truman's notes, the

[298] Abbe, "Radium in Surgery."

two patients described as having advanced cancer both died shortly after beginning treatment. In many of the other cases it remains unclear as to whether or not radium treatment worked, or for how long. The patient with rectal cancer, for example, experienced a dramatic reduction in the size of the tumor and a corresponding improvement in the quality of his life, but the case notes describe the tumor beginning to grow again after only a couple of months' remission.[299] Reporting on his therapeutic experiments and results in 1906, Robert Abbe informed his colleagues that he had little success with what he described as "cancer in its graver form." Of the 125 cases reported for the paper, Abbe described 40 as belonging to this category. Abbe did not explain precisely what he meant by the term, but since all were tumors (as opposed to, say, leukemia), it seems probable that Abbe was confronting cancers that had already metastasized. Whatever his criteria, Abbe informed his audience that radium treatment had sometimes, as in the case of Mrs. G., shrunk tumors, but that the treatment "must candidly be said to have yielded no results worthy to justify a statement that radium has established any claim to be a specific cure." Even when therapy "gave evidence of control," the patients still died.[300]

Such results did not, however, change the fact that radium therapy produced more positive outcomes than negative ones, even when the results fell short of "miraculous." Truman's fifth patient, "Mr. S.," was typical of the more mundane results. Truman saw the patient off and on for five years, beginning in 1904, for an "epitheliomia of lip." Radium treatment never totally eliminated the problem; the patient's lip never entirely healed, and he had to return to Abbe periodically for additional treatments when the problem recurred. Nevertheless, occasional radium treatments kept the epithelioma for the remainder of the patient's life, and Truman included an entry in the file noting that when the subject died of

[299] Abbe, "Early Radium Cases."
[300] Abbe, "Radium in Surgery.," 4.

"nephritis" in 1916, he still had his "lip intact."[301] Mr. S.'s case thus encapsulated the way in which the Abbes' experience both affirmed and undercut the radium mythos. Radium really did have extraordinary power, but that power had very distinct limits.

Radium Production

Scarcity was another part of the radium mythos that grew more complicated in the real world of radium therapy. The purest, most powerful radium salts existed only in vanishingly small quantities; the total worldwide supply amounted to a couple of grams, at most. The Abbes had 15 milligrams, divided between two tubes—one containing five, the other, ten milligrams of radium chloride—supplemented by several less powerful samples, which they ultimately rejected as not producing adequate results. Every patient receiving radium treatment had to share those two tubes, meaning that only a small number of patients could receive treatment at any given time. In 1904, the German manufacturers began to produce high-activity salts comparable to those sold by the Curies, but the supply remained extremely limited. By 1906, the Abbes had added just two new tubes—60 and 20 mg, respectively—of Giesel's radium bromide to their collection, along with two tiny, 10 mg samples. Robert Abbe referred to them as "cells"; they would later be more popularly described as "seeds."[302]

Scarcity and the State

Radium scarcity resulted primarily from technological barriers, but it also had a public policy dimension. On the technical side, the barriers to radium extraction confronted by the Curies remained intact. Radium only appeared in vanishingly small quantities—one ton of uranium pitchblende might yield only a few grains of radium chloride—and no method existed for extraction beyond versions of the labor-, chemical-, and time-intensive process pioneered by

[301] Abbe, "Early Radium Cases."
[302] Abbe, "Radium in Surgery.," 1.

the Curies. Would-be radium producers could scale up the size of operations, employing more people to stir more tubs of boiling chemicals; the main French producer, the Nogent-sur-Marne plant that opened in 1907, employed this methodology. But actual efficiency gains, in terms of either speeding up the process of extraction or increasing the amount of material extracted from one ton of ore, remained elusive.[303]

A lack of high-quality sources of uranium-bearing ore further complicated the technical problem of extraction. The Curies had taken advantage of a ready source of pitchblende: piles of the stuff set aside as waste material from mining operations in Bohemia. But the rate of return on the process—for the Curies, it took approximately eight tons of pitchblende to produce one tenth of a gram of pure radium chloride—meant that industrial-scale processing would rapidly exhaust existing supplies. Responding to this fear, the Austrian government, in 1903, stepped in to tightly control, and largely deny, the further export of Joachimsthal pitchblende. Today, people tend to think of export controls on nuclear material as entwined with weapons and proliferation issues, but the practice actually far predated the bomb, and, in an interesting twist, it related not to the *danger* of nuclear materials, but rather to the perceived value and scarcity of radium. If radium proved to be a miraculous wonder drug, the imperial Austrian government meant to keep the miracle at home.[304]

Responding to both the price and the loss of Austrian supply, would-be radium entrepreneurs around the world began to look for other sources of ore. Several European governments involved themselves in the search. When the Royal Society of London tried to obtain its own pitchblende from Austria, it relied on an intervention by the Prince of Wales to obtain the material. When the grudging bequest of the Austrian government amounted to only half a ton, King Edward responded by putting his support behind a British Radium Corporation,

[303] Harvie, *Deadly Sunshine*, 56.
[304] Mullner, *Deadly Glow*, 18.

formed to commercially extract radium from pitchblende mined in Cornwall. Similar efforts, also backed by government support, took place in France and Germany, and in every case, the discovery of new sources of uranium ore coincided with the implementation of export controls.[305]

In her investigation of the radium industry, historian Maria Rentetzi found an interesting dynamic at work. In the early years, radium production required "a unique and novel cooperative tone." The need to process massive quantities of ore meant that "chemists and physicists were to forced to initiate collaboration with industrialists." The intervention of the state, however, kept local researchers—Sir William Ramsay, in Britain, Karl Ulrich, in Austria, the Curies and Emile Armet de Lisle, in France—firmly in control of the extracted final product. De Lisle's plant, at Nogent-sur-Marne, produced French radium for French researchers and French medical technology companies. The British Radium Corporation produced radium from Cornwall pitchblende for the British Radium Institute, which was responsible for overseeing British radium research and supplying the material to British hospitals.[306]

These facts suggest an interesting reinterpretation of the radium scarcity mythology. Contemporary newspaper accounts of radium consistently emphasized its theoretical price, which seemed to rise in every headline. By June of 1903, the *San Francisco Chronicle* estimated it at $170,000 per ounce. By 1905, the number had risen to "$3,000,000 an ounce," according to a breathless article in the *Washington Post*. As previously discussed, some of the price inflation reflected the false equivalence of pure radium metal with the more easily-obtained radium salts. But it also reflected a mistaken understanding of the radium market, as the wealthy American

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[305] Special Cable to The Washington Post, "RADIUM PRICE SOARS: $2,500,000 an Ounce Is Now Demanded for It. QUARTER OF POUND IS FOUND Once It Was Possible to Buy the Pitch- Blende Extract at $2 a Milligram. Sir William Ramsay to Attempt the Production on a Commercial Basis. One Romance of Science.," *The Washington Post (1877-1922)*, October 31, 1909.
[306] Rentetzi, "The U.S. Radium Industry," 439; Harvie, *Deadly Sunshine*, 61–62.

industrialist Joseph Flannery would learn firsthand, in 1909. When Flannery's sister was diagnosed with cancer, he traveled to Europe in hopes of purchasing radium for her doctor to use in an experimental treatment. Flannery declared that he would pay any price for radium, but he could find no sellers. Flannery's failure reflected the common mythology about the scarcity of radium: "A Demand That Cannot Be Satisfied," according to a contemporary story in the *New York Times*, which went on to tell readers, "How great is the world's demand and how limited the world's available supply may be gathered from the fact that the present price of radium is in the neighborhood of $40,000,000 a pound."[307]

Flannery's experience, however, was misleading; in reality, there was medical grade radium to be had. Robert Abbe took delivery of 87.5 milligrams of "500,000 x activity" radium from the Nogent-sure-Marne plant in October of 1908, and although the price was steep—8,800 francs, or about $1700—it was hardly unobtainable. What did Abbe have that Flannery lacked? Amidst the receipts for his radium purchases in 1908 and 1909, Abbe saved a series of telegrams with instructions for payment and delivery of radium tubes, signed "Wickham." "Wickham" was Louis Frédéric Wickham, a respected French dermatologist doing radium research in Paris (also, "a gentleman, a charming fellow, and has a lovely wife!" according to the letter of introduction that Abbe kept in his files) who apparently met Abbe at the International Dermatology Conference, in September of 1907. Wickham's institutional connections gave him access to radium from the French plant, and he in turn brokered sales of the material to his colleague in New York. Simply put, the mythology of radium's scarcity—its "unobtainable" status—resulted

[307] "THE METAL RADIUM: Its Properties, Possibilities, Limitations and Value," *San Francisco Chronicle (1869-Current File)*, June 14, 1903; "BOOM IN PRICE OF RADIUM.: Rate of $3,000,000 an Ounce Is Now Demanded for Metal.," *The Washington Post (1877-1922)*, May 18, 1905; Mullner, *Deadly Glow*, 24; Harvie, *Deadly Sunshine*, 60; "PRICE OF RADIUM NOW UP TO $40,000,000 A POUND: It Has Practically No Commercial Use, But Its Value in Laboratory Experiments Has Created a Demand That Cannot Be Satisfied. To Its Manufacture Many Countries Contribute Material, All of Which Goes Through a Very Elaborate Process at the Nogent Factory.," *New York Times*, May 3, 1908, sec. Magazine Section.

not from the technical barriers to production, but rather from the institutional barriers erected by governments. The key barrier to access lay not in wealth, but in connections.[308]

U.S. production and disruption

One notable exception to the export control regime existed: the United States of America, home to some of the largest uranium-bearing ore deposits in the world. The ore in question was carnotite, a bright yellow mineral found in the sandstone formations of the American southwest. Carnotite has a relatively low uranium concentration and thus contains almost impossibly miniscule amounts of radium, on the order of 1 to 180 *million* parts radium to ore. The ore's relative abundance, however, offset its low quality. In the arid region of the southern Colorado-Utah border, prospectors found carnotite in large, relatively easy-to-mine deposits. Moreover, the United States government did not maintain export controls on uranium or radium. Despite the myriad logistical difficulties of shipping tons of ore from the deserts of Utah to processing centers in Europe and England, foreign companies like British Radium rushed to stake claims.[309]

Americans had not completely ignored the potential of carnotite. Stephen Lockwood, an entrepreneur from Buffalo, New York, attempted to set up a refining operation using carnotite in 1903, but the poor efficiency of recovery and high cost of chemicals made the process unprofitable, and the company closed in 1908. In 1910, however, a new American player

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[308] "Letter to Robert Abbe, Introducing Louis Wickham," October 5, 1906, Earliest Sale of Radium in America - with Subsequent Correspondence (scrapbook), Historical Medical Library of the College of Physicians of Philadelphia; Louis Wickham, "Letter to Robert Abbe, Concerning Sale of Radium," September 30, 1908, Earliest Sale of Radium in America - with Subsequent Correspondence (scrapbook), Historical Medical Library of the College of Physicians of Philadelphia; "Receipt for Sale of Radium to Robert Abbe" (Bureau des Commandes, Nogent-sur-Marne, October 16, 1908), Earliest Sale of Radium in America - with Subsequent Correspondence (scrapbook), Historical Medical Library of the College of Physicians of Philadelphia; Louis Wickham, "Telegram to Robert Abbe Regarding Sale of Radium," November 25, 1909, Earliest Sale of Radium in America - with Subsequent Correspondence (scrapbook), Historical Medical Library of the College of Physicians of Philadelphia.

[309] Harvie, *Deadly Sunshine*, 67–68; Mullner, *Deadly Glow*, 18–20.

entered the field: the Standard Chemical Company, based in Canonsburg, Pennsylvania. Standard was headed by Joseph Flannery—the same wealthy industrialist who had fruitlessly quested in Europe for radium to treat his sister's cancer—and Flannery would succeed where Lockwood failed, massively altering the radium market in the process. Flannery actually had a history with carnotite. His General Vanadium Company was a vertically integrated producer of vanadium alloy steel, and although the company's primary supply of the element came from mines in Peru, General Vanadium had experimented with using carnotite as an alternate source. Spurred on both by their sister's death and the scarcity mythos—and its promise of an unquenchable demand for radium, at any price—Joseph and his brother, James, set up a new business, the Standard Chemical Company.[310]

It took three years of effort, and enormous difficulty, but in 1913 Standard Chemical managed to produce its first successful batch of radium. Here again, however, the Flannery brothers ran into the fundamental disconnect between scarcity as an element in the radium mythos and what "scarcity" actually meant in the radium market. As one might guess, based on the experience of Robert Abbe, the newspaper claims about doctors and hospitals clamoring for radium proved overblown, at least in North America. In reality, doctors who wanted radium badly enough had generally already found it, and doctors who had been unwilling or unable to pay princely sums for radium salts from Europe remained equally uninterested in Standard's still-prohibitively-expensive product. Instead, the same government-funded national institutes that controlled the radium supply in Europe snapped up most of Standard's production.[311]

Standard's activities, in combination with attempts by British Radium and other foreign producers to purchase large concessions in Colorado, finally attracted the attention of the U.S. government, and specifically of the Bureau of Mines. Charles L. Parsons, head of the bureau's

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[310] Mullner, *Deadly Glow*, 20–28; Rentetzi, "The U.S. Radium Industry."
[311] Mullner, *Deadly Glow*, 27; Rentetzi, "The U.S. Radium Industry."

mineral technology division, undertook a public crusade to address what he described as a foolish injustice: "The United States today is in the humiliating position of being forced to purchase at extravagant prices from abroad such radium as its hospitals can afford for experimental purposes, while we have been supplying the ores from which it is made."[312] In arguing for Congressional action, Parsons put an interesting twist on the radium discussion, arguing, in essence that its famous scarcity was an illusion engineered by some combination of scheming foreigners and a greedy "Radium Trust"—Standard Chemical, recast as Standard Oil. His public efforts led to umbrage taking in newspapers around the country. The *Washington Post*, for example, indignantly informed readers that, although "the United States has the greatest known supply of radium-bearing ores in the world, not one gram has been produced here," and concluded that Americans had been outsmarted by "the foresight of foreign scientists."[313]

The Bureau found some support in Congress, especially following the cancer death of New Jersey congressman Robert Bremner. Bremner's doctor, Howard Kelly, was a prominent supporter of radium therapy and research, and Kelly succeeded in making Bremner a *cause célèbre* for radium-based cancer research with a series of stories in the *New York Times*.[314] Bureau officials proposed two government strategies for radium, both based on European models. To promote U.S. supplies and use, the Bureau of Mines would create a National Radium Institute tasked both with producing radium for U.S. use and with handling distribution to scientists, hospitals, and researchers in the United States. The Institute would study all aspects of radium production, operating its own mining concessions and refining plant, in an attempt to

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[312] Special to The New York Times, "AMERICA IGNORES HER RADIUM MINES: Has Two-Thirds of the World's Supply, but Never Produced Any of It. RICH ORES SENT TO EUROPE Federal Bureau Discovers Fact When Old World Laboratories Buy Colorado Carnotite.," *New York Times*, May 5, 1913.
[313] "U.S. RICH IN RADIUM.: Europe Gets Chief Supply of Ores Here, Says Expert.," *The Washington Post (1877-1922)*, May 5, 1913.
[314] Harvie, *Deadly Sunshine*, 93–94.

find cheaper or more efficient methods for extracting radium from U.S. ores. It would also, like its European counterparts, subsidize radium-based research, including both medical and technical applications.[315] With regard to the existing marketplace in U.S. carnotite and radium, the Bureau proposed to nationalize the existing mining concessions and impose strict export controls—possibly, a full ban—on carnotite ore.[316]

Congress ultimately accepted half of the plan. It appropriated funds for a National Radium Institute, with both mining concessions and a refining operation in Paradox Valley, Colorado, but the institute would not provide free or subsidized radium to U.S. users, and Congress declined to nationalize the mines or implement export controls because of strong opposition in Colorado and Utah.[317] For its part, the National Radium Institute had real success. As promised, the Institute ultimately managed to produce radium for approximately $37K per gram, roughly a quarter to a third of its prior market price.[318] When the Institute presented Congress with its first two tubes of radium chloride—totaling 171 mg—the presentation "stopped business in the House," as "150 members crowded around" to examine the tubes of

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[315] "GREATEST RADIUM MINES: Will Be Developed In The Paradox Valley Of Colorado," *The Sun (1837-1988)*, October 27, 1913; "UNCLE SAM WARS ON RADIUM TRUST: Builds Laboratory to Test Ores from Federal Lands Ill Colorado for New Element. EUROPE GOBBLING FIELD. Beds Produce Almost Three Quarters of World's Supply of Alleged Cancer Cure.," *Chicago Daily Tribune (1872-1922)*, January 18, 1914.

[316] "ACT TO PRESERVE RADIUM ORE LAND," *Chicago Daily Tribune (1872-1922)*, January 19, 1914.

[317] "CONSERVATION OF RADIUM.: OPPOSING INTERESTS CONFER WITH SECRETARY LANE; Western Men Protest Against the Foster Bill, but Agree That There Should Be No Monopoly of the Precious Substance--Eastern Doctors Will Advocate Measure Today.," *Los Angeles Times (1886-1922)*, January 19, 1914; Special to The New York Times, "FEDERAL CONTROL OF RADIUM DOOMED: While Legislation Has Hung Fire Prospectors Have Gobbled Up Remaining Deposits. BILL RUINED BY CHANGES Senate Committee Amendments Make Government Monopoly Doubtful -- How Europe Takes Out Ore.," *New York Times*, March 23, 1914.

[318] "RADIUM'S COST REDUCED: Secretary Lane Announces U. S. Produces It For $37,000 A Gram DUE TO DR. HOWARD A. KELLY Bureau Of Mines Turns Out Five Grams, Portion Of Which Comes To Kelly Senatorium," *The Sun (1837-1988)*, November 22, 1915; "RADIUM AT LOW COST: Production From Colorado Ores Success, Says Lane. REDUCED TO $36,050 A GRAIN Bureau of Mines Reports It Has Developed Manufacture Past the Experimental Stage -- Deposits Rich, but Closely Held -- Conservation and Working by Government Are Urged.," *The Washington Post (1877-1922)*, August 1, 1915. There is some question about the actual cost of the National Institute's radium; critics of the scheme argued that the National Institute had not truly accounted for all of its costs, particularly with regard to salaries and other bureaucratic overhead. Nevertheless, the institute clearly made some actual efficiency gains, and it could have doubled the price of its product and still come out ahead.

gently glowing material. When House Speaker Clark asked the Chairman of the Committee on Mines and Mining to retire to the lobby with his samples, in order that business might go on, "two-thirds of the House trailed out after the Illinois member."[319]

By some measures, in fact, the National Radium Institute was *too* successful. U.S. production of high-purity radium salts grew to more than 22 grams in 1914; for comparison, estimates of the total world supply in 1913 came to just a quarter pound, or 113 grams. But even as production increased, the overall market for the element collapsed, as the start of the hostilities in Europe led the various European national institutes that had provided the primary market for radium to cease their purchases. Although the U.S. government had supported improvements in the technology of carnotite mining and refining, it still declined to financially support researchers, and even at its new, lower price, radium still cost well above what many private users (hospitals, researchers, etc.) could afford, and high enough to discourage institutional purchasers from acquiring more than very small quantities of the material. In 1915, U.S. production fell to a mere 6 grams, with many carnotite mines producing only the minimum amount of ore necessary to maintain their concessions. The mythology of scarcity would live on, but the age of actual scarcity was over, and the National Radium Institute closed down.[320]

Radium as Consumer Product

Thus far, the discussion in this chapter has focused on physicians using high-purity radium salts to treat diseases like cancer. This analytical approach puts radium into a technological frame similar in many ways to that of x-ray therapy, emphasizing the role of therapists in working out how to use the new radiation technologies and making therapeutic decisions—what diseases to treat, what methodology to use, the balance between safety and

[319] Special to The New York Times, "AMERICAN RADIUM SHOWN.: Success of the Kelly-Douglas Plant Announced in Congress.," *New York Times*, January 28, 1915.
[320] "RADIUM $9,000,000 A POUND.: War Causes Slump In Production of the Metal.," *New York Times*, January 31, 1916.

therapeutic efficacy—on behalf of patients who come to the doctor's office looking for medical intervention in response to a specific ailment. Reinforcing this narrative, doctors like Robert Abbe often used radium for the same purposes as x-rays, to treat skin cancers and other dermatological conditions.

There was, however, another side to the "radium therapy" narrative. Whereas the production of x-rays required access to very specific kinds of equipment, the types of radiation produced by radium samples occurred naturally in the environment, in ways that ordinary people could encounter. For many patients, radium was the key ingredient in a consumer health product used to treat generalized ailments or promote wellness, rather than a tool for shrinking tumors. For these users—and for the sellers of those products—radium therapy was built on the mythology of *helios*; they would consume the healing power of the sun.

Emanation: Over the Counter and In the Bath

So-called "emanation" was a key element in the evolution of radium as a consumer health product. As previously discussed, the radioactive decay of a radium atom results in the creation of two smaller atoms: one helium, the other radon, itself a radioactive element. It took several years, however, for scientists to work out the exact nature of radon and even longer to put a name to it. Instead of referring to "radon," radium users spoke of "radium emanation."[321] Radioactive decay is an ongoing process; any radium source will steadily release radon gas. Because of the ongoing creation and decay of radium in the Earth's crust, a steady supply of new radon is constantly being created. Radon is a heavy gas, so it tends to collect in caves, mines, basements, and other low spots. It can also diffuse into water, meaning that subsurface springs and aquifers often contain radon. Radon has a relatively short life (its most common isotope has

[321] William Ramsay, "Radium Emanation," *Science* 26, no. 657 (1907): 158–158.

a half-life of 3.8 days); the radioactive gas rapidly decays to form polonium, continuing a chain that eventually results in the production of stable lead.

Almost as soon as researchers worked out the basic facts of emanation—that radium steadily produced a separate, radioactive gas—they began to speculate about and experiment with using emanation as a therapeutic tool. Frederick Soddy developed a particular enthusiasm for this line of research, and in July of 1903 he published one of the first articles laying out a methodology for treating patients with emanation. Soddy recommended putting a radium sample into a sealable container, filled most of the way with water. Radon gas ("emanation") released by the radium would bubble up through the solution into the airspace; patients could then inhale the trapped, radioactive gas. Soddy suggested that the method would allow for the treatment of otherwise inaccessible lung diseases, like consumption (tuberculosis).[322]

Thus far, this narrative has focused primarily on physicians working with radium, but the discovery of emanation opened up a new avenue of radiation therapy. Medical consumption can take place apart from, and often in competition with, the practice of professional healers. In particular, patient self-care represented a significant source of competition for doctors in the medical marketplace: patients hoping to avoid the doctor's office turned instead to a wide variety of other practitioners and purveyors of supplements, tonics, remedies, equipment, and services. Through emanation, radiation therapy would spread into this market.[323] For good or ill, radiation therapy would be the purview not only by doctors, but of consumers.

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[322] Frederick Soddy, "Radium. A Method Of Applying The Rays From Radium And Thorium To The Treatment Of Consumption," *The British Medical Journal* 2, no. 2221 (1903): 197–99.

[323] In his 2003 article, "Health in the Home: A Tradition of Print and Practice," Charles Rosenberg found that "guides to regimen and midwifery" traveled alongside copies of the Bible in the first colonist ships to arrive in North America, and books like Nicholas Culpeper's *English Physician; and Complete Herbal* and William Buchan's *Domestic Medicine* eventually appeared in American editions alongside a host of works native to North America, like *Every Man his own Doctor: Or the Poor Planter's Physician*. As the title of the latter work suggests, patients saw self-care as a way to specifically avoid the cost and inconvenience of consulting with a physician. Charles E. Rosenberg, ed., *Right Living: An Anglo-American Tradition of Self-Help Medicine and Hygiene* (Baltimore: Johns Hopkins University Press, in cooperation with the Library

The initial arrival of emanation as a consumer health product would involve the world's oldest health industry: the resorts and bath houses built on and around natural hot springs. In 1903, J. J. Thomson's announced that he had found radium emanation in the water of very deep wells. Following Thomson's announcement, the operators of spas and health resorts at natural springs across the United States and Europe began to test their water and tout its radioactivity. The public associated such facilities with health; "taking the waters" had a history a history that, in Europe, stretched back to before the Roman Empire. Following Thomson's discovery, spa operators began to claim that science had finally discovered the mechanism that differentiated hot springs and other salubrious waters from the contents of the local pond. A typical example of the form appeared in the June 17, 1905 issue of the *British Medical Journal*, under the heading "Radio-Activity of the Harrogate Sulphur Water." A local physician had, "in view of the fact that the water of what is known as 'The Old Sulphur Well' plays such a valuable part in the Harrogate Treatment," sent a sample of the water to William Ramsay's lab for analysis, where it had tested positive for radon. Similar announcements, doubling as advertisements for "the Sulphur Waters of Baden, Austria," the "Wychia Water," of Droitwich, and every other spring or bath spot that could find it, appeared periodically throughout the rest of the decade, in medical journals, newspapers, and advertisements.[324]

In 1908, a German firm doing business as the Radium Heil Gesellschaft (RHG), or "Radium Healing Society," began to market several products that took advantage of this quality, including a series of "Radium Emanation Activators": jugs designed to produce radon-infused

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Company of Philadelphia and the College of Physicians of Philadelphia, 2003), 2–3 & 5; on alternative medicines, see also James C. Whorton, *Nature Cures: The History of Alternative Medicine in America* (Oxford ; New York: Oxford University Press, 2002).

[324] Lavine, *The First Atomic Age*, 101–4; H. Douglas Wilson, "Radio-Activity Of The Harrogate Sulphur Water," *The British Medical Journal* 1, no. 2320 (1905): 1330–1330; "Reports and Analytical Records FROM THE LANCET LABORATORY.," *The Lancet*, Originally published as Volume 1, Issue 4297, 167, no. 4297 (January 6, 1906): 38; "VIENNA.," *The Lancet*, Originally published as Volume 1, Issue 4319, 167, no. 4319 (June 9, 1906): 1646–47.

water for individual consumption. The crockery containers incorporated a radioactive mineral, such as pitchblende or carnotite, which would release radon gas into the water stored in the jug. Consumers were instructed to drink the water from the jugs on a regular schedule—with breakfast and dinner, for instance—as a cure for various ailments, a preventative treatment against aging and degeneration, and as a source of general "potency." Emanation jugs could be produced with relatively little ease, since they did not require "pure" radium salts; virtually any uranium-bearing ore, incorporated into the body of the device, would do. Numerous manufacturers followed RHG's lead, and a variety of devices intended for "Radium Emanation Therapy" soon appeared on the market. In the United States, the most popular brand sold under the name "Radium Ore Revigator," patented in 1912.[325]

Paint, Demand, and World War 1

Despite this early start, however, radium health products remained relatively rare until the 1920s. There were several reasons for both the lag and the eventual explosion in demand for radium emanation therapy. As previously discussed, prior to World War 1, access to the most common sources of radium-bearing ore remained for the most part tightly controlled by a mixture of public and private interests primarily interested in exploiting the ore for medical research focused on using high-grade radium salts to treat cancer. The radium industry was dominated by highly-capitalized, vertically-integrated consortiums, including both private companies, like Standard Chemical, and government-sponsored actors, like the British Radium Corporation and the U.S. National Radium Institute. These players showed little interest in marketing products to average consumers; instead, they focused on the high-dollar research market. That focus reflected, in part, the priorities of both the American and various European

[325] Lavine, *The First Atomic Age*, 101–7; Harvie, *Deadly Sunshine*, 148–57.

governments, all of whom treated the “miracle” of radium as a matter of national security, often to the point of putting in place restrictions on mining and export controls.

Under these circumstances, radium emanation therapy might well have remained an insignificant sideshow. The onset of the First World War, however, dramatically altered the status quo. With the start of the hostilities in Europe, the market for research-grade radium salts collapsed as governments reoriented their spending priorities and the various national institutes that had provided the primary market for radium ceased their purchases. In this sense, the war destroyed the market for radium, since the buyers had come primarily from Europe. Eventually, however, the conflict would help to launch a new radium boom, and the U.S. government would once again play a key role. This time, instead of throwing its money into improving the supply of radium, the U.S. government would become the world’s largest source of nuclear demand, helping to fund the post-war explosion of a radium therapy as a consumer health product, rather than solely as an experimental cancer drug.

In his early experiments with radium, the erstwhile William Hammer had experimented with using some of the material to create a paint that would glow in the dark. So-called “radium paint” actually contained very little of the element. The glow came not from the radium, but rather from the fluorescence of other materials in the paint (such as a zinc-sulfide phosphor) excited by radiation coming from the radium. Hammer concluded, however, that the incredibly high cost and extreme scarcity of radium made the large-scale production of the paint impractical, particularly in light of medical interest in using radium for cancer treatment. Glow-in-the-dark paint, while clearly useful, paled in importance when set against a cure for a dread disease.[326]

[326] Mullner, *Deadly Glow*, 15.

In 1914, the *Washington Post* ran a short story, under the headline "Demonstration with Radium," describing a lecture by Frank Hess. Hess extinguished the lights in the lecture hall and passed around a watch, its numerals traced in radium paint, that glowed in the dark.[327] Radium-for-paint made no sense in 1902, but by 1914, the price of radium had dropped, the supply had expanded, and the U.S. producers of radium were already watching their overseas customer base disappear. Into this picture stepped an Austrian immigrant, doctor, and entrepreneur, Sabin Arnold von Sochocky. Von Sochocky, like many would-be radium entrepreneurs, began by experimenting with using radium in therapy. Unlike the Abbes, however, von Sochocky did not have enough personal wealth or a successful-enough practice to fund his work. Searching for a steady source of research funds, von Sochocky hit on the clever idea to create luminous paint using a mixture of "regular" radium, ^{226}Ra, and mesothorium (^{228}Ra), a cheaper, less stable, and more readily available isotope.[328]

Von Sochocky initially painted watches in small batches and sold them to acquaintances, but he quickly recognized that he had stumbled onto a potential blockbuster product with his glow-in-the-dark paint. Von Sochocky found another investor, fellow physician George S. Willis, and started a company, the Radium Luminous Materials Corporation (eventually renamed "United States Radium Corporation," or "U.S. Radium").[329] From early on, U.S. Radium's directors had their eyes on military customers. In 1916, for example, company spokesman A. G. Hamlin published an editorial in *Aviation and Aeronautical Engineering* gathering complaints from pilots about the difficulty of using their electrically illuminated instruments at night, and

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[327] "DEMONSTRATION WITH RADIUM: Frank L. Hess, of Geological Survey, Lectures to Brookland Brotherhood.," *The Washington Post (1877-1922)*, March 11, 1914.
[328] Mullner, *Deadly Glow*, 42–43; Harvie, *Deadly Sunshine*, 165–66.
[329] Claudia Clark, *Radium Girls, Women and Industrial Health Reform: 1910-1935* (Chapel Hill, NC: University of North Carolina Press, 1997), 14–15; Mullner, *Deadly Glow*, 41–43; Harvie, *Deadly Sunshine*, 165–67. Different sources disagree on the starting date of the company, as either 1914 or 1915.

calling on the military to equip its planes with a full suite of radium-illuminated instruments because "the safety of the military night aviator depends upon it."[330]

It was an easy sell; the benefits of low-light, self-luminous materials in military applications were readily obvious. Radium paint was relatively cheap and could be produced in a variety of application-specific mixtures for everything from instrument and watch dials to signs, clothing, and tape. The material glowed just brightly enough for comfortable use without spoiling the user's night vision or revealing positions to distant snipers. Soldiers going "over the top" in a night attack would put radium-paint tabs on the backs of their jackets and tunics to prevent friendly fire incidents between the first and second waves of an assault. Engineers and pathfinders used luminous stakes and tape to mark obstacles and designate safe routes for relief parties making the dangerous night journey into No-Man's Land to recover bodies or wounded soldiers. And since the paint, unlike electric bulbs, required neither power nor maintenance and worked regardless of environmental conditions, it was an excellent solution for naval and aircraft instrument panels.[331]

From Soldiers to Consumers

The United States officially entered World War I in 1917, and the resulting military spending single-handedly revived the moribund radium industry. Estimates published in 1919 described military orders as accounting for 95% of the radium market. Noting the success of U.S. Radium, companies like Standard Chemical reorganized to focus on paint and consumer products, rather than on producing pure salts for researchers. In 1915, many carnotite mines had stopped operation; in 1917, U.S. Radium, unable to obtain enough material to satisfy its military cutomers' needs, moved into Paradox Valley, Colorado—formerly the home of the

[330] A. G. Hamlin, "Efficient Instrument Illumination for Military Night Fliers," *Aviation and Aeronautical Engineering (1916-1920)* 1, no. 5 (October 1, 1916).
[331] "Luminous PAINT Night Battle Aid," *San Francisco Chronicle (1869-Current File)*, January 13, 1918.

National Radium Institute—to set up its own carnotite mining and refining operation. By the end of the war, almost every U.S. soldier had a radium-dial wristwatch, and when those soldiers returned home, they brought with them an appreciation for the convenience of glow-in-the-dark products. By both reorienting the radium industry and radically expanding consumer familiarity with radium, World War 1 thus reshaped not only the market in radiation therapy, but in radiation more generally.

Both changes significantly impacted the perception—the mythology—of radium as a health product. The myth of scarcity would go down as a casualty of the war; although radium remained expensive, consumers would now perceive it as a luxury good, rather than as an unobtainable philosopher's stone. The mythology of extraordinary power had shifted. Consumers understood that radium was special: it did things, like glow in the dark, that other products could not. But the vision of radium as a source of near-miraculous power had receded, replaced by the mundane reality of radium as an everyday product. The mythology of helios, however, was stronger and more ubiquitous than ever after the war. Consumers did not need to hear about helium production or atomic energy to see the connection between radium and the sun. They could see the evidence with their own eyes, glowing in their own homes.

As military demand for radium fell after the war, the consumer products market exploded; in the one-year period from 1919 to 1920, the number of glowing clocks and watches produced in the United States doubled, going from two to four million units. Producers like U.S. Radium and Standard Chemical also began to diversify their product offerings, with DIY paint sets (U.S. Radium's "Radium Illuminating Set" included paint, adhesive, thinner, a mixing cup and rod, and a camel-hair brush) and glow-in-the-dark switches. Manufacturers of consumer health products began to incorporate radioactive materials into bandages, ointments, and tonics.[332]

Radium in the doctor's office

Doctors also experienced the changes in the radium market. Twenty years after the Abbes had conducted their first experiments on themselves and on patients, the basic methodology of radium cancer treatment had not really changed; treatment still consisted of placing radium in close proximity to a diseased area, leaving it there for an extended period of time, and then retrieving the sample for later use. The availability of radium, however, had radically increased. Prior to the war, physicians like Robert Abbe had to purchase their materials through special networks of friends and acquaintances; now, they could simply peruse a sales pamphlet and put in a purchase order with a local distributor. Prices had fallen precipitously, and in addition to selling tubes of radium salt, producers like the Radium Chemical Company, in Pittsburgh, offered a variety of apparatus, making it easier for doctors to use radium with patients.[333]

The radium salt was still stored in tiny glass tubes, but instead of using the tubes on their own, physician would purchase a variety of housings, each with its own advantages and uses. Metal capsules, for example, were intended to "screen" the radium sample, blocking its alpha radiation output while allowing the more penetrating beta and gamma rays through. Screw eyes on the end of each capsule simplified retrieval, with the physician leaving a silk string dangling from the end of the capsule, or allowed a surgeon to sew the capsule in place for multi-day exposures. Instead of slicing into a tumor and sticking a glass tube in, physicians could now purchase needle-type housings; the doctor could stick the needle directly into the tumor or

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[332] Mullner, *Deadly Glow*, 42–44; Harvie, *Deadly Sunshine*, 165–67.
[333] Pittsburgh PA Radium Chemical Company, "Radium Applicators," 1923, http://www.cppdigitallibrary.org/admin/items/show/4355.

other treatment area, exposing more of the radiation-emitting sample's surface area to the diseased tissue. The needles, like the capsules, featured various aids to use and retrieval, including screw eyes and extra-long shafts. Radium sellers offered a wide variety of handles, flexible cables, and other implements designed for thrusting samples into various bodily orifices. Moreover, radium producers offered educational materials, lectures, training sessions and other support services to doctors.[334]

Prescription Light

Sometimes, new entrants to a marketplace expand the pie for everyone, rather than simply stealing away business. For many physicians, the expanded availability of radium as a consumer health product would create new business opportunities. Patients now saw radium therapy as more than just a cancer treatment, and companies like Radium Heil Gesellschaft offered physicians new ways to take advantage of the radically increased consumer interest in radium by repackaging the pre-war concept of “emanation therapy.” With products like the Saubermann Radium Emanation Activator, companies with access to radioactive materials now sought to target physicians not simply with seeds or salts, but rather with “medicalized” forms of emanation therapy. Consumer products, it turned out, could be easily repackaged as drugs.

The medicalization process began with the construction of the device. Prior to the war, consumer-grade emanators like the Revigator and its brethren were essentially crockery jugs, indistinguishable, but for their labeling, from non-radioactive earthenware containers. RHG’s emanatory, by contrast, consisted of a transparent glass vessel with porcelain fittings. The design, and the accompanying sales materials, emphasized the “hygienic” nature of the vessel,

[334] Heber Robarts, *Practical Radium: The Practical Uses of Radium in the Treatment of Obstinate Forms of Disease* (St. Louis: s.n, 1909), 52–58; Radium Chemical Company, “Radium Applicators”; Rentetzi, “The U.S. Radium Industry.” Combining, as they do, medieval design aesthetics with the purpose of inserting radium into one’s body, the pictures of some of these instruments, while totally reasonable from a functionality standpoint, are terrifying.

taking advantage of the same aesthetics that ruled hospitals and bathrooms thanks to the newly-ascendant germ theory of disease. Equally important, it made the device look impressive when sitting in a doctor's office. The units' radium source was held in an internal housing at the center of the glass tank; thanks to the increased availability of higher-activity sources (rather than just raw carnotite or pitchblende) the company could afford to equip its units with salts pure enough to gently glow—"the best guarantee," sales materials point out, "for the presence of Radium to the patients."[335]

The business model proposed for such devices also emphasized the medical expertise of physicians. Philadelphia radium salesman Frank Hartman stamped all of his materials prominently with versions of the phrase, "We do not dispense to the laity direct," and sales materials made it clear that physicians should act like physicians, rather than sellers of health tonics. Rather than simply directing patients to drink from the jug, the Saubermann sales materials advised physicians to use the two-stage design of the emanator to measure out precisely calibrated volumes of water for patients, offering different amounts based on "the diagnosis of each individual case," because every physician should know that "there is no such thing as a standard dose" of medicine. The clear subtext was that physicians should represent the device to patients not as an unusually fancy, glowing water cooler, but rather as a therapeutic device requiring expert medical knowledge for its operation. Physicians were advised to calibrate and prescribe dosages based not on volumes of water, but on "Mache

[335] U. S. A. Radium Limited, "Saubermann Radium Emanation Activator," ca 1900, http://www.cppdigitallibrary.org/admin/items/show/4351; Frank Hartman, trans., "Radium Emanation Therapy (Sales Circular). From 'German Promotional Materials, 1920s; Frank Hartman Adaptations.'" (Radium Heil Gesellschaft, n.d.), Box 3, Folder 14, Frank Hartman papers, 1904-1977, College of Physicians of Philadelphia Historical Medical Library; Frank Hartman, trans., "Some Facts Regarding Emanation Therapy. From 'Radium Promotional Material, 1920s.'" (Frank Hartman Radium Products, n.d.), Box 3, Folder 15, Frank Hartman papers, 1904-1977, College of Physicians of Philadelphia Historical Medical Library.

Units" that would describe the theoretical energy-content of the water, and to advise patients to come in once a day for three to nine months, depending on the ailment under treatment.[336]

For many middle-class patients, the ability to purchase small quantities of radium water at a physician's office brought emanation therapy within financial reach for the first time, making radium therapy available to a much larger swath of the population. Advertisements for the devices soon began to include testimonials like that of physician Francis E. Park, of Stoneham, Mass., who found that courses of emanation therapy as short as one or two weeks could provide "a remarkable uplift of nerve strength" for patients suffering from afflictions like seasonal allergies, so that "the irritating matter, whatever it may be, no longer causes trouble." The patients described by Park in his letter are middle-aged professionals—people wealthy enough to afford the occasional treatment for hay-fever (at Saubermann's suggested rates, a prescription would have gone for roughly the modern-day equivalent of between $100 and $150) but not enough to obtain their own home emanators. For physicians, emanators represented a source of ongoing revenue. Patients had to come in regularly, and most could not fill their "emanation" prescriptions at an alternative retailer, such as a pharmacy. Emanators also represented a marketing opportunity, making it possible to tap into the lucrative radiological marketplace in a way that both emphasized the value of medical expertise and offered ancillary business opportunities, as a patient coming in for emanation might mention another malady in need of treatment or puchase a health tonic. Moreover, emanators served an

[336] Radium Limited, "Saubermann Radium Emanation Activator"; Hartman, "Radium Emanation Therapy (Sales Circular). From 'German Promotional Materials, 1920s; Frank Hartman Adaptations.'"; Hartman, "Some Facts Regarding Emanation Therapy. From 'Radium Promotional Material, 1920s.'"

advertising function, when patients told friends and family members about the glowing curiosity they had seen in the doctor's office.[337]

The Trouble with Radium

In 1921, American socialite, journalist, and magazine editor Marie Meloney arranged for Marie Curie and her daughters, Irene and Eve, to visit the United States. Meloney and Curie first met in 1920, when Curie agreed to do an interview for Meloney's magazine, *The Delineator*. The two women developed a friendship, and Meloney made arrangements for and accompanied Curie on both her 1921 tour and a subsequent visit in 1929. On May 23rd, The College of Physicians of Philadelphia held a special meeting with Madame Curie as the guest of honor. The meeting brought together Curie and Robert Abbe, who had a special interest in her visit. Due in part to his early work with radium, Abbe had become a great fan of Curie, with whom he carried on an occasional correspondence that lasted until Abbe's death in 1928.[338]

The meeting held in Curie's honor included a special presentation, by Abbe, of scientific mementoes that he had collected and donated to the College of Physicians. One of these was a quartz piezo-electrometer—one of the instruments used by the Curies in their early efforts to identify and isolate the first radium samples. The quartz piezo electrometer was shipped to the United States in March, ahead of Madame Curie's visit, and presented formally at the meeting on May 23rd. Curie got up and gave a few remarks about the device, but Abbe, noting that the scientific celebrity appeared "greatly tired from her Washington Experience," encouraged Curie

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[337] Radium Limited, “Saubermann Radium Emanation Activator”; Hartman, “Radium Emanation Therapy (Sales Circular). From ‘German Promotional Materials, 1920s; Frank Hartman Adaptations.’”; Hartman, “Some Facts Regarding Emanation Therapy. From ‘Radium Promotional Material, 1920s.’”

[338] The College of Physicians of Philadelphia, “Invitation to a Special Meeting of the College of Physicians with Speakers Marie Curie and Robert Abbe,” 1921, http://www.cppdigitallibrary.org/admin/items/show/4362; Marie Curie, “Letter from M. Curie to Robert Abbe,” 1921, http://www.cppdigitallibrary.org/admin/items/show/4391; *In Memory of Robert Abbe*. Abbe was an ardent fan of the Curies. Among other demonstrations of his affection, he named his pet swans “Marie” and “Pierre.”

not to overtax herself with a long speech. The piezo electrometer remains on display in the College of Physicians to this day, minus its original quartz crystal, which had a high level of radioactive contamination.[339]

Although the use of radium for cancer treatment offered hope to previously hopeless patients, Marie Curie's exhaustion almost certainly reflected the trouble with radium. As the Abbes' case notes showed, radium burns could be severe enough to leave permanent scars. By the 1920s, the risks of radium exposure were fairly well recognized—so much so that radium salesmen, like Frank Hartman, would provide safety materials to their clients. Hartman's recommendations made no secret of the danger of exposure; the first line on his card read, "Do not under any consideration handle radium in the palm of your hand." Hartman also provided what we would describe today as "hazardous materials" services, coming to clients' offices with a radiation detector to conduct clean-up in the case of a spilled or broken tube of radium.[340]

The most significant danger of radium, however, came from accidental consumption. Radium comes from the second column of the periodic table--the same column as calcium. When a person ingests radium, it, like calcium, gets deposited in bone tissue, where it causes a variety of health problems. In large quantities, radium can destroy the surrounding tissue, causing bones to weaken and fail, or lead to painful and deadly bone cancers. Between 1915 and 1920, a steady trickle of warnings about the dangers of working with radium appeared in various medical journals, mostly as anecdotal accounts from researchers at radium labs. Eventually, the danger of radium ingestion was confirmed and made very public by the terrible case of the so-called "Radium Girls": women hired to paint watch faces with glow-in-the-dark

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[339] Philadelphia, "Invitation to a Special Meeting of the College of Physicians with Speakers Marie Curie and Robert Abbe"; Curie, "Letter from M. Curie to Robert Abbe"; *In Memory of Robert Abbe*.
[340] Frank Hartman Radium Service, "Radium Department, 'Do Nots' and Things To Do" (Digitized by the Historical Medical Library of The College of Physicians of Philadelphia, ca 1900), http://www.cppdigitallibrary.org/admin/items/show/4352.

radium paint. The women continuously licked their brushes to give them a precise tip, and ingested radium paint in the process. Beginning in 1920, women from the U.S. Radium factory in Orange, New Jersey, began to report jaw pain and problems with their teeth. Doctors and dentists found that the bones in the women's faces were literally rotting from within; all would eventually die of complications from radium poisoning.[341]

The body eventually does rid itself of radium, so lower, more occasional exposures do not produce the same immediate, obvious harm suffered by the Radium Girls. But since ionizing radiation disproportionately affects rapidly dividing cells, like those in the bone marrow, long-term exposure to radium can lead to anemia, as the tissues responsible for blood-cell production accumulate permanent damage over time. Both Robert Abbe and Marie Curie suffered this fate. Abbe died in 1928, at the age of 76. As the autopsy report noted, Abbe's bones showed no signs of lingering radioactivity, but they were weakened and misshapen, leading the coroner to record the primary diagnosis as Paget's disease. The autopsy report also noted that samples taken from the right femur contained "a large amount of amorphous brownish material not at all resembling normal red marrow," and blamed bone marrow atrophy for the "blood destruction" and chronic anemia that had necessitated numerous blood transfusions towards the end of Abbe's life. Marie Curie suffered with similar health problems. She died in 1934, at the age of 66, of what is usually described as aplastic anemia (although it may have been a form of leukemia).[342]

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[341] Clark, *Radium Girls, Women and Industrial Health Reform: 1910-1935*; Mullner, *Deadly Glow*; Harvie, *Deadly Sunshine*; LL Carter, “Glow in the Dark Tragedy (Radium Girls),” *AMERICAN HISTORY* 42, no. 4 (2007): 32–32.

[342] St Luke’s Hospital Medical Board, “Report of Autopsy on the Body of Dr. Robert Abbe” (Digitized by the Historical Medical Library of The College of Physicians of Philadelphia, 1928), http://www.cppdigitallibrary.org/admin/items/show/4356; *In Memory of Robert Abbe*; Harvie, *Deadly Sunshine*, 46–47 & 123.

Despite the evidence of danger, however, radium health products like the Saubermann emanators would only become more popular in the decade following Madame Curie's visit to the United States, culminating eventually in the massive popularity of radium-laced over-the-counter health tonics. The most famous of these, Radithor, would do massive sales for years until one of its high-profile endorsers, steel magnate and socialite Eben Byers, suffered a gruesome death as the radium in his body caused his bones to literally disintegrate. High-profile deaths like these eventually led to bans on the products, but only after a long inquiry and only after thousands of buyers had exposed themselves to radium.[343] The fate that befell Byers and other Radithor drinkers was unfortunate, but it was also ludicrous—a totally foreseeable tragedy more or less perfectly predicted by the fate, a few years earlier, of the Radium Girls.

The problem lay with the mythology of helios—the idea that radium represented, like the sun, a "natural" source of energy. Consumers had become acquainted with radium products through military use during the First World War, and this familiarity made radium products feel safe. Radium had received substantial public press as a potential miracle cure in the years prior to the war, and consumers accepted the claim of radium sellers, in particular, that radioactive products reproduced the benefits of "natural" sources of therapy and health, like sunlight and hot springs.[344]

For doctors, pecuniary interest provided an obvious motive for promoting emanation therapy, but a not unreasonable case also existed to suggest that emanators represented a way to harness the benefits of radiation while eliminating the dangers of radium exposure. The

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[343] "STEEL MAN KILLED BY 'RADIUM WATER': Costly Medicine He Took as Tonic Proves Fatal to Eben M. Byers," *Washington Post (Washington, D.C. : 1877)*, 1932; Ron Winslow, "The Radium Water Worked Fine Until His Jaw Came Off," *Wall Street Journal*, 1990.

[344] "Radithor, the Modern Weapon of Medical Science: A Complete Treatise on Internal Radioactive Therapy" (Bailey Radium Laboratories, Inc., 1928), Box 14, Folder 11, Frank Hartman papers, 1904-1977, College of Physicians of Philadelphia Historical Medical Library; Macklis RM, "Radithor and the Era of Mild Radium Therapy"; "STEEL MAN KILLED BY 'RADIUM WATER': Costly Medicine He Took as Tonic Proves Fatal to Eben M. Byers"; Winslow, "The Radium Water Worked Fine Until His Jaw Came Off."

linkage of radon with hot springs also suggested an ethical way forward for sellers of emanation therapy cognizant of the risks of exposure to radium. Hot springs and spa waters, after all, had a long-tested reputation for healing people, not harming them. If the salubrious power of such places stemmed from the diffusion of radon gas into the water, then it logically followed that physicians need not worry about the safety of the products coming from their emanators. Indeed, this exact line of reasoning appears in the sales literature on Saubermann jugs. In sales circulars, the company's copy-writers explicitly and repeatedly emphasized the distinction between radium therapy and emanation therapy. The writer of "Some Facts Regarding Radium Emanation Therapy," for example opened with a declaration in bolded, capitalized letters that "the first and most important fact…is that **RADIUM ACTS BY ITS EMANATION ONLY**," before warning doctors that, while "radiation emanation … is **ABSOLUTELY HARMLESS** … it is by no means certain that Radium itself is equally harmless when introduced into the organism." One has a hard time imaging a distinction like this one in literature aimed at lay-consumers; it is a declaration calculated to appeal to someone who knows enough about radium to worry about the risks of exposure.[345]

In both cases, market participants, perhaps to their detriment, saw exposure to radioactivity not as a new or exotic or dangerous treatment, but rather as simply the latest iteration of older, "natural" types of therapy. At the end, as in the beginning, the relationship between radium and consumers would be defined more by the perception of the thing than by its realities. Unfortunately, the last and most pernicious myth of radium—that it was safe—would prove as powerful as its predecessors.

[345] Radium Limited, "Saubermann Radium Emanation Activator"; Hartman, "Radium Emanation Therapy (Sales Circular). From 'German Promotional Materials, 1920s; Frank Hartman Adaptations.'"; Hartman, "Some Facts Regarding Emanation Therapy. From 'Radium Promotional Material, 1920s.'"

CHAPTER 4 – THE DISAPPEARING ROENTGENOLOGIST

In the library stacks of the College of Physicians of Philadelphia, the early books on x-ray technology, radium, light therapy, and other forms of energy-based treatment all cluster together on the very last row of shelves. Amidst the early titles is a green-bound volume entitled *Practical Radium*, authored by Heber Robarts. Robarts was the founding editor of *The American X-Ray Journal* and the first president of the Roentgen Society of the United States, but *Practical Radium*, published in 1909, did not celebrate the glories of x-ray medicine. Comparing the two sources of radiation, Robarts concluded that the "big, complicated and inconvenient apparatus" used to produce x-rays "appears indeed ridiculous" when set next to a tiny capsule of radium. Elsewhere, a chart purporting to offer a side-by-side comparison of the two therapies borders on comical. Under the "Energies" category, a bracket drawn around the various types of radiation proclaims them "feeble" and "mostly wasted" when coming from an x-ray emitter. X-rays, according to the chart, are "dangerous," "uncertain," and unreliable, whereas radium is safe, giving certain, reliable results.[346]

Why did Heber Robarts experience such a change of heart on the subject of x-ray therapy? That Robarts had been diagnosed with x-ray induced cancer in 1907 could not have helped matters, but several clues suggested a more personal dimension to Robarts's disillusionment. In the preface to *Practical Radium*, Robarts gave a brief summary of his time working with x-rays, beginning with his founding of the *American X-Ray Journal*. The account concludes with the following lines:

> …the Roentgen Ray Society of America was formed in my office, 310 Chemical Building, Saint Louis. I was president of this society at the New York meeting, in the Grand Central Palace, in December, 1900, and at the Buffalo meeting, in the University of Buffalo, in September, 1901. These meetings were well attended by doctors representing most of the states and Canada, but the croakers did not come.

[346] Robarts, *Practical Radium*, 2 & 38.

> Years have passed, and what Agassiz said about croakers is again proven true. He thus analyzed them: first, they condemn your discovery and innovation; second, they deride and laugh at you; third, they suddenly change front and pose as the parent thinker of all you have done. It is significant to mention these contentions in the medical profession, for radium must go through the same order of oppression. Jealousy and mental limitation is ever in the road of progress…[347]

A second clue was added by one of *Practical Radium*'s readers. In Philadelphia, the College of Physicians was (and still is) the home of the Philadelphia Roentgen Ray Society, founded by another x-ray pioneer, Lester Leonard, whose portrait still hangs in the library reading room. The Philadelphia roentgenologists ensured that the College of Physicians library acquired a full run of the major English-language journals related to radiation. They also obtained copies of the latest textbooks and monographs in the field, including Heber Robarts's *Practical Radium*. The society's members were zealous in building their corner of the collection, and several of the volumes show evidence of their attention—marginalia and other ephemera. The note scrawled in sloppy script at the top of *Practical Radium*'s table of contents reads, "This book is the work of a Quack—unreliable, unscientific, self-advertising."[348]

This chapter examines the professionalization debate that took place within the early x-ray community by following the owners of the *American X-Ray Journal* through the tumultuous period from 1901 to 1903. During this time period, *AXJ* founder and editor Heber Robarts became so disillusioned that he permanently abandoned the x-ray community, selling his stake in the journal to Chicago x-ray therapist H. Preston Pratt, who turned the editorship over to another Chicagoan, T. Proctor Hall. Pratt and Hall intended to use the journal as a weapon in their fight with other members of the Chicago Electro-Medical Society. My purpose in taking a closer look at the intrigues unfolding around the journal's various owners is to bring a more personal, human dimension to a subject—specialization and professionalization in American

347 Ibid., xiv–xv.
348 Ibid., vii.

medicine—that historians of medicine have often viewed from a distance. For x-ray doctors like Heber Robarts and Emil Grubbé, who found themselves on the losing side, the disputes that divided x-ray therapists were intensely personal and often painful.

Heber Robarts's fall from x-ray grace resulted primarily from professional wounds—some of them vigorously self-inflicted—sustained in the battle over professional identity that enveloped roentgenologists after the initial enthusiasm of the Röntgen rush had worn off. The central question concerned who should count as an x-ray "doctor"; the anonymous reader who scrawled the word "quack" in Robarts's book essentially sought to shove him beyond the professional pale. Often, the debate took the form of arguments over licensure. In its August, 1902 issue, for example, *The American X-Ray Journal* (*AXJ*) included a short report on the Chicago Electro-Medical Society's "Resolution to Restrict the Use of X-Rays." For the members of the Chicago society, the resolution's text was a salvo in the bitter conflict that would soon split the organization. It read, in part:

> "Whereas physicians are the only class of persons recognized by law as having the necessary knowledge and training to justify them in the therapeutic use of dangerous agents . . . the Medical Practice Act of the State of Illinois should be so interpreted, or if necessary so amended, as to make it unlawful for any person not a legally qualified medical practitioner to expose to the x-rays, for any purpose whatever, any part of the living human body, for hire or expectation of reward."[349]

Many of the earliest x-ray "therapists" did not have a medical license or degree. Individuals like Emil Grubbé and Sydney Rowland often had a background in medicine, and even some formal medical training, but they did not have medical degrees or credentials. As a result, early professional organizations catering to x-ray users, like the Roentgen Society of London and its counterpart in the United States, more or less welcomed all comers. But as acceptance of radiation technology in the medical community grew, a rift developed between the orthodox, licensed physicians and their fellow x-ray and radium users.

[349] "Resolution to Restrict the Use of X-Rays," *The American X-Ray Journal* 11, no. 2 (August 1902): 1109.

Professionalization progress narratives

To understand that rift, one must first have some sense of the context within which the early radiation therapists found themselves. Given the degree to which scientific discovery and new research challenged and conflicted with the old medical traditions, nineteenth-century orthodox physicians faced a serious challenge to their legitimacy, and orthodox practitioners responded, in part, by embracing the progress narrative, portraying medical practice as a discipline evolving with and informed by the scientific process. As members of a scientific, progressive discipline, orthodox physicians essentially promised, in Warner's words, to make "active efforts to advance medical knowledge and practice" and "readily alter its practices if such change would bring about better care."[350]

The professionalization progress narrative runs along slightly different paths in the United States and the United Kingdom, but the basic endpoint—an emphasis on particular types of medical education as the prerequisite for a state-approved license—holds in both contexts. The professionalization progress narrative in medicine thus celebrates licensure as an essential feature of a "modern" or "scientific" system of medicine. For radiation therapists, the question became whether they would be included amongst the orthodox insiders, and thus share in the market benefits of professionalization, or excluded and shunned as unprofessional, non-physician hucksters and quacks.[351]

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[350] In reality, of course, a space existed between the *possibility* of change and its actual embrace. Tomes found that a majority of Anglo-American physicians still rejected the germ theory in 1880, and even in 1895, skeptics could be found publicly mocking members of "the antiseptic club" as hypochondriacs and hysterics. Warner, *The Therapeutic Perspective*, 163; Tomes, *The Gospel of Germs*, 27 & 91.

[351] In his study of medical licensure, sociologist Jeffrey Lionel Berlant found that orthodox medical professionals promoted it as "necessary to protect the public from medical practice by unskilled and untrained persons." Of course, in return for such protections, those same professionals argued that they should benefit from legal privileges; as suggested by the title of Berlant's book, *Profession and Monopoly*, licensure could easily serve as a way for orthodox physicians to exclude competitors from the medical marketplace. Jeffrey L. Berlant, *Profession and Monopoly: A Study of Medicine in the United States and Great Britain* (Berkeley: University of California Press, 1975), 129.

In the United Kingdom, the critical inflection point for professionalization came in 1858, when Parliament passed the Medical Act. The law established a General Medical Council of the United Kingdom (GMC) to oversee physician certification and to create a register of qualified practitioners.[352] As a result of various compromises, passage of the Medical Act marked the beginning of a long battle over professionalization and licensure, rather than a resolution of the issue. Still, it established a clear point of departure. Going forward, the debate in British medicine would revolve around who should control licensure and by what standard, rather than over whether or not the government should license physicians in the first place.

The U.S. professionalization narrative often begins with the first meeting of what would become the American Medical Association, in 1846. The issue that occasioned the initial meeting, standards for medical education, was the result of panic, rather than power; orthodox

[352] The Medical Act aimed to explicitly overturn an older order dominated by what amounted to a medieval guild system. That system placed medical certification under the tripartite authority of the Royal College of Physicians (RCP), founded in 1518, the Royal College of Surgeons (RCS), which traced its origins to the 1540 founding of the Barber-Surgeons Company, and the Worshipful Company of Apothecaries (WCA) which had received its charter in 1617. These fifteenth- and sixteenth-century "corporations"—the term used to describe the three major guilds—in turn built a tight affiliation with the traditional colleges at Oxford and Cambridge, particularly with respect to the regulation of physicians. By the mid-nineteenth century, however, a variety of factors had conspired to erode both the power of the corporations and their usefulness to both the public and the British government. Perhaps the greatest was that the Industrial Revolution and explosive population growth led to a proliferation of cities and towns across the United Kingdom that the corporations, based in London, were poorly-suited to manage. Despite having local and regional affiliates, the organizations mostly exhibited interest and exercised day-to-day control only in London and its environs. Historian John Chamberlain, in his history of British medical regulation, concluded that by 1858, "the medical marketplace in the countryside was unorganized, largely unregulated, and dominated by women and 'quacks' (defined by the colleges as individuals who had not passed [the Oxbridge] exams) who operated in direct competition with a few officially licensed practitioners (typically apothecaries)." As Chamberlain's explanation suggests, "quackery" in this context referred to pedigree as much as to practice. Some country doctors actually had formal medical educations and degrees, but from the "wrong" institutions; others had come to practice through a process of apprenticeship. Led by physician / force-of-nature Charles Hastings, of Worcester, doctors outside of London had begun to organize in 1832 under the auspices of the Provincial Medical and Surgical Association, which would eventually become the British Medical Association (BMA). The association promoted itself as the representative of physicians across all of England and Wales *except* London, forming a counterbalance to the corporations and the colleges. M J D Roberts, "The Politics of Professionalization: MPs, Medical Men, and the 1858 Medical Act," *Medical History* 53, no. 1 (January 2009): 37–56; John M. Chamberlain, *Social Issues, Justice and Status : Doctoring Medical Governance: Medical Self-Regulation in Transition* (New York, NY, USA: Nova, 2010); Paul Vaughan, *Doctors' Commons: A Short History of the British Medical Association* (London: Heinemann, 1959).

physicians were responding to the wholesale dismantling of state licensing statutes throughout the United States. U.S. licensing laws actually predated the Revolution, and they provided orthodox physicians with a measure of both legitimacy and market cachet, but they tended towards toothlessness, with laws in most places possessing neither meaningful educational standards nor effective enforcement mechanisms. Beginning in the 1820s, moreover, opponents of licensing, and especially unlicensed non-orthodox practitioners, like the Thomsonians, successfully harnessed a wave of populist sentiment to repeal the laws, and by the Civil War, licensure had effectively disappeared from the United States.[353]

In the 1870s and 1880s, however, states once again began to enact licensing statutes, in part because non-orthodox practitioners, like the homeopaths and Eclectics, reversed their opposition to licensure, motivated both by the danger of "unskilled competitors" and by the public acceptance of their legitimacy as physicians—under the new standards, graduates of homeopath medical schools, for example, qualified for licenses. The licensing push proved very successful; by 1895, most jurisdictions in the United States had some sort of licensing requirement. In general, licensing laws tended to develop over time; states usually started by passing a diploma requirement, followed eventually by restrictions on which schools' diplomas the licensing board would accept, and finally by a demand that graduates of all schools pass state-approved examinations. Taken together, these demands emphasized medical education as the path to professional status: an individual had to go to the right type of school and learn a prescribed curriculum in order to advertise him or herself as a "doctor." The American Medical Association proved adept at hitching its star to the issue of licensure; its members would

[353] The organization sought primarily to raise and standardize the requirements for medical degrees. In *The Social Transformation of American Medicine*, Paul Starr describes the AMA over the next half-century as a relatively powerless organization, unable to provide "some benefit or penalty, apart from the collective good, that [would] induce participation," and consequently cursed with few resources, a small member base, and an empty treasury. Paul Starr, *The Social Transformation of American Medicine* (New York: Basic Books, 1982), 44–58, 92.

ultimately become the essential gatekeepers of standards in American medical education and practice.[354]

For both British and American radiation therapists, and especially for x-ray therapists, the issue of licensure and medical education would create significant tensions, both within the community, between degreed and non-degreed practitioners, and beyond it, between radiation therapists and traditional physicians. The barrier to entry for x-ray practice was relatively low; the equipment, though expensive, was widely available, and "treatment," in its most basic iteration, simply required the placement of a subject in physical proximity to an energized emitter. As a result, many "therapists" entered practice without having completed a formal medical education—some without having completed any medical education at all. In seeking to build their legitimacy on the possession of specialized technical knowledge, rather than a medical degree, x-ray therapists without an orthodox medical education were explicitly challenging the licensure status quo, with conflict the predictable result.

The X-Ray in Quackery

The Chicago Electro-Medical Society had its first meeting on June 25, 1901, just six months after the first full convention of the American Roentgen Ray Society. From the Chicago society's point of inception, several important elements distinguished the two organizations and highlighted the debate unfolding between licensed doctors and everyone else. All of the Chicago founders listed themselves not as "roentgenologists," but as "doctors"; the group made a point of including the initials "M.D." after each name in its list of officers. Of particular note were the erstwhile, and now degreed, Emil Grubbé and a Chicago physician named H. Preston Pratt.[355] The American Roentgen Ray Society had opened its ranks to non-medical x-ray enthusiasts,

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[354] Ibid., 102–4 & 120–21.
[355] "The Chicago Electro-Medical Society," *The American X-Ray Journal* 11, no. 2 (August 1902): 1108–9.

including “technical electricians, chemists, teachers of chemistry and physics, [and] specialists and experts in electro-technique,” and had specifically separated itself from the American Medical Association on the grounds that x-ray operators without medical degrees should not be excluded from society meetings.[356] The Chicagoans, by contrast, explicitly restricted membership in their organization. “Persons outside the medical profession who are interested in the work of the society,” according to the group’s constitution, could participate only as “Associates,” rather than as full members. The Chicago society also adopted a tighter focus, eschewing general interest in electrical and x-ray phenomena in favor specifically of therapeutic research.[357]

For the Chicagoans and other physicians working with x-rays, the move to divide themselves from non-degreed roentgenologists was a necessary one in the face of growing skepticism by other physicians. A typical critique appeared in the March 2, 1901 issue of the *Journal of the American Medical Association* (*JAMA*), under the heading, “The X-Ray in Quackery.” The *JAMA* piece consists of a single paragraph—little more than a blurb—containing both a general and a specific warning about x-ray-based medical malpractice. The editor specifically warned readers to watch out for the advertisements of “specialists proclaiming the special advantages of the x-ray apparatus in their hands for modest females, enabling them to be examined without disrobing or otherwise exposing themselves.” Reflecting more generally on x-ray technology, the editor expressed surprise that “the x-ray should have been left so long unutilized by the quack.” He advanced a dual hypothesis that Crookes tube technology lay “a little above the intelligence of the average charlatan” and that “the regular profession has

[356] Jicinsky, “The Roentgen Society of the United States: Constitution,” 724; Robarts, “President’s Address,” 807.
[357] “The Chicago Electro-Medical Society.”

progressed so rapidly in discovering its remedial and diagnostic utilities that the quacks could not well keep up."[358]

At first glance, the essay seems largely congratulatory of x-ray practitioners and their technology: x-rays are mostly not used for quackery; real therapists are outrunning potential quacks; the technology is too sophisticated for charlatans. The author concluded, however, that unscrupulous users would inevitably adopt the x-ray, because "in several respects, it has a peculiarly happy adaptability" for fraud, given that "the buzz and the blue flames with brass and glass accessories would go far to create the mental impressions desired by the quack, and the unquestioned scientific associations of the methods would help to increase his victim's confidence." In other words, the very use of x-ray technology constituted grounds for skepticism—roentgenologists might not be quacks, but if someone *were* a quack, the author points out, this is precisely the sort of medical practice that would appeal.[359]

"Ignorance has even dared to write!"

Interestingly, the announcement of the Chicago society's formation did not appear in the *American X-Ray Journal* until more than a year had passed. Closing professional ranks around licensed physicians had put the Chicagoans at odds with *AXJ* founder and American Roentgen Ray Society president Heber Robarts. In the same month that the Chicagoans held their first meeting, the June, 1901 edition of *The American X-Ray Journal* ran two blistering editorials by Robarts. His first piece took aim at *JAMA* for the March 2 quackery story. The second piece criticized a report released by the American Surgical Association's "Committee on Medico-Legal Relation of the X-Rays." In his editorials, Robarts expressed contempt for licensure laws like the one embraced by the Chicagoans, lamenting that "medical laws are quite severe

[358] "The X-Ray in Quackery," *The Journal of the American Medical Association* XXXVI, no. 9 (March 2, 1901): 577–78.
[359] Ibid.

throughout the States and yet quackery is, if possible, more aggressive now than formerly." According to Robarts, medical degrees and licensure laws only "improved [the quacks'] standing, because besides flaunting the sheepskin they gild the frame about their State Board license. They are made in law and equity our equal."[360]

Robarts was not alone in decrying existing licensure procedures and the contemporary system of medical education. The offending March 2, 1901 issue of *JAMA*, besides discussing x-rays, also ran the text of a speech that seemed to fit neatly into Robarts's critique. Robert T. Morris decried osteopaths as "trying to find an easy way into a profession which requires long and arduous study" and "wishing to assume too great responsibilities on a small margin of medical education." According to Morris, the licensure system deferred too much to osteopathic medical colleges whose graduates were but "crudely prepared for practice."[361] Likewise, the article running immediately after the x-ray editorial proclaimed "Medical Anarchy in Texas" in response to the state's decision to accept "all diplomas of whatever qualifications for practice."[362]

Robarts's position, however, was different in kind from the more general orthodox critique of licensure. Where existing licensure laws and the existing system of medical education presented an insufficient barrier to malpractice, the *JAMA* editorial board, much like the members of the Chicago Electro-Medical Society, advocated strengthening and enforcing the existing system. The prescription for change sought to buttress, rather than to overturn, the existing laws, and it assumed that the existing cadre of licensed, orthodox physicians should be the ones to fix the system. Robarts, by contrast, utterly rejected the authority of the existing

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[360] "The X-Ray in Quackery," *The American X-Ray Journal* 8, no. 6 (June 1901): 919–20.
[361] "Address Before the New York State Assembly Committee on Public Health," *The Journal of the American Medical Association* XXXVI, no. 9 (March 2, 1901): 567-.
[362] "Medical Anarchy in Texas," *The Journal of the American Medical Association* XXXVI, no. 9 (March 2, 1901): 578.

medical establishment; in his telling, licensed orthodox physicians represented the greatest possible threat to good medical practice—the false prophets looking to lead the flock astray. The editorial aimed at *JAMA* offered a typical example of Robarts's claims. Fulminating that "ignorance has even dared to write," he railed against physicians who complained about "the inaccuracies of the x-rays":

> "It is absolutely known that there is no such thing as inaccuracies of the x-rays. These inaccuracies are wholly the errors of the operator. Do we have inaccuracies of the law of gravitation? Of reflection? Of magnetism? Of radial energy? When these fixed laws of nature are misunderstood then the would-be-wise sage of maturity leaps into the medical press, with the credit of his former bearing, and affects the professor's part. They cut for bullets, and failing to find them under the triangles, wires and fluoroscope, see a new vista and proudly assume the role of the world's teacher and tell how careful we should be in depending upon what we see. What an inglorious assumption… The medical profession should awaken to the realization of the responsibility the subject bears to us and question seriously every writer who can tell of the faults of the x-rays. Let him answer to his own faults first."[363]

The Professional Made Personal

Lest his words leave any ambiguity, Robarts spelled it out for his audience in the starkest possible terms: citing "the old definition of the term," the editor declared that any writer, "soever [sic] wise he may be in all other domains, if ignorant in [x-ray technology] and writes as a teacher he is a quack." For Robarts, the *only* measure of quackery lay in right or wrong thinking. Moreover, Robarts couched the debate in intensely personal terms: when he penned the aforementioned line about quacks "flaunting the sheepskin" and framing their licenses, Robarts intended it to apply not to some particular suspect class, like the osteopaths, but rather to anyone who disagreed *with Robarts*, in his assumed role as the stand-in or champion of roentgenologists as a group.

[363] "The X-Ray in Quackery," June 1901.

Heber Robarts was a licensed surgeon with a degree from Missouri Medical College, in St. Louis, and fifteen years in practice prior to his work with x-rays.[364] He had no reason to fear a credentialing requirement for roentgenologists. But Robarts had always presented himself as a sort of embodiment of x-ray users. In his role as editor of the *American X-Ray Journal*, Robarts always adopted the royal "we":

> "This is a pioneer journal of x-ray work. We are not imitators. We are casting our hopes among the needs and wants of man. We expect encouragement. While it cannot be expected nor desired that we shall escape just criticism, and it may be contumely, yet our aim shall be to improve each coming journal, encouraged as we are in the faith and usefulness of our mission."[365]

It may be that this very sense that Robarts had of himself—either as the x-ray everyman or the Roentgenological Moses, depending upon one's reading of the previous quote—helps to explain Robarts's willingness to stake his reputation so decisively on the professionalization issue. Whether for reasons of kingdom building or out of loyalty to his fellow x-ray enthusiasts, including those without medical degrees, Robarts could not accept the rapidly developing orthodoxy in medicine, with its emphasis on degrees and licensure. Robarts did acknowledge the potential dangers of quackery or incompetence, but he felt that those issues should be handled internally, amongst roentgenologists, and his definition of "roentgenologist" included those without medical degrees. Robarts spelled out his alternative solution to the problem of quackery, at least with regard to x-rays, as part of his editorial message to *JAMA*, promising that "we will begin to standardize uses of the x-rays and raying will become as accurate in the hands of the profession as they are free from inaccuracies."[366]

Robarts had first articulated this idea in his presidential address to the first convention of the American Roentgen Ray Society. Standardization, according to Robarts, would provide an

[364] Brown, "Heber Robarts (1852-1922)."
[365] Robarts, "Announcement."
[366] "The X-Ray in Quackery," June 1901.

alternate pathway to legitimize practitioners: by training in and following a set of procedures developed and approved by the existing group of roentgenologists, *anyone* could become an x-ray therapist.[367] To this end, the *American X-Ray Journal* began to devote itself to the ambitious project of publishing "a series of articles covering the entire teachings of the uses of the x-rays." Robarts's previous collaborator, J. Rudis-Jicinsky, authored the series of "lessons" as a sort of serialized textbook, published at the beginning of *AXJ* issues beginning in April of 1902.[368] Robarts's vision of a sort of correspondence class for aspiring roentgenologists had some basis in reality; the elimination of licensure statutes in the nineteenth century had enabled a few enterprising doctors to open up correspondence medical schools, granting degrees through remote study. But the distance-learning model was never going to work for x-ray medicine. Correspondence medical schools were basically for-profit diploma mills. By 1902, orthodox organizations, like the American Medical Association, had already repudiated such schools, and one purpose of new licensure laws, like the Medical Practice Act cited by the Chicago Electro-Medical Society, was to drive such institutions out of business.

Heber Robarts, X-Ray Outcast

The April, 1902 issue of the *American X-Ray Journal* ran the first entry in the series of explanatory articles that Heber Robarts had envisioned as the basis for standardization in roentgenology. It was also Robarts's last turn as editor of the journal and the end of his x-ray career. In attempting to thread the needle of concerns about quackery in a way that would satisfy both physician and non-physician x-ray users, Robarts had eventually satisfied no one, and as the two sides squared off for battle, Robarts found himself ejected from both camps.

[367] Robarts, "President's Address." For a more extensive discussion of the first meeting of the Roentgen Society and Robarts's remarks, see Chapter 1, "X-Ray Therapy in the Röntgen Rush, 1895-1900."
[368] Heber Robarts, "A B C of X-Rays (Ed. Note)," *The American X-Ray Journal* 10, no. 1 (April 1902): 1020.

The beginning of the end had come nine months earlier, at the second meeting of the Roentgen Ray Society of America.[369] The accounts of the meeting published in the *American X-Ray Journal* mostly took a laudatory tone. “Time has again shown,” wrote Robarts, “that the complainer and the wrecker fall together, while the enthusiast and the builder go on together.” Admonishing his readers that “if we are a fraternity, then in union there is strength,” Robarts proclaimed that “each member is strengthened by the strength of the whole,” and he called on them to “continuously look for light—more light, remembering again that it is the builder that constructs—not the wrecker. Our mission is onward, while the destroyers sleep by the wayside.” But the editor’s description of events at the meeting also took on a wistful and melancholy turn: although “we follow beacon lights in science and forget maledictions,” Robarts concludes that “we reach for the stars, and our ambition is not fulfilled.”[370]

The subsequent, October issue of the *AXJ* opened with a full-page portrait of Rudis-Jicinsky, captioned, “Retiring Secretary of Roentgen Society of America, whose potent service made this organization a success.” The first article was a letter to the editor, written by “Dr. Carl Beck, of New York City.” In reference to the September meeting of the Roentgen Society of the United States, Beck lamented that Robarts “did not accept the presidency again,” adding that “the best men of the society have note done one-tenth as much as you did to build the society up under the most difficult circumstances. If I would have been able to follow my intention to be present,” Beck concluded, “I would have given expression to these feelings of mine.” Robarts followed up the text of the letter with a cryptic explanation of the editorial choice to publish it: “Constant readers of *The American X-Ray Journal* will recall the studious effort of the editor to

[369] Previously, the “Roentgen Society of the United States”—the organization voted to change its name at the meeting in order to reflect the membership “from Canada, where the society has a healthy contingency”; Heber Robarts, “The Roentgen Ray Society of America,” *The American X-Ray Journal* 9, no. 3 (September 1901): 963–67.
[370] Ibid.

suppress all flattery of himself or personal mention where it could be avoided. But there is a far-reaching motive in this reference that will be understood by many now and by all a little later."[371]

The choice to turn the October issue into a celebration of the original officers of the Roentgen Ray Society seems to have been a deliberate—if, by Robarts's standards, relatively tame—protest against the displacement of the original officers of the organization by a new faction. The precise events of the overthrow remain apparently unrecorded; the official record does not appear to contain minutes from the business meeting, and Robarts never explicitly described or referred to it. Even the textual interpretation offered above would seem questionable, but for two corroborating pieces of evidence: 1) Robarts's own sharp reversal of enthusiasm for x-rays and 2) the various obituaries and remembrances written after Robarts's death. The former of these requires a further bit of consideration. How did Robarts get to the seeming rejection of x-rays on display in *Practical Radium*? For all his talk of the "complainer and the wrecker," after all, Robarts had issued a call for roentgenologists to close ranks after the September meeting, and his veiled comments in the October issue certainly did not indicate an abandonment of either roentgenology or its community. Moreover, if Robarts had grievances with the society, editorship of the *American X-Ray Journal* gave him a perfect platform for criticism.

That platform, however, had apparently grown shaky. Neither the November nor the December, 1901 issue of the *AXJ* had anything to say about the Roentgen Ray Society; indeed, the issues contained remarkably little in the way of editorial comment generally, particularly in comparison to previous instantiations of the journal. And after the December issue, the *American X-Ray Journal* abruptly disappeared for three months. When it reappeared, in April of

[371] Carl Beck, "A Letter.," ed. Heber Robarts, *The American X-Ray Journal* 9, no. 4 (October 1901): 975–76.

1902, the editor made no comment about the gap in publication; when the subsequent, May issue appeared, it listed "Charles P. Renner, M.D., M.E.," as editor. The journal's new editor made no mention of the change—no announcement, no explanation, no introduction. Heber Robarts, founder and first editor of the *American X-Ray Journal*, founding member and first president of the Roentgen Ray Society of America, was simply gone.

What happened to Heber Robarts?

The precise circumstances of Heber Robarts's downfall remain somewhat murky, but the circumstantial evidence paints an interesting picture. With regard to the *American X-Ray Journal*, the publication break may have related to financing. The *AXJ* had missed its publication schedule in the past, and Robarts had complained about the constant necessity of fundraising for both the journal and the Roentgen Ray Society. In his only comments about new Roentgen Ray Society President G. P. Girdwood, Robarts, noting that Girdwood had "cut short his European trip to be present at Buffalo," coyly assured readers that the new president would not need "to write letters every day in the year, neither will he need to question himself: 'Am I a missionary or a financier?'"[372] Another possibility is that Robarts had already decided to wash his hands of roentgenology after his ouster at the September meeting, and that his calls for comity were simply the face-saving moves of a man looking to leave as a martyr, rather than as a loser. Perhaps the search for someone to take over the *AXJ* editorship had outlasted Robarts's willingness to keep up his duties.

Percy Brown ascribed to the latter position in his 1936 memorial to x-ray pioneers—a book with the melancholy title *American Martyrs to Science Through the Roentgen Rays*. In his chapter on Robarts, Brown claimed that Robarts "disposed of his proprietary control of the *American X-Ray Journal*" because the editor "could foresee no realization of his hopes and ideals

[372] Ibid.

for a journal or for a national organization thoroughly dedicated to the interests of the 'new science.'" As suggested by the previous excerpts discussed in this chapter, Heber Robarts had no shortage of dire names for his professional rivals, but in none of his mentions of the "croakers," "wreckers," "destroyers," and so on had Robarts ever actually explained what happened in the Roentgen Ray Society. Brown, however, argued that Robarts lost out because "little or no provision was made as to restriction of membership" in the organization's original constitution. Brown remained hazy on the motivation for this choice, blaming it alternately on "laxity" or the "pressure of necessity that has ever been a concomitant of pioneering," but he concluded that the group's "ranks of earnest workers became infiltrated by those whose interests were merely commercial and who would exploit without compunction a young and struggling scientific body to further their own ends." Fortunately, in Brown's telling, "Robarts lived to see a complete restitution of the principles for which he stood," as, "by 1905, the salutary influence of Goodspeed, Bullitt and Lester Leonard," each of whom served as president of the Roentgen Ray Society, "had completely purged it of the corrosive pervasion that had so sorely tried Heber Robarts, and which would have surely resulted in its ultimate disintegration."[373]

Brown's insistence that Robarts had fallen victim to villainy was probably an example of the winners writing the history. Brown, himself a licensed radiologist with a medical degree and having held the same office as Heber Robarts—president of the Roentgen Ray Society of the United States—published his book less than a decade after the AMA had officially begun a campaign against commercial x-ray clinics operated either by non-physicians or those without specialist training in radiology.[374] Brown had entered x-ray practice in 1903 as an internist at Boston Children's Hospital, the year after Robarts left the community.[375] Brown repeatedly

[373] Brown, *American Martyrs to Science through the Roentgen Rays*, 203–4.
[374] Weisz, *Divide and Conquer*, 141.
[375] "Without Armor," *Time* 56, no. 17 (October 23, 1950): 99.

made clear his commitment to “the dignified growth of the specialty of radiology.” In the Robarts memorial, Brown claimed that the Roentgen Ray Society had only reached “the full flower of its scientific prosperity” by acknowledging that, in the words of another Roentgen Ray Society President, William Evans, “with medicine appropriating the new science to its use,” the community had “no place for workers in x-ray without medical training.” Elsewhere in the book, Brown presented Leonard’s presidency of the Roentgen Ray Society as an act not unlike Christ’s purging of the Temple market, with the true radiologists “sifted from a heterogeneous imbroglio” by Leonard “by reason of his inexorable attitude on the matter of functional forthright in scientific bodies.”[376]

Percy Brown’s tale of x-ray profiteers sneaking into the organization through a door left open by oversight or desperation seems at odds with Robarts’s own actions and public comments. Besides publicly railing against the existing system of licensure, Robarts had rejected suggestions that the Roentgen Society organize under the auspices of the American Medical Association. That was not an oversight; Robarts explained the decision at length in his presidential address to the first full meeting of the society.[377] William Evans, though still eager to downplay Robarts’s advocacy of non-physicians, probably got closer to the mark when he wrote that “Dr. Robarts’s influence in this organization was unfavorably affected by the undesirable activities of several who had attached themselves to him . . . it should be stated that he was in no way discredited, but rather he was a victim of the strife which developed between the medical and non-medical members of the society.”[378]

[376] William A. Evans, “American Pioneers in Radiology,” in *The Science of Radiology*, ed. Otto Glasser (Springfield, Ill: Charles C. Thomas, 1933), 28; Brown, *American Martyrs to Science through the Roentgen Rays*, 119 & 204.
[377] Robarts, “President’s Address.”
[378] Evans, “American Pioneers in Radiology,” 28.

For his own part, Robarts never decisively named the "croakers" or explained whether he saw himself as a victim of profiteers, led astray by hangers-on, or simply a physician who did not accept the developing consensus on orthodoxy and licensure. It does seem possible, however, that Robarts experienced a change of heart on his support for non-physicians in the aftermath of his ouster from the Roentgen Ray Society presidency, and regardless of whether or not he countenanced the move, the *American X-Ray Journal* would soon shift to favoring medical professionalization. The Chicagoans were on the march, and they intended to wage their battle against non-physician x-ray therapists from the pages of the journal, beginning with a purge of the unbelievers from within their own ranks.

New Management

In July of 1902, a letter from the editor of the *Journal of Gynecology and Surgery, St. Louis*, appeared in the *American X-Ray Journal*:

> "The *American X-Ray Journal* (St. Louis) for May comes in a new dress with the name of Chas. P. Renner, M.D., as editor, Dr. Heber Roberts [sic] presumably retiring. However, from appearance one would think 'it is the voice of Jacob but the hand of Esau.' Bro. Roberts will undoubtedly be still heard thru this ever-interesting journal."[379]

Renner's short reply read, "We thank you, Bro. Lamphear, for this notice and trust you may award us with more extended ones later on. Matter of x-ray interest we will gladly read in your most valuable journal."[380] The letter writer had a point; many of the minor features, advertising copy, gossipy blurbs, and editorial asides in the issues edited by Renner bore the distinctive stamp of Heber Robarts, either in authorial voice or in editorial interest. Renner may have included the letter and his reply for its value as farce; the July issue would mark his last appearance with the publication. The nature of Renner's tenure at the journal remains slightly

[379] Charles P. Renner, ed., "Letter to the Editor (Concerning Change of Editor)," *The American X-Ray Journal* 11, no. 1 (July 1902): 1085.
[380] Ibid.

unclear, but he seems to have been mostly serving as a caretaker until Robarts could find a permanent home for the enterprise. Renner was a colleague of Robarts's in St. Louis, and it seems likely that Robarts had either assembled much of the material for the issues in advance or that he continued the behind-the-scenes work of assembling the material but no longer wished to attach his name to it. In either case, beginning with the August issue, the *AXJ* would once again come under new editorial control, this time of a pair of doctors from Chicago: T. Proctor Hall and H. Preston Pratt.

When the August issue of the *AXJ* arrived in subscribers' mailboxes, its header contained two important changes: the aforementioned change in editorship, dropping Renner's name in favor of Hall and Pratt, and a change in location, from "St. Louis" to "St. Louis and Chicago." Whereas Renner's name had simply replaced Robarts's without comment, the August issue of the *AXJ* gave Robarts a proper send-off. The outgoing founder published a letter congratulating the new owner and "managing editor," Pratt, on "the wholesome and enviable position you occupy before the scientific and professional world."[381] The new editor of the journal, T. Prescott Hall, printed a response, entitled "Priority in X-Ray Journalism," in which he expressed "a deep debt of gratitude for [Robarts's] unselfishness and unswerving honesty of purpose," which had made the new owner and editor "heir to a journal of stainless reputation and genuine worth."[382] The last issue to bear Robarts's distinctive stamp—an article, authored by Robarts, which discussed the problem of contaminated drinking water in St. Louis and, perhaps fittingly, contained not a single mention of x-rays, radiation, or therapy—ran in the September issue.

More than a year after the event had taken place, the August issue finally ran a notice on the formation of the Chicago Electro-Medical Society, along with that society's resolution on

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[381] T. Proctor Hall, "Priority in X-Ray Journalism," *The American X-Ray Journal* 11, no. 2 (August 1902): 1114–16.
[382] Ibid.

restricting the use of x-rays to physicians properly accredited under Illinois's licensure law. Commenting on the resolution, the new *AXJ* editors expressed firm agreement that "only those who have some knowledge of physiology and therapeutics should be permitted to use x-rays" in the practice of medicine, and suggested that "medical societies, in their own interest as well as in the interest of the public, should bring this matter before the various state legislatures for action."[383] Their position represented a shift from that of the journal's previous editor, Heber Robarts, but it could not have come as much of a surprise to anyone who had read the short piece on the founding of the Chicago Electro-Medical Society. T. Proctor Hall was listed as the Secretary of the new society, and H. Preston Pratt's name appeared in a list of the founding members.[384]

The appearance of both the resolution and the editorial commentary in the pages of the *AXJ* signaled new points of focus for the journal. It remains unclear as to whether the Chicagoans had sought out Heber Robarts with the intent of buying out his stake in the journal or whether Robarts had extended the offer, but whatever the case, the new editors made clear from the beginning that they intended to use the *AXJ* as a promotional vehicle for the Chicago Electro-Medical Society and its members. Beginning with the August issue, the serialized instructional course—J. Rudis-Jicinsky's "Lessons for Beginners"—received new branding, appearing "under the auspices of The Chicago College of X-Ray and Electro-Therapeutics." Each issue of the journal also carried a summary of the Chicago society's latest meeting.

The Chicagoans intended to make their city into the center of x-ray activity in the United States. The May, 1902 issue of the *AXJ* contained an announcement notable both for its content and its desultory tone: "The Roentgen Society of American will meet sometime in December in annual session, unless changed by the managing board. Chicago will probably be the place of the

[383] "Resolution to Restrict the Use of X-Rays."
[384] "The Chicago Electro-Medical Society."

meeting."[385] Under its new editorship, the journal returned to earlier form, proclaiming to readers that the convention in Chicago "promises to be the best meeting in the history of the society," featuring "a very fine program" with "several of the leading men of Medicine and Science."

The Battle of Chicago

Readers of x-ray publications might have found the rebranding of the *American X-Ray Journal* a bit confusing; as of 1902, the Chicago Electro-Medical Society already had a house journal, the *American Electro-Therapeutic and X-Ray Era*. In fact, both the Chicagoans' acquisition of the *AXJ* and their endorsement of the Illinois Medical Act were strategic moves made by a rebellious faction of physicians looking to capture control of the Chicago Electro-Medical Society away from what they would later describe as crass commercial interests. The dispute came to a head at the organization's September 30 meeting, as recorded in the *AXJ*. One of the members, Charles H. Treadwell, read a paper on "The Use of the X-ray in the Treatment of Cancer." Following a brief discussion, the paper's author "then moved that, in view of the fact that there are two factions within the society, and that the society's usefulness has come to an end in consequence, the Chicago Electro-Medical Society do now disband." The society's president, Gordon Burdick, called for a vote on the motion, declared it passed, and "withdrew from the room, along with all who supported the motion," including Treadwell, Emil Grubbé, and Robert Friedlander.

The remaining members elected replacement officers and created a Judiciary Committee to investigate Burdick "for conduct unbecoming an officer of the society." They also voted to replace the *Era* with the *AXJ* as "the official organ" of the group. In an editorial meant

[385] Charles P. Renner, ed., "Untitled Editorial Announcements," *The American X-Ray Journal* 10, no. 2 (May 1902): 1043.

to explain the whole affair for *AXJ* readers, Hall blamed the “puerile attempt” to disband the society on a “disturbing element” of “non-medical or associate members . . . supported by the president and two or three members of the society.” Lamenting that “the commercial spirit finds its way into even the healing art,” Hall called the incident “a warning to all to draw the lines strictly according to medical ethics.”[386]

Open warfare between the two groups would continue for years, taking several bizarre twists along the way. While Pratt and Hall had been negotiating for control of the *American X-Ray Journal*, the walk-outs had pursued their own scheme, applying to the state government of Illinois for an official charter for the “Chicago Electro-Medical Society.” The Burdick faction apparently revealed this document with a flourish at the September 30 meeting, claiming that it gave them exclusive rights to the name. In the subsequent months, two Chicago Electro-Medical Societies would meet in the city, each publishing its activities in a separate journal. A lawsuit based on the proper application of Robert’s Rules of Order went against Burdick, et al., but did not result in an injunction, and the two squabbling sides remained in their corners throughout 1903, emerging occasionally to poke one another in the eye, as when the *Era* managed to lure away the titan of written x-ray instruction, J. Rudis-Jicinsky. The whole sordid affair dragged on for years.

In the meantime, however, the Pratt and Hall faction had revealed their larger ambitions: the construction of a new professional group, the American Electro-Medical Society, intended to “bring together all those who are interested in this branch of medical work.” This new society, according to the announcement in the *American X-Ray Journal*, would differ from the Roentgen Society of America by having a structure much like that of the AMA, with national,

[386] T. Proctor Hall, “Chicago Electro-Medical Society,” *The American X-Ray Journal* 11, no. 4 (October 1902): 1181–82; T. Proctor Hall, ed., “Commercialism in Medical Societies,” *The American X-Ray Journal* 11, no. 4 (October 1902): 1183.

state, and local branches; more importantly it would differ from both the Roentgen Society and the American Electro-Therapeutic Association "in not having an unlimited membership," thus preventing non-physicians from "sitting in judgment upon the ethical standing of physicians who apply for membership."[387]

Licensure, Specialization, and the Marketplace

It becomes clear, at the point where the members of the American Electro-Medical Society vie with the Roentgen Ray Society of the United States and the American Electro-Therapeutic Association while prosecuting a feud between the two Chicago Electro-Medical Societies, that the historical value of this incident lies not in the narrative of a particular organization—most of these did not survive in their original form—but rather in what the tussle says about professionalization and specialization in American medicine. In his comparative history of medical specialization, the aptly named *Divide and Conquer*, George Weisz argued that "in the United States, freedom to establish private hospitals and medical schools left considerably less social distance between the medical elite and rank-and-file practitioners and created more fluid social conditions." Weisz also found that "the competitive environment fostered by large numbers of private institutions" made the "market pressures and economic incentives . . . if not more potent, then more visible and transparent in the United States."[388]

One of the interesting features of the early x-ray community was that it included so many participants who saw themselves as medical practitioners despite having little or no formal medical training. Of the walk-outs from the September 30 meeting, Grubbé and Burdick had medical degrees, but Treadwell and Friedlander did not. Treadwell had been a physics professor at Syracuse University prior to becoming involved with x-rays; Friedlander was an

[387] T. Proctor Hall, ed., "Editorial: The American Electro-Medical Society," *The American X-Ray Journal* 12, no. 2 (February 1903): 63.
[388] Weisz, *Divide and Conquer*, 63.

engineer and equipment manufacturer specializing in electro-medical apparatus. For this reason, it made sense that the Pratt and Hall faction framed the battle over control of the Chicago Electro-Medical Society as a dispute over licensure. As previously discussed, non-medical x-ray practitioners would remain a significant source of competition for physicians well into the 1930s. But the inclusion of Grubbé and Burdick amongst the conspirators does not make sense in that framing; these men had nothing to fear from a licensure dispute. Rather, this was a specialist dispute, playing out more or less exactly along the lines described by Weisz.

The advertisements in the March, 1902 issue of the *American Electro-Therapeutic and X-Ray Era* make clear that Hall and Pratt—soon to acquire the *American X-Ray Journal*—had no compunctions about making money on x-rays. In a full-page advertising spread, the pair is listed as the principals in two joint ventures: Dr. Pratt's X-Ray and Therapeutical Laboratory, which provided both diagnostic services and therapy, and the aforementioned Chicago College of X-Ray and Electro-Therapeutics, billed as "a post-graduate college in which physicians can get the greatest possible amount of scientific and practical knowledge in the shortest possible time," leading to "the degree of Doctor of Electro-therapeutics." The latter institution clearly fits Weisz's description of the small, for-profit institution. In addition to the degree program—awarded in just three months of study—it offered correspondence courses and weekly mini-programs arranged by a staff of "fifteen of the leading specialists of Chicago."[389]

The faculty list, however, did not include Professor Treadwell. He taught for the Edison College of Electro-Therapeutics, which offered a Master of Electro-Therapeutics degree that physicians could complete in two weeks for a mere $25. Also of note, neither Treadwell's Edison College nor Hall and Pratt's Chicago College employed Emil Grubbé or Gordon Burdick; those two sat instead on the faculty of the Illinois School of Electro-Therapeutics, which, though it

[389] J. O. M. Hewitt, ed., "Advertisements," *American Electro-Therapeutic and X-Ray Era* 2, no. 1 (January 1902): 16.

could not confer degrees, would provide students with “a handsomely engraved certificate of attendance.” Grubbé, like Pratt, was running a dual-operation, with the Illinois School of Electro-Therapeutics housed in his own Illinois X-Ray and Electrotherapeutic Laboratory. Burdick, meanwhile, split his time between his teaching duties and Dr. G. G. Burdick’s X-Ray and Therapeutic Laboratory,” which specialized in “the treatment of all cutaneous diseases by x-ray.”[390]

Clearly, the market for x-ray services aimed not only at patients, but at physicians, was a competitive place, and it seems clear that these marketplace tensions had much to do with the fracturing of the Chicago Electro-Medical Society. In that sense, the specifics of this narrative bear out Weisz’s theory about specialist markets in American medicine. As is so often the case, however, considering a very specific episode with a discrete cast of characters, like the one in Chicago, reveals that the relatively large-scale, impersonal forces described by Weisz played out at the local level in intensely personal ways: professional slights, hurt feelings, denied opportunities.

Sticks and Stones

Emil Grubbé never quite seemed to fit in with his colleagues at the Chicago Electro-Medical Society. Based on a reading of his memoir and some of his other writings, Grubbé was probably a hard man to get along with. He had a tendency towards bombast and self-aggrandizement, and the possibility of glory and wealth clearly played a role in his attraction to x-rays. One of the claims dearest to Grubbé’s heart was his assertion that he had conducted the first experiments with x-ray therapy, in February of 1896. As previously discussed, the validity of

[390] Ibid., 12–16.

Grubbé's claims to priority are difficult to adjudicate, but his description of events was not particularly implausible.[391]

Grubbé's colleagues at the Chicago Electro-Medical Society, however, utterly rejected not just his assertion of therapeutic priority, but even the lesser claim that Grubbé belonged to the founding generation of roentgenologists. In 1902, the group assembled a committee to investigate claims of priority. The resulting report, circulated to the *AXJ* and other outlets in May of 1902, did not even mention Grubbé. Instead, it claimed that priority for the first treatment of humans fell to H. Preston Pratt, based on his publicized treatment of two patients with stomach cancer, in April of 1896. The choice was not an entirely unreasonable one. The custom of adjudicating priority based on publication dates was well-established by this time, and the committee members adopted a similar methodology, although they made use of both publication dates in medical journals and stories that ran in major newspapers.[392] Interestingly, Grubbé to some extent accepted the legitimacy of that judgment in his memoirs, explaining that, "At the time I gave these early treatments with the x-ray, I could not myself make a clinical report or write a paper giving the details, because I had not yet graduated as a physician and consequently did not have access to the medical journals."[393]

Other evidence, however, suggests something more visceral in the exclusion of Grubbé from the official record. Pratt's supporters seem to have taken particular issue with Grubbé; the dispute had, for whatever reason, turned personal. In November of 1902, a few months after the schism in which Grubbé left the meeting, an editorial blurb appeared in the *AXJ* commenting on a paper Grubbé had submitted to the *Medical Record*. The tone of the whole thing makes a

[391] Grubbé, *X-Ray Treatment: Its Origin, Birth, and Early History*.
[392] "Priority in X-Ray Therapeutics: A Summary of the Work Done in 1896 (Report of the Research Committee of the Chicago Electro-Medical Society, Adopted March 25, 1902)," *The American X-Ray Journal* 10, no. 2 (May 1902): 1042–43.
[393] Grubbé, *X-Ray Treatment: Its Origin, Birth, and Early History*, 55.

strong impression: "The doctor is to be congratulated upon the extent of his work for one who has been at it such a comparatively short time. Dr. Grubbé is mistaken, however, in supposing that he is 'doing pioneer work in this line.' He appears to be ignorant of the fact that the pioneer work in this direction was done both here and in Europe during the years 1896 and 1897…" After lobbing a few insults at Grubbé's theory of the therapeutic action of x-rays and mocking Grubbé's (totally correct) claim that cold cathode tubes could and did produce a wide variety of x-rays at different wavelengths, the editor concluded his piece with a final patronizing send-off: "The success that Dr. Grubbé reports in his treatments must be encouraging to others who, like him, are comparatively new in the work."[394]

In working with documents like these, one cannot ever really know for sure the contents of the writers' hearts. Pratt and Hall had very good reasons to dislike Emil Grubbé, given the latter's participation in the schism of their society. Nevertheless, this column, in combination with several others written on the issue of priority (which Pratt apparently prized very highly), *feels* like a piece of professional bullying. The editors of the *AXJ* had, after all, reached out to make this attack, spotting Grubbé's paper in another journal and bringing it up specifically for the purpose of ridicule. In perusing the *AXJ*, no similar attacks appear against Burdick or the other September 30 conspirators.

In reading Emil Grubbé's x-ray memoir, written almost fifty years later and at the end of a professional career in radiology that was highly successful by any measure, it seems clear that he still keenly felt the sting of his treatment at the Chicago Electro-Medical Society. Grubbé returned the long-held grudge; despite numerous mentions of the Chicago x-ray community, he omitted any mention of Pratt in an otherwise-detailed roundup of early work in x-ray therapy. In the long list of professional associations that followed his name on the title page, Grubbé

[394] T. Proctor Hall, "X-Ray in the Treatment of Cancer," *The American X-Ray Journal* 11, no. 5 (November 1902): 1213.

included his status as a “Charter Member” of the Radiological Society of North America and the American Roentgen Ray Society. Of the Chicago Electro-Medical Society, he made no mention.

The stories of Emil Grubbé and Heber Robarts reveal the human dimension of controversies and forces that we often think about in very impersonal ways. Heber Robarts was, in the long run, clearly on the wrong side of the debate over licensure and training; his attempt to keep roentgenology separate from the larger medical establishment was doomed from the outset. For Robarts, however, the story turned not on unrealistic expectations or the growing consensus around licensure, but rather on the betrayal of small-minded usurpers. In the case of the Chicagoans, for all their talk of purging unsavory “commercial elements” from the society of high-minded physicians, Pratt, Hall, and their colleagues in the Chicago Electro-Medical Society were fundamentally engaged in commercial maneuvering, using their status as physicians against competitors in the marketplace. Moreover, even this lens fails to capture the way in which a competition for prestige and clients that bore out Weisz’s analysis nevertheless seems to have turned on basic questions of likeability and group dynamics: Pratt was in, and Grubbé was out. Out of such capricious material are the larger trends of history constructed.

CONCLUSION – SEEN BUT NOT FELT

Frank Hartman, of Philadelphia, entered the radium business after his army service in the First World War. Hartman kept a journal of his activities, which included providing hazardous material services, cleaning up and recovering spilled or lost radium. In his journal entries, Hartman repeatedly expressed his fears about exposure to radium; in one case, when summoned to clean up an office where a tube of radium had exploded, scattering material all over the room, Hartman described it as the day he had prayed would never come. In later life, Hartman volunteered for testing programs intended to determine whether or not he had radioactive material in his body. In all cases, Hartman received a clean bill of health, but he volunteered several times for testing, never truly believing that he could have escaped unscathed from his long experience as a broker and handler of radioactive material.[395]

For a modern, educated audience, many of the images, descriptions, and photos presented in this study—the picture of a doctor's hand beside a live emitter, the chart listing erythema doses, the caption explaining that a photo required a fifteen minute exposure, the x-ray photo of an infant's skeleton—range from cringe-worthy to deeply upsetting. Frank Hartman would have understood these reactions. Hartman knew how to handle radium safely, and he managed to make a career out of it. But even with all of his experience, and even with no evidence that his protocols had failed, Hartman apparently always assumed that radium was so dangerous, and radiation so pernicious, that the best safety protocols might be insufficient and that his work with radium had probably poisoned him in some way. It was this gut-level distrust of radiation that the first generation of radiation therapists failed as a group to develop, even

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[395] Frank Hartman, "'#15. 50 MGM. Radium Tube Exploded. Radium recovered.' Undated Entry in the Hartman Radium Diary." (diary entry, n.d.), Box 2, Folder 5, Frank Hartman Papers, 1904-1977, The Historical Medical Library of the College of Physicians of Philadelphia.

after they had to acknowledge evidence of the danger. The question that demands an answer is "why?"—why did radiation therapists not feel any urgency about the dangers of exposure?

The answer to that question varied substantially from therapist to therapist, but three factors in particular stand out. First, medical treatment in general, and cancer treatment in particular, involved a great deal of risk in 1895, which made the risks of radiation exposure feel less pressing. Second, the ways in which people understood or misunderstood radiation tended to obscure the problem of risk. Finally, early radiation therapy, like many novel technologies, appealed to a particular kind of person; today, we might call that person a "technological optimist" or "futurist." As a result, early x-ray therapists created a community that celebrated and reinforced positive narratives about radiation therapy at the expense of reasonable fears about the hazards of the technology.

Savior and Threat

Stanley Joel Reiser, in a discussion linking the development of dialysis machines and the artificial respirator, described "technological medicine" as paradoxical—"at once a savior and a threat."[396] Radiation technologies clearly fit this conception, and the savior-threat paradox is one of the most compelling elements of the x-ray therapy narrative. When therapists began treating diseases like cancer and lupus vulgaris with x-rays or radium, they realistically had few or no other treatment options available. Radiation therapy, moreover, could produce not just results, but semi-miraculous outcomes, at least in some cases, as regularly shown by before-and-after patient pictures published in the *Archives of the Roentgen Ray*. Out of control tumors and disfiguring infections seemed to melt away under the influence of x-rays or radium seeds. Radiation therapy—using both x-rays and radioactive materials—is still used today as a treatment for cancer because it works. It is tempting, therefore, to assume that early radiation

[396] Reiser, *Technological Medicine: The Changing World of Doctors and Patients*, 51.

therapists simply could not see the hazards of the technology because they were blinded by the enormous potential benefits.

As this study has shown, however, the problems of radiation exposure were neither hidden nor unknown. X-ray and radium therapists began to experience and publicly report the negative consequences of exposure more or less as soon as they began to work with radiation, first with painful dermatitis, and later with cancer, anemia, and even a few cases where patients died of acute radiation poisoning. These harmful effects of radiation exposure represent the other half of Reiser's paradox. Incidents of radiation harm, such as Clarence Dally's death, appeared in venues like the *New York Times*. It would have been ludicrous for practitioners to claim ignorance of the threat of radiation, and enough patients knew of the danger, according to Matt Lavine, that doctors took to offering reassurances in their advertising.[397] But although therapists could, and did, talk endlessly about dermatitis, the first generation of x-ray and radium therapists mostly never showed evidence, either in their writing or their behavior, of actual unease with their tools.

The question of why this might be the case—why did practitioners find it so easy to erase the threat side of the equation?—is interesting because it has not disappeared in the century since x-ray therapists began to ply their trade. Today, regulations of various kinds have raised barriers to deploying new medical technologies, but medical innovation remains a hazardous business, as demonstrated recently by the Theranos scandal. Much as x-ray advocates had prematurely promised a cure for cancer, Theranos founder Elizabeth Holmes promised a major leap forward in the technology of blood tests that she ultimately could not deliver. Holmes, like Heber Robarts, vastly overestimated what she could achieve and

[397] "C. M. Dally Dies A Martyr to Science: Was Burned While Experimenting with X-Rays"; Lavine, "The Early Clinical X-Ray in the United States."

underestimated the difficulties that lay ahead. As a result, patients suffered from botched tests.[398]

Interestingly, most observers of the Theranos disaster have concluded that it was not purely a scheme to defraud investors or patients. Elizabeth Holmes aggressively sought venture capital and corporate partnerships, but she seems to have done so in the belief that she could, with enough time and money, eventually deliver on her technological promises. Money, in other words, was a motivator, but it was not *the* motivator. Holmes clearly wanted to get rich, but by all accounts she wanted to do it with technological innovation, rather than fraud. Similarly, although the desire to make money clearly motivated early radiation therapists, simple greed does not seem adequate to explain the willingness of individuals across the community to overlook the hazards of radiation exposure. As discussed in the first three chapters, the "golden rule" concept of physician-patient ethics meant that therapists accepted the same risks as their patients. It seems unlikely that Truman Abbe, for example, was operating principally out of greed when he gave himself the same radium exposure as he gave to his early patients.

A Context of Risk

Setting aside ignorance and greed opens the way to other explanations of radiation therapists' insensitivity to the risks of radiation exposure. One plausible explanation has to do with context. At more or less the same moment that x-ray and radium therapy appeared on the scene, surgeons were engaged in an increasingly vicious battle with cancer. Theory held that a skilled surgeon could cut cancer from the body, at least in instances of discreet tumors or diseased tissue. From the point of view of surgeons, moreover, the combination of new

[398] Nick Stockton, "Everything You Need to Know About the Theranos Saga So Far," *WIRED*, accessed October 9, 2016, https://www.wired.com/2016/05/everything-need-know-theranos-saga-far/; John Carreyrou, Michael Siconolfi, and Christopher Weaver, "Theranos Dealt Sharp Blow as Elizabeth Holmes Is Banned From Operating Labs," *Wall Street Journal*, July 8, 2016, sec. Business, http://www.wsj.com/articles/u-s-regulator-bans-theranos-ceo-elizabeth-holmes-from-operating-labs-for-two-years-1467956064.

anesthetic technologies and anti-/aseptic techniques had radically increased the scope of what they could accomplish on patients.

Cancer surgery, however, had a relatively low rate of success. All too often, malignancy would reappear beyond the removal area—often, right at the edges of the previous surgery. Predictably, some surgeons responded with a kind of medical scorched-earth policy: they cut more and more tissue from afflicted patients, amputating limbs well above the line of malignancy or cutting large chunks of tissue from around a cancerous area. Noting that cancer often seemed to travel along the lymphatic system and reappear in lymph nodes upstream of the original tumor, surgeons began to aggressively excise as many of the lymph nodes and as much lymphatic tissue as possible with ever more "radical" surgeries, often massively disfiguring their patients as they, for example, removed collarbones to get at the lymph nodes in the patient's upper torso. But even these measures proved insufficient; the prognosis for cancer patients, remained extremely poor.[399] In this context, as Deane Butcher pointed out in his article on experimental x-ray treatments for myoma, the lesser hazards of radiation therapy, such as dermatitis, though painful, still seemed like a reasonable tradeoff to both doctors and patients when compared with the prospect of a surgical "mutilation" that might not even arrest the disease.[400] In the context of cancer treatment, the comparison between the two therapies made the risks of radiation feel less scary and more acceptable.

It seems likely that radiation therapy benefited from comparisons with surgery in part because of the ability of patients to perceive the risks of surgery. X-ray dermatitis was, by all

[399]Emil Grubbé actually credited his approach of seeking immediate amputation whenever he found cancer—"the more radical, the better"—with allowing him to live for many years longer than most of his colleagues. Pioneering roentgenologist Elizabeth Fleischman exemplified the worst possible outcome, accepting, as a remedy for cancer on her finger, "amputation at the shoulder," with "the army and scapula, with the clavicle removed," only to have the cancer appear four months later at the line of amputation. Grubbé, *X-Ray Treatment: Its Origin, Birth, and Early History*, 84; Brown, *American Martyrs to Science through the Roentgen Rays*, 48; Siddhartha Mukherjee, *The Emperor of All Maladies: A Biography of Cancer* (New York: Scribner, 2010).
[400] Butcher, "The Intensive X-Ray Treatment of Myoma."

accounts, incredibly painful, but patients cannot see pain, and they have difficulty imagining it, in comparison to a visually striking result, like amputation. Moreover, the visual manifestations of x-ray damage most closely resembled sunburn, in the mild form, or an open sore, in more serious cases. Most people have experienced sunburns and had sores at some point in their lives. Though uncomfortable, neither of those conditions produces debilitating pain of the type described by dermatitis sufferers.

Economists describe the thought processes above, whereby a person underestimates negative consequences, with the term “discounting,” where the “cost” of an otherwise desirable action, like trying to remove cancer from the body, describes the negative side effects or results of the choice. When perceptual limitations make it difficult for humans to accurately assess costs, those costs get discounted. The inability of humans to perceive and account for risk grows sharpest when a temporal lag separates an action from its negative consequences, as happens with the worst cost of radiation exposure: future diseases, like cancer. Radiation treatment, in short, had the capacity to turn both therapists and patients into what are known as “hyperbolic discounters”—able in theory to recognize the savior-threat paradox but unable in practice to actually assess the risk. The most obvious example of hyperbolic discounting was the use of x-ray emitters for the cosmetic removal of body hair, but one encounters it repeatedly in the x-ray literature, as in the example presented in Chapter 2, of cold cathode users unwilling to use existing, obtainable equipment to deal with the problem of vacuum regulation.[401]

The Unhelpful Metaphor of Light

Another cause of discounting amongst radiation therapists was the combination of humans’ inability to perceive the real-time effects of radiation and our mental strategy for dealing with imperceptible phenomena. Simply put, humans can neither see nor feel ionizing

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[401] Herzig, “Removing Roots.”

radiation (even a person exposed to an immediately lethal dose of radiation will experience a short time lag before the effects begin to show). As a result, we have to think about and discuss radiation and its effects in abstract, metaphorical terms.

Light was an obvious metaphor for ionizing radiation. As discussed in Chapter 3, therapists explicitly linked radium to the sun (to the point of suggesting that the stellar core might actually be made of radium), but x-rays also received this treatment. Throughout the various materials associated with x-ray and radium therapy, a reader encounters one image over and over again: an angelic woman holding aloft a light source. Sometimes the angel bears a torch, as she does in the sales pamphlet for Radithor, a health tonic laced with radium. In x-ray publications, the angel usually lifts the glass bulb of a cold cathode x-ray emitter. Often, she was pictured in outer space, shining x-rays down on the globe of Earth. A few publishers dispensed with the angel; the *Archives of the Roentgen Ray* put orbs at the four corners of its cover, with a different kind of ray depicted from each source, with zig-zagging lightning bolts for electrotherapy, squiggles for radiotherapy, and rays of sunlight for phototherapy. For its promotional pamphlet, *The Romance of Radium*, the Radium Dial Company opted for a simple black background with a bright starburst at the center. The editor of the *American X-Ray Journal*, Heber Robarts, predictably saw no need to limit himself to choosing between the two approaches; the cover of his journal featured the angel, hovering in space with her Crookes' tube, surrounded by a collection of no less than ten oil lamps.

Light is, in many respects, the perfect stand-in for radiation, but insofar as therapists working with radiation already tended to discount its risks, the light metaphor may well have been catastrophic. To start with, the link between skin cancer and sunlight exposure remained unknown in 1895; insofar as early radiation workers understood the potential direct danger of sunlight, they were thinking about sunburns. Visually, a low-level x-ray or radium burn—an erythema dose—does look very much like a sunburn, with a reddening of the skin, often accompanied by tenderness, that fades fairly rapidly, over a period of days. But even though sunburns and low-level x-ray or radium burns have a similar presentation, the actual damage represented by the latter two events is an order of magnitude worse than the former, especially with regard to potential long-term damage and increased cancer risk. To describe erythema as similar to sunburn, as so many x-ray doctors did, cannot help but discount the risk of radiation exposure.

Moreover, the sunburn analogy gets at the trickiest part of understanding the risk from radiation: the problem of penetration. Simply put, sunlight only does damage at the surface level of the human body—it stops at the skin. A sunburn sufferer does not have to wonder whether he or she might also have been sterilized by that nap on the beach. And even though x-ray and radium users clearly understood that ionizing radiation could penetrate the body, they all too often seemed to lose sight of that fact when it came to thinking about the risks of radiation. A typical example of this split between understanding the penetrating nature of radiation and applying that knowledge to the danger of radiation was the myoma article discussed in Chapter 2, where no less an expert than the editor of the *The Archives of the Roentgen Ray* decried erythema as criminal while raising no objection to experiments intended to expose patients to many times the erythema dose internally.[402]

The most painfully obvious version of this extremely disconnect was in what radiologists did not write about: ways to narrowly control the radiation output of x-ray tubes, as discussed in Chapter 2. By far the most common uses of radiation therapy in the period covered by this study related to skin treatment, from cancer to hair removal. But that treatment necessarily exposed the recipient to more penetrating forms of ionizing radiation, since both radium samples and

[402] Butcher, "The Intensive X-Ray Treatment of Myoma."

cold cathode emitters produced a spectrum of radiation capable of higher and lower degrees of tissue penetration. It is a strange but unavoidable fact that many therapists—people who clearly cared about their patients, like Margaret Sharpe—wrote extensively about various methods of skin protection and its importance while more or less ignoring the issue of emitter output. Sunburn provided a deeply flawed way of understanding the threat of radiation, but at least it framed the issue in terms that therapists could appreciate. Unfortunately, it also sucked up all of the attention. Therapists did not conceive, and thus, sharply discounted, a type of damage for which they had no analogue, taking in places that they could not see, in favor of worrying about the problem to which they could relate.

Technological Optimism and Enthusiasm

The context of risk and the obscuring quality of light metaphors help to explain why therapists tended to discount the hazards of radiation exposure. In reading the words of early radiologists, however, another factor looms much larger than either of these: technological optimism and enthusiasm. When Heber Robarts sat down to write *Practical Radium*, he was already using it to treat his own x-ray induced cancer; Robarts, of all people, had every reason to believe in the hazards of radiation. The claims in the Preface of his book therefore come across as almost shocking. First, he assured readers that radium was an absolutely trustworthy therapeutic agent; "negative reports emanating from some persons pretending to use radium" should be rejected on their face. Moreover, Robarts promised his readers that, with radium exposure, "no injury is inflicted to normal cells, but, on the contrary, the cells are infused with renewed vigor, which assists in the separation of malignant tissue."[403] Similarly bizarre examples of this sort of thinking recur throughout the early radiology literature.[404] That Robarts would

[403] Robarts, *Practical Radium*, XII & 45.
[404] See, for example, Turner, *Radium, Its Physics and Therapeutics*, 138–39.

have ascribed therapeutic power to radiation made sense, given Robarts's own experiences with x-ray therapy, but to dismiss the dangers of radiation exposure, and describe it as an exclusively salubrious force, *while using it to treat x-ray induced cancer*, seems ridiculous to the point of absurdity.

Absurd, but not new. Readers who had followed Robarts during his x-ray years might have recalled his claim that x-rays could be used to cure "those diseases known by the profession as practically incurable," and that "with the x-rays, failures are not known and the cures are given without giving pain or leaving scars." Robarts spoke those words at a speech in December of 1900, by which time the problem of x-ray dermatitis had been well-aired in both the professional medical press and the newspapers. In other words, he did not so much develop an absurd enthusiasm for radium as perform an act of transmutation on his absurd enthusiasm for x-rays. Moreover, the language Robarts used to describe detractors—as discussed in Chapter 4, the "croakers," "wreckers," and so on—along with his over-the-top denunciations of writers and groups that expressed any pessimism towards x-ray technology made clear that Robarts viewed extreme enthusiasm in the defense of x-rays as no vice and pessimism in assessing the dangers of radiation as no virtue.

Robarts, moreover, clearly believed his own claims. He spent the remainder of his life using radium to treat the progressive cancer that would ultimately end his life, in 1922. Nor was Robarts alone in this sort of unadulterated enthusiasm towards radiation technologies. If anything, it was a quality that linked many of the pioneers in radiology. In the final years of his life, Robert Abbe carried on a correspondence with Marie Curie that can only be characterized as hero worship, culminating in his offer, graciously accepted by the French scientist, to name a pair of swans “Marie” and “Pierre.”[405] The pioneering roentgenologist Elizabeth Fleischman began her career when she and her husband, a doctor, “borrowed enough money to purchas x-ray apparatus, including a fluoroscope”; the two experimented, “with each other as subjects, oblivious to all else but its fascinating revelations,” until both developed acute dermatitis. Subsequently, “in spite of warning,” Fleischman opened up an x-ray practice specializing in dental photography; she died of cancer in 1905. Of Louis Andrew Weigel, who died in 1906, Percy Brown wrote, “in the midst of this labor . . . in the marvelous discovery to which he had become so ardently devoted, Weigel was beset by insidious physical reactions to which, at first, he paid but slight attention.”[406] As described in Chapter 1, the British roentgenologist William Webster responded to an x-ray burn on his hand by switching hands.[407]

Taken together, these examples suggest that early radiation workers allowed their enthusiasm and excitement to blind them to the threat written right in front of them, in their own bodies. Moreover, these optimists represented not the exception, but rather the rule in their community. When new owners took over *The American X-Ray Journal* in 1902, they proclaimed that “an x-ray apparatus is the most important implement of the doctor’s armamentarium,” and that “the therapy of the x-ray as predicted in 1896 has been fully realized, and in fact all the prophecies made by Dr. Robarts in the Announcement of the first journal have been fulfilled.”[408]

Optimistic Unto Death

The tendency towards optimism in communities organized around technological innovation is the product of fairly obvious factors. Working with new technologies inevitably

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[405] Portland ME Mosher Press, “In Memory of Dr. Robert Abbe 1851-1928” (Digitized by the Historical Medical Library of The College of Physicians of Philadelphia, 1929), http://www.cppdigitallibrary.org/admin/items/show/4263. Abbe was very, very excited about the swans.
[406] Brown, *American Martyrs to Science through the Roentgen Rays*, 48 & 58.
[407] Webster, “‘Practical X-Ray Work,’ a Paper Delivered to the Roentgen Society.”
[408] Hall, “Priority in X-Ray Journalism.”

entails frustration and difficulty. X-ray therapists spent hours tinkering with finicky and complex electrical apparatus. The first radium therapists paid small fortunes for samples the size of rice grains. Barriers to entry like these inevitably sort for the enthusiast and the optimist, because why else go to the trouble? Given a truly pessimistic outlook on the possibilities of a new technology, it makes more sense to do something else, rather than waste time in the pursuit of failure. Moreover, pessimism is no fun. Optimism and enthusiasm about technology are the emotional equivalents of venture capital—the optimist gets to enjoy the fruits of his or her labor, in terms of feeling good, prior to having actually succeeded. That advance on good feelings, in turn, can help to sustain the innovator through the difficult and often frustrating labor of actually making a new technology work. But optimism and enthusiasm, like other forms of capital, can also be misspent. It can insulate people from the real, painful costs of their decisions. In his own heart, Heber Robarts saw himself as the man who had helped to cure cancer. Heber Robarts was a man who died of cancer.

In many technological contexts, such misallocations have relatively little impact on the world. If a new video game works poorly, a few people lose their jobs, but probably none of them die. In medical innovation, however, the costs of misallocation are often paid in human suffering. Perhaps the single most useful insight to draw from the early history of radiation therapy is that we need more of the pessimism of Frank Hartman. Hartman's pessimism towards radiation did not prevent him from forwarding the cause of radiation therapy, nor did it prevent him from achieving success in his part of the field; he retired a well-to-do and respected member of the community. But Hartman's healthy fear of radiation motivated him to do things that benefited not just him, but numerous other radiation workers. They received his safety training, or saw one of his safety placards, or called him in to clean up a contaminated office. Hartman lived in very real fear, and so he lived.

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