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INFORMAL REPORT

THE RISE OF RADIATION PROTECTION: SCIENCE, MEDICINE AND TECHNOLOGY IN SOCIETY, 1896-1935

Daniel Paul Serwer

December 1976

BIOMEDICAL AND ENVIRONMENTAL ASSESSMENT DIVISION
NATIONAL CENTER FOR ANALYSIS OF ENERGY SYSTEMS

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Foreword

Before World War II, radiation protection was primarily of concern in medicine. Though there continue to be important problems arising from medical irradiation, in the post-War era the most prominent public concern with radiation protection has been energy-related. No technology has ever been subjected to closer scrutiny for possible harm than nuclear power. Only grudgingly are we realizing every source of electricity has dangers, and that a decision to build a coal-fired rather than a nuclear plant involves trade-offs between the risks associated with the entire coal fuel cycle (from mining to end-use) and the risks entailed by the entire uranium fuel cycle. Analysis of these energy-related risks is the prime responsibility of the Biomedical and Environmental Assessment Division of the National Center for Analysis of Energy Systems.

In carrying out these analyses, we consistently find that public perceptions vary widely as to awareness of the extent of risk. We have thus become interested in perception of risk by the public, a subject that raises social questions and value conflicts of the sort discussed in an historical mode in this informal report, which Mr. Serwer prepared at Princeton before coming to Brookhaven. Public concern, professional responsibility, interdisciplinary cooperation, and the activities of government and of the courts are pervasive themes wherever society in the 1970s confronts harmful consequences arising from science, medicine and technology. Similar themes arose soon after the discovery of X-rays and radium. This history is in some important respects a microcosm of current developments in areas far removed from radiation protection in medicine.

The absolute solution of public concern and perceived risks is impossible. This study sheds light on the mechanisms society uses to come to terms with the risks it creates for itself; we are distributing it in this preliminary form in the hope of disseminating that understanding further.

Leonard Hamilton

Abstract

The history of radiation protection before World War II is treated as a case study of interactions among science, medicine and technology. The fundamental concerns include the following:

a) How are medical and technical decisions with social impacts made under conditions of uncertainty?

b) How are social pressures brought to bear on the development of science, medicine and technology?

c) What does it mean for medicine or technology to be "scientific"?

d) Why do professional groups seek international cooperation?

e) What roles do various sorts of professionals and organizations play in controlling the harmful side effects of science, medicine and technology?

These questions are addressed in the specific context of protection from the biological effects of X-rays and radium in medical use.

In this context, science, medicine and technology are found to have interacted strongly with social concerns expressed primarily through the courts and the news media. Professional perceptions that medical radiology might be threatened are seen repeatedly to have motivated promotion of protection measures, and in the absence of such perceptions professionals did not proceed readily to limit the risks of radiation even when they themselves suffered much of the harm. Uncertainties concerning radiation protection

were often resolved under strong social pressures from within the profession and from the general public.

The links between medical radiology and scientific studies of radiation are found to have been multifold, and the application of scientific understanding to biological events is found to have been only one of a number of interactive mechanisms between medical radiology and scientific studies of radiation. Medical radiology until World War I used scientific discoveries without substantial input from scientific experiment or theory. This separation between laboratory and clinical approaches was critical to the history of medical radiology and of radiation protection. Only the War, and in particular its effects on the careers of a small group of German physicians and physicists, brought substantial applications of scientific knowledge in medical radiology, and even then practical considerations rather than scientific ones dictated key decisions on protection and dosage questions.

International cooperation on protection and measurement questions is found to have depended strongly on competitive, and often nationalist, rivalries. Physicists and physician-specialists played key roles in achieving by 1928 international standards for radiation protection, though there remained substantial differences in the ways in which laboratory and clinical research workers regarded protection matters.

Preface

This case study concerns the interactions among science, medicine, and technology within a social context. The particular branches of science and medicine in question, both at one time known as "radiology," originated in discoveries of the late nineteenth century: Röntgen's discovery in late 1895 of X-rays and the Curies' discovery in 1898 of the radioactive element radium. The science of radiology undertook experimental and theoretical investigations of X-rays and of radioactive substances within academic institutes and laboratories. Initially comprised of physicists and chemists, the scientific radiological community came later to include biologists as well. Such famous names as Rutherford, Curie and Becquerel contributed to this late nineteenth and early twentieth-century science of rays. Medical radiology applied X-rays and radium for diagnostic and therapeutic purposes. The primary instituional setting for both research and practice in medical radiology was the clinic, sometimes private and sometimes attached to a hospital or university.

The technology with which we shall be concerned serviced medical radiology, which required X-ray tubes, radium applicators, measuring instruments, protection devices, and a variety of auxiliary equipment. Scientists and physicians contributed to this clinical technology, but so too did a diverse group of skilled craftsmen. Before 1896, these craftsmen had been glassblowers, instrument makers, electricians, and mechanics; after 1896, they became X-ray equipment manufacturers. Only during World War I did X-ray and radium technology begin to rely heavily on the science of radiology and on academically trained professionals, and as a result the scientific and medical traditions would eventually be drawn into a single radiological community. Before this important development, however, the scientific and the medical radiological communities were largely separate.

Either scientific or medical radiology might merit historical treatment on its own. My interest, however, lies in their interactions with the industrially advanced societies that fostered their development before World War II: the United States and Western Europe, especially Britain, France, Germany and Austria. These interactions arose because X-rays and radium, in addition to their many medical benefits, also posed health risks. Radiation damage and radiation protection are not creatures of our post-World War II nuclear age. Both those responsible for applying X-rays and radium in the clinic and their patients suffered injuries, and as a result the biological effects of X-rays and radium have been the subject of public concern since shortly after their respective discoveries. Today's newspaper editorials on the risks of nuclear reactors had their counterparts fifty years ago, when editors were concerned with the risks of exposure to X-rays and radium in medical use. Even today, medical irradiation contributes much more than routine reactor discharges to the dose of radiation received by an average American. Though now overshadowed by other concerns in the public eye, radiation protection in medicine is still an important problem for science, medicine and technology.

The early, pre-World War II concern with radiation protection in medicine arose from the discovery during the four decades following 1896 that X-rays and radium had a variety of acute and long-term biological effects. Let me offer here an outline of major events that I shall discuss in detail in later chapters. In 1896, within a few months of Röntgen's discovery of X-rays, it became widely known that exposure to the X-ray tube caused human hair to fall out (epilation), reddened and inflamed the skin (erythema), and could also cause more severe skin irritations (dermatitis). In 1902 and 1903, several other less obvious effects were reported: X-rays and radium caused sterility in both males and females; they caused changes in the blood and blood-forming organs; and they induced cancer. In the two or three years before World War I, the effects on the blood and blood-forming organs were found to lead to leukemia and to a sometimes fatal pernicious anemia, but general recognition of these consequences did not come until around 1920. All of these biological effects were the results of exposure to X-rays and radium in medical use, and I shall be largely concerned with tracing the consequences of these clinical discoveries. In addition, radiation-induced genetic mutations were demonstrated for the first time in 1927, under laboratory conditions, and between 1925 and 1930 the health effects of radioactive materials in industry began to arouse concern. By 1930, radium and mesothorium (a radium isotope) had been shown to cause inflammation of the bone (osteitis) and bone sarcoma in workers exposed while painting luminescent watch dials with radium paint. Radon, a gas produced by the radioactive decay of radium, was strongly suspected by 1930 of causing lung cancer in arsenic and uranium miners exposed in the course of their work. A monument to X-ray and radium victims of all countries carried 169 names when it was dedicated in Hamburg in 1936.[1] Many others remained anonymous.

As a result of these discoveries of radiation effects, a series of national and international institutions concerned with radiation protection developed during the first four decades of the twentieth century. One of my primary concerns will be to relate the discovery of radiation effects to the evolution of these institutions. Radiation protection recommendations first began appearing in the medical radiological literature around 1902. Before World War I, X-ray protection and X-ray measurement, a closely related topic, had become continuing concerns of the German Röntgen Society, which played a leadership role in this area as it did in medical radiology as a whole. The Germans issued their first formal set of protection guidelines in 1913, a precedent that the British Röntgen Society followed in 1915. After World War I, concern with radium as well as X-ray protection grew rapidly and led to the establishment of several national protection committees in the early 1920s. In 1925, the first International Congress of Radiology created an International Commission on X-ray Units, and in 1928 the second such Congress created an International Commission on X-ray and Radium Protection. By 1935, the terminal date for the present study, the International X-ray and Radium Protection Commission had explicitly adopted as the basis of its X-ray protection recommendations the tolerance dose, which was thought to be a dose below which no harmful effects would occur.

The institutions concerned with radiation protection before World War II were largely professional institutions, not governmental ones. It was the scientific and medical radiological communities, not administrative or legal actions, that promulgated the protection recommendations. The history of radiation protection before World War II thus offers an opportunity to study how professional mechanisms work in the absence of positive intervention by government authorities (though with the threat of their intervention often present). It might be possible to view these mechanisms as part of the self-regulatory behavior to be expected of a profession, but as we shall see their operation depended heavily on what the medical radiological community viewed as threats from the broader society conveyed through the courts, the news media, and insurance companies. Unfortunately, I shall not be able to offer a full history of the popular reaction to X-rays, radium and the injuries they caused, but I shall trace at certain critical junctures professional perceptions of how the public viewed medical radiology and I shall show how these professional perceptions provided a compelling motive for radiation protection measures.

The results of this effort to delineate interactions between professional behavior and social demands cannot be translated unthinkingly to today's problems. Though I shall argue that radiation protection in the past was in large part a professional response to public pressure, it would be a mistake to conclude that I believe radiation protection could or should return to this pattern. At the same time, I believe that many of the mechanisms to be discussed are still operative, and that the reliance on government authorities changes form more than substance. To be sure, government authorities today take the final decisions on radiation protection standards. More often than not, however, these decisions are the culmination of a process that has reached far beyond the narrow circles of those formally responsible. That wider process relies on professional mechanisms and public pressures strikingly similar to those that existed before government authorities became involved. It is reasonable, I think, to suggest that governmental involvement has not removed decisions on complex medical and technical issues from the tug and pull of professional-public interactions, but that the role of government is to absorb the blows from each side and thereby prevent the adversaries from doing each other serious harm.

Whatever the role of government, we still rely on nongovernmental mechanisms more than we are generally aware, not only in radiation protection but also in other areas where modern science, medicine and technology pose serious risks to society. The closest parallels to the problems that radiation posed in the past lie among today's concerns with the safety and efficacy of drugs, with the hazards of food additives, with the effects of occupational exposure to industrial and agricultural chemicals, and with the effects of non-occupational exposure to environmental chemicals. Looser, but still substantial analogies can be drawn with other instances where science-based technology and medicine pose social risks of uncertain magnitude. In tracing the rise of radiation protection through international agreement among professionals on the tolerance dose, I hope in part to bring current events into a deeper perspective. In this perspective, social control over modern medicine and technology no longer looks utopian. It looks instead like a harsh reality with which we have been living for a long time, and from which we cannot escape. The important questions for the future are "which parts of society should be involved?" and "how much control is adequate?"

In undertaking the present case study, I have been guided by a number of fundamental assumptions concerning modern science and its interactions with medicine, technology and society. Medicine and technology are today often assumed to be scientific endeavors, and the application of scientific knowledge is considered routine. By contrast, I have assumed that science and medicine are socially distinct institutions, that they in some important respects utilize different criteria in coming to conclusions, and that the application of scientific knowledge or the use of scientific methods in medicine (and in technology) is therefore a complex process involving different community perceptions and standards.[2] I shall distinguish between the scientific and medical radiological communities not on the basis of stated intentions of individual authors, but rather on the basis of occupational roles, institutional affiliations, and journals utilized.[3] One might also think of using the different citation patterns that Price has claimed characterize science and technology, on the assumption that medicine might resemble technology more than science. I suspect, however, that the medical radiological literature would show the cumulative pattern and exponential growth that Price views as characterizing scientific publication from a very early date, as it does today.[4] I agree with Price's basic point--that technology (and, I believe, medicine) should be presumed independent of science until proved otherwise--but one must recognize that a nonscientific community, thinking of itself as scientific, may put a good deal of effort into citing the contemporary scientific literature and into copying the scientists' citation pattern.

With the notions of "scientific" medicine and "scientific" technology often comes the assumption that medical and technical decisions can be made on a "scientific" basis. I assume that medical and technical decisions with social impacts are usually made under conditions of uncertainty, and that many factors besides reason can therefore enter into consideration. Accordingly, I do not treat science, medicine and technology as institutions entirely separate from the rest of society. One need not doubt the dedication of a profession or of its practitioners to lofty goals in order to recognize that professional institutions and their members are interacting continuously with social pressures. I assume that these pressures can affect not only the status and prerogatives of the profession, but also its intellectual development. One especially important aspect of the intellectual development of a discipline is its interactions with other disciplines, and I shall assume that social pressures as well as the inherent character of the subject matter can play a major role in the formation of interdisciplinary endeavors. Another important aspect of intellectual development within a discipline is its international character. Here, too, I shall assume that more than the subject matter is involved, and that international cooperation depends on social pressures both within and outside a profession.

The degree to which I can demonstrate the validity of these assumptions even within the narrow confines of radiation protection is limited, but I ask the reader's indulgence in suspending his disbelief and in following the detailed story of this very small segment of modern history. I am convinced that in the present state of the historiography case studies can shed more light on such basic issues than the more usual broad-ranging discussions under the rubrics "science and society" or "science and politics." The single most dramatic case of interactions between science and society is surely the atomic bomb, but for all the vast literature on this subject I think it has been singularly unproductive of interesting insights. Of other specialized studies, I would cite James Whorton's Before Silent Spring and Richard French's Antivivisection and Medical Science in Victorian Society as two that point in interesting directions.[5] Neither, however, tries hard enough to draw general implications from the particular cases. Such studies will, in my view, be more fruitful the more they are able to keep broader questions in focus while delving into the complexities of a special case. The task, as I know only too well, is not an easy one.

Let me state the bare outlines of the story that follows. In Chapter 1, I describe the separate development of scientific and medical radiology in the first few years after 1896. Radium will be introduced only briefly, and in the next two chapters it will play a contrapuntal role. In Chapter 2, I shall discuss the initial reactions to the discovery that exposure to the X-ray tube had biological effects, and in the process the substantial effects of public concern on professional behavior will become apparent. In Chapter 3, the first decade of radiation protection as a professional concern will bring to the fore the issue of physician control over radiological practice and the issues of protection and measurement techniques in the clinic. Pre-war developments in science that would later be important to medical radiology will be discussed in Chapter 4, which will also describe in general terms the impact of the War on medical radiology. Only in Chapter 5 will the science of radiology, partly as a result of World War I, contribute substantially to the medical applications of X-rays. Also in Chapter 5, radium protection will come to be treated on a par with X-ray protection. Radiation measurement and protection will become subjects of international cooperation in Chapter 6, but only after a period of intense nationalism. Physicists, and a newly evolving group of physician-specialists, will be critical to this development. Chapter 7 in the current text is merely a preliminary sketch of questions to be dealt with more fully in the future. It will reconsider a key theme emerging from the earlier chapters, namely the separation between the laboratory and the clinic, in the context of radiation-induced mutation. It will also discuss the relationship between this discovery and contemporary politics, which had a great deal to do with the way in which uncertainty over the

importance of genetic effects was resolved.

This study is for me an effort to fuse two strikingly separate worlds. During the past eight years, I have divided my time between academic pursuits and policy oriented research with international organizations. I would like to acknowledge the kind assistance of those who have made this fusion possible: Thomas Kuhn, for his continuing criticism and patience; Alexander Hollaender, Oscar Schacter, Francesco Sella, David Sowby and Peter Thacher, for stimulating discussions of present and future as well as past problems; Theodore Brown, Charles Gillispie, Thomas Howe, Geoffrey Kabat and Jerome Ravetz, for their careful readings of parts of this text and for their comments. The Danforth and Rockefeller Foundations not only provided essential financial assistance, but through Lillie Mae Rose and Elmore Jackson, respectively, offered flexibility and understanding.

Many individuals provided specific assistance and materials, including Elof Axel Carlson of the State University of New York at Stony Brook, Antoinette Béclère of the Centre Antoine Béclère, Dipl.-Ing. H. Graf, A. Hilpert of the Fachnormenausschuss Radiologie im Deutschen Normenausschuss, Ernst Streller of the Deutsches Röntgen-Museum, Lauriston Taylor of the National Council on Radiation Protection and Measurements, and an anonymous employee of the Mallinckrodt Chemical Works.

The British Institute of Radiology and the American Institute of Physics offered welcome hospitality as well as the use of their facilities. The largely anonymous staffs of the Science and Technology

Research Center at the New York Public Library and of the Library of the New York Academy of Medicine have struggled hard to find the all too often misplaced volumes listed in their superb collections. Stephen Carey of the Academy merits special thanks for his efforts. Bless the benefactor who will give the New York Public and the Academy the means they merit!

Valuable comments and opportunities to sharpen many points have resulted from presentations of this research in various stages of preparation at the MIT Technology Studies Program, at the Biomedical and Environmental Assessment Group of Brookhaven National Laboratory, at the History of Science Department of Harvard University, and at the History and Philosophy of Science Program of Princeton University. To Eugene Skolnikoff, Nathan Sivin, Leonard Hamilton, Everett Mendelsohn, and to all those who were amused and bemused, my thanks.

1. Hans Meyer, ed., Ehrenbuch der Röntgenologen und Radiologen aller Nationen, Sonderband 22 zu Strahlenth. (Berlin/Wien: Urban and Schwarzenberg, 1937).

2. For a general presentation of the view that science and technology are different sorts of intellectual activities governed by different criteria of belief, see Edwin Layton, "Technology as Knowledge," Tech. Cult., 15 (1974) 31-41; for the opposite view, see Jerome R. Ravetz, Scientific Knowledge and its Social Problems (Oxford: Clarendon Press, 1971). For some comments on the analogous view of science and medicine, see O. Temkin, "Basic Science, Medicine and the Romantic Era," Bull. Hist. Med., 37 (1963) 97-129.

3. In order to make the identification of occupational roles and institutional affiliations easier, I shall include in the footnotes in parentheses after the author's name whatever degrees, titles or affiliations are given in the reference in question. Physicians will generally be identifiable from this information. Of the nonphysicians, members of the scientific community will often be readily identifiable, but there will remain a group of nonphysicians who are difficult to place. Most of them belonged to the technological part of the medical radiological community. They included a variety of skilled craftsmen as well as a number of people with professional credentials in electrical engineering.

4. Derek J. de Sola Price, "Citation Measures of Hard Science, Soft Science, Technology and Nonscience" in Carnot E. Nelson and Donald K. Pollock, Communication Among Scientists and Engineers (Lexington, Massachusetts: D. C. Heath, 1970). For the conformity of the current medical radiological literature to Price's "scientific" pattern, see E. R. N. Grigg, "Information Science and American Radiology," Radiol. Tech., 41 (1969) 19-30.

5. James Whorton, Before Silent Spring: Pesticides and Public Health in Pre-DDT America (Princeton, N.J.: Princeton University Press, 1974) and Richard French, Antivivisection and Medical Science in Victorian Society (Princeton, N.J.: Princeton University Press, 1975).

List of Abbreviations

Acta Radiol.	Acta Radiologica
Amer. J. Cancer	American Journal of Cancer
Amer. J. Phys.	American Journal of Physics
Amer. J. Rönt.	American Journal of Röntgenology
Amer. J. Sci.	American Journal of Science
Amer. J. Surg.	American Journal of Surgery
Amer. X-Ray J.	American X-Ray Journal
Ann. Electro.	Annales d'Électrobiologie, d'Électrotherapie et d'Électrodiagnostic
Ann. Med. Leg.	Annales de Médicine Légale de Criminologie et de Police Scientifique
Ann. Phys.	Annalen der Physik
Arch. Derm. Syph.	Archiv für Dermatologie und Syphilis
Arch. Elec. Med.	Archives d'Électricité Médicale
Arch. Hist. Ex. Sci.	Archive for the History of the Exact Sciences
Arch. Int. Hist. Sci.	Archives Internationales d'Histoire des Sciences
Arch. Med. Exp.	Archives de Médicine Expérimentale
Arch. Mikro. Anat.	Archiv für Mikroskopische Anatomie
Arch. Pathol.	Archives of Pathology
Arch. Physiol. Mensch. Thier.	Archiv für die gesammte Physiologie des Menschen und der Thiere
Arch. Radiol. Electroth.	Archives of Radiology and Electrotherapy
Arch. Rönt. Ray	Archives of the Röntgen Ray
Ber. Deut. Chem. Ges.	Berichte der Deutsche Chemische Gesellschaft
Beit. Klin. Chir.	Beiträge zur Klinischen Chirurgie
Berlin. Klin. Wschr.	Berliner Klinische Wochenschrift
Biochem. Z.	Biochemische Zeitschrift

Bost. Med. Surg. J.	Boston Medical and Surgical Journal
Brit. J. Hist. Sci.	British Journal of the History of Science
Brit. J. Radiol.	British Journal of Radiology
Brit. Med. J.	British Medical Journal
Bull. Acad. Med. (Paris)	Bulletin de l'Académie de la Médicine (Paris)
Bull. Ass. Franc. Cancer	Bulletin de l'Association Française pour l'Étude de Cancer
Bull. Hist. Med.	Bulletin of the History of Medicine
Bull. Off. Soc. Franc. Electroth. Radiol.	Bulletin Officiel de la Société Française d'Électrotherapie et de Radiologie
Bull. Soc. Radiol. Med. (Paris)	Bulletin et mémoires de la Société de Radiologie Médicale de Paris
C. R. Acad. Sci. (Paris)	Comptes Rendus (Académie des Sciences, Paris)
C. R. Soc. Biol. (Paris)	Comptes Rendus (Société de Biologie, Paris)
Deut. Med. Wschr.	Deutsche Medizinische Wochenscrift
Deut. Zeit. Ges. Gericht. Med.	Deutsche Zeitschrift für die Gesammte Gerichtliche Medizin
DIN-Mitt.	DIN-Mitteillungen
Elec. Eng.	The Electrical Engineer
Fortschr. Röntgenstr.	Fortschritte auf dem Gebiete der Röntgenstrahlen
Gaz. Hop.	Gazette des Hôpitaux
Gen. Elec. Rev.	General Electric Review
Health Phys.	Health Physics
Hist. Stud. Phys. Sci.	Historical Studies in the Physical Sciences
Inter. Ass.	International Associations
J. Amer. Med. Ass.	Journal of the American Medical Association
J. Cancer Res.	Journal of Cancer Research
J. Hist. Biol.	Journal of the History of Biology
J. Hist. Med.	Journal of the History of Medicine

J. Radiol. Electrol.	Journal de Radiologie et d'Électrologie
J. Rönt. Soc.	Journal of the Röntgen Society
Lancet	The Lancet
Lyon Med.	Lyon Médicale
Med. News (N. Y.)	The Medical News (New York)
Med. Press	The Medical Press
Monat. Prak. Derm.	Monatsheft für Praktische Dermatologie
Munchen. Med. Wschr.	Münchener Medizinische Wochenschrift
Nach. Ges. Wiss. (Göttingen)	Nachrichten von der Gesellschaft der Wissenschaften zu Göttingen
Naturwiss.	Die Naturwissenschaften
Nat. Phil.	The Natural Philosopher
N. Y. Med. J.	New York Medical Journal
Paris Med.	Paris Médicale
Pers. Amer. Hist.	Perspectives in American History
Phila. Med. J.	Philadelphia Medical Journal
Phil. Mag.	Philosophical Magazine
Phil. Trans.	Philosophical Transactions
Phys. Rev.	Physical Review
Phys. Z.	Physikalische Zeitschrift
Presse Med.	La Presse Médicale
Proc. Camb. Phil. Soc.	Proceedings of the Cambridge Philosophical Society
Proc. Nat. Acad. Sci.	Proceedings of the (U. S.) National Academy of Sciences
Proc. Phys. Sci.	Proceedings of the Physical Society
Proc. Roy. Soc.	Proceedings of the Royal Society
Proc. Roy. Soc. Med.	Proceedings of the Royal Society of Medicine
Public Health Repts.	Public Health Reports

Radiol. Med.	La Radiological Medica
Radiol. Tech.	Radiologic Technology
Radium	Le Radium
Radium Biol. Heilk.	Radium Biologie und Heilkunde
Sitzungsber. Akad. Wiss. (Wien)	Sitzungsberichte der Kaiserlichen Akademie der Wissenschaften (Wien)
Sitzungsber. Akad. Wiss. (Heidelberg)	Sitzungsberichte der Heidellberger Akademie der Wissenschaften
Sitzungsber. Phys. Med. Ges. (Würzburg)	Sitzungsberichte der Physikalisch-Medicinische Gesellschaft zu Würzburg
Strahlenth.	Strahlentherapie
Tech. Cult.	Technology and Culture
Trans. Amer. Inst. Elec. Eng.	Transactions of the American Institute of Electrical Engineers
Trans. Amer. Rönt. Ray Soc.	Transactions of the American Röntgen Ray Society
Trans. Amer. Surg. Ass.	Transactions of the American Surgical Association
Verh. Deut. Rönt. Ges.	Verhandlungen der Deutsche Röntgengesellschaft
Verh. Ges. Deut. Naturf. Artz.	Verhandlungen der Gesellschaft Deutsche Naturforscher und Ärtze
Verh. Phys. Med. Ges. (Würzburg)	Verhandlungen der Physikalisch-Medicinische Gesellschaft zu Würzburg
Wien. Klin. Rund.	Wiener Klinische Rundschau
Wien. Klin. Wschr.	Wiener Klinische Wochenschrift
Z. Induk. Abst. Vereb.	Zeitschrift für Induktive Abstammungs- und Vererbungslehre
Z. Phys.	Zeitschrift für Physik
Z. Röntk.	Zeitschrift für Röntgenkunde
Z. Tech. Phys.	Zeitschrift für Technische Physik

Chapter 1: Introduction

Wilhelm Conrad Röntgen's discovery of X-rays is an archetypal story of modern science: the lone research worker in an esoteric field, the observation of a phenomenon others had missed, the feverish weeks of experimentation, the rush to publication, the enormous potential for applications beyond the narrow sphere in which the discovery originated, and the widespread public enthusiasm. This classic story has often been told, and I need only recall a few salient points.[1] Röntgen, a professor of physics at the University of Würzburg, had been experimenting with cathode rays, which were produced by a high voltage electric discharge at the negative pole (or cathode) of a partially evacuated glass bulb or tube. The precise nature of cathode rays, which today we regard as electrons, was then in dispute. The designation "ray" merely implied that whatever their nature, cathode rays propagated in straight lines, like the rays of light. Philipp Lenard, a young physicist at Bonn, had found that he could make cathode rays pass through a thin metal foil inserted like a window into the glass wall of the discharge tube.[2] Thus alerted to the possibility of rays outside the tube, Röntgen late in 1895 found much more penetrating rays than those Lenard had observed. These new rays originated wherever the cathode rays struck the wall of the discharge tube regardless of whether it was equipped with a foil window. Like cathode rays. Röntgen's new rays exposed photographic plates and could not be reflected or refracted, but unlike cathode rays they could also not be deflected by a magnet and their absorption in matter did not appear to depend solely on density.

The apparatus required to produce X-rays was widely available in the 1890s, and the equipment used by Röntgen differed in only small ways from that of his predecessors and successors. Central to the apparatus was the discharge tube, which was an evacuated glass bulb with metal electrodes sealed into its walls. Glass-blowers had been making such tubes for physicists since the 1850s. Röntgen had purchased some of his tubes commercially, and both scientific and medical radiology soon depended heavily on commercial tube manufacturers. Röntgen permitted his cathode rays to strike the glass wall of the tube; shortly after his discovery, it became standard to allow them to strike a metal target (the anticathode) placed in the center of the tube, a procedure that produced a higher intensity of X-rays. To evacuate his tube, Röntgen used a self-acting version of a common vacuum pump in which liquid mercury falling repeatedly in a tube with a closed end created a vacuum. Röntgen kept the tube attached to the pump in order to maintain a sufficiently high vacuum, but this procedure soon became unnecessary. The vacuum could be made high enough, and the seals tight enough, to permit a tube to be evacuated in advance by the manufacturer. Variations in the vacuum, however, strongly affected the output of X-rays. As a tube warmed up, the vacuum generally became worse (as the occluded gas was released from the electrodes and the walls) and then better, and over its lifetime a tube gradually became so highly evacuated that it could no longer be excited. Until the invention of the Coolidge tube, which I shall describe in Chapter 4, a good deal of skill and ingenuity were applied to inventing devices for keeping the vacuum within workable limits. To excite his discharge tube, Röntgen used a Ruhmkorff coil, which was one of the standard induction coils of the day; it continued in use for more than a decade after 1896. This coil converted a low-voltage current of about 20 amperes to a much higher voltage current of several milliamperes. Röntgen might also have used, as some of his successors did, a generator of static electricity (an influence machine) to produce the high-voltage electric discharge. Both Ruhmkorff coils and induction coils were readily available from electrical equipment manufacturers.

Thus there was nothing unique about Röntgen's equipment, and it would soon become even more readily available than it had been before 1896. Röntgen submitted a paper for publication describing his findings in late 1895.[3] On New Year's Day of 1896, he sent reprints of this "preliminary communication" to colleagues, enclosing with some of these reprints photographs demonstrating the ability of X-rays to penetrate matter. The most spectacular of the photographs showed the bones of a hand; such photographs would soon become as well known to the world of the 1890s as photographs of the earth taken from space were to the 1960s. One of the reprints and a set of photographs fell into the hands of a Viennese newspaperman who wrote an account of the discovery that spread rapidly from Vienna to London, New York, Paris, and from there to the rest of the world. Before the end of the first week of 1896 the news of X-rays was known throughout Europe and the United States.

Röntgen has been celebrated ever since his discovery for his contribution to medicine as well as to physics. Medical interest in

X-rays lay initially in their diagnostic potential, which had been discussed even in the first newspaper reports of the discovery. Within a few weeks, both physicians and nonphysicians reported numerous diagnostic successes. From swallowed pins and pennies attention shifted quickly to embedded bullets, broken limbs, kidney stones, and soft tissues, some of which were readily made visible by the injection of opaque substances. A large number of examinations was undertaken to determine what a given pathological condition might look like in the sometimes deceptive shadow images that X-rays projected on to a photographic plate (radiography) or on to a fluorescent screen (radioscopy). The demand for refinements in technique was met with a flood of inventions. Contrast media, devices for taking stereoscopic pictures, localization techniques for foreign bodies, regulators to control the vacuum in the X-ray tube, and improved X-ray plates and films were often invented independently in several different places. The reason for simultaneous discovery was not a common research tradition but rather a common set of technological capabilities. New techniques developed from scattered beginnings, small improvement by small improvement. With hundreds of people contributing, overall progress was rapid.[4]

The relationship between these early developments in diagnostic technique and contemporary physical knowledge raises issues of general interest. It is often assumed that a technique based on scientific discovery necessarily relies heavily on scientific knowledge, and that such a technique thereby offers the certainty in its results that is associated with the underlying science. There are, of course, techniques that do rely heavily on science in both medicine and technology: the reliance of immunology on bacteriology, and of transistor technology on solid state physics, may serve as examples. This pattern is not, however, a necessary one. X-rays were unquestionably "scientific" in pedigree; they were discovered by an academic physicist working within a scholarly tradition of experimentation with cathode ray tubes. Röntgen, however, made medical radiology possible without contributing further to it. He and his colleagues in physical laboratories generally left the development of medical applications to others, and the relatively few technical innovations the physicists offered did not usually rely on the specialized knowledge of their discipline. For more than two decades, advances in diagnostic radiology were more often the result of *bricolage* than of applied science.[5]

Similarly, the discovery that exposure to an X-ray tube could cause "physiological" effects, as the effects we would now term "biological" were then called, was independent of both physical and biological science. In the first few months of 1896, several investigators, thinking that X-rays resembled ultraviolet light, had anticipated that they might affect bacteria. The experiments undertaken to test this expectation yielded negative results. X-rays did not appear to kill bacteria, as had been hoped, or even to limit their growth.[6] At about the same time that this disappointment was becoming apparent, the first reports of other, unanticipated, biological effects began to appear. A number of diagnostic practitioners reported that patients were suffering from loss of hair (epilation), reddening and inflammation of the skin (usually termed "erythema"), and a more severe dermatitis resembling a third degree burn.[7] Both practitioners and patients were surprised that the effects often appeared after a delay, sometimes of hours and sometimes of days or weeks, but the appearance of epilation, erythema and dermatitis on parts of the body that had been close to the X-ray tube during irradiation made it clear that the tube was in some way responsible. Thus it was clinical accident, and not scientific knowledge, that first brought the biological effects of exposure to the X-ray tube to light.

Dermatologists, already emerging as a specialty group within medicine, seized this discovery as a promising tool and applied the X-ray tube to the treatment of a wide variety of skin diseases. Sometimes the X-ray tube worked, especially in conditions such as eczema, lupus vulgaris (tuberculosis of the skin), and hypertrichosis (excessive growth of hair).[8] The success in the treatment of lupus vulgaris, which was known to be caused by the tubercle bacillus, stood in direct contradiction to what little was known from experiments about the effect of X-rays on bacteria. The contradiction prompted further experimentation, but it did not slow the therapeutic use of the X-ray tube in cases of lupus vulgaris and other bacterial skin diseases.[9] By 1900, success with rodent ulcer, a tumor of epithelial cells, had been reported.[10] With the shift in medical and popular attention around the turn of the century from tuberculosis to cancer, X-rays took on greater promise in both the professional and the public mind.[11] The possibility of a cancer cure was to remain a major factor in the growth of biological and medical work with X-rays, and later with radium.

Progress in X-ray therapy, as in diagnosis, was largely the result of small contributions, many of which were made independently in several places. I shall discuss in Chapter 2 some of the thinking behind these efforts, but it would be a mistake to put too much weight on theory or experiment in the early development of X-ray therapy. Trial and error, only occasionally guided by conceptions of the nature of X-rays and their interactions with biological material, was the basic technique. The standard that prevailed among practitioners was not intelligibility, but effectiveness. It mattered little whether the biological effects of the X-ray tube could be understood. If the treatment worked, it could be used, regardless of the state of the related scientific research. The subject of treatment was the patient, and the results were reported as case histories. A physician might append a few words at the end of an article speculating that the effects of X-rays on biological material were similar to their effects on the photographic plate, and therefore chemical or "actinic" in character. Or he might guess that the biological effects were essentially trophoneurotic, affecting the electrical condition of the nervous system first and only secondarily causing damage to other tissues.[12] But on the whole these speculations remained at a very general level, often testifying more to the assumption that biological facts could be explained in physical terms than to any continuing interest in unraveling the detailed mechanisms at work.

Why was scientific input into medical radiology so meager? Especially in Germany, where there was a long tradition of close connections between science and medicine, practical applications of physical and chemical knowledge might be expected. The annual Conferences of German Scientists and Physicians had been meeting since 1822, and Röntgen's "preliminary communication" had been published in the Proceedings of the Physical-Medical Society of Würzburg, a society devoted to invigorating medicine with scientific methods.[13] The explanation for the paucity of applied science even in this science-based branch of medicine is two-fold: neither biology nor physics had much to offer in these early years of the use of X-rays in medicine, and medicine could not make ready use of the limited scientific knowledge available. X-rays had taken everyone by surprise.

The physicist faced three interrelated problems: the nature of the X-rays; the processes generating them in a discharge tube; and the mechanism of the interaction of X-rays with matter. In 1897 and 1898 a theory treating these problems emerged that by 1900 achieved widespread acceptance among physicists. This theory treated the X-rays as electromagnetic pulses generated by the deceleration of cathode rays, which were by then viewed as streams of negatively charged particles. If in passing through matter, X-rays interacted primarily with elastically bound electrons, such pulses might be very penetrating and also cause the known emission of secondary X-rays. This pulse theory was occasionally challenged by those physicists who believed X-rays to be particles arising from the discharge of cathode rays when they struck a solid body, and in 1907 the English physicist William Henry Bragg would precipitate a major controversy over the nature of X-rays (and the gamma rays of radium) with the claim that they were particles rather than pulses. The pulse theory, however, remained dominant until at least 1912.[14]

Productive though it was of physical experiment and discovery, the pulse theory had little to offer medical practitioners. Even if they understood the theory, as on the whole they did not, those who were using X-rays for therapy and diagnosis had no means of linking their biological materials with the physicist's picture of matter. Not until well after 1900 did research on the biochemical effects of radiation prove fruitful. The colloidal aggregate theory of proteins would eventually provide a bridge between the physical process of ionization and the observed biological effects. In the years before 1900, however, medical use of X-rays established itself without any firm link to scientific theory and experiment. Biology and physics took longer to make something useful of this discovery than did medicine, which put it to work almost immediately.

Similarly, radium had been put to use in medicine without relying on scientific knowledge soon after its discovery in 1898. Pierre and Marie Curie made their discovery of this radioactive element while trying to find the reason for the surprisingly intense emission from the uranium ore pitchblende of rays similar to X-rays.[15] Physicists would continue to dispute whether the rays emitted by radium were identical to X-rays, but physicians were already referring by 1900 to the "radioactivity" of the X-ray tube. Both newly discovered phenomena affected photographic plates, caused fluorescence, and ionized air. It was a small step to imagine that radium, like the X-ray tube, might have biological effects. Once sufficient quantities of radium became available, it was another small step to the relevant trials. A series of reports in 1900 and 1901 put the matter beyond doubt: radium, like the X-ray tube, could "burn."[16] Radium was then quickly applied in therapy, beginning as X-rays had with dermatological ailments.

Medical interest in radium increased further with the discovery that a radioactive gas, known today as radon but then called "radium emanation," was present in the atmosphere, in soil, and in mineral springs.[17] In Continental Europe, mineral springs were frequently used in therapy, and it appeared reasonable to suggest that the curative effects of drinking and bathing these waters might depend on the presence of radium emanation. The measurement of the emanation dissolved in mineral waters soon became a minor outdoor sport, with wide surveys conducted in France, Germany and Austria.

Radium still posed fundamental problems for both physicists and chemists. The notion that its radioactivity arose from the transmutation of one element into another, a notion that we today regard as true, was put forward around the turn of the century, but the Curies continued to believe in the theory that had led them to use the term "radioactive."[18] This theory held that there were highly penetrating rays throughout space to which certain elements, the radioactive ones, were sensitive, and from which they could extract energy that was re-emitted as Becquerel rays. Debate on this fundamental scientific problem, however, had no discernible bearing on the medical uses of radium and radium emanation, which developed apace on the basis of clinical trials.

Despite their initial lack of grounding in contemporary science, X-ray diagnosis and X-ray and radium therapy were clinical successes and spread rapidly. Well before 1900, diagnostic X-rays were prerequisite to many surgical procedures.[19] By early 1901, one American hospital had made 8000 diagnostic X-ray exposures in 3000 cases.[20] I have unfortunately been unable to find any contemporary data on the number of X-ray practitioners, but I would guess that by 1900 there were at least several hundred diagnostic X-ray installations in each of the countries of primary concern, namely Austria, England, France, Germany and the United States. X-ray and radium therapy were less widely used, but the success of X-ray therapy in treating a number of dermatological ailments was well established by 1900, and radium emanation during the first few years after the turn of the century was beginning to be administered by bathing, drinking and inhalation at health spas. X-ray installations appear to have been especially common, as one would expect, in Röntgen's Germany; in the Curie's France, radium was relatively more important to medical radiology.

Medicine is in part an economic pursuit, and neither the legitimate practitioners nor the considerable number of quacks could have survived, much less thrive as they did, without public support. An initial burst of public enthusiasm, and continuing public interest, encouraged the adoption of medical techniques that required investments, albeit modest ones, in specialized equipment.[21] New clinics, within hospitals and outside, grew up quickly. Specialists, alert to new techniques that could establish their competitive edge over the general practitioners more firmly, were quick to install X-ray apparatus, and health spas vigorously advertised the radium content of their waters. To appreciate the impact of the negative public reaction that I discuss below, it is important to realize that acceptance of X-rays and radium was very rapid. The vested interest in their continuing use, though small in economic terms, was significant for the individual X-ray practitioners, health spas, and equipment manufacturers.

Growth of medical radiology under popular pressure put strains on the professional mechanisms available, which in any case were not well suited to the difficulties X-rays and radium posed. Only in Austria, where medicine was tightly professionalized and acutely aware of its prerogatives, did physicians gain immediate and exclusive control of the medical uses of X-rays and radium. In England, France, Germany and the United States, nonphysician practitioners hung out their shingles, primarily for diagnostic work with X-rays. The question of physician control over the use of X-rays was to continue, as we shall see, to pose problems. Whatever the merits of the case later, the surging demand in the first few years after 1896 permitted nonphysicians with photographic, electrical and glass-blowing skills to compete effectively. The nonphysicians survived in part because physicians referred patients to them, and also because hospitals often employed nonphysicians to run their diagnostic X-ray units. With the exception of physicians who before 1896 had been using electricity to diagnose and to treat their patients, a group of which we shall say more in Chapter 2, physicians were not well equipped to use X-rays. However important it might be for diagnosing and treating disease, a medical degree rarely testified to the types of skills required in building and maintaining high vacuum electrical discharge tubes and the auxiliary equipment.

While straining the existing professional mechanisms, the use of X-rays in medicine also generated new institutions. Often young, the devotees saw in X-rays an opportunity to be first in discovery and to advance rapidly. The intense interest led to the establishment of Röntgen Societies in Britain (1897), Germany (formed initially in Berlin but expanded to a national society in 1905) and the United States (1900). The British and German Röntgen Societies will prove especially important to the history of radiation protection because they permitted physicians and non-physicians to participate on an equal footing. Membership in the French Society for Medical Radiology (founded in 1909 by an expansion of the Parisian Society for Medical Radiology) was limited to physicians, and the American Röntgen Ray Society restricted nonphysicians to a lower category of membership.

In addition to these professional societies, medical use of X-rays (and later radium) created a new professional literature. The volume of articles on medical applications of X-rays and on the associated equipment was unprecedented. Within two years, the medical radiological literature was outgrowing the capacity of the existing medical journals, and a number of specialty journals were established in the first decade after 1896. In Germany, the Fortschritte auf dem Gebiete der Röntgenstrahlen was founded in 1897, and the Verhandlungen der Deutschen Röntgengesellschaft in 1905. In Britain, there was the Archives of the Röngten Ray, which was founded in 1898 as the Archives of Skiagraphy, and later also the Journal of the Röntgen Society (first published separately from the Archives in 1904). In the United States, the American X-Ray Journal (founded in 1897) gave way as the leading journal to the Transactions of the American Röntgen Ray Society after 1902, which was joined by the American Quarterly of Röntgenology after 1906. In France, the Archives d'Électricité Médicale, which had been founded in 1893, became the major outlet for medical X-ray work, and Le Radium served both the scientific and medical communities.

With the exception of Le Radium, the new journals did not attract original work in physics or chemistry, and the scientific radiological community was readily distinguishable from the medical radiological community. The physicists working on X-rays and radioactivity generally used the existing journals like the Philosophical Magazine in Britain, the Annalen der Physik and the Physikalische Zeitschrift in Germany, and the Comptes Rendus (as well as Le Radium) in France. The material published on physical aspects of X-rays in the medical radiological journals was either derivative or highly practical. The authors were often associated with manufacturers of X-ray tubes and auxiliary equipment. In Germany, where medical radiology showed more interest in scientific developments than elsewhere, most of the "scientific" and "technical" articles were contributed until World War I by Bernhard Walter, a diligent but thoroughly second rate physicist, and by Friedrich Dessauer, an X-ray tube manufacturer who would only after the War obtain a doctorate in physics. In England, the nonphysician members of the Röntgen Society were mostly tube manufacturers, electrical engineers, and interested dilettantes. A series of International Conferences of Medical Radiology and Electrology met seven times between 1900 and 1914. This series was entirely separate from the contemporary international scientific conferences on "Radiology and Ionization" (Liège, 1905) and on "Radiology and Electricity" (Brussels, 1910).

This gap in institutions between medical and scientific radiology corresponded to a difference in methodologies that we shall find of great importance. I shall state the difference here in radical terms without expecting to find it in so pure a form in the story that follows. In the scientist's laboratory, experiment and theory are, ideally, tightly linked. The experimenter tries to work within clearly defined theoretical presuppositions that enable him to ask questions about how a small and isolated piece of the natural world behaves. In the medical clinic, the procedure is different. The clinician observes a relatively broad expanse of nature and tries to bring some sort of recognizable order to it without the experimenter's capacity for controlling the conditions under which observations are made. Delicate and precise instruments may be used in the clinic, but to observe rather than to test. By accumulating ordered experience, and not necessarily understanding, the clinic aims to achieve practical results. Thus through years of making rounds with his more experienced elders, the young physician is trained to recognize a large number of diseases and the appropriate courses of treatment while understanding in scientific detail the causes of only a textbook few.[22]

There is no question about which of these methods is more precise. The clinical approach leaves a great deal of room for personal judgment, which means both wide scope for individual genius as well as wide scope for error. There is, however, also no question but that the scientific approach fails to offer solutions to many practical problems. As Wilfred Trotter pointed out in 1932, physiology could give no explanation of the most common symptons of which patients complain: feeling ill, pain, sleeplessness, vomiting, loss of appetite, and constipation.[23] Despite much research effort, the situation is not very different today. A physician can nevertheless learn to recognize and treat hundreds of diseases that cause these symptoms. The disadvantage of the scientific approach is precisely what makes it work so well: "Experiment...isolates the event to be studied from the common order of nature, and causes it to occur in circumstances as far as possible simplified and subject to specification."[24] Even today, this approach has been used on only limited aspects of the vast territory of medical practice, and much of medicine continues to be what Trotter called a "practical art" rather than an applied science.[25]

The methods of the practical arts are not limited to medicine. In chemistry, the periodic table has survived, despite the discovery of physical laws that make it superfluous in theory, as a device to organize experience rather than to explain it. The table permits a great deal of practical work to be done without recourse to what the physicist would regard as a proper explanation of chemical phenomena in quantum mechanical terms. When a chemist said, as he might have until the last few years, "xenon does not react because it belongs to the eighth period, the inert gases," he was stating neither a tautology nor the consequence of a physical law. Rather, he was stating the result of a vast quantity of experience, with which he need not have been personally familiar since the position of xenon in the periodic table neatly summarized it for him. In the history of radiation protection, we shall encounter mnemonic devices simlar to the periodic table, though not so conveniently graphic, and we shall recognize them as tools of a practical art. One of these was for decades regarded as the foundation of radiation biology, namely the "law" of Bergonié-Tribondeau. This law stated that cells were affected by radiation more strongly the greater their reproductive activity, the longer they took in mitotic division, and the less their morphology and functions had been differentiated. This statement summarized, and continues to summarize, many observations, but there were, and are, exceptions to it. Physical laws like Newton's or Maxwell's cannot have exceptions and still remain valid. The law of Bergonié-Tribondeau still stands because it is in most cases correct and therefore remains useful. It is closely akin to the engineer's rule of thumb, which often serves him better in his daily work than physical laws. In medical radiology, such rules--concerning which kinds of radiation to use for different purposes, the length and frequency of the exposures required, and the protection measures that were appropriate--grew out of the clinical experience of many practitioners, who would sometimes accord them the status and respect usually reserved for physical laws.

Between the largely separate worlds of scientific and medical radiology there were from the first three important links that would prove important to the development of radiation protection. First, the two communities used similar materiel. On the one hand, medical radiology in the early years relied directly on scientists to obtain radium. This reliance would, as we shall see, keep the medical applications of radium in much closer touch with related scientific work than the medical applications of X-rays, which were developed by a much larger group of often isolated practitioners. On the other hand, scientists could purchase X-ray equipment readily because it was produced commercially for the medical radiological market. It took only a few months in the spring of 1896 for glassblowers and for manufacturers of electrical equipment, many of whom already manufactured medical electrical equipment, to place on the market a bewildering variety of X-ray tubes, induction coils and interrupters, influence machines, and auxiliary equipment like fluoroscopes, tube stands, and examining tables. Both the scientific and the medical communities took an active interest in innovations in X-ray and radium technology, and although certain items were considered suitable only for the laboratory and not for the clinic, or _vice versa_, there was a common interest in X-ray tubes, the manipulation of radium, and the measurement of both.

The second major link between scientific and medical radiology lay in the goals that the medical radiological community set for itself. Acutely aware of its scientific origins, medical radiology aimed to be scientific and became an important component of what I shall call the "program of scientific medicine." In retrospect, it is obvious that the discovery of X-rays and radium by scientists did not make their medical use any more certain in its clinical results than if, for example, they had been discovered by a glass-blower (as might well have been the case for X-rays). The image of X-rays and radium as distinctively modern and advanced tools was nevertheless a strong one. Precisely what it meant to be "scientific" varied considerably. "Scientific" medicine sometimes meant medicine based on scientific theory. Of the people who thought along this line, some were reductionists who wanted explanations of the biological effects of radiation in physical or in chemical terms, but there were others who were satisfied with explanations in terms of cytology or bacteriology. To still other research workers, "scientific" medicine meant the use of measurements or of experiments within medicine rather than explanations in scientific terms. Among these people there were different views of the precision required of measurements and different opinions on what constituted an experiment. I shall distinguish below among these different meanings of "scientific" medicine, and I shall go to some pains to show that the results of research efforts along these different lines were not necessarily consistent with each other. For the moment, however, I want to emphasize that all these claims to "scientific" status betray the strong inspirational force of the program of scientific medicine. As a result of the widespread adherence to this program, medical radiology was in principle more open to scientific input than many other branches of medicine.

The third link between scientific and medical radiology was conceptual: they both thought in terms of the "quantity" and "quality" of radiation.[26] It would be decades before the two communities would come to an agreement on how to measure these parameters, but both communities talked of the quantity and quality of X-rays and of radium as if these were self-evident concepts. For X-rays, quantity was the amount of radiation; the term was often used interchangeably with "intensity" (which might also mean quantity per unit time) and was analogous to the intensity of light. For radium, quantity meant the amount of a substance. Quality, which was analogous to the different colors of light, meant the differing ability of radiation to penetrate matter: "harder" rays were more penetrating and "softer" rays less penetrating.[27] With the discovery of radium, the usage was quickly extended to the radiation it emitted: the softer radiation that could be deviated with a magnet (later broken down into positively charged "alpha" particles and negatively charged "beta" particles) and the much harder, "gamma" radiation that was not deviable in a magnetic field and was even more penetrating than the hardest X-rays known at the time.[28]

1. For the historian of science, the classic telling of this story is George Sarton, "The Discovery of X-rays," Isis, 26 (1937) 340-69. Most of the details there and elsewhere come from Otto Glasser, Wilhelm Conrad Röntgen and the Early History of Röntgen Rays (Charles C. Thomas, 1934), which for lack of other materials is usually treated as a primary source. A. Romer has briefly discussed the lack of reliable primary sources and the consequent limitations on writing the history of the discovery of X-rays in "Accident and Professor Röntgen," Amer. J. Phys., 27 (1959) 275-77. A full, critical discussion of the sources with reprints of the original articles in English and German can be found in Herbert S. Klickstein, Wilhelm Conrad Röntgen, "On a New Kind of Rays": A Bibliographical Study (Mallinckrodt Chemical Works, 1966). Unfortunately, this excellent study is not readily available in even the best libraries, and I am indebted to the Mallinckrodt Chemical Works for providing me with a copy. There are many variations on Sarton and Glasser, including G. E. M. Jauncey, "The Birth and Early Infancy of X-Rays," Amer. J. Phys., 13 (1945) 362-79; A. W. Crane, "The Research Trail of the X-Ray" in A. J. Bruwer, Classic Descriptions in Diagnostic Röntgenology (Springfield, Illinois: Charles C. Thomas, 1964); and E. A. Underwood, "W. C. Röntgen and the Early Development of Radiology," Proc. Roy. Soc. Med., 38 (1944-45) 697-706. W. Robert Nitske, The Life of Wilhelm Conrad Röntgen: Discoverer of the X-Ray (Tucson: University of Arizona Press, 1971) must be considered a plagiarism of Glasser, despite some embellishments. Not all the embellishments are accurate.

2. Philipp Lenard (Bonn, Physik. Inst. d. Univ.), "Ueber Kathodenstrahlen vom atmosphärischen Druck und im äussersten Vacuum," Ann. Phys., 51 (1894) 225-67.

3. W. C. Röntgen, "Eine neue Art von Strahlen (Vorläufige Mitteilung)," Sitzungsber. Phys. Med. Ges. (Würzburg) (1895) 132-41, am 28 Dezember wurde als Beitrag eingereicht. Inadequate translations can be found in numerous places, including the following: Glasser, note 1 above, 16-28; Klickstein, ibid.; and N. Feather, X-Rays and the Electric Conductivity of Gases, Alembic Club Reprint No. 22 (Edinburgh: E. and S. Livingstone, 1958).

4. See, for accounts of early techniques, the following: A. J. Bruwer, Classic Descriptions in Diagnostic Röntgenology (Springfield, Illinois: Charles C. Thomas, 1964); P. Pizon, La Radiologie en France, 1896-1904 (Paris: l'Expansion scientifique française, 1970); Ruth and Edward Brecher, The Rays: A History of Radiology in the United States and Canada (Baltimore: Williams and Wilkins, 1969); Hans R. Schinz, Sechzig Jahre Medizinische Radiologie: Probleme und Empirie (Stuttgart: Georg Thieme, 1959); P. Fleming Møller, History and Development of Radiology in Denmark, 1896-1950 (Copenhagen: Nyt Nordisk Verlag-Arnold Busek, 1968). These references are but the cream of a vast retrospective literature, some of which is listed in the "Annotated 'Radiohistoric' Bibliography" in E. R. N. Grigg's bizarre but informative tome, The Trail of Invisible Light (Springfield, Illinois: C. C. Thomas, 1965), pp. 822-64.

5. For the meaning of bricolage and its relationship to science, see C. Levi-Strauss, The Savage Mind (Chicago: University of Chicago Press, 1966), pp. 16-22.

6. F. Mink, "Zur Frage über die Einwirkung der Röntgensche Strahlen auf Bakterien und ihre ev. therapeutische Verwendbarkeit," Munchen. Med. Wschr., 43 (4 February 1896) 101-102 and (3 March 1896) 202; and T. Glover Lyon (Senior Assistant Physician to the Victoria Park Chest Hospital), "The Röntgen Rays as a Cure for Disease," Lancet, 74 (1 February 1896) 326 and (22 February 1896) 513-14.

7. The first report was probably J. Daniel (Physical Laboratory at Vanderbilt University), "The X-Rays," Science, (10 April 1896) 562-63, signed 23 March 1896, but there was a flood of similar reports soon thereafter, see for example Marcuse, "Dermatitis und Alopecie nach Durchleuchtungsversuchen mit Röntgenstrahlen," Deut. Med. Wschr., 22 (23 July 1896) 481-83. For other early reports, see Otto Glasser, "First Observations on the Physiological Effects of Röntgen Rays on the Human Skin," Amer. J. Phys., 28 (1932) 75-80.

8. L. Freund, "Ein mit Röntgen-Strahlen behandelter Fall von Naevus pigmentosus pilferus," Munchen. Med. Wschr., 47 (6 March 1897) 429-34, based on a lecture at the k. k. Gesellschaft der Aerzte in Wien on 15 January 1897, and Albers-Schönberg, "Über die Behandlung des Lupus und des chronischen Ekzems mit Röntgenstrahlen," Fortschr. Röntgenstr. 2 (1898-99) 20-29.

9. Albers-Schönberg, ibid., and an editorial, "The Action of X-Rays on Microorganisms," Arch. Rönt. Ray, 3 (1898) 1-2 provide extensive references. Claims of unequivocal success in killing bacteria and in inhibiting their growth also helped to promote further efforts, see for example H. Rieder, "Wirkung der Röntgenstrahlen auf Bakterien," Munchen. Med. Wschr., 45 (1898) 101-104.

10. See T. Sjögren and E. Sederholm (Stockholm), "Beitrag zur therapeutischen Verwertung der therapeutischen Verwertung der Röntgenstrahlen," Fortschr. Röntgenstr., 4 (1900-01) 145-70, where they claimed priority on the basis of a report to the Gesellschaft der schwedischen Ärtze on 19 December 1899.

11. There can be few topics in the history of social attitudes toward disease more worthy of detailed investigation than this one, which appears to have escaped even cursory treatment. Cancer as a "dread" disease of the first rank, worthy of special public and professional attention, appears to date from the period 1900-1905, see for example the (British) Cancer Research Fund, First Report (1903).

12. For an example of the comparison with the photographic plate, see Gocht (Assistenarzt, aus der chirurgischen Abteilung des Neuen allgemeinen Krankenhaus in Hamburg), "Therapeutische Verwendung der Röntgenstrahlen," Fortschr. Röntgenstr., 1 (1897) 14-22. For an example of emphasis on trophoneurosis, see Albers-Schönberg, note 8 above.

13. On the history of the Versammlungen der Gesellschaft Deutscher-Naturforscher und Ärtze, see R. Hinton Thomas, Liberalism, Nationalism and the German Intellectuals (1822-47) (Cambridge: W. Heffer, 1951).

14. On the pulse theory, see Bruce R. Wheaton, The Extension of the Electromagnetic Spectrum: Determining the Nature of X- and γ-rays, 1896-1915 (Princeton Ph. D. thesis in progress).

15. For the Becquerel papers in translation, see Alfred Romer, The Discovery of Radioactivity and Transmutation (New York: Dover, 1964). For the Curie papers in translation and a narrative account of this work, see Alfred Romer, Radiochemistry and the Discovery of Isotopes (New York: Dover, 1970), pp. 63-75 and the "Historical Essay," pp. 3-8. See also Lawrence Badash, "Chance Favors the Prepared Mind: Henri Becquerel and the Discovery of Radioactivity," Arch. Int. Hist. Sci., 70 (1965) 55-66.

16. H. Becquerel and P. Curie, "Actions physiologiques des rayons du radium," C. R. Acad. Sci. (Paris), 132 (1901) 1289-91 is the standard reference, but largely because of the fame of the authors. They were aware of F. Giesel, "Ueber radioaktiven Stoffe," Ber. Deut. Chem. Ges., 33 (1900) 3569-71, received 7 December 1900, and of a report by Walkhoff in Photographische Rundschau (October 1900).

17. J. Elster and H. Geitel (Wolfenbüttl), "Über die Radioaktivität der im Erdboden enthaltenen Luft," Phys. Zeit., 3 (1902) 574-77, received 3 September 1902. See also W. Gerlach, "Johann Philipp Ludwig Julius Elster," in Dictionary of Scientific Biography, 4 (New York: Charles Scribner's Sons, 1971), pp. 354-57.

18. For the development of the idea of transmutation, see the original papers and narrative in Romer (1964), note 15 above, pp. 86-150. For Marie Curie's original presentation of this theory, see her first article on Becquerel rays, "Rays Emitted by the Compounds of Uranium and Thorium," (originally "Rayons emis par les composes de l'uranium et du thorium," C. R. Acad. Sci. (Paris), 126 (12 April 1898) 1101-1103) as translated in Romer, ibid., pp. 65-68, especially 67-68. For the Curies' continuing defense, see for example their "On Radioactive Substances" (originally "Sur les corps radio-actifs," C. R. Acad. Sci. (Paris), 134 (13 January 1902) 85-87) as translated in Romer, ibid., pp. 121-23.

19. See, for example, the generally favorable report by J. William White (M. D., Philadelphia), "The Röntgen Rays in Surgery," Trans. Amer. Surg. Ass., 15 (1897) 59-88.

20. E. A. Codman (Surgeon to Out Patients, Massachusetts General Hospital, Skiagrapher to Children's Hospital), "No Practical Danger from the X-ray," Bost. Med. Surg. J., 144 (28 February 1901) 197.

21. One retrospective account puts the cost of a minimal X-ray installation at 30 pounds sterling in 1896, see C. Thurstan Holland, "X-rays in 1896," in A. J. Bruwer, note 4 above. Similar equipment in Germany cost about 600 marks, which was about the same amount, see the advertisement by the firm of Ferdinand Ernecke in E. Wunschmann, Die Röntgeschen X-Strahlen. Gemeinverständlich dargestellt (Berlin: F. Schmidt, 1896) as reproduced in Glasser, note 1 above, at 352. Screens for radioscopy,

photographic plates, suitable furniture, and a reasonable number of tubes would more than double this minimum. The tube itself was negligible in cost; the induction coil was the most expensive component.

22. The methodological distinction between the clinic and the laboratory appears, explicitly or implicitly, in many places. At the risk of attributing a distinction to authors who would not think it valid in the form in which I shall use it, I would cite the following sources: Claude Bernard, An Introduction to the Study of Experimental Medicine, tr. H. C. Greene (New York: Dover, 1957), pp. 9 and 18, where the distinction is made in terms of "experimental" vs. "empirical"; Donald Fleming, "Emigré Physicists and the Biological Revolution," Pers. Amer. Hist., 2 (1968) 152-189, where (p. 160-61) the distinction is made in terms of an instinctual difference between physicists and biologists in responding to evidence; and Levi-Strauss, note 5 above, where the distinction is made in terms of "the scientist creating events (changing the world) by means of structures and the 'bricoleur' creating structures by means of events," at 22.

23. Wilfred Trotter, "Art and Science in Medicine," an address delivered at the opening of the 1932-33 session at the University College Hospital Medical School, in The Collected Papers (London: Humphrey Milford for the Oxford University Press, 1941), pp. 85-101.

24. Wilfred Trotter, "Observation and Experiment and their Use in the Medical Sciences," Brit. Med. J. (26 July 1930) reprinted, ibid., pp. 104-27, at 104.

25. Ibid.

26. In French, quantité and qualité. In German, quantity was originally Menge but became Quantität with the shift, discussed below, from exposure to dose; quality was Härte, Qualität or sometimes Penetrationsvermögen.

27. The idea of quality, analogous to the different colors of light, had already been introduced for cathode rays, see Philipp Lenard, "Ueber die magnetische Ablenkung der Kathodenstrahlen," Ann. Phys., 52 (1894) 23-33.

28. For the development of this classification scheme, see Thaddeus J. Trenn, "Rutherford on the Alpha-Beta-Gamma Classification of Radioactive Rays," Isis, 67 (1976) 61-75.

Chapter 2: Two-edged Swords: X-rays and Radium, 1896-1902

While medical applications of X-rays developed rapidly between 1896 and 1900, X-ray protection was at best a minor concern. Lack of knowledge *per se* was not the reason for this inattention to harmful side effects. Exposure to X-ray tubes was known to have harmful effects; these effects were, after all, the basis of X-ray therapy. Many operators, however, took no measures to protect themselves or their patients. Of those who did take protective measures, the majority probably used a grounded aluminum or tin sheet. Lead, though used by some practitioners, was considered unnecessarily inconvenient and even dangerous by others. Many operators tested the hardness of a tube by exposing their own hands and viewing the resulting image on a fluorescent screen. The X-ray tube was, in general, not enclosed. Although physicists routinely recorded some measure of the quantity and quality of X-rays, measurements were not generally made during medical applications. Moreover, the bulk of the X-ray practitioners, physician and non-physician, around 1900 were pleased with the situation. The number of cases of harm to patients was, they thought, decreasing rapidly. X-ray therapy was curing a widening range of dermatological ailments. The reddening, scaling and open sores that had developed on the hands of many X-ray practitioners were usually considered minor ailments. This chronic dermatitis was considered a small price to pay for the benefits obtained from the application of X-rays in medicine.

Change would come rapidly after 1900, as we shall see. Measures to protect the patient and the operator would become routine, however inadequate the procedures used may appear by today's criteria. By the end of 1902, the weight of professional opinion would shift against the use of aluminum and tin shielding, grounded or not. Methods of measuring the quantity and quality of the rays would come into general use. Operators who tested hardness with their own hands would be considered foolhardy at best. The professional societies and journals would actively promote precautions. While far from the precision it was to acquire in the decades to follow, X-ray protection would by 1903 be a recognized problem for science, medicine, and society.[1] In sharp contrast, radium protection was still unknown.

Why did X-ray practitioners before 1900 use what they themselves would regard by 1903 as grossly inadequate and misdirected methods of protection? What made these practitioners shift gears after 1900, embracing both X-ray measurements and X-ray protection as essential professional concerns? Why did radium protection not become a matter of concern? We shall be concerned here with the story of radiation protection, but the implications of this specific case are broader. We are today all too familiar with a pattern of medical and technological innovation in which the widespread adoption of a technique leads to harmful side effects and subsequent retrenchment. Disinterested expertise appears to be an obvious solution, and many professional communities would prefer to see problems arising from medical and technological innovation resolved within relatively narrow circles of expertise and out of view of the public. Medicine in particular has established self-regulatory mechanisms that should, it can be argued, protect both practitioners and patients from harm. There are, I think, fundamental difficulties with this picture of disinterested expertise and professional self-regulation. The term "expertise" suggests a degree of objectivity that often does not, and sometimes cannot, exist: those who know a good deal about something by definition have an interest in it. The interest may be intellectual or professional rather than financial, but it is an interest nevertheless and may affect the weighing of evidence. As for the self-regulatory mechanisms, they certainly exist, but their operation may depend on pressures from outside a professional community. In order to see the interested character of expertise and the dependence of self-regulation on outside pressures in the case of radiation protection, let us return briefly to the period before 1900 to consider the procedures used in X-ray therapy and the thinking that guided therapeutic efforts. I shall then consider the conjoined intellectual and social pressures that led to change after 1900. The contrasting case, radium, will require only cursory treatment.

Although X-ray protection _per se_ was not a concern before 1900, clinical experience had led practitioners to adopt what they believed to be conservative and cautious therapeutic procedures. Patients, it was generally thought, showed wide variability in their reactions to treatment. This idiosyncracy contrasted sharply with the presumed invariability of the physical agency, the X-ray tube. In reporting cases, the practitioner specified the parameters of the tube, such as the equivalent spark gap, the "secondary" current through the tube (or sometimes only the "primary" current flowing into the induction coil), the number of breaks per minute of the interrupter, the distance of the patient from the anticathode, the duration of the exposure, or whatever other parameters had been found by clinical trials to affect the course of treatment. No need arose for measuring the dose of X-rays delivered, and indeed the notion of "dose" did not yet exist. The presumed variability of the biological material made therapy an art. Knowing when a sufficient exposure to the X-ray tube had been administered was a matter of judgment, preferably informed by both long experience and a medical degree. Generally, physicians solved this matter of judgment simply. Exposure was continued until a light skin reaction was visible. The erythema was the signal both that the treatment was working and that it should be discontinued. This practical method favored the use of harder tubes, which "burned" less readily, and of short, repeated exposures over a period of weeks or even months.[2]

Many practitioners administered this treatment with little or no concern for the mechanism of the resulting effects, but some X-ray practitioners and medically oriented research workers had their own notions about the nature of X-rays and thereby drew conclusions about how they acted on biological materials. These notions were analogies that placed the new discovery within the context of existing biological knowledge. On the one hand, X-rays were considered closely akin to light, and especially to ultraviolet light. Since violet, and ultraviolet, light was known to be a disinfectant, this kinship led to the anticipation of bactericidal effects of X-rays.[3] The analogy to ultraviolet also led to the treatment of lupus vulgaris with X-rays, in imitation of therapeutic successes with ultraviolet lamps.[4] On the other hand, X-rays were considered closely akin to electricity. The way in which X-rays were produced testified strongly to their "electrical" character, as did their ionization of gases. This latter analogy to electricity, though far from a theory whose implications could be worked out in mathematical fashion, was a step in assimilating X-rays into medical thinking and had profound consequences for work on their biological effects.[5]

The biological effects and medical uses of electricity, after falling into disrepute during the 1830s and 1840s, were again the subject of intensive investigation in the late nineteenth century, with interest reviving in the 1860s and probably peaking in the 1880s and early 1890s. Along with other "physical" techniques like baths and massage, electrotherapy played an important role during a period of relative nihilism in chemcial therapeutics. Although today much of this tradition is forgotten, electrodiagnosis and electrotherapy were in the 1890s often considered especially modern and promising areas of scientific medicine. Medical electricity was also considered an undeveloped area in which over-enthusiastic adherents and charlatans put forward excessive claims, as should be expected of a rapidly developing specialty. Because of the similarity in the equipment and skills needed for radiotherapy, many of the early X-ray practitioners came from electrotherapy. With his knowledge of batteries, static generators, induction coils and electrodes, the electrotherapist was far ahead of other physicians in being prepared to set up and maintain an X-ray tube. Moreover, X-rays were welcomed into medical electricity, and until World War I were often treated as part of that broader and older tradition.[6]

The association with medical electricity strongly conditioned the reaction of many physicians to the discovery of biological effects of exposure to the X-ray tube. Initially, X-rays themselves were often assumed to the be the causal agent, but a variety of other candidates were soon put forward. These included ultraviolet light from the fluorescing tube, ozone produced around the tube or in the skin, metal particles from the anode, and the static electric charges surrounding the tube. Only the last of these suggestions achieved a significant level of acceptance. This electrical view became dominant among medical practitioners through 1900 and persisted for several years thereafter.[7] Attributing the biological effects to the static charges surrounding the tube, which was often placed within inches of the patient, rather than to the X-rays themselves placed the new technique in the sphere of electrotherapy and avoided the uncertainty concerning the nature of the X-rays. Moreover, there was clinical evidence. A "brush" discharge of diffuse sparks could sometimes be seen between the X-ray tube and the skin of the patient. Experience had taught medical practitioners that increasing the distance between the tube and the patient would often prevent harm. The static electric field around the tube would not extend far, certainly not as far as the X-rays themselves, so this effect of distance favored the electrical view.[8] So, too, did the reports that a grounded aluminum screen would protect the patient.[9] Occasional reports appeared of intensified therapeutic effects when the patient was placed on an insulated stage, a common practice in some electrotherapeutic techniques.[10] Some practitioners claimed that an X-ray tube excited with an influence machine rather than with an induction coil did not cause burns.[11] This claim suggested that it was the electrical means used to excite the tube rather than the X-rays themselves that were responsible. Ungrounded lead, it was said, might be harmful if used for shielding because it would condense the harmful static charges near the skin of the patient.[12]

Experimental evidence against the electrical view was readily obtained. It was a simple matter to expose a finger to the X-ray tube, protecting part of it with ungrounded lead and part of it with grounded aluminum or tin shielding. By 1898 this experiment and comparable ones with other biological materials had been performed, but the unequivocal results against the electrical view had little effect.[13] X-rays were still relatively new, the professional communities were still in the process of formation, and the standards of proof were still uncertain. Moreover, the weight of the clinical evidence was on the side of the electrical view. No matter how decisive the outcome, the experimental evidence was based on a mere case or two, and these occurred in the artificial conditions of the laboratory. The practitioner valued the vast experience of the clinic more highly. The recently founded journals turned not to laboratory experiment, but rather to surveys of clinical experience to decide the issue of what caused the injuries. By collecting cases of injury and studying the conditions under which they occurred, the practitioners hoped to find ways of preventing their recurrence. The surveys, which were begun in 1897 and completed by 1899, failed to reach definitive conclusions, and the electrical view continued to dominate in the medical community.[14]

The advocates of the electrical view greatly enhanced their position by shifting the burden of proof. X-rays were presumed to be innocent of causing any biological effects. Those who believed the X-rays were the causal agent were asked to demonstrate that none of the other possible agents was responsible.[15] The electrical view was thus bolstered by the demonstration that high-voltage electrical charges could, in the absence of X-rays, cause erythema and epilation.[16] Likewise, demonstration that electric charges could kill bacteria became evidence for the electrical view since exposure to the X-ray tube was known to have a curative effect in the clinic on a disease like lupus.[17] Since the majority view was that X-rays did not kill bacteria, those who believed X-rays had biological effects also had the burden of accounting for the successful therapy. X-rays might stimulate the body's natural defense mechanisms, but this argument was unconvincing without supporting evidence.

Decisive evidence that X-rays themselves caused the biological effects was finally offered in 1900, but it was only slowly over the next several years that the proponents of the electrical view gave way. With the evidence came a new technique of X-ray therapy, a need for dose measurements, and greatly intensified concern for X-ray protection.

The initiator of these changes was a young Viennese physician, not yet habilitated, who was working at a private clinic. In the course of treating six patients with X-rays in late 1899 and early 1900, Robert Kienböck was forced to switch to a softer tube when his harder one was punctured.[18] All six patients, regardless of how much they had been exposed to the harder tube, quickly developed skin reactions. This clinical experience suggested to Kienböck that X-rays, which were more intense from the softer tube, caused the burns rather than the static electric charge surrounding the tube, which would be more intense around the harder tube. A series of straightforward experiments decided the issue. Kienböck exposed one side of a living organism to a soft tube and one side to a hard tube. The reaction appeared more rapidly on the side exposed to the softer tube. The point closest to the focus of the cathode rays reacted soonest and most dramatically. Masking with lead showed that shadows were cast from the single point where the X-rays originated rather than from the surroundings of the tube. Rubber, a good electric insulator, did not protect the skin. Biological effects were observed only on the side of the anticathode struck by the cathode rays, the direction in which Kienböck assumed all the X-rays to be emitted.[19]

Replying to the advocates of the electrical view, Kienböck offered explanations for the phenomena they had adduced as evidence. The grounded aluminum or tin foil might delay and lessen the reaction because it absorbed X-rays, but it did not prevent harm altogether. Because the current through the tube excited with an influence machine was less, it produced a lower intensity of X-rays than a tube excited with an induction coil, and therefore burned less readily. Biological effects were more readily produced in close proximity to the tube because the intensity of the X-rays fell off with the square of the distance.[20]

Kienböck also offered a new method of X-ray therapy. Idiosyncracy, he claimed, was irrelevant. Different tissues reacted differently, but the same tissues in different individuals of the same age reacted in the same way. Instead of irradiating a patient with a hard tube until a reaction appeared, Kienböck suggested fewer and shorter sittings with a soft tube, preferably one whose quality could be regulated. After these sittings, the treatment would be suspended and the physician would wait for results to appear. The quantity of X-rays to which the tissue was exposed was the key variable in this method, and the notion of dose in medical radiology dates from its adoption. Kienböck's method required more attention to the tube and its output and less attention to the patient and his idiosyncracy. Since high doses were to be given in a few sittings, without waiting for a reaction, there was a clear need for a means of measuring X-ray dosage.

Kienböck's proofs may in retrospect seem trivial and his new method obvious, but this was not the reaction of his opponents. To them, Kienböck appeared to be making extravagant claims, especially when he advocated the use of fewer exposures and higher doses. He would, they warned, find with further experience that X-ray therapy was more difficult than his mechanical approach suggested. Routinized application of X-rays without regard for the special characteristics of individual patients could only lead to injury. Kienböck's method was inherently dangerous because the reactions only appeared after exposure, while the method in use fractionated the exposures so that they could be stopped as soon as the first signs of a reaction appeared. One opponent offered an experiment in which the static charges around the cathode section of an X-ray tube appeared to kill a bacterial culture while the X-rays radiating from the anode failed to do so. But the primary argument against Kienböck was the weight of the evidence. So many patients had been treated by so many physicians with relatively hard X-ray tubes surrounded with static electric charges. How could they all be wrong?[21]

The majority electrical view was wrong, but it took more than Kienböck's experiments, and other evidence of the same sort, to turn the tide. These experiments may have been a necessary step, but they were not sufficient to rescue X-ray therapy from electrotherapy. In part, the other influences at work lay outside both science and medicine. Popular reaction to cases of X-ray injury, and the fear that popular reaction would bring limitations on the use of X-rays in medicine, were essential to hastening the rejection of the electrical view and the acceptance among X-ray practitioners of the need for protection and dosimetry. Also important was the recognition that chronic X-ray dermatitis among practitioners could no longer be considered a minor ailment. Thus radiation protection was by no means an automatic response by a professional community to the simple fact of X-ray injuries. Public concern was essential to activating the self-regulatory mechanisms available in the medical radiological community.

An initial public outcry over X-ray injuries had occurred in 1897 and 1898 in the lay press. The X-ray burns shocked a public that had greeted the new technology with almost unrestrained enthusiasm less than two years earlier. The X-ray practitioners reacted by mounting the surveys of X-ray injuries mentioned above. Public reaction, in other words, brought practitioners together to act jointly in defense of their profession, a pattern that we shall see repeated. Ambiguous though the results of the surveys were on the question of whether X-rays were the causal agent, they in general concluded that dermatitis resulted largely from idiosyncratic reactions of especially sensitive individuals, and that the risk to most people was small. The presence on the hands of many physician and nonphysician practitioners of chronic dermatitis, which had been recognized as early as 1897, was discounted.[22]

The public was assured that below a certain level of exposure no harm would be done, and that diagnostic X-rays could be given without any risk. An organizer of the Röntgen Society survey concluded optimistically:

> We may, I think, safely assert that the length of exposure necessary to produce an injury is at least three or four times that required to obtain a radiograph with the improved apparatus now at our disposal, even when the most opaque parts of the body are concerned, and then only when the patient is specially susceptible to the electrical forces which cause the injury.[23]

Indeed, the need for unblurred radiographs and greater efficiency in taking them had led by 1900 to improved photographic plates, intensifying screens, and X-ray tubes whose vacuum could be controlled. These improvements, even without any measures taken specially for protection, had significantly decreased the number of reports of acute injury. The errors of the past, it appeared, were being avoided, and the future would see even greater results from the medical use of X-rays. The weight of the evidence was substantial: a 1902 survey reported that "only one case in 5000 has been injured, and less than half of these seriously."[24]

A second round of public reaction came in the years 1900-02 in the courts.[25] Here the problem confronting X-ray practitioners was not to be solved solely by allaying public fears. Harm had been done, and patients were suing for damages. As early as 1898 an unsuccessful suit brought in Germany had led to recommendations for reducing exposures and increasing the distance between the tube and the patient.[26] Beginning in 1900, there was a series of successful suits. A climax of sorts was reached in 1902, when a criminal judgment of "negligent bodily injury" against a German physician was followed by a suit for damages of 36,000 marks. The defendant was a particularly irascible and incoherent advocate of the electrical view who had failed to protect the clothed parts of the patient's body.[27] This physician became the extreme against which a more moderate position could be defined. Reinforcing the intellectual impact of Kienböck's experiments, the law suits changed professional standards and clinical practice. Shortly after the suit for 36,000 marks was filed, recommendations appeared in the professional literature that aimed to protect the patient, the operator and the manufacturer. These recommendations assumed that X-rays, not static electric charges, caused observed injuries.

The second factor precipitating this concern for protection was the discovery that chronic X-ray dermatitis was developing into carcinoma of the skin, then termed "epithelioma" (a term now reserved for benign tumors). The dermatitis had been a painful and debilitating ailment. It had not responded to a wide variety of treatments, and even after healing relapses were frequent. There is, however, an enormous qualitative difference in the reaction to malignant and benign disease. Physicians, no less than laymen and perhaps more, responded dramatically to the discovery in late 1902 that one of Edison's assistants was suffering from X-ray induced carcinoma. Death followed in 1904 after successive amputations aimed at saving him from a malignancy that had spread from his hands up his arm.[28] As we shall see, this death was but one of several that were to generate increased interest in radiation dosimetry and protection during the next few years. Cancer, the dread disease that X-rays were supposed to cure, they also caused. The discovery, it should be noted, was once again the result of clinical accident rather than laboratory experimentation. Scientific understanding still had little to offer in guiding experimenters to the discovery of biological effects.

The protection recommendations that resulted from the concern about law suits and about cancer could be little more than a wise man's view of the measures that would avoid harm. Heinrich Albers-Schönberg, the wise man, was the editor of the leading German radiological journal, Fortschritte auf dem Gebiete der Röntgenstrahlen. He would play a continuing role in the development of protection measures until his death from X-ray induced injuries in 1921.[29] First among the precautions Albers-Schönberg suggested in 1903 was to permit only competent physicians to apply X-rays, a suggestion that we shall meet again. Beyond control of clinical practice by physicians, Albers-Schönberg offered what he considered reasonable exposure times and distances (less than four minutes at 30 centimeters no more than three times per day). He also recommended the use of lead shielding around the tube and between the operator and the tube. Testing the hardness of the tube with one's own hands he thought highly inadvisable, especially since chronic dermatitis absolutely excluded the physician from surgical or obstetrical practice. These recommendations were common sense informed by experience. There was no experimental or theoretical reason to believe that the recommendations were adequate, or that they were not excessive. No one had studied dose-response relationships, as later the radiation biologists would. There was nevertheless a good deal of reason to believe that the recommendations would provide some measure of protection to the patient from physical harm and to medical radiology from law suits. As Albers-Schönberg put it, "In order to assure the physician protection from such unfortunate occurrences [as a suit for 36,000 marks] and at the same time to protect the public from the possibility of burns, I have drafted some rules for the radiographic examination of patients, whose observance can guarantee an almost certain protection."[30] Albers-Schönberg was unquestionably sincere. He was already suffering from chronic dermatitis himself. To some of his colleagues, the recommendations he proposed seemed overly strict.[31] No one challenged the assumption, implicit in the recommendations, that below some exposure threshold, no harm would be done.

More important than the details of these early recommendations was the introduction of X-ray dose measurements. Kienböck had urged that practitioners keep careful records of more than a dozen parameters while administering therapeutic doses, but this so-called "indirect" method never came into use in the form in which he envisaged it. Instead, a number of "direct" methods of dose measurement that relied on chemical changes caused by X-rays, as well as some simpler indirect methods, were introduced beginning in 1902. These will be discussed in detail in the next chapter. Here the key point is that the introduction of even the crudest measurement of X-ray quantity was not automatic for either physician or nonphysician practitioner, in therapy or in diagnosis. For

X-rays, measurement was introduced into medical practice to serve the need for protection, which was not generally recognized for four or five years after the initial applications of X-rays in medicine. To mark the change, the advocates of X-ray measurements referred to the earlier approach as "more scientific."[32] As we shall see, the methods of measuring dose were in fact highly practical and their connections with contemporary physical science extremely tenuous, but the claim to scientific status was made nevertheless.

Measurements of the quantity of radium would, as we shall see, enter the clinic more readily than X-ray measurements had, but the purpose of radium measurements was not protection. Not until just before World War I would radium protection become even a peripheral professional concern, and not until after the War would it be treated on a par with X-ray protection. Lack of radium injuries was not the reason for this delay. Physicists and physicians who worked with radium extensively, foremost among them the Curies, suffered obvious effects on their fingers well before 1903. At worst, raw and itching fingers seemed a small price to pay for working with this wonderful new element, just as a few years earlier chronic X-ray dermatitis had seemed a minor ailment to X-ray practitioners. Marie Curie treated her damaged hands as a badge of honor, and in her laboratory protection measures were not encouraged.[33] Pierre Curie collaborated with two physicians in demonstrating that small quantities of radium emanation administered by respiration could kill mice and hamsters, but this research on laboratory animals did not lead to calls for radium protection.[34] Emanation was readily detectable in the breath and urine of people who worked with radium, but since they had not suffered any acute symptoms this fact was taken as proof that the amounts of emanation involved were harmless.[35] In the absence of severe injuries, law suits, and public outcry, the professionals most directly concerned discounted the need for radium protection measures.

1. This early period in the history of X-ray protection has been most accurately discussed in N. Ratkőczy, "Geschichtliches über Strahlenschädigung und Strahlenschütz," Strahlenth., 141 (1971) 311-20 and 425-38. James D. Nauman provides some interesting excerpts from British and American materials of this period in "Pioneer Descriptions in the Story of X-ray Protection," in A. J. Bruwer, Classic Descriptions in Diagnostic Röntgenology (Springfield, Illinois: Charles C. Thomas, 1964), pp. 311-39. Most other secondary treatments either begin later or fail to take seriously the issue of identifying the agent causing the biological effects.

2. For a review of this "many sittings" approach, see Magnus Möller, (Docent für Dermatologie und Syphilis in Stockholm), "Der Einfluss des Lichtes auf die Haut in gesundem und krankhaftem Zustande," in Gustav Born, et. al., eds., Bibliotheca Medica, Abtheilung DII, Heft 8 (1900), especially pp. 126-27. For specific instances, see A. Gasmann (I Assistent der dermatolog. Universitätsklinik des Prof. Jadassohn in Bern) and H. Schenkel (techn. Leiter des Röntgen-Instituts des Insel-Spitals), "Ein Beitrag zur Behandlung der Hautkrankenheiten mit Röntgenstrahlen," Fortschr. Röntgenstr., 2 (1898-99) 121-32 and J. Hall-Edwards, "The Röntgen Rays in the Treatment of Cancer," Arch. Rönt. Ray, 7 (December 1902) 45-9. Hall-Edwards believed "that the production of a limited amount of dermatitis is a sine qua non to successful treatment...the amount of good done is in direct ratio to the amount of dermatitis produced, so long as this does not exceed the scientific limit," at 46 and 47. See also the editorial, "X-ray Dermatitis," Arch. Rönt. Ray, 8 (October 1903) 79-82.

3. For the bactericidal effects of violet and ultraviolet light, see for example Arthur Downes (M. D.) and Thomas P. Blunt (M. A. Oxon.), "On the Influence of Light on Protoplasm," Proc. Roy. Soc., 28 (1878-9) 199-212, communicated by J. Marshall (F. R. S., Surgeon to University College Hospital); and E. Duclaux, "Influence de la lumière du soleil sur la vitalité des micrococcus," C. R. Soc. Biol. (Paris), 4 (1885) 508-10, séance du 25 juillet 1885.

4. Niels R. Finsen, 1903 Nobelist in medicine, was the inventor and prime promoter of treatment with electric arc light (often known as Finsen light), see especially his "The Treatment of Lupus Vulgaris by Concentrated Chemical Rays," tr. from La Semaine Médicale of 21 December 1897 by J. H. Sequeira in N. Finsen, Phototherapy (London: Edward Arnold, 1901). See also A. Äggebo, Niels Finsen: Die Lebensgeschichte eines grossen Arztes und Forschers, tr. from Danish by M. Backmann-Isler (Zurich: Rascher, 1946).

5. The analogies to light and electricity and the notion that something could be intermediate in character between them was also used by LeBon for his "black light," see Mary Jo Nye, "Gustave LeBon's Black Light: A Study in Physics and Philosophy in France at the Turn of the Century," Hist. Stud. Phys. Sci., 4 (1974) 163-95, at 173.

6. Most standard sources fail to make more than passing mention of the electrotherapeutic tradition. For surveys, see H. A. Colwell, "A Sketch of the History of Electrotherapy," Arch. Radiol. Electroth., 21 (1917) 320-26 and his Essay on the History of Electrotherapy and Diagnosis (London: Heinemann, 1922); see also John S. Coulter, Physical Therapy (New York: Paul B. Hoeber, 1932). An interesting account of the Viennese electrotherapists, including one Dr. Sigmund Freud, within the context of physiotherapy can be found in Erna Lesky, Die Wiener medizinische Schule im 19. Jahrhunderts (Graz-Köln: Bohlau, 1965), pp. 334-401. For a timely review of the effects of high-frequency alternating current and its therapeutic uses, see D'Arsonval, "Action physiologique et thérapeutique des courants à haute fréquence," Ann. Electro., 1 (1898) 1-28, communication faite en avril 1897 à la Société internationale des électriciens, and Oudin, "Les courants de haute fréquence et de haute tension dans les maladies de la peau et des muqueuses," ibid., 86-113. For an important contemporary text, see W. Erb, Electrotherapeutics, Vol. VI of von Ziemssen's Handbook of General Therapeutics, tr. A. de Watteville (New York: William Wood, 1887). Overshadowed by radiotherapy, electrotherapy declined rapidly after World War I because it was unable to establish itself as a creditable specialty with the bulk of physicians, except perhaps in France. Today's radiotherapy and physical therapy can, however, be traced in part to this stillborn branch of medicine.

7. A retrospective opinion poll would be difficult. The best readily available evidence for the dominance of the electrical view is that contemporary sources on both sides of the issue from 1898 through 1900 treated it as the majority view, see for example the following: F. Dollinger, "Zweiter Bericht über die Arbeiten auf dem Gebiete der Röntgenstrahlen in Frankreich," Fortschr. Röntgenstr., 2 (1898-99) 36-43 and 73-75; A. Rodet and H. Bertin-Sans (laboratoire de Microbiologie et de Physique médicale, Université de Montpellier), "Influence des rayons X sur la tuberculose expérimentale," Arch. Elec. Med., 6 (15 October 1898) 413-31; Schiff and Freund, "Rapport sur l'état actuel de la radiothérapie," Comptes-rendus des séances du 1er Congrès International d'Electrologie et de Radiologie Médicale, Paris, 27 juillet-

1er aout 1900 (Lille: Bigot Frères, 1900), pp. 218-29 with discussion; and R. Kienböck (Röntgen-Institut im Sanitorium Fürth, Vienna), "Über die Einwirkung des Röntgen-Lichtes auf die Haut," originally delivered at the k. k. Gesellschaft der Aerzte (Vienna) 19 October 1900 and printed with revisions in Wien. Klin. Wschr., 13 (13 December 1900) 1153-66. The electrical view held on even longer in some circles, see the account of a discussion at the Röntgen Society, "The Relation Between X Rays and Allied Phenomenae," Arch. Rönt. Ray, 7 (June 1902) 3-7 and also Francis R. Williams (M. D., Harvard; graduate of MIT; Visiting Physician at the Boston City Hospital; Fellow of the Massachusetts Medical Society; Member of the Association of American Physicians and of the American Climatological Association; Fellow of the American Association for the Advancement of Science), The Röntgen Rays in Medicine and Surgery (New York: MacMillan, 1901). Williams, whose book was the leading text in the United States, was still recommending the use of a grounded aluminum screen for protection of the patient in the third edition, 1903.

8. Destot, "Les troubles physiologiques et trophiques dus aux rayons X," C. R. Acad. Sci. (Paris), 124 (17 May 1897) 1114-1116, presentée par M. Bouchard.

9. Chester L. Leonard (Skiagrapher to the University Hospital and Assistant Instructor in Clinical Surgery, University of Pennsylvania), "The X-ray 'Burn': its Productions and Prevention. Has the X-ray Any Therapeutic Properties?" N. Y. Med. J., 68 (2 July 1898) 18-20. Leonard thought a grounded aluminum sheet provided "absolute protection."

10. Schürmayer (Hannover), "Die Schädigungen durch Röntgenstrahlen und die Bedeutung unserer Schutzvorrichtungen," Fortschr. Röntgenstr., 5 (1901-02) 44-48, delivered at the 73. Versammlung deutscher Naturforscher und Ärtze in Hamburg (22-29 September 1901).

11. G. A. Frei, "X-rays Harmless with the Static Machine." Elec. Eng., 22 (23 December 1896) 651, as quoted in Nauman, note 1 above, and Destot in the discussion following Schiff and Freund, note 7 above, at 228.

12. Schürmayer, note 10 above.

13. For the "finger" experiment, see E. Thomson, "Röntgen Ray Burns," Amer. X-Ray J., 3 (November 1898) 452-53. Thomson had believed from the first that the effects were due to the X-rays themselves, see E. A. Codman, "The Cause of Burns from X-rays," Bost. Med. Surg. J., 135 (19 December 1896) 610-11. Thomson, it should be noted, had developed an induction coil that was in competition with a static machine developed by Frei, note 11 above. There may therefore have been vested interests influencing both Thomson's report and Frei's. For experiments comparable to Thomson's with bacteria, see H. Rieder, "Wirkung der Röntgenstrahlen auf Bakterien," Munchen. Med. Wschr., 45 (1898) 101-104.

14. For the initiation of the surveys, see "X Ray Traumatism," Arch. Rönt. Ray, 2 (1898) 61 and ibid., p. 92, and Albers-Schönberg, "Aufforderung zu einer Sammelforschung über die Wirkung der Röntgenstrahlen auf den menschlichen Organisms," Fortschr. Röntgenstr., 1 (1898-99) 226-27. The inconclusive outcome of the Röntgen Society inquiry is apparent in E. Payne, "Notes on the Effects of X Rays," Arch. Rönt. Ray, 3 (1899) 67-69 and D. Walsh, "Focus-Tube Dermatitis," ibid., 69-73. The inconclusive outcome of the German survey, which was mounted in imitation of the English one, is apparent in the report on the 30 July 1900 session of the Congrès International d'Électrologie et de Radiologie médicales in Fortschr. Röntgenstr., 4 (1900-01) 99. A similar American inquiry did not attempt to answer the question of what was causing the X-ray injuries, leaving it to "the electricians," see N. S. Scott, "X-Ray Injuries," Amer. X-Ray J., 1 (1897) 57-66, but the editor of the journal made his position clear, "With all that has been written for the lay press, medical journals and scientific publications, I am unable to find a rational conclusion [sic] for the belief that X-rays ever injured in any instance human tissue," ibid.

15. For the reversal of the burden of proof, see the reply to Thomson of C. L. Leonard (M. D., Assistant Instructor in Clinical Surgery and Instructor in Skiagraphy, University of Pennsylvania), "Röntgen-Ray Dermatitis," Amer. X-Ray J., 3 (November 1898) 453: "...we must first eliminate all causes that experience has shown are capable of producing like results under different circumstances..."

16. Leopold Freund (aus dem pathologisch-anatomischen Universitätsinstitute und dem Institute für Radiographie und Radiotherapie in Wien), "Die physiologischen Wirkungen der Polentladungen hochgespannter Inductionsströme und einiger unsichtbaren Strahlungen," Sitzungsber. Akad. Wiss. (Wien), 109, Abtheilung III (1900) 583-654, vorgelegt in der Sitzung 12 July 1900. For a briefer statement of the mature electrical view, see E. Schiff and L. Freund (Universitätsdocent in Wien), "Der gegenwärtige Stand der Radiotherapie," Wien. Klin. Wschr., 13 (1900) 827-29, nach einem auf dem XIII internationalen dermatologischen Congresse in Paris gehaltenen Vortrage. Schiff and Freund, who were leading figures in radiology in Vienna, converted to the electrical view around 1898.

17. Ibid.

18. For a biography, see Konrad Weiss, "Robert Kienböck--80 Jahre," Strahlenth., 84 (1951) 161-64.

19. Kienböck, note 7 above. Kienböck's work was confirmed by W. Scholz (Privatdocent an der Universität Königsberg, frühere Assistenartzt an der dermatolog. Universitätsklinik zu Breslau) in a Habilitationschrift completed in June 1901, "Ueber den Einfluss der Röntgenstrahlen auf die Haut in gesundem und krankem Zustande," Arch. Derm. Syph., 59 (1902) 87-104, 241-60 and 421-45. See also the experiments in which exposure to an X-ray tube killed guinea pigs protected by a grounded metal cage, as reported by W. Rollins, "X-Light Kills," Bost. Med. Surg. J., 144 (14 February 1901) 173.

20. Kienböck, ibid.

21. Ibid., 1053-55, where an account is given of the discussion following Kienböck's oral presentation of his paper. The flaw in the experiment with bacteria was that the culture exposed to the X-rays was shielded with a grounded aluminum sheet to eliminate the effect of the static charges, while the culture exposed to the static charge around the cathode portion of the tube was not. As Kienböck pointed out, this aluminum sheet kept out sunlight as well, which is presumably what killed the bacteria in the other culture.

22. For photographs of a case of dermatitis on the hands of a physician that began in 1896 and became chronic in 1897, see J. Hall-Edwards, "Chronic Dermatitis of Both Hands," Arch. Rönt. Ray, 8 (1905) 92. Acute and chronic X-ray dermatitis were described in Oudin, Barthelemy and Darier (Paris), "Über Veränderungen an der Haut und den Eingeweiden nach Durchleuchtung mit X-Strahlen," Monat. Prak. Derm., 25 (1 November 1897) 417-46, vorgetragen auf dem Internat. medizinischen Kongresse zu Moskau (or see the short report, "Accidents cutanés causés par les rayons X," Gaz. Hop., 70 (1897) 1041-42). The distinction between acute and chronic cases was common thereafter, but apparently no further chronic cases were reported until P. G. Unna, "Die chronische Röntgendermatitis der Radiologen," Fortschr. Röntgenstr., 8 (1904-05) 67-91.

23. Payne, note 14 above.

24. E. A. Codman (Harvard Medical School and Massachusetts General Hospital), "A Study of the Cases of Accidental X-Ray Burns Hitherto Recorded," Phila. Med. J., 9 (8 March 1902) 438-42 and 499-503. Codman, who seems to have been an agnostic on the electrical view, had earlier complained in reference to Rollins, note 19 above, "Such sensational headlines as 'X-Light Kills' are apt to give the wrong impression. The fact that the X-ray is in daily use in the large hospitals without harmful results should be put in blacker type than the death of two guinea pigs," see "No Practical Danger from the X-ray," Bost. Med. Surg. J., 144 (28 February 1901) 197.

25. The most extensive, but by no means complete, survey of these cases is in G. Holzknecht (Sachverständiger für das medizinische Röntgenfahren am Landesgericht in Strafsachen in Wien), "Die forensische Beurteilung der sogenannten Röntgenverbrennungen," Fortschr. Röntgenstr., 6 (1902-03) 145-50 and 177-184. In Germany, physicians acted as expert advisors to the court in such cases. It is striking that in the United States there were already calls for medical judgments before cases came to court and for medical defense unions "to check the nefarious schemings of those pathogenic bacteria of the body politic, the 'shyster' lawyers, to whom by far the larger proportion of such suits owe their origin," in "Actions for Malpractice," N. Y. Med. J., 68 (2 July 1898) 21-22, at 22.

26. H. Gocht (Sekundärarzt der Klinik, aus der chirurgisch-orthopädischen Privatklinik des Prof. A. Hoffa in Würzburg), "Anklage wegen 'fahrlässige Körperverletzung' nach Anwendung der Röntgenstrahlen (Röntgendermatitis)," Fortschr. Röntgenstr., 2 (1898-99) 110-14.

27. For the defendant's own indignant view of the proceedings, see B. Schürmayer, "Röntgentechnik und fahrlässige Körperverletzung," Fortschr. Röntgenstr., 6 (1902-3) 24-43. Holzknecht, note 25 above, gives a different view of this case, but Holzknecht, it should be noted, was a strong supporter of Kienböck. Holzknecht himself later paid damages of £1450 for burns inflicted in 1902, see J. Rönt. Soc., 3 (July 1906) 22.

28. The case of Clarence Dally, who had worked with X-rays since 1896, is described in P. Brown, American Martyrs to Science Through the Röntgen Rays (Springfield, Illinois: Charles C. Thomas, 1936). Edison himself had this to say: "In the case of our Mr. Dally the damage is serious; but now, when we know just how continued exposure to the rays affects the living tissue, we can go ahead safely. Ample protection can be obtained by using a screen of lead about 1/4 inch thick....I...would continue the experiments myself, but my wife won't let me," from an interview in the Daily Mail as quoted in "Mr. Edison and the X Rays," Arch. Rönt. Ray, 8 (August 1903) 45. At about the same time as Dally's carcinoma was being reported, another case was demonstrated by Frieben at the Ärztliche Verein, Hamburg (21 October 1902), see the report of the ensuing amputation in Sick, "Fall von Karzinom der Haut, das auf Boden eines Röntgenulcus entstanden ist," Munchen. Med. Wschr., 50 (1903) 1445, from the report of the 23 June 1903 meeting of the Biologische Abteilung des ärztlichen Vereins Hamburg.

29. For biographies, see the obituaries in Fortschr. Röntgenstr., 28 (1921-22) 197-205 and in Strahlenth., 13 (1922) 538-48 (including a list of publications).

30. H. Albers-Schönberg (in Hamburg), "Schutzverkehrungen für Patientin, Ärzte und Frabrikanten gegen Schädigungen durch Röntgenstrahlen," Fortschr. Röntgenstr., 6 (1902-03) 235-38, reprinted from Zbl. Chir., 30 (1903) 637-41. Recommendations for protection of patients, but with an upper limit of exposure time four times as long at approximately the same distance, had already appeared in England, see the editorial "Dermatitis," Arch. Rönt. Ray, 5 (May 1901) 84-85. These recommendations were also explicitly a response to law suits for damages.

31. Max Levy-Dorn (Berlin), "Schutzmassregeln gegen Röntgenstrahlen und ihre Dosierung," *Deut. med. Wschr.*, *29*^{2} (1903) 921-24.

32. Belot, "De l'importance du dosage et de la méthode dans le traitement röntgenothérapique de quelques affections néoplasiques," *Verh. Deut. Rönt. Ges.*, *1* (1905) 184-88, at 185: "Les méthodes du premier groupe /which used only a single sitting and measured the dose/ sont plus scientifique..."

33. Robert Reid, *Marie Curie* (London: Collins, 1974), especially pp. 121, 125, and 273.

34. C. Bouchard, P. Curie and V. Balthazard, "Action physiologique de l'émanation du radium," *C. R. Acad. Sci. (Paris)*, *138* (6 June 1904) 1384-87.

35. J. Elster and H. Geitel, "Über die Aufnahme von Radiumemanation durch den menschlichen Körper," *Phys. Z.*, *5* (1904) 729-30, eingegangen 15 Oktober 1904 and S. Loewenthal, "Über die Einwirkung von Radiumemanation auf den menschlichen Körper," *Phys. Z.*, *7* (1906) 563-64.

Chapter 3: A Decade of Practical Posology, 1903-13

> The collaboration of the X-ray physicist and the X-ray technician should enable the X-ray therapist to make the exact fundamentals of research science useful for his medical practice. Only with the closest consideration of natural laws will methods be devised that achieve practical value for medical science.
>
> --"Introduction," Strahlentherapie, 1 (1912) 2.[1]

> Let us not forget...that a series of well-established clinical observations is as solid a foundation on which to theorize as are laboratory experiments upon the acceleration of an electroscopic leak...the wise physician, while familiarizing himself in so far as may be with all the advances of physics and chemistry, will regard these sciences not as infallible guides, but as handmaidens to his art. He will remember that there are more things in life than can be weighed in a balance or measured by the micro-millimetre scale.
>
> --Francis Hernamen-Johnson, M. D., "Theory and Practice in Ray Therapeutics," Journal of the Röntgen Society, 9 (1913) 86.[2]

By the beginning of World War I, radiation protection had developed dramatically from its modest beginnings in the recommendations Albers-Schönberg made in 1903. The need for protection from effects in deep-lying tissues as well as from dermatitis had been thoroughly demonstrated. A broad array of protective devices was commercially available, and professional societies were actively promoting protection measures. Both X-rays and radium were subject to quantitative measurements, but with different levels of precision and accuracy. Radium measurements by 1913 were internationally comparable to within a few parts per thousand, with the laboratory and the clinic virtually on a par in measuring capabilities. Clinical X-ray measurements were much less precise, and the laboratory and the clinic used different measuring instruments as well as different units of X-ray quantity and quality.

How did protection gain a strong hold on the medical radiological community in the decade before World War I? How did devices for protection and measurement become widespread, and why were the results so different for X-rays and for radium?

To answer these questions, I shall turn first to two discoveries, made in 1903, of effects of radiation on deep-lying tissues. The scope of radiation therapy would expand dramatically as these discoveries of deep effects were put to use in clinical practice. The discovery of deep effects would also greatly expand public and professional fears concerning the effects of radiation, as would the confirmation after 1903 of X-ray induced carcinoma. Physicians would react to these fears by claiming exclusive control over the diagnostic and therapeutic uses of X-rays. At the same time, the medical radiological community would recognize that physician control alone could not avoid harm or satisfy the public. Protection and measurement techniques were essential. With their introduction, scientific and medical radiology would be drawn closer together in principle, but the laboratory and the clinic continued to use different methods and units of X-ray measurement. In radium work, economic considerations and institutional constraints would force a much greater degree of uniformity in measurement technique. Radium protection, however, would remain a haphazard affair, with public and professional enthusiasm for the benefits of radium and radium emanation far outweighing isolated voices of concern.

Scattered reports of deep effects after radiation exposure had appeared before 1903; but they had been discounted as isolated events without any general bearing on medical radiological procedures. A published report on a stomach cancer that improved with exposure to X-rays went unnoticed, though it has often been cited since.[3] Symptoms reported as side effects of diagnostic or therapeutic irradiation--including heart palpitations, bellyaches, and nausea--were too obviously subjective to attract sustained attention.[4] Physicians would soon view the reports of illness after irradiation in a different light, and damage to the lining of the stomach and the intestines may account for at least some of the symptoms.[5] Before deep effects became well known, however, the patients' complaints were for the most part regarded as spurious. Patients, after all, were understandably excited when undergoing X-ray examination for the first time. The noisy clatter of mechanical interrupters, the mysterious glow of the tube and of the fluorescent screen, and the awesome profusion of electrical apparatus aggravated the natural anxiety accompanying this novel experience. Evidence of effects on the deeper layers of the skin likewise failed to attract attention. As early as 1898, histological investigation had suggested that the primary lesion in X-ray dermatitis was not in the most superficial layer of the skin, the epidermis, but rather in the blood vessels of the lower-lying _corium_. X-rays appeared to destroy the _tunica intima_, the elastic inner lining of the blood vessels, and the resulting expansion of the vessels caused the symptomatic swelling and redness of the skin.[6] This effect on deep-lying blood vessels was, however, attributed to a special sensitivity of the tunica intima, and it did not therefore arouse a general interest in the possibility of deep effects. So far as radium was concerned, there were before 1903 reports of fatigue among laboratory workers, but this common symptom was usually attributed to long hours of work.[7] X-rays and gamma rays were known to penetrate the body, but their biological effects were assumed to be limited in depth.

This assumption that only superficial effects occurred had some justification in Kienböck's experiments and in contemporary physics. The electrical view was rapidly forgotten, and along with it the primary purpose of Kienböck's experiments. Thereafter, Kienböck was said to have established that only absorbed radiation could cause biological effects. Rays that passed through biological material without being absorbed were believed to have no effects. Absorption of homogeneous X-rays (that is, X-rays of the same degree of hardness) and of gamma rays obeyed an exponential law:

$$I = I_o e^{-\lambda d} \qquad \text{(A)}$$

where I_o was the initial intensity of the rays, I was the intensity after passage through a material of thickness d, and λ (the absorption coefficient) was a constant that, for any given hardness of the rays, was characteristic of the material in which the absorption took place. Since intensity according to this law fell off exponentially with depth, it was reasonable to expect biological effects caused by absorbed radiation to be confined to the surface layers of tissue.

This expectation that no effect would occur in deeper tissues proved incorrect. It was a reasonable expectation that would have been supported by the weight of the evidence available early in 1903. But without a theoretical basis for predicting how large an effect should be, and without the capacity to carry out the appropriate experiments, even a vast quantity of evidence cannot prove that an effect does not occur. A few observations can, however, demonstrate that it does occur. The "no effect" assertion is therefore singularly vulnerable in the clinic; it is less vulnerable in the laboratory, where tests of a predicted effect can lead to definitive conclusions. We shall nevertheless see the "no effect" assertion made repeatedly where no experiment has been done, and even when no confirmatory experiment is possible. The "no effect" assertion is a common response to public concerns with the impact of a new technique, a response the public appears to demand and that professionals are frequently willing to provide in order to protect their profession. When the "no effect" assertion turns out to have been incorrect, as it often does, we should not however assume that there was intentional chicanery involved, though of course in specific instances there may have been. In 1903, X-ray practitioners stood to lose even more than the public from being incorrect about deep effects. With many practitioners already suffering from chronic dermatitis and the public aroused, the leadership of the medical radiological community was clearly in favor of recognizing radiation risks and taking protection measures. It is to this attitude that the rapid response to the discovery of deep effects should be attributed. But before 1903 it was overwhelmingly obvious and straightforward to assume that no deep effects occurred; it was also wrong.

The discovery of deep effects stemmed from clinical experience. Albers-Schönberg demonstrated early in 1903 that guinea pigs and hamsters could be sterilized by exposure to X-rays without causing surface lesions.[8] He left no trace of his motivations in looking for sterility, but it appears likely that he had been treating eczema of the scrotum with X-rays.[9] For a physician already concerned with the effects of X-rays, but probably unconcerned with the exponential absorption law, it would be a short step to wonder whether such therapy might be causing harm to the reproductive capacity of his patients. At about the same time, an American physician reported that he had cured a case of pseudoleukemia, a disease in which lymphoid tissue proliferates, by irradiating lymph nodes.[10] The physician in question believed pseudoleukemia, which is today known as Hodgkin's disease, to be "microbial" in origin, and he was probably working on the largely discredited assumption that X-rays had a bactericidal effect. This report of clinical success nevertheless raised in the minds of other physicians and biologists the question of whether X-rays affected the hematopoietic, or blood-forming, organs and the blood itself. Affirmative answers were rapidly forthcoming from several quarters.[11] Neither the discovery of sterilization nor the discovery of effects on the blood and blood-forming organs depended on knowledge of contemporary physics, which might even have proved misleading. Trial and error was still proving fruitful in mapping unexplored territory.

The discovery of sterilization and of effects on the blood and blood-forming organs had far-reaching implications for radiation therapy. No longer was radiation limited to the treatment of dermatological ailments. From 1903 on, deep therapy became the most exciting area for clinical trials. Rather than looking at the exponential absorption of radiation and concluding that the effects would be confined to superficial layers of tissue, practitioners began to ask how greater amounts of radiation could be delivered to deeper tissue without causing dermatitis.[12] More penetrating rays were obviously preferable. Filtering X-rays through leather or metal could remove the softer rays that were absorbed in the skin and caused dermatitis. The gamma rays of radium were soon recognized to be especially useful since they were more penetrating than the hardest rays available from an X-ray tube. If the X-ray tube or radium were moved farther away from the patient, a greater proportion of the radiation was absorbed in the deeper layers relative to the mores superficial layers of tissue.

This procedure thus enabled greater doses to be delivered to deeper tissues without causing dermatitis in the over-lying skin. Irradiation from several different directions likewise permitted greater doses to be delivered to deeper tissues.[13] Deep therapy was to take a giant step during and after World War I with the invention of hot-cathode X-ray tubes, but already in the first decade of the twentieth century there was intense interest and widespread debate concerning treatment of leukemia and other blood diseases, of uterine fibromas by irradiation of the ovaries, and of other deep-lying neoplasia.[14]

In addition to the discovery of deep effects, and the advent of deep therapy, 1903 and succeeding years saw the confirmation of X-ray induced carcinoma. Edison's assistant, mentioned in Chapter 2, was not an isolated case. In the years 1903 to 1911, fifty-four other cases were reported.[15] Of these, twenty-six were physicians, twenty-four were technicians, and four were patients. Twenty-six cases were reported in the United States, a plurality that probably resulted from the widespread experimentation with X-rays here after 1896. Germany and England each reported thirteen cases. Only two were reported in France, probably because of the continued use there of relatively low intensity tubes excited by static generators rather than by induction coils.[16] The pain of the chronic lesions was "almost unequaled by any other disease." Surgery usually proved incapable of halting the spread of the malignancies, and repeated amputations became the rule.[17] Eleven deaths were unequivocally attributed to X-ray induced carcinoma between 1904 and 1911, including the deaths of eight physicians.[18] At least one death occurred annually during those years.

X-ray induced carcinoma, along with deep effects, posed threats not only to the health of individual practitioners and patients, but also the viability of medical radiology as a profession. In this regard, the effects on the blood and blood-forming organs were at first relatively unimportant. Only after World War I, when their fatal consequences became known to the public, did blood diseases caused by X-rays begin to play a major role in the history of radiation protection, as I shall discuss in Chapter 5. In the decade before the War, the confirmation of X-ray induced carcinoma and the discovery of sterilization had the greatest impact on public and professional perceptions.

Within medical radiology, the risk of sterility aroused strong fears for the reproductive capacity of both practitioners and patients. Clinical studies soon confirmed that many radiologists were aspermatic.[19] Histological investigations in laboratory animals showed that X-rays destroyed the epithelium of the seminiferous canal; the fully developed sperm were not necessarily harmful, but the cells that produced them were destroyed.[20] Laboratory experiments showed that X-rays could also cause atrophy of mammalian ovaries.[21] The social implications were dramatic:

> Thus aspermia in the male, sterilization in the female, these are two of the most fearful discoveries that very recent experiments bring us, and is one not justified in saying that here the use of X-rays in medicine has consequences that extend into the sphere of the interests of the individual? that it touches directly on the most serious of all social problems, the reproduction of the species?[22]

Some practitioners welcomed X-rays as an ideal tool for birth control.[23] Others thought birth control fundamentally immoral and antisocial, or even unpatriotic.[24] All recognized that with the discovery of sterilization came a new and important impact on society with which medical radiology as a profession would have to reckon.

Professional fear of the public reaction to sterility was a good deal stronger than the reality would appear to justify. The newspapers seem to have continued to be more interested in the X-ray burns than in what the practitioners regarded as the more serious social peril.[25] It was, however, easy to imagine the concerns the public might express and the heightened prospects for government intervention in regulating the use of X-rays. This partly imagined threat to the profession had profound effects, just as the more tangible threat of law suits had had a few years earlier.

So far as X-ray induced carcinoma was concerned, the medical radiological profession generally viewed the moment of greatest danger as past, but public concern forced the risk of carcinoma to a high priority in protection considerations. The introduction of protection measures would, the professionals thought, avoid burns and chronic dermatitis altogether and reduce the risk of carcinoma to a negligible level. The cases occurring after 1903 were, in this professional view, the unavoidable backlog of the previous lack of caution. In the professional literature, the announcements of death became routine: "He will be long remembered for his skill and kindness, and his name will ever be inscribed in the scroll of the book of Martyrs to Science."[26] The maudlin imagery of martyrdom became standard. Beneath it, however, lay anxiety not only for the welfare of self and colleagues but also for the profession, which was threatened with government intervention and declining interest among physicians.[27] The attitude of the profession toward the victims was therefore ambivalent, including not only sympathy for their plight but also fear of the effect that excessive public reaction might have on medical radiology:

> One cannot but regret the sensational articles on the subject of burns and other unfortunate accidents that have happened to workers with X-rays. Of course we cannot but sympathize with any who in the pursuit of their profession have received injuries by incautiously exposing themselves to the rays, but by allowing accounts of these misfortunes to appear in the lay press no good is gained, and a great deal of harm is done, people have been alarmed, and many individuals to whom the application of radiations for radiograhpic or therapeutic purposes would be of great benefit refrain from seeking their aid for fear of the harmful consequences.[28]

By making the victims of X-ray induced carcinoma martyrs to science, the medical radiological community could hope to justify the loss of life and also thereby off-set the negative public reaction. Not until around 1910 was neoplasia produced in laboratory animals using radiation, but well before then the clinical discovery had become a major factor in shaping the development of medical radiology and radiation protection.[29]

Among the physician practitioners, the first line of defense for the patient, and for medical radiology, from deep effects and from X-ray induced carcinoma seemed obvious: as Albers-Schönberg had urged, diagnostic and therapeutic use of X-rays should be the exclusive preserve of physicians. On this issue, the medical radiological community sought government intervention. The newly-formed German Röntgen Society in 1905 passed a resolution urging that use of X-rays be limited by law to physicians.[30] The French Academy of Medicine followed suit in 1906, rejecting an alternative resolution that would have required physician supervision during operation of an X-ray tube by a specially trained person.[31] In England, physicians in 1905 urged legal action in a resolution of the Electrotherapeutic Society.[32] They also exerted exclusive control over the Archives of the Röntgen Ray, which had been the official organ of the Röntgen Society, and formed an Electro-therapeutic Section of the British Medical Association, thus creating a forum for medical radiology dominated by physicians.[33] This move forced the Röntgen Society, which aimed to promote cooperation between physicians and nonphysicians, to found its own Journal.[34] The Röntgen Ray Society of America, which included non-physicians, was forced to found its own Transactions when physicians took complete control of the American X-ray Journal, which expanded its scope to include electrotherapy.[35] The campaign against nonphysician practitioners appears to have fallen short of establishing any new legal prohibitions in Europe or in the United States. The effort to exert physician control over medical radiology did, however, contribute to the reduction in the number of nonphysicians in practice, a reduction that fears of the effects of X-rays had already initiated. The campaign against nonphysicians also established a split in professional institutions that was only slowly bridged during the next two decades.[36]

While physicians seized the opportunity to exert exclusive control over medical radiology, many practitioners (physician and nonphysician), technicians, and manufacturers thought further responses to the discovery of deep effects were also in order. If reserving the practice of medical radiology to physicians was wise, it was because of their general medical knowledge rather than their knowledge of X-rays. The dermatitis from which so many physician practitioners suffered, and the law suits filed against them, showed that physicians were not especially well-equipped for radiation protection.[37] Moreover, the discovery of deep effects greatly increased the need for specialized knowledge of radiation, for dosage measurements, and for protective devices. Physicians were active in meeting these new demands, but so too were nonphysicians. On the biological side, the discovery of effects on the production of germ cells and of radiation-induced carcinoma attracted the attention of research workers in biology, as we shall see in Chapter 4. On the physical side, the discoveries of 1903 intensified interest in dosage measurements and protective devices and led to a decade of practical efforts that I shall describe here in detail.

The protective devices introduced after the discovery of deep effects and of X-ray induced carcinoma in 1903 generally aimed to avoid exposure of the operator of the X-ray tube and those parts of the patient's body that were not being treated or diagnosed. In principle, all unnecessary exposure was to be avoided. Albers-

Schönberg and others advocated that the operator stand in a double-lined lead box while using an X-ray tube, which was to be enclosed in a lead box that allowed only a narrow pencil of rays to escape.[38] Lead-impregnated glass and rubber appeared on the market.[39] The glass was used to line fluoroscopic screens and in "X-ray proof" spectacles (in order to protect the practitioner during radioscopic examination) and also to manufacture X-ray tubes with unleaded windows (through which only the X-rays that were to be used could pass). Lead-impregnated rubber was preferable to lead sheet for protecting the patient's body because it was more flexible and durable as well as easier to disinfect. An English observer reported as early as 1905, after attending the founding session of the German Röntgen Society:

> A wonderful collection of shields, or 'Schutz-Apparatus' was exhibited. Gloves, aprons and spectacles were universally worn. A mannikin was exhibited clothed in armour of X-ray proof, from eyes to boots, not forgetting the mustache, the most cherished ornament of the German physician.[40]

German manufacturers continued to lead the field, but protection devices also spread rapidly to other countries.

Though protection aimed to avoid all unnecessary exposure, clinical practice required compromises. Few operators used the lead-lined boxes.[41] Radioscopy, which necessarily exposed the operator during his viewing of the fluorescent screen, remained in widespread use, both because some diagnoses required it and because it allowed more patients to be examined in less time.[42] Many practitioners took care to avoid the primary beam of X-rays leaving the tube, but they often paid no attention to the scattered

X-rays arising from materials struck by the primary beam.[43] Some practitioners thought it sufficient to merely stand behind or above the anticathode, because they mistakenly assumed, as Kienböck had, that X-rays were emitted only on the side of the anticathode struck by the cathode rays.[44] Thus, the need for protection after 1903 was not in undispute, but under the actual conditions of practice in the clinic the degree of protection achieved varied widely. The individual practitioner had to consider the need for speed, convenience and simplicity as well.

Protection measures were not, however, left entirely up to individual practitioners. Public reaction threatened the profession, and the profession responded by discussing protection at its meetings, appointing special committees, and subjecting protection to decisions of the medical radiological community as a whole. Recommendations that had community endorsement replaced the recommendations of a single wise man. The German Röntgen Society played the leading role in these developments. In this Society, the issue of physician control had not led to a serious split, and nonphysicians continued to play a major role. In England, an attempt to establish professional standards through the physician-controlled Electrotherapeutic Society of the Royal Society of Medicine failed.[45] The British Röntgen Society, in which both physicians and nonphysicians continued to participate, was the primary forum for discussion of protection measures, especially dosage measurements. In France, Austria, and the United States, countries that had active medical radiological communities but no organizations in which physicians and nonphysicians participated on an equal footing, there were few signs of organized promotion of radiation protection. This pattern is one we shall see repeated. There were two reasons for it: though the biological effects of radiation were of medical interest, the choice of protection and measurement methods involved physical and engineering questions with which physicians were not equipped to deal; and the non-physicians, including many who were associated with the manufacturers of X-ray tubes, were much more ready to take action than the physicians, whose economic interests were more directly at stake and who maintained the professional equilibrium with which they had been trained to observe disease. Like the general public, the nonphysicians in the medical radiological community were more readily shocked. In a sense, they represented the public reaction in an attenuated form, though with the advantage of being in a position to take positive action.

From its founding in 1905 until World War I, the German Röntgen Society formed several committees concerned with aspects of radiation protection. Generally, these committees originated in discussions after relevant papers at the annual congresses of the Society. The papers were often grouped together in such a way as to make the formation of a committee a natural outcome of their presentation, so it is likely that the committees reflected the interests of the Society's leadership. In 1905, at the first congress, a group of papers on dosage measurements led to the formation of a Commission for the Determination of Permanent Standards for the Measurement of X-Ray Intensity (later scaled down in its objective and renamed the Special Committee for Scientific and Practical Measurement Methods). We shall discuss below the work of this Commission, which reported in 1907 and ceased to exist in 1912. In 1909, two papers offered evidence of damage to bone during rapid growth, a danger that aroused public and professional concerns because X-rays were being used to treat ringworm in school children by epilating their scalps.[46] These papers led to a Special Committee for the Survey of the Influence of Röntgen Rays on Body Growth, which reported in 1910 that such injuries occurred in man very infrequently, but

> their possibility is nevertheless present; thus X-ray therapy shall only be engaged in by physicians, and generally by those sufficiently trained; in the hands of every layman and incompetent it is a very dangerous affair that can cause irreparable injury.[47]

When in doubt, the medical radiological community reiterated the need for physician control.

The German Röntgen Society also, however, went farther than the issue of physician control in pressing radiation protection. The Society adopted in 1910 a set of "Theses" concerning radiation damage that had been recommended to it by a physician who had frequently acted as an expert in court cases.[48] These Theses established general standards of professional conduct, including obligations to provide and use protection apparatus and dosage measurement devices, to obtain liability insurance, and to permit only experienced personnel to work in X-ray clinics. The implication was that a physician who abided by the theses would be better off in any legal action taken against him. Three years later, in 1913, the German Röntgen Society reinforced the general obligations of the Theses with an "Instruction Sheet" of protection rules, which was to be posted in X-ray clinics and in workshops where tubes were made and tested.[49] The rules established as the minimum protection during lengthy irradiations 2 mm of lead, or its equivalent in other materials. The Instruction Sheet also authorized workers in X-ray clinics and employees of tube manufacturers to refuse work if adequate protection was not provided. Otherwise, compliance was voluntary, though here again abiding by the rules might prove useful in defending against law suits. Compliance might also make it easier to obtain insurance to cover liability for injuries to patients and practitioners.[50] The 2 mm of lead was a practical compromise that did not interfere unduly with clinical manipulations. The specific figure had very little, if any, support from laboratory experimentation. Clearly, however, important developments had taken place in the wake of the discovery of deep effects and X-ray induced cancer: professional discussion of protection measures had gone far beyond the issue of physician control, and the German medical radiological community was establishing protection standards that aimed to limit doses to both patients and practitioners.

This detailed discussion of radiation protection included discussion of X-ray measurement techniques, which came into use more slowly than protection devices.[51] Dose measurements, as I noted in Chapter 2, became an issue with Kienböck's demonstration that X-rays themselves caused biological effects and with his associated suggestion that therapy should be administered in fewer and shorter sittings. One of Kienböck's supporters, Guido Holzknecht, introduced the first dosimeter designed for clinical use, a device termed a "chromoradiometer."[52] It was simply a yellow disk of unspecified composition that turned darker on exposure to X-rays. The unit of dose, H (after the inventor of the device), was one-third of the dose required to produce a slight erythema on normal adult skin. The chromoradiometer was supposed to be, above all, a practical device that any physician could utilize without unduly complicating his clinical procedures. The disk was simply placed on the part of the body to be exposed and compared with a standard scale of three shades of yellow, each corresponding to 1 H. A minimum of knowledge and manipulation was required, there was no need for the operator to understand how the color change came about, and the device could be read directly without any calculation.

The discovery of deep effects and the confirmation of X-ray induced carcinoma in 1903 vastly increased the incentive to use X-ray measurements and led to the introduction of other dosimeters. Holzknecht's chromoradiometer, though it remained in use in modified form at least until World War I, failed to attain universal acceptance. It was difficult to read because of the small differences in the shades of yellow and the variability of the available light. Holzknecht's colleagues resented his decision to keep the composition of the salts used in the chromoradiometer a secret.[53] Two French physicians designed a "pastille" made of barium platinocyanide.[54] This device was similar to Holzknecht's, but it was easier to compare with a standard scale as dehydration of the barium platinocyanide caused a color change from the bright green tint A to the dark yellow tint B with exposure to a quantity of X-rays supposedly equivalent to 3 _H_. Designed initially for measuring dosage in the treatment of ringworm, the Sabouraud-Noiré pastille became the most commonly used clinical dosage device for all therapeutic procedures before World War I, despite the dependence of the pastille readings on the light available and on the ambient temperature and humidity, and despite the requirement that it be placed halfway between the anticathode and the part of the body to be exposed.[55] Also popular was Bordier's variant of the Sabouraud-Noiré pastille, which was placed directly on the body and compared with a standard scale of four tints, corresponding to the "principal reactions required in radiotherapeutic treatment."[56] Kienböck proposed a photographic "quantimeter" based on the comparison of exposed silver bromide paper with a standard scale. His unit, _K_, was originally equal to one-half of Holzknecht's _H_, though both were later changed.[57]

These "direct" methods and units of X-ray dosage were the most commonly used ones in the decade after 1903, but almost any physical or chemical effect of X-rays could be made the basis of a new method. The release of iodine gas from a solution of iodine in chloroform, the precipitation of calomel (mercurous chloride) from a solution of ammonium oxalate and mercuric chloride, the decrease of electrical resistance of selenium, and many other X-ray effects were used to design measurement devices for the clinic.[58]

These numerous "direct" methods of measuring X-ray quantity did not exhaust the ingenuity of the inventors. Also available were a number of "indirect" methods based on the parameters of the X-ray tube rather than on measurement of its output. The quantity of X-rays produced was thought to be proportional to the current through the tube. This high-voltage "secondary" current depended on the type of interrupter used and the frequency of the interruption, so it was not related in a simple way to the low-voltage "primary" current used to excite an induction coil. The secondary current had to be measured either by a milliammeter, or by the heat generated in the anticathode or in the wall of the tube behind the anticathode. Of the various milliammeters available, the "Deprez-D'Arsonval" instrument, which had a small pivoting coil in the field of a fixed electromagnet, seems to have been most commonly used.[59] Special tubes equipped to measure the heat generated by secondary cathode rays in the wall behind the anticathode were available in Germany, but they do not appear to have been widely used even there.[60]

With some important exceptions that I shall mention below, both "direct" and "indirect" measurements of X-ray quantity were assumed before World War I to be independent of X-ray quality.

The term "dose" was used loosely, sometimes referring only to quantity and sometimes encompassing measurements of quality as well. The precise relationship among these parameters was undefined. Practitioners measured quantity and quality separately and used the results to specify clinical conditions. The effects of quality differences on measurements of quantity were generally not considered.

Since quality determined the sharpness of an image on the photographic plate, clinical methods of measuring this parameter had been in use before 1900, and the introduction of quality measurements cannot be linked directly with the need for protection. Interest in quality measurements did, however, increase markedly after Kienböck's work and the discovery of deep effects. "Hardness" of X-rays was originally associated with the degree of evacuation of the tube. It soon became apparent, however, that the essential factor was the voltage across the tube. The length of a gap in parallel with the tube at which sparks would begin to jump was the simplest method of measuring the voltage, and various "spintermeters" of this sort came into clinical use.[61] The sparking potential measured with a parallel spark gap, however, was many times the potential at which most of the current actually flowed through the tube, so several voltmeters were introduced to measure this lower, effective potential.[62] More difficult to use than the parallel spark gap, these voltmeters were less common in the clinic.

In addition to these "indirect" methods of measuring X-ray quality, there were also a number of "direct" methods.

The simplest of these was the "phantom hand," which was nothing more than a pasteboard replacement for the practitioner's own hand with which to test the image on a fluorescent screen.[63] Such devices were probably in common use during the first decade of the twentieth century, though they were mentioned only occasionally in the medical literature. More frequently mentioned, though probably less commonly used, were the direct methods that gave a numerical reading. Several of these were based on the same phenomena: the difference in the absorption of a given output of X-rays in two materials. In one common version, aluminum disks of various thicknesses were mounted with a single disk of silver. The X-rays passed through this "penetrometer" and struck a fluorescent screen. By comparison of the brightness of the spots on the screen, the thickness of aluminum that reduced the intensity of the X-rays by the same fraction as the silver could be chosen. The thicker the aluminum disk, the harder the X-rays. The results were specified according to one of a number of arbitrary scales (Benoist, Benoist-Walter, or Wehnelt), depending on the particular instrument used.[64]

For the practitioner, choosing among these different methods of measuring X-ray quantity and quality was a difficult task. Each method had an advocate, if only its inventor or manufacturer, and different clinics developed their own preferences. Reviews of the methods available became standard in the medical radiological literature, and both the British and German Röntgen Societies in

1906 and 1907 mounted efforts to compare and evaluate measurement techniques.[65] The results were inconclusive. The English survey, based on experiments undertaken by two nonphysicians, pinpointed the shortcomings of some of the methods of measuring quantity, but it failed to propose a practical solution.[66] One physician commented, "...to measure a Röntgen ray tube is very much like measuring a will o' the wisp; it is one of the most freakish and capricious things which it is possible to deal with."[67] The German survey, undertaken by a commission of physicians and nonphysicians on the basis of their collective experience, concluded that there was not enough information for a definitive choice on the basis of accuracy or on the basis of theoretical considerations. Practical considerations could therefore be overriding.[68]

This emphasis on practical considerations in selecting a measurement method contrasted sharply with emphasis on the scientific character of a medical procedure that relied on measurements. Especially in therapy, the introduction of measurements of quantity and quality brought a claim to scientific precision:

> The current great progress that radiotherapy makes each day is to learn to dose with more and more precise rigor the quantity and the quality of the rays used. Just as there is a medicamentous posology, there exists today a genuine radiologic posology. Is it not obvious that only the physician can examine and settle such delicate questions, and that it is only on this condition that radiotherapy can be a method that is scientific in its procedures, and effective in its results?[69]

The public was repeatedly assured that with these new scientific methods the mistakes of the past could not be repeated.[70] To be sure, new techniques had made both diagnosis and therapy easier and faster: self-regulating tubes that eliminated the widest variations in X-ray quality; transformers that used both phases of alternating current to produce stronger and more continuous high-voltage currents; and water-cooled anticathodes that could withstand the more intense bombardment of cathode rays.[71] Neither these inventions nor the methods of measuring X-ray quantity and quality, however, owed much to scientific knowledge or the laboratory. Within their own professional organizations, out of sight of the newspapers and the public, practitioners agreed that the methods the laboratory physicist used to measure X-ray quantity and quality, which I shall describe shortly, were unsuitable for use in the clinic.[72]

How good, or bad, were the clinical measurements? The answer depended on the needs of their users. X-ray practitioners were generally satisfied with the techniques available for measuring quality directly. Only an occasional voice of concern was raised over the known inhomogeneity of the rays from most tubes, and the consequent ambiguity in a specification like "Benoist 5," a reading that could result from X-ray beams that differed significantly in the quality of the rays of which they were composed. There were anomalies in the readings of quality when X-rays were filtered through one of the materials of which a penetrometer was made, since the secondary rays arising from a given material were known to be readily transmitted through that same material. Practitioners did not, however, acknowledge this difficulty.[73] Many physicians doing superficial therapy were also satisfied before World War I with the most commonly used devices for direct measurement of X-ray quantity, or with devices of their own design. As one English physician put it, "I have always three or four means of measurement at hand. I have been working with Kienböck's method, as well as with those of Bordier and Sabouraud, and with the meter I have described to you, and they all work very exactly together."[74] It was among the practitioners doing deep therapy that serious practical and conceptual problems arose. Working at the limits of contemporary capabilities, deep therapists posed questions that would prove of interest to physics as well as to medicine.

Two related practical problems arose for the deep therapist. First, he had to choose a filter that would enable him to deliver higher doses to deep-lying tissues without causing damage to the patient's skin. Aluminum and leather were most commonly used, but some physicians claimed that very thin silver filters were preferable.[75] The advocates of aluminum and of filter generally believed that the filter raised the average hardness of the X-ray beam by absorbing more of the softer than of the harder rays, while the advocates of silver claimed that it selectively removed only those rays that caused skin burns.[76] Secondly, the deep therapist had to measure doses and compare them with doses measured by his colleagues, who used different X-ray tubes and filters and therefore rays of different quality. Such comparisons made it apparent that the existing techniques of measuring X-ray quantity did not give comparable results when quality changed. In France, it appeared that barium platinocyanide varied widely in its response to X-rays of different qualities. As a result, the H units measured in Lyons were "very different from those measured by our colleagues in Paris," perhaps different by a multiplicative factor of four or five.[77] In Germany, deep therapists at Freiburg who used Kienböck's silver bromide strips to measure doses reported delivering safely as much as 200 times the dose normally required to produce erythema to the skin of women whose ovaries were being irradiated.[78] Most deep therapists did not believe that such high doses were possible without causing harm.[78]

Related to both the filtration problem and the problem of measuring doses was a question that began to attract experimental attention around 1910: did the harder rays used in deep therapy have the same biological effects as the softer rays used for superficial therapy? Many practitioners believed that softer rays were more efficacious since softer tubes often caused epilation and erythema more quickly during diagnosis and brought about more rapid results in superficial therapy, but this clinical observation failed to take into account the different absorption of harder and softer rays in the skin. The experiments undertaken to resolve the question of the variation of biological effects with quality yielded ambiguous answers.[80] At the same time, these experiments helped to clarify the concept of dose, to separate it from the notion of quantity, and to reveal the shortcomings of the clinical measurement methods in use.

The clearest pre-War statement of these developments was in a review of measurement techniques prepared in 1913 by Theophil Christen, a Swiss physician who had obtained a doctorate in mathematics before turning to medicine.[81] To Christen, it was essential to distinguish between the quantity or intensity of X-rays, which he dentified as the energy passing through a given surface, and the dose, which he defined as the energy absorbed in a given volume. This physical dose, which was not necessarily equivalent to the biologically effective dose, was proportional to intensity, and, since the harder rays were less absorbed, inversely proportional to hardness. With this distinction in mind, the usefulness of the existing direct clinical techniques became highly doubtful. As instruments to measure the quantity of X-rays moving through a given surface, they were inadequate because their readings depended on the absorption of the X-rays in a test body like barium platinocyanide, and this absorption varied with hardness. As instruments to measure the dose of X-rays absorbed, they were inadequate because there was no guarantee that absorption in the test body was similar to absorption in a human body. The measurement of absorbed dose would, as we shall see in Chapter 5, provide the occasion for much further discussion and lead to the adoption of a less practical posology based on ionization methods, which were virtually unknown in the clinic before World War I. For the moment, however, Christen's 1913 definition of absorbed dose was a purely theoretical statement, with no instrumental means of entering clinical practice. Despite efforts to design more convenient devices for measuring the ionization of air caused by X-rays, ionization methods continued to be considered suitable apparatus for the laboratory, but not for the clinic.[82]

There were several reasons for this lack of acceptance of ionization methods among X-ray practitioners. The ionization methods were more precise, but they required calculations. Practitioners preferred instruments that would provide an immediate reading. Medical practitioners also needed measurements that were comparable among different clinics. Ionization measurements were comparable only when made with the same instrument, and there was no means of comparing X-ray quantity as determined by ionization in different laboratories. Moreover, many practitioners considered the chemical changes they used to measure X-ray quantity more appropriate to producing biological effects than ionization. The prevailing assumption was that biological effects were basically chemical, not physical. There was a continuing hope that one of the chemical methods of measurement would parallel the desired therapeutic effects. As we shall see, ionization was to become the focus of reductionist notions among physicians and biologists, but to the X-ray practitioner before World War I the word still meant little.

Physicists had generally preferred ionization as the basis for their measurements of X-ray quantity and quality since the discovery in 1896 that X-rays ionized air. If air exposed to X-rays was placed in an electric field, there was a "leakage" current that could be used to measure the ionization, which physicists assumed to be a measure of X-ray quantity or intensity. Two types of devices were used to measure the amount of charge produced. In an electroscope, two pieces of gold leaf or other conductor were hinged to an insulator so that they diverged when charged to a high voltage. The rate of fall of the gold leaf was proportional to the leakage current and therefore provided a measure of the ionization of the air. Alternatively, the ionization could be produced between the plates of a condenser charged to a high voltage, and any one of a number of sensitive electrometers could be used to measure the leakage current. Generally, the smaller capacity of the electroscope made it preferable for measuring smaller amounts of ionization. In both devices, it was essential that the voltage be above a certain minimum, the saturation voltage, at which the rate of discharge of the electroscope or the current measured by the electrometer became independent of the applied voltage. As Ernest Rutherford and J. J. Thomson had explained, the saturation voltage was the voltage required to prevent recombination of the ions before they reached the gold leaf of the electroscope or the parallel plates of the condenser.[83]

For specifying X-ray quality, the physicist used the absorption coefficient (λ in equation A above) or sometimes the thickness of a material required to reduce the X-ray intensity to half its original value (the half-value layer). To determine the absorption coefficient or the half-value layer, the physicist generally used ionization measurements of X-ray intensity after passage through aluminum.

Medical users of radium, unlike X-ray practitioners, relied heavily on these ionization methods in their clinical work. The need for protection, which had been so important to the introduction of dosimetry to the X-ray clinic, did not play a role in the adoption of measurement techniques for radium. Therapy with radium and radium emanation passed through an initial period of almost unqualified enthusiasm from about 1903 to about 1906 and thereafter survived a period of scepticism among physicians who thought exaggerated claims had been made. By 1910 radium therapy was recovering with a more realistic estimate of its potential, which seemed high in the treatment of some dermatological ailments, cancerous growths and arthritis. The public had remained enthusiastic throughout, with many people "trying radium" for almost any ailment that could not be treated in some other way.[84] After 1910, enthusiasts and sceptics, laymen or physicians, were happy to see the establishment of medical radium institutes.[85] To the enthusiasts these institutes appeared to offer readier availability of radium treatments. To the sceptics, the radium institutes meant more rigorous control of the clinical trials. Radium emanation by 1910 was widely available in Continental Europe at health spas.

In this widespread administration of radium and radium emanation, very few patients suffered acute clinical harm. There had been at least one death of a laboratory worker due to a burn caused by radium "imprudently carried in his pocket," but this incident does not appear to have aroused public concern.[86] Ernest Rutherford, Britain's leading research worker with radioactivity, told the Röntgen Society in 1911 that radium had never affected him and that his assistant simply wore rubber gloves to avoid damage to his hands.[87] Without public protest, radium protection continued to lag behind X-ray protection as a subject of professional concern, and protection was thus not the reason for the use of ionization methods in radium clinics.

The reasons for the reliance on ionization methods among medical users of radium were largely practical. The sources of radium, as I have noted, were few. After 1903, when the Curies won the Nobel Prize and the therapeutic effects of radium started to be widely discussed, the price of radium rose to about $100 per milligram, where it stayed until the opening of the Congo uranium mines around 1922.[88] At this astronomical price, precise measurements of the purity and amount of radium being sold, or even loaned, were an obvious commercial necessity. For the ordinary practitioner, the price of radium was so high that it was entirely out of reach. A few biologists and research-oriented physicians, were, however, able to borrow radium from the Curies, from the Viennese Academy of Science, from the German manufacturer Giesel, from the French manufacturer Armet de Lisle, or from individual physical laboratories. With the radium generally came instruction in ionization measurements.[89]

There was no difficulty obtaining radium emanation, since it occurred naturally in mineral waters, but in concentrations that could only be detected by ionization techniques. The sensitive electroscopes required were readily available. Physicists had been using them extensively before 1898, and also thereafter, for the study of atmospheric electricity.[90] With the discovery of radium emanation in mineral springs, physicians and nonphysician practitioners at health spas learned quickly how to use electroscopes, though not without making serious errors.[91] By 1910, ionization measurements of radium emanation for medical purposes were common. In Germany and Austria, the results were most often expressed in the "Mache-unit," which was the amount of emanation that would produce a charge of one one-thousandth of an electrostatic unit in a given electroscope.[92] In France, the unit in which amounts of radium emanation were expressed was usually the milligram-second, which was the amount of emanation produced by a one milligram sample of radium in one second.[93] In England and the United States, both units were used, as was the amount of ionization produced by a given amount of uranium.

The ionization measurements used in scientific radiology and in radium work were much more precise than the clinical X-ray dosage techniques physicians regarded as "scientific." By 1913, radium measurements were comparable on an international basis to one part in a hundred, an achievement in standardization that I shall discuss further in Chapter 6. Physicists nevertheless emphasized the practical character of ionization methods. The physicist knew only too well that the process of ionization was not one that he understood. By 1906, it was clear that the prevailing theory of X-rays and gamma rays, the pulse theory described briefly in Chapter 2, could not account readily for the expulsion of an electron from an atom. The pulse, which spread as it left the source of the X-rays or gamma rays, would decrease in intensity with the square of the distance. No single pulse would have enough energy localized in a given direction to cause ionization and to give the secondary electrons as much energy as they were known to have.[94] For the physicist, the "scientific" way of measuring the energy of X-rays or gamma rays was to measure the heat produced when they were absorbed. The physicist's attitude toward this procedure was similar to the X-ray practitioner's attitude toward ionization measurements: theoretically desirable, but impractical. Only occasionally was the heat produced by X-rays and gamma rays measured directly, sometimes producing inexplicable results.[95] For most purposes, ionization measurements sufficed.

Thus, by 1913 the contrasts between X-ray and radium dosimetry were striking. X-ray practitioners used measurements of dose in large part for purposes of protection and emphasized the scientific character of the therapy they administered. In fact, however, clinical X-ray techniques were unknown to scientific radiology and were designed to meet practical requirements. Medical users of radium, disregarding the question of protection, nevertheless used the more precise ionization techniques familiar to the scientific laboratory. These techniques were, however, regarded as practical, not scientific.

1. "Zur Einführung," Strahlenth., 1 (1912) 2: "Die Mitarbeit der Röntgenphysiker und Röntgentechniker soll den Röntgentherapeuten instand setzen, sich die exakten Grundlagen der forschenden Naturwissenschaft für seine medizinische Praxis nutzbar zu machen. Nur unter genauester Berücksichtigung der Naturgesetze können Methoden ersonnen werden, die für die medizinische Wissenschaft praktischen Wert erlangen."

2. The occasion for this statement requires explication. Hernamen-Johnson had, following J. J. Thomson's suggestion, attempted to use the characteristic X-rays discovered by C. J. Barkla and C. A. Sadler as a therapeutic agent, see J. J. Thomson, (Cavendish Professor of Experimental Physics, Cambridge; Professor of Physics, Royal Institution), "Röntgen Rays in Therapeutics: A Suggestion from a Physicist," Brit. Med. J., 2 (1910) 512-14, an address to the Section of Radiology and Medical Electricity of the British Medical Association, July 1910; and Francis Hernamen-Johnson (M. D.), "Secondary X-radiations: Their Uses and Possibilities in Medicine," Proc. Roy. Soc. Med., 5^1 (1911-12), Electro-therapeutic Section 87-111, session of 16 February 1912. Hernamen-Johnson announced success with characteristic X-rays before realizing that his equipment permitted the primary beam as well as the secondary rays to strike the part being treated. The outburst quoted above came when he realized the error, which he blamed entirely on the physicist.

3. V. Despeignes (Ancien chef des travaux à la Faculté de médicine de Lyon), "Observation concernant un cas Cancer de l'estomac, traités par les rayons röntgens," Lyon méd., 82 (1896) 428-30 and 503-506.

4. Oudin, Barthelemy and Darier (Paris), "Über Veränderungen an der Haut und den Eingeweiden nach Durchleuchtung mit X-Strahlen," Monat. Prak. Derm., 25 (1 November 1897) 417-45.

5. M. Seldin (Dr. Med., Bobruisk, Russland), "Über die Wirkung der Röntgen und Radiumstrahlen auf innere Organe und den Gesamtorganismus der Tiere," Fortschr. Röntgenstr., 7 (1904) 322-39, submitted as an Inaugural Dissertation in March 1904 for the Medical Faculty at the University of Königsberg. Damage to the stomach and intestines was first demonstrated by Cl. Regaud, Th. Nogier and A. Laccasagne, "Sur les effets redoutables des irradiations étendues de l'abdomen et sur les lésions du tube digestif determinées par les rayons de

Röntgen," Arch. Elec. Med., 21 (1912) 321-34, communication presentée au Congrès de l'Association française pour l'Avancement des Sciences à Nimes, en août 1912. It should be noted, however, that by 1912 much harder X-rays were in use (those of Regaud, et. al., were filtered through 2 mm of aluminum) and many of the earlier reports may have been spurious.

6. A. Gasmann (aus der dermatologischen Universitätsklinik des Herrn. Prof. Jadassohn in Bern), "Zur Histologie der Röntgenulcera," Fortschr. Röntgenstr., 2 (1898-99) 199-207.

7. Fatigue among radium workers was later regarded as the result of effects on the blood, see F. Gudzent (Assistent der I. Medizinischen Klinik) and L. Halberstaedter (Assistenten des Instituts), "Über berufliche Schädigungen durch radioaktive Substanzen," Deut. med. Wschr., 40 (1914) 633-35, aus dem Radiuminstitut für biologisch-therapeutische Forschung der Charité in Berlin (Direktor: Geheimrat His).

8. Albers-Schönberg (Dr. med.), "Ueber eine bisher unbekannte Wirkung der Röntgenstrahlen auf den Organismus der Tiere," Munchen. Med. Wschr., 50^2 (27 October 1903) 1859-60.

9. The routine character of this procedure is evident from a later statement of Holzknecht's: "Dieser schweren Schädigung [sterility] müssen auch alle Patienten verfallen, deren Skrotum mit üblichen therapeutischen Dosen (Ekzem) beschickt wird," in a discussion at the German Röntgen Society, Verh. Deut. Rönt. Ges., 1 (1905) at 239.

10. N. Senn (M. D., surgeon), "The Therapeutical Value of the Röntgen Ray in the Treatment of Pseudoleukemia," N. Y. Med. J., 77 (18 April 1903) 665-68.

11. For effects on the spleen and lymph nodes, see H. Heineke (Assistent der chirurgischen Klinik in Leipzig), "Ueber die Einwirkung der Röntgenstrahlen auf Tiere," Munchen. Med. Wschr., 50 (1903) 2090-92 and "Ueber die Einwirkung der Röntgenstrahlen auf innere Organe," 51 (1904) 785-86. For effects on the blood and on bone marrow, see Ch. Aubertin and E. Beaujard, "Action des rayons X sur le sang et les organes hématopoiétiques," C. R. Soc. Biol. (Paris), 58 (4 février 1905) 217-19, laboratoires de MM. Béclère et Blum.

12. G. Perthes (aus dem chirurgische-poliklinischen Institut der Universität Leipzig), "Versuch einer Bestimmung der Durchlässigkeit menschlicher Gewebe für Röntgenstrahlen mit Rücksicht auf die Bedeutung der Durchlässigkeit der Gewebe für die Radiotherapie," Fortschr. Röntgenstr., 8 (1904) 12-25.

13. See, for example, Holzknecht, "Die Lösung des Problems in der Tiefe, gleich viel und mehr Röntgenlicht zu applizieren, wie an der Oberfläche (Homogen- und Zentralbestrahlung)," Verh. Deut. Rönt. Ges., 4 (1903) 73-74.

14. For a hint of how quickly deep therapy developed, see the following: on leukemia and other blood diseases, the review by P. Krause (Privatdozent, Breslau), "Zur Röntgenbestrahlung von Bluterkrankungen (Leukaemie, Pseudoleukaemie, Lymphomatosis, perniciöse Anemie, Polycythaemia mit Milztumor)," Fortschr. Röntgenstr., 8 (1904-05) 209-35; on uterine fibromas and other gynecological ailments, see the annual reports by H. Albers-Schönberg, "Röntgentherapie in der Gynäkologie," (the title varies slightly) in Verh. Deut. Rönt. Ges., 5 (1909)-8 (1912).

15. Otto Hesse (Assistent der Kgl. medizin. Univ.-Poliklinik in Bonn, Direktor Prof. Dr. Paul Krause), "Das Röntgenkarzinom," Fortschr. Röntgenstr., 17 (1911) 82-92, which is presumably an abbreviated version of his Symptomatologie, Pathogenese und Therapie des Röntgenkarzinoms (Leipzig: J. A. Barth, 1911). Krause reported the same figures in his "Zur Kenntnis der Schädigung der Haut durch Röntgenstrahlen. 3. Beitrag zur Kenntnis des Röntgenkarzinoms," Verh. Deut. Rönt. Ges., 7 (1911) 101-104. The fifty-four cases did not include malignancies that developed after the treatment of lupus vulgaris, which was thought to develop frequently into carcinoma, see J. Belot (chef du service d'électrotherapie et de radiologie du docteur Brocq à l'hôpital Saint-Louis), "La radiothérapie ne donne pas les cancers," Bull. Soc. Radiol. Med. (Paris), 2 (1910) 34-44.

16. B. Walter (Hamburg), "Bericht über die Röntgenausstellung des 2. Internationalen Kongresses für medizinischen Elektrologie und Radiologie in Bern, 1-6 September 1902," Fortschr. Röntgenstr., 6 (1902-3) 56-58. Italian manufacturers also showed "influence" machines at this exposition.

17. The quotation is from Charles Allen Porter (M. D.), a surgeon who reported on many operations and skin grafts he had done on 47 cases of chronic dermatitis in "The Pathology and Surgical Treatment of Chronic X-Ray Dermatitis," Trans. Amer. Rönt. Ray. Soc. (1908) 101-70, at 159.

18. Krause, note 15 above.

19. F. Tilden Brown (M. D.) and Alfred T. Osgood (M. D.) (New York), "X-rays and Sterility," Amer. J. Surg., 18 (1905) 179-82. All of those examined who had done extensive X-ray work for more than three years showed no spermatozoa in the seminal fluid, but none had suffered obvious effects on the scrotum.

20. J. Bergonié and L. Tribondeau, "Actions des rayons X sur le testicule du rat blanc," C. R. Soc. Biol. (Paris), 57 (Réunion biologique de Bordeaux, séance du 8 novembre 1904) 400-402; ibid. (séance du 6 décembre 1904) 592-95; and C. R. Soc. Biol. (Paris), 58 (Réunion biologique de Bordeaux, séance du 17 janvier 1905) 154-58. See also Frieben (Dr., aus dem Röntgen-institut und Institut für medizinische Diagnostik von Dr. Albers-Schönberg und Dr. Frieben, Hamburg), "Hodenveränderungen bei Tieren nach Röntgenbestrahlungen," Munchen. Med. Wschr., 50^2 (1903) 2295.

21. J. Bergonié, L. Tribondeau and D. Récamier, "Action des rayons X sur l'ovaire de la lapine," C. R. Soc. Biol. (Paris), 58 (Réunion biologique de Bordeaux, séance du 17 février 1905) 284-86 and L. Halberstaedter (Assistenarzt der dermatologischen Universitätsklinik zu Breslau, Dir. Geheimrat Prof. Dr. Neisser), "Die Einwirkung der Röntgenstrahlen auf Ovarien," Berlin. Klin. Wschr., 42^1 (16 January 1905) 64-66. Halberstaedter got the same results with radium bromide as with X-rays.

22. "Rapport sur les conditions légales de l'emploi médicale des rayons Röntgen," au nom d'une Commission de MM. Brouardel, Debove, Gariel, Gueniot, Hanriot, Motet, C. Perier, Pouchet et Chauffard, rapporteur, Bull. Acad. Med. (Paris), 55 (1906) 50-64 and the subsequent discussion, 76-95, at 55: "Ainsi azoospermie chez l'homme, stérilization chez la femme, telles sont deux des plus redoutables revelations que nous apportent des expériences toutes récentes, et n'est-on pas en droit de dire qu'ici la Röntgenisation déborde par ses

conséquences le cadres des intérêts individuels? qu'elle touche directement à la plus grave peut-être de toutes ces questions sociales, à la reproduction de l'espèce?" The Commission had been set up in response to a proposal by Debove, who was concerned with the "péril social" posed by the sterility of women, "Sur l'emploi des rayons Röntgen," Bull. Acad. Med. (Paris), 53 (1905) 486, séance du 23 mai 1905.

23. Philipp (Aus Dr. Philipp's Röntgeninstitut in Bonn), "Die Röntgenbestrahlung der Hoden des Mannes," Fortschr. Röntgenstr., 8 (1904) 114-19: "Was aber diese Versuche für den Arzt Besonders lockend machen musste, war die Aussicht, eventuell hierdurch ein langersehntes soziales Heilmittel zugewinnen, in der Form einer bequemen und schmerzlosen Sterilisierungsmethode," at 116. Dr. Philipp described two successful male sterilizations.

24. Hennecart (prakt. Arzt), "Nécessité d'une législation spéciale pour les Rayons Röntgen," Verh. Deut. Rönt. Ges., 1 (1905) 205-209. Hennecart was especially concerned that women might seek sterilization and that existing laws did not prohibit it: "N'est-ce pas un de leurs devoirs les plus essentiels de favorisier tout ce qui peut contribuer à la richesse de leur pays, au developpement de sa population?...Je suppose le cas suivant, qui serait le plus commun. Une femme saine est soumise sur sa demande ou sur son contentement, à l'action des Rayons Röntgen dans le but de supprimer sa fonction de réproduction. Il ne s'en suit aucun accident (röntgendermite). Elle devient à jamais sterile. Cette femme, le ou les opérateur (médicins ou non-médicins) sont-ils possibles d'une peine quelconque?" (at 206). Hennecart then surveyed the existing French legislation and concluded that the answer was no.

25. The continuing interest in X-ray dermatitis was annoying to practitioners, who anxiously assured the public that the greatest danger was to the operator, see "Editorials," Arch. Rönt. Ray, 8 (September 1903) 63-4.

26. "Mr. Wilson of the London Hospital," J. Rönt. Soc., 7 (April 1911) 48-9. Mr. Wilson was a "lay worker," a fact that may have made this announcement more modest than the usual.

27. For the expectation of government intervention, see for example Max Levy-Dorn (Berlin), "Schutzmassregeln gegen Röntgenstrahlen und ihre Dosierung," *Deut. med. Wschr.*, 29^{2} (1903) 921-24 and Alfred E. Dean, "Les victimes de la radiodermite en Angleterre," *Arch. Elec. Med.*, *16* (1908) 484-87, at 486. For the shortage of personnel, especially in radioscopy, see A. D. Reid, "Presidential Address: Survey of the Year's Work in Electrotherapeutics," *Proc. Roy. Soc. Med.*, 5^{1}, Electrotherapeutical Section (1911-12) 1-8, at 5: "The inducement at present offered to medical men to take up this work, which under the best conditions is one of danger to health, is at present totally inadequate, and we are conscious of the fact that at present very few names are known to us as entering this branch. Several of the small hospitals find it impossible to get medical men to undertake the charge of their departments, and undoubtedly there will be not only a shortage but a dearth of men who will be willing to run the risk of devoting their lives to radiology."

28. *J. Rönt. Soc.*, *3* (1903) 49.

29. For one effort that produced hyperplasia but no real tumor, see C. W. Rowntree (Hunterian Professor at the Royal College of Surgeons and Surgical Registrar at the Middlesex Hospital), "X ray carcinoma, and an experimental inquiry into the conditions which precede its onset," (Hunterian Lecture at the Royal College of Surgeons, 17 March 1909) *Lancet*, *1* (20 March 1909) 821-24. Experimental success in producing a neoplasm was first reported by P. Marie, J. Clunet and G. Raulot-Lapointe, "Contribution à l'étude du développement des tumeurs malignes sur les ulcères de Röntgen," *Bull. Ass. Franc. Cancer*, *3* (1910) 404-26.

30. "Der Röntgenkongress erklärt: Die Untersuchung und Behandlung mit Röntgenstrahlen ist eine rein ärztliche Leistung. Dem muss in der allgemeinen und der Medizinalgesetzgebung Rechnung getragen werden. Auch diejenigen Ärtze, die Röntgenuntersuchungen von anderen machen lassen, müssen dies beachten," see *Verh. Deut. Rönt. Ges.*, *1* (1905) 240. The proposal for this resolution originated with Hennecart, note 24 above, who suggested it because he thought that physicians would not perform an immoral act like sterilization: "Le souci de notre dignité professionelle et de notre bon renom auprès de la clientèle est un frein suffisament puissant." From the discussion of the resolution, it can be inferred that this view was not unanimously held.

31. "Considérant:

Que l'emploi médical des rayons Röntgen peut déterminer des accidents graves;

Que certaines pratiques peuvent créer un danger social;

Que seules les docteurs en médicine, officier de santé ou dentistes diplômes (en ce qui concerne la pratique odontologique) sont capables d'interpreter les résultats obtenus au point de vue diagnostic et du traitement des maladies:

L'Académie est d'avis que

L'application médicale des rayons Röntgen, par des personnes non pourvues des diplômes ci-dessus, constitue un acte d'exercice illégal de la medicine," note 22 above, at 64, with approval voted at 95. The alternative resolution, which failed to gain any significant support in the discussion, is at 81:

"L'Académie est d'avis:
1° Qu'un enseignement soit institué pour la pratique des rayons Röntgen;
2° Que nul ne puisse, sans un diplôme special et sans le contrôle médicale, faire l'application des rayons Röntgen;
3° Que les positions officielles acquises et justifiées par des travaux anterieurs soient respectées."

Efforts to limit medical radiology to physicians had begun earlier in France, see Antoinette Béclère, Antoine Béclère (Paris: J. B. Ballière, 1972) 58-60.

32. See the reference to this resolution, passed unanimously in John Hall-Edwards, "On X-ray Dermatitis and its Prevention," Proc. Roy. Soc. Med., Electrotherapeutical Section, 2 (20 November 1908) 11-34, at 25.

33. The change in the journal occurred with the November 1903 issue, when a nonphysician (Ernest Payne, M. A. and A. I. E. E.) was dropped as an editor, leaving a physician (J. Hall-Edwards, (L. R. C. P. (Edinburgh) and F. R. P. S.) in charge. At the same time the title was changed to Archives of the Röntgen Ray and Allied Phenomena (namely, phototherapy, electrotherapy and thermotherapy) and an editorial announced the intention "to safegard as far as possible the interests of the medical profession," Arch. Rönt. Ray, 8 (1903-4) 95. The Electrotherapeutic Section of the British Medical Association was formed in July 1903, see the "Programme of Annual Meeting," a supplement to Brit. Med. J., 175 (1903).

34. The Journal of the Röntgen Society appeared in July 1904 with the explanation that the Archives, which before November 1903 had been "the only journal in which the transactions of the Röntgen Society of London are officially reported," no longer had a member of the Society as an editor.

35. This development had begun before the discovery of deep effects, as physician electrotherapists sought control of the American X-ray Journal. In 1902, a "publisher's announcement" declared that the journal would "devote its columns to the education of the medical profession in X-Ray and Electro-Therapeutical Practice." This announcement followed the sale of the journal by its founder, Heber Robarts (M. D., M. E.), to T. Proctor Hall (Ph. D., M. D.), see Amer. X-Ray J., 11 (1902) 1114-15. The official version of this story, told in the anonymous The American Röntgen Ray Society, 1900-1950 (Springfield, Illinois: Charles C. Thomas, 1964) pp.5-6 and sanctioned in Ruth and Edward Brecher, The Rays: A History of Radiology in the United States and Canada, (Baltimore: Williams and Wilkins, 1969), at 304, would have it that Robarts was "euchred" out of his journal by the electrotherapists, but the contemporary evidence indicates that Robarts himself would have been counted among the electrotherapists, see H. R., "Injurious Forces from X-ray Tubes," Amer. X-Ray J. (1902) 1049-50. The Transactions of the American Röntgen Ray Society first appeared in 1903 with a report of the third annual meeting (10 and 11 December 1902).

36. In 1908, the English physician who had proposed the resolution for physician control at the Electrotherapeutic Society commented on the failure to obtain a legal prohibition, "Parliament as a whole is not at all friendly to the medical profession, and it considers that profession is capable of taking care of itself." At the same time, he admitted, "the X-rays, at any rate in the provinces, are not very much used by the quacks. I think that quacks have been frightened by them," see Hall-Edwards, note 32 above, at 25.

37. This point was made forcefully by Paul Reynier during the debate on medical control at the French Academy of Medicine. In introducing the alternative resolution quoted in note 31 above, he exclaimed: "Croyez-vous qu'il suffise de dire que la radiologie sera du ressort exclusivement médical pour... éviter [les accidents]? Helas! trop de procès où des médicins ont été condamnés à des dommages et intérêts pour brûlures sont là pour démontrer qu'il ne suffit pas d'être médecin pour manier sans accident ces terribles rayons!" Generally, the reply was that physicians should nevertheless

be in charge because they had diplomas and licenses, see Debove, note 22 above, at 92 and "Dangers of X Rays," reprinted from The Family Doctor of 5 September 1903 in Arch. Rönt. Ray, 8 (October 1903) 84.

38. H. Albers-Schönberg, "Technische Neuerungen," Fortschr. Röntgenstr., 7 (1903-04) 137-49.

39. For the introduction of lead-impregnated rubber, see G. Holzknecht and R. Grünfeld (aus dem Röntgenlaboratorium des k. k. allgemeinen Krankenhauses in Wien), "Ein neues Material zum Schutz der gesunden Haut gegen Röntgenlicht und über radiologische Schutzmassnahmen im Allgemeinen," Munchen. Med. Wschr., 50^2 (14 July 1903) 1202-1205.

40. W. Deane Butcher, "The Röntgen Congress at Berlin," J. Rönt. Soc., 2 (July 1905) 6-10.

41. Levy-Dorn, note 27 above.

42. See the French Academy's report, note 22 above, at 52: "l'examen radioscopique est le plus économique des procédés (et pour la pratique hospitalière un tel avantage est capital)."

43. B. Walter (Hamburg), "Über den Schutz des Untersuchers gegen sekundäre Röntgenstrahlen," Verh. Deut. Rönt. Ges., 6 (1910) 51-57.

44. For evidence of this assumption, see the drawings in Robert Kienböck (aus dem Röntgen-Institut im Sanatorium Fürth in Wien), "Ueber die Einwirkung des Röntgen-Lichtes auf die Haut," Wien. Klin. Wschr., 13 (13 December 1900) 1153-66 and the comments on the dangers of secondary radiation from the glass walls of the tube in B. Walter, note 43 above. Many tube boxes before World War I appear to have been built without backs, probably to facilitate cooling and to avoid what was considered an unnecessary expenditure.

45. The Section passed a resolution in 1908 calling for a committee to consider protection measures and to formulate rules, see the discussion following Hall-Edwards, note 36 above, at 31. This proposal was to be brought before the council of the Royal Society of Medicine at its next meeting, but there is no indication in the succeeding Proceedings of the Section that the committee was created.

46. Forsterling (Mörs, Niederrhein), "Wachtumsstörungen nach Röntgenbestrahlung," and Krukenberg (Elberfeld), "Gehirnschädigung durch Röntgenbestrahlung," Verh. Deut. Rönt. Ges., 5 (1909) 68-75. In England, there were similar concerns, precipitated by a decision of the Education Committee of the London County Council requiring X-ray treatment of ringworm in school children, see Dawson Turner (M. D.), letter to The Times (30 March 1909); "The Röntgen Ray Treatment of Ringworm," Lancet, 1 (15 May 1909) 1399-1400; J. M. H. Macleod (M. A. St. And.; M. D. Aberd.; M. R. C. P. Lond.; Physician for Diseases of the Skin, Victoria Hospital for Sick Children, Chelsea; Assistant Physician for Skin Diseases, Charing Cross Hospital; Lecturer on Dermatology, London School of Tropical Medicine), "The X Ray Treatment of Ringworm of the Scalp With Special Reference to the Risks of Dermatitis and the Suggested Injury to the Brain," ibid., 1373-77; H. G. Adamson (M. D. Lond.; M. R. C. P. Lond; Physician for Diseases of the Skin, St. Bartholomew's Hospital), "A Simplified Method of X Ray Application for the Cure of Ringworm of the Scalp: Kienböck's Method," ibid., 1378-81; Dawson F. D. Turner (B. A., M. D., F. R. C. P. Edin., F. R. S. E., Lecturer on Medical Physics, Surgeons' Hall, Edin.; Examiner in Physics, R. C. P. Edin. and R. C. P. Lond. and University of Edinburgh) and T. J. George (L. R. C. P., L. R. C. S. Edin., Carnegie Assistant to Lecturer in Physics, School of Medicine, Royal Colleges, Edin.), "Some Experiments on the Effects of X Rays in Therapeutic Doses on the Growing Brains of Rabbits," Brit. Med. J., 2 (1910) 524-26, from the Section of Radiology and Medical Electricity, British Medical Association, July 1910.

47. "Bericht des Sonderausschusses für die Sammelforschung über den Einfluss der Röntgenstrahlen auf das Körperwachstum," Verh. Deut. Rönt. Ges., 6 (1910) 16-17: "ihre Möglichkeit ist doch vorhanden; deshalb darf Röntgentherapie nur von Ärtze, und zwar von genügend hierfür vorgebildten, getrieben werden; in der Hand eines jedes Laien und Unkundigen ist sie ein sehr gefährliches Wagnis, das irreparable Schädigungen stiften kann."

48. For the proposal of the theses, see Gocht (Halle a. S.), "Röntgenschädigungen," Verh. Deut. Rönt. Ges., 5 (1909) 72-73. For their adoption, see the "Bericht der Kommission zur Beratung der Thesen Bezüglich Röntgenverbrennungen," Verh. Deut. Rönt. Ges., 6 (1910) 15-16.

49. The "Sonderausschuss zur Schaffung eines Merkblattes für Schutzregeln" distributed a draft Merkblatt to the 1912 Congress of the German Röntgen Society, but no discussion was held because the committee had not yet reached full agreement, see the report of the Chairman (A. Köhler), Verh. Deut. Rönt. Ges., 8 (1912) 16. The Instruction Sheet was complete by 1913, when it was decided to print 10,000 copies, suitable for posting, that would be distributed free to manufacturers, see Verh. Deut. Rönt. Ges., 9 (1913) 14. I am indebted to Mr. A. Hilpert, Geschäftsführer of the present-day Fachnormenausschuss Radiologie, for providing a transcription of this instruction sheet from the original in his files.

50. I have unfortunately not been able to find any pre-War discussions of the question of liability insurance for injuries to patients, but Albers-Schönberg reported in 1913 that the Stuttgart Allgemeine Versicherungsgesellschaft had reclassified X-ray injuries to physicians and technicians as accidents (for which insurance would be made available) instead of treating them as occupational diseases (for which insurance would not have been available). The Council of the German Röntgen Society responded with approval and suggested that the premiums should be determined by considering radiology in the same danger class as surgery, see Verh. Deut. Rönt. Ges., 9 (1913) 15. During the War, Albers-Schönberg emphasized the importance of carrying liability insurance for damage to patients, see Albers-Schönberg (Prof.) and Lorenz (Dr.) (aus dem Röntgeninstitut des Allgemeinen Krankenhauses St. Georg in Hamburg), "Die Schutzmittel für Aerzte und Personal bei der Arbeit mit Röntgenstrahlen," Deut. med. Wschr., 41[1] (1915) 301-305.

51. Butcher, note 40 above, reported (at 10): "In only one of the Röntgen-ray institutions which I visited did I see any instruments used for therapeutic dosage." Similarly, an American physician who had visited Germany reported in 1906, "they do not pay so much attention to the dosage of the ray," M. K. Kassabian in the discussion following Ennion G. Williams, (M. D., Richmond, Virginia), "The Regulation and Measurement of the Therapeutic Dose of the Röntgen Ray," Trans. Amer. Rönt. Ray Soc. (1906) 84-95.

52. G. Holzknecht, "Eine neue einfache Dosierungsmethode in der Röntgentherapie," Wien. Klin. Wschr., 15 (1902) 1180-81, with discussion, or "Eine neue, einfache Dosierungsmethode in der Radiotherapie (Das Chromoradiometer)," Wien. Klin. Rund., 16 (1902) 685-87. For a biography of Holzknecht, who died of radiation injuries in 1931, see R. Kienböck, "Holzknecht semper vivus," Strahlenth., 58 (1937) 497-98.

53. One source says the chromoradiometer was a fused mixture of hydrogen chloride and sodium carbonate, see J. Cramer Hudson, "Röntgen-Ray Dosimetry," in Otto Glasser, The Science of Radiology (Springfield, Illinois: Charles C. Thomas, 1933).

54. R. Sabouraud (Chef du laboratoire de la Ville de Paris à l'hôpital Saint-Louis) and Henri Noiré (adjoint au laboratoire), "Traitements des teignes tondantes par les rayons X à l'École Lailler (Hôpital Saint-Louis)," Presse Med., 12^2 (1904) 825-27.

55. For the dependence on temperature and humidity, see H. Bordier (Professeur agrégé à la Faculté de médicine de Lyon) and J. Galimard (Préparateur de chimie à la Faculté de médicine de Lyon), "Actions des rayons X sur les platinocyanures et en particuliers sur celui de baryum. Cause de leur régéneration. Conséquences pratiques de cette étude," Arch. Elec. Med., 13 (1905) 323-26. For the effect of the available light, see Regaud and T. Nogier, "Estimation différente des doses de rayons X suivant les divers modes d'éclairage du chromoradiomètre," Arch. Elec. Med., 19 (1911) 458-60, Communication au Congrès de l'A. F. A. S., Section d'Électricité Médicale, août 1911.

56. H. Bordier (Lyon), "Radiometric Methods," Arch. Rönt. Ray, 11 (1906-07) 4-13, at 9. The four Bordier tints corresponded to the following: 1, epilation after twenty days; 2, erythema; 3, true dermatitis; 4, ulceration and necrosis.

57. R. Kienböck, (Privatdozent, aus dem Radiologischen Institut der Allgemeinen Polikilinik in Wien), "Über Dosimeter und das Quantimetrische Verfahren," Fortschr. Röntgenstr., 9 (1905-06) 276-295.

58. For iodine in chloroform, see L. Freund, "Ein neues radiometrisches Verfahren (Vorläufige Mitteihung)," Wien. Klin. Wschr., 17 (1904) 417-18, vorgetragen in der Sitzung der k. k. Gesellschaft der Aerzte in Wien am 8 April 1904; for the precipitation of calomel, see G. Schwarz, Fortschr. Röntgenstr., 10 (1906-07) 251, in a report on the 25 May 1907 session of the k. k. Gesellschaft der Artze in Wien; for the decrease of selenium resistance, see G. Athanasiadis (Athen, Physik. Laboratorium d. Univers.), "Wirkung der Röntgenstrahlen auf den elektrischen Widerstand des Selens," Ann. Phys., 27 (1908) 890-96.

59. D'Arsonval, "Dispositif permettant de se rendre identiques les tubes à rayons X," C. R. Acad. Sci. (Paris), 138 (1904) 1142-45 and Walter (Assistent a. Physkal. Staats-Labor. Hamburg), "Über die Messung der Intensität der Röntgenstrahlen," Verh. Deut. Rönt. Ges., 1 (1905) 126-34.

60. Alban Köhler (Wiesboden), "Über Dosierung in der Röntgentherapie und Vorgänge im Innern der Röntgenrohre," Fortschr. Röntgenstr., 11 (1907) 1-12.

61. For the earliest of these clinical devices, see Antoine Béclère (Médecin de l'hôpital Saint-Antoine), "La mesure indirecte du pouvoir de pénétration des rayons de Röntgen à l'aide du spintermètre," Arch. Elec. Med., 8 (15 avril 1900) 153-57.

62. Klingelfuss (Basel), "Die Einrichtung zur Messung der Röntgenstrahlen mit de Sklermoter," Fortschr. Röntgenstr., 16 (1910-11) 64-65; J. Bergonié, "Mesure du degré radiochromomètrique par le voltmetre électrostatique dans l'utilisations en médicine des rayons de Röntgen," C. R. Acad. Soc. (Paris), 144 (1907) 28-29, presentée par M. d'Arsonval; and Heinz Bauer, "Über einige konstruktive Neurungen," Verh. Deut. Rönt. Ges., 5 (1909) 122-26, especially 125-26 and "Das Qualimeter," Verh. Deut. Rönt. Ges., 7 (1911) 137-39, with discussion.

63. Beck, "Zum Selbstschutz bei der Röntgenuntersuchung," Fortschr. Röntgenstr., 6 (1902-03) 268.

64. For the aluminum/silver instrument described, see L. Benoist, "Définition expérimentale des diverses sortes de rayons X par le radiochromomètre," C. R. Acad. Sci. (Paris), 134 (1902) 225-27, presentée par M. Lippmann; for other versions, see B. Walter, "Zwei Härteskalen für Röntgenrohren," Fortschr. Röntgenstr., 6 (1902-03) 68-74 and A. Wehnelt, "Über eine Röntgenröhre mit veränderlichem Härtegrad und über einen neuen Härtemesser," Fortschr. Röntgenstr., 7 (1903-04) 221-22.

65. The best of the pre-war reviews of clinical measurement techniques, which I shall discuss below, was Th. Christen (Dr. med. et phil., Privatdozent an der Univ. Bern),

Messung und Dosierung, Ergänzungsband 28 of Fortschr. Röntgenstr. in a series that comprised the Archiv und Atlas der normalen und pathologischen Anatomie in typischen Röntgenbildern (Hamburg: Lucas Gräfe und Sillem, 1913). See also W. Deane Butcher, "The Measurement of X-rays," J. Rönt. Soc., 4 (February 1908) 36-45, read 6 February 1908 and discussed 5 March 1908, at 59-71; and H. Guillemot, "Les quantitomètres en radiographie et en radiothérapie," Arch. Elec. Med., 16 (1908) 763-72.

66. Lord Blythswood (LL. D., F. R. S.) and Walter Scoble (A. R. C. Sc., B. Sc.), "A Test of Kienböck's Quantimeter," J. Rönt. Soc., 3 (1906) 36-38 and "The Relation Between the Measurements from a Focus Tube, with a View to Determine Which are Proportional to the Intensity of the Röntgen Rays," J. Rönt. Soc., 3 (1907) 53-67, with discussion. Scoble concluded that ionization methods were best, see his "X-Ray Measurement: the Present Position," J. Rönt. Soc., 3 (1907) 99-102, but this solution was not considered practical for the clinic, as I shall discuss below.

67. W. Deane Butcher, J. Rönt. Soc., 3 (1907), at 62.

68. Kommission zur Festsetzung fester Normen für die Messung der Intensität der Röntgenstrahlen, "Bericht," Verh. Deut. Rönt. Ges., 3 (1907) 15-26, read by the Rapporteur, Wertheim-Salomonson (Professor of Radiography and Neuropathology at the University of Amsterdam), at the session of 31 March 1907. The report led to a proposed resolution, at 33 (it is not clear whether it passed): "Bei jeder Messung sollen Daten angeben werden, die Stärke der Röntgenstrahlen charakterisieren.

Die Intensität soll in der Weise angegeben werden, dass die Dosis reproduzierbar sei.

Alle gangbaren Messmethoden, sowohl die direkten als auch die indirekten können dafür gebraucht werden.

Eine bestimmte Methode lässt sich zur Zeit noch nicht empfehlen.

Falls eine photographische, photometrische oder ähnliche Methode benutzt wird, so sollen die Messungergebnisse womöglich mit der Wirkung einer Hefnerkerze verglichen werden."
This last point, which referred to the standard amyl acetate lamp used in measuring illumination, went unheeded, the Commission was renamed the Sonderausschuss für wissenschaftliche und praktische Messmethoden, and it was disbanded, without reporting again, at the eighth congress (1912). The Commission had been created at the first congress in 1905 in response to a proposal by Friedrich Dessauer, see Verh. Deut. Rönt. Ges., 1 (1905) 238.

69. See the "Rapport sur les conditions légales...," note 22 above, at 60: "Le grand progrès actuel que fait chaque jour la radiothérapie, c'est d'apprendre à doser avec une rigueur de plus en plus précise la quantité et la qualité des rayons employér. De même qu'il y a une posologie medicamenteuse, il existe aujourd'hui une véritable posologie radiologique. N'est-il pas évident que seul le médicin peut examiner et trancher ces questions si délicates, et que ce n'est qu'à cette condition que la radiotherapie peut être une methode scientifique dans ses procédés, éfficace dans ces résultats."

70. See, for example, C. Thurstan Holland (M. R. C. S., L. R. C. P.), "Presidential Address," J. Rönt. Soc., 1 (December 1904) 25-37, at 36: "At the same time these articles in the daily press are calculated to do harm, as many of the general public reading them may be led to conclude that there is a danger of chronic dermatitis and cancer being caused by having an X-ray examination made, or by being treated with X-rays....It may be definitely stated that no harm whatever can follow from a properly conducted examination, and I think one is justified in saying that the treatment by X-rays in skilled hands is also harmless." Or, see G. E. S. Phillips, "Presidential Address," J. Rönt. Soc., 6 (January 1910) 1-14, at 4: "The public are still a little nervous as to X-ray burn. Apart from the fact that a satisfactory remedy seems to have been found [???], I take it we are agreed that 'burning', in these days, is due either to the ignorance or carelessness of the operator. It would perhaps be well, therefore, if an authoritative and reassuring statement upon the matter were issued by the Council of this Society."

71. For these technical developments, see Robert Knox (M. D.), "Recent Improvements in Radiographic Technique," J. Rönt. Soc., 6 (October 1910) 110-13 and W. Deane Butcher, "The Amsterdam Congress," Presidential Address, J. Rönt. Soc., 5 (January 1909) 1-8.

72. See, for example, the "Bericht," note 68 above.

73. This problem had been pointed out even before the discovery of characteristic X-rays, see B. Walter, "Über das Röntgensche Absorptionsgesetz und seine Erklärung," Fortschr. Röntgenstr., 8 (1904-05) 297-303.

74. Howard Pirie (M. D., B. Sc.), "Practical Observations on Everyday X-ray and Electrical Work," J. Rönt. Soc., 6 (October 1910) 105-10. Or see C. E. S. Phillips (F. R. S. E.), "The Measurement of Radioactivity and X-Rays," J. Rönt. Soc., 3 (April 1907) 89-99, at 93: "...I gather that the need for a very precise method of comparing X-rays is not so pressing as some appear to think. At least one medical practitioner has pointed out that the errors due to matching the tint of a Sabouraud disc are small relatively [sic] to those arising from the idiosyncracy of the patient. Cases of X ray burn are now happily rare and it therefore seems that added to a good practical experience, the methods of Kienböck and others are accurate as far as they go. While I agree that more convenient means should be looked for, a greater degree of accuracy does not appear to be required than is already attainable at the present time."

75. R. von Jaksch (Hofrat, Professor, Prague), "Über Metallfilter," Verh. Deut. Rönt. Ges., 8 (1912) 71-76, with discussion.

76. J. Belot (chef du service d'Électrologie et de Radiologie à l'Hôpital Saint-Louis), "La filtration en radiothérapie," Arch. Elec. Med., 18 (1910) 648-61.

77. Cl. Regaud and Th. Nogier (Agrégés à la Faculté de Médicine de Lyon), "Les effets produits sur la peau par les hautes doses de rayons X selectionnées par filtration à travers 3 et 4 millimètres d'aluminum. Applications à la Röntgentherapie," Arch. Elec. Med., 22 (1913) 49-66 and 97-128, at 103: "Il est donc evident que les unités H que nous mesurons à Lyon sont très differentes de celles que mesures nos confrères à Paris."

78. C. J. Gauss and H. Lembcke (Freiburg i. B.), Röntgentiefentherapie, ihre theoretischen Grundlagen, ihre praktischen Anwendung und ihre klinischen Erfolge an der Freiburger Universitätsfrauenklinik, 1. Sonderband zu Strahlenth., mit einem Vorwort von Prof. Dr. B. Krönig (Berlin/Wien: Urban and Schwarzenberg, 1912).

79. See the discussion of dosimeters in Verh. Deut. Rönt. Ges., 10 (1914) 187-91 and "Rundscreiben der Sonderkommission für Dosimetervergleich," Fortschr. Röntgenstr., 23 (1915-16) 69-70, and the comments of Meyer, at 75-76.

80. Some experiments showed no effect of quality so long as the absorbed doses were the same, see for example Guilleminot, "Actions biologiques comparées des radiations du radium et des radiations de röntgen. Loi d'efficacité biochimique des radiations," Comptes rendus et Communications III^e Congrès international de la physiothérapie (Paris: Masson, 1911), pp. 674-84. Others showed that the harder rays had greater effect for equal absorbed doses, see for example Hans Meyer and Hans Ritter (Kiel), "Experimentelle Untersuchungen zur biologischen Strahlenwirkung," Verh. Deut. Rönt. Ges., 8 (1912) 126-35, with discussion. Still others thought that different cells reacted differently to radiation of different qualities, see Regaud and Nogier, note 77 above.

81. Christen, note 65 above. For an obituary of Christen by Bernhard Walter, see Fortschr. Röntgenstr., 28 (1921-22) 391-92.

82. See especially the instruments designed by P. Villard, "Instruments de mesure à lecture directe pour les rayons X," Arch. Elec. Med., 16 (1908) 692-99 and his "Radiosclérométre," ibid., 236-35.

83. J. J. Thomson (M. A., F. R. S., Cavendish Professor of Experimental Physics, Cambridge) and E. Rutherford (M. A., Trinity College, Cambridge, 1851 Exhibition Scholar, New Zealand University), "On the Passage of Electricity through Gases Exposed to Röntgen Rays," Phil. Mag., 42 (November 1896) 392-407, read before Section A of the British Association, 1896.

84. See the complaints about this public attitude in C. Mansell Moulin (Consulting Surgeon to the London Hospital, Vice President of the Royal College of Surgeons), "The Treatment of Malignant Growths by Radium," J. Rönt. Soc., 7 (July 1911) 67-75. In the discussion, J. MacKenzie Davidson, a medical radiologist who was knighted in 1911, commented (at 73): "It is very unfortunate that the public have an idea that radium does cure cancer. I think by 'trying' radium people often end their existence a little sooner than they would otherwise do."

85. The London Radium Institute began operations in 1911, see its "First Report, 14 August 1911-31 December 1912," Brit. Med. J. (25 January 1913). The Pasteur Institute and the University of Paris agreed to build the Curie Institute in 1912, but it was not operational until after World War I. The Radiumhemmet in Stockholm opened before the War.

86. This death is mentioned, without a name or further reference, in the report to the French Academy of Medicine, note 22 above.

87. Ernest Rutherford, "The Radioactivity of Thorium," J. Rönt. Soc., 7 (April 1911) 23-30.

88. "At last supplies of pure radium salts are coming to hand both of home manufacturers and from abroad; the price is high, about £20 per milligramme, but it is something that it is obtainable at any price...," J. Rönt. Soc., 7 (January 1911) 16. In 1923, the price fell to around £15 per milligramme, see the South African Mining and Engineering Journal, 17 February 1923, BARP Clipping File IV at the Library of the British Institute of Radiology. See also Thomson, note 2 above, who indicated that the price in 1903 had been 8 s. per milligram, and said of the subsequent rise to £20 per milligram, "there is no doubt that this enormous rise in price has been due to the widespread belief that radium had been found to be a cure for cancer..."

89. For reviews of radium therapy, see P. Oudin (Paris), "État actuel de la radiumthérapie," Comptes rendus des séances du 3e Congrès International d'Électrologie et de Radiologie Médicale, Milan, 5-9 September 1906 (Lille: Camille Robbe, 1906), pp. 113-127; S. Loewenthal, ed., Gundriss der Radiumtherapie und der biologischen Radiumforschung (Wiesbaden: J. F. Bergmann: 1912); and Paul Lazarus, Handbuch der Radium-Biologie und Therapie, einschliesslich der anderen Radioaktiven Elemente (Wiesbaden: J. F. Bergmann, 1913).

90. See, for examples of this vast literature, Julius Elster and Hans Geitel, "Beobachtungen des atmosphärischen Potentialgefälles und der ultravioletten Sonnenstrahlung," Ann. Phys., 48 (1893) 338-73, and H. Mache, "Beiträge zur Kenntnis der atmosphärischen Elektrizität XXI: Über die Genesis der Ionen in der Atmosphäre," Sitzungsber. Akad. Wiss. (Wien)., Abt. IIa, 114 (1905) 1377-88. Elster, Geitel and Mache turned their electroscopes to use in measuring radioactivity as well.

91. Errors occurred, for example, when practitioners introduced a sample into an electroscope without taking account of the consequent change of the electroscope's capacity, see for example the criticism of Herr Saubermann (an advocate of radium emanation in therapy) in Heinrich Mache and Stefan Meyer

(aus dem II. Physikalischen Institut und dem Institut für theoretische Physik an der k. k. Universität in Wien), "Über die Radioaktivität der Quellen der böhmischen Bädergruppe: Karlsbad, Marienbad, Teplitz-Schönau-Dux, Franzensbad sowie von St. Joachimsthal," Sitzungsber. Akad. Wiss. (Wien), Abt. IIa, 114 (1905) 355-85, vorgelegt in der Sitzung am 16 Februar 1905, at 376; and also S. Russ' criticism of W. S. Lazarus-Barlow's (M. D., F. R. C. P.) claim to have discovered substances that would retard the leak of an electroscope, in the discussion following the latter's "Radioactivity and Animal Tissues," J. Rönt. Soc., 6 (April 1910) 33-51, at 40. For reviews of the therapeutic uses of radium emanation, see Lowenthal and Lazarus, note 89 above and also Lachmann (Bad Landeck i. Schl.), "Die Radiumemanation in der Balneologie," Strahlenth., 2 (1913) 153-69.

92. The unit was introduced by Heinrich Mache (aus dem II. physikalischen Institute der k. k. Universität in Wien), "Über die Radioaktivität der Gasteiner Thermen," Sitzungsber. Akad. Wiss. (Wien), Abt. IIa, 113 (1904) 1329-52 and Mache and Meyer, note 91 above.

93. P. Curie and A. Laborde, "Sur la radioactivité des gaz, qui se dégagent de l'eau des sources thermales," C. R. Acad. Sci. (Paris), 138 (1904) 1150-53, transmise par M. Potier.

94. This "spreading difficulty" would generate many proposals and debates over the next twenty years, but for one of its earlier manifestations see W. Wien (Physikalische Institut, Würzburg), "Über die Energie der Kathodenstrahlen im Verhältnis zur Energie der Röntgenund Sekundärstrahlen," aus der Wüllner-Festschrift mit einigen Zusätzen, Ann. Phys., 18 (1905) 991-1007, received 27 November 1905. Wien suggested that the pulses were stored up in an atom until there was sufficient energy to trigger the expulsion of an electron, and that the secondary electron got some of its energy from the atom rather than from the X-ray pulse.

95. The earliest of the efforts to measure the heating effect of absorbed X-rays was E. Dorn, "Ueber die erwärmende Wirkung der Röntenstrahlen," Ann. Phys., 63 (1897) 160-76, Halle, 8 August 1897 (Die Ergebnisse sind der Naturforschenden Gesellschaft zu Halle am 8. Mai d. J. mitgetheilt). Especially well-known and often-cited was Ernest Rutherford (M. A., B. Sc., Macdonald Professor of Physics, McGill University,

Montreal), "Energy of Röntgen and Becquerel Rays, and the Energy required to produce an Ion in Gases," Phil. Trans., 196A (1901) 25-59, communicated by Professor J. J. Thomson, received 15 June 1900 and read 21 June 1900. Others, however, had difficult finding any heating, see Franz Leininger (Würzburg, Phsikal.-Institut), "Notiz über Energiemessungen der Röntgenstrahlen," Phys. Z., 2 (1900) 691-93, eingegangen 3 August 1901. Moreover, the notion of a "trigger" effect, as in Wien, note 94 above, cast doubt on using the heating effect as a measure of the energy of X-rays since some of the heat would have been due to energy originating in the atom.

Chapter 4: Developments in Science and the Impact of War, 1907-18

While X-rays and radium were entering clinical use before World War I, laboratory research in physics, biology and X-ray technology was producing discoveries that after 1914 would bring major changes to medical radiology. In the history of physics, the demonstration of X-ray diffraction in 1912 by three German physicists is considered a critical event, and it might seem reasonable to assume that it was critical for medical radiology as well. As we shall see, two other events in physics proved of much greater importance: the discovery in 1907 of characteristic secondary X-rays by the English physicists C. G. Barkla and C. A. Sadler; and the cloud-chamber photographs showing the paths of secondary cathode rays taken by another English physicist, C. T. R. Wilson, in 1911. In biology, extensive laboratory experimentation led to a theory of radiation effects that relied on the colloidal aggregate theory of proteins, and that appeared to explain the biological effects of radiation in physical terms. In X-ray technology, the major event was the invention in 1913 of the high-vacuum, hot-cathode X-ray tube by W. D. Coolidge, an American physicist and electrical engineer. I shall describe these developments in physics, biology and X-ray technology briefly in this chapter and consider their relevance to medical radiology in general terms. I shall also discuss how World War I affected medical radiology, but I shall leave for Chapter 5 the detailed account of how the pre-War laboratory research proved its usefulness in the clinic during and after the War.

The work in physics that I shall discuss was all closely tied to the question of the nature of X-rays. Barkla, who with Sadler discovered characteristic X-rays, was a leading exponent of the pulse theory that we have described above. Wilson's cloud-chamber photographs supported the views of W. H. Bragg, who from 1907 to 1912 advocated a particle theory of X-rays and gamma rays. Bragg and Barkla were enmeshed in these years in a complex controversy over the nature of X-rays and gamma rays that was resolved by the experimental work of Max Laue, Paul Knipping and Walter Friedrich.[1] Their X-ray diffraction patterns demonstrated that X-rays were neither Barkla's pulses nor Bragg's particles, but ordinary electromagnetic waves like light. I shall not attempt here to offer a full narrative account of the debate over the nature of X-rays and gamma rays before World War I, but I shall instead aim to summarize what a physicist in 1914 might know about the interaction of X-rays with matter and to consider the degree to which this knowledge was explicable on the prevailing view that X-rays were electromagnetic waves.

The X-ray diffraction experiments that Laue convinced Knipping and Friedrich to undertake are remembered today as a major event in the history of physics. The detailed story of the experiments and their interpretation is a complex one. Physicists did not accept Laue's original explanation of the diffraction patterns, and that explanation is not the one in use today.[2] To those outside physics, however, it appeared that the nature of X-rays had been rapidly and unequivocally elucidated. They were not electromagnetic pulses, which would have no determinable wavelength. Instead, X-rays were electromagnetic waves like light but with much shorter wavelengths. The "softer" X-rays had longer wavelengths closer to ultraviolet light and the "harder" X-rays had shorter wavelengths. Crystals diffracted X-rays just as a sufficiently fine slit or grating diffracted light. Using the interference patterns, it was possible to determine the X-ray wavelengths, and by 1914 it was also possible to calculate them from the known absorption coefficients of homogeneous X-rays.[3]

As fundamental as the discovery of the nature of X-rays was to physics, it failed to solve important problems and had little effect on medical radiological research or practice. The results of the X-ray diffraction experiments were known in medical radiology, and the language of "wavelengths" partially replaced the earlier language of "hardness" and "penetrating power" in referring to X-ray quality. Interference measurement of X-ray wavelengths remained, however, a laboratory technique and did not enter the clinic.[4] Moreover, the nature of X-rays was only one of the three interrelated problems the physicist had faced since 1896. The other two were the nature of the processes generating X-rays and the mechanism of the interaction of X-rays with matter. The diffraction experiments and the theory that accounted for them failed to solve these problems. The notion that X-rays were produced by deceleration of cathode rays remained much as it had on the pulse theory. But in 1915, when it was discovered that the maximum frequency of the X-rays produced by Coolidge's tube was proportional to the voltage across the tube, the electromagnetic wave theory could provide no better explanation than the pulse theory.[5] Moreover, the electromagnetic wave theory did nothing to solve the "spreading difficulty" that I described in Chapter 3. Like pulses, waves would spread from their point of origin and have insufficient energy to cause the observed ionization when they were absorbed in air.

It was partly to solve the "spreading difficulty" that Bragg had proposed a particle theory of X-rays. On his theory, the ionization produced by the absorption of X-rays was due to secondary cathode particles. An X-ray or gamma corpuscle was to be regarded as a "neutral pair," which Bragg described as a cathode particle combined with enough positive electricity to neutralize its negative charge. In passing through matter, the neutral pairs would occasionally be torn apart by collisions with atoms, releasing cathode rays. These "secondary" cathode rays would then cause the ionization usually attributed to the X-rays themselves.[6] In 1911, C. T. R. Wilson succeeded in making the paths of the charged particles involved in the ionization of a gas visible and thereby lent strong support to Bragg's view of this process. By triggering a lamp with a mechanism that also caused the expansion of a chamber saturated with water vapor and exposed to X-rays, Wilson photographed the water droplets that condensed along the paths of the charged particles immediately after the expansion. These "cloud-chamber" photographs showed clearly the short paths of the secondary cathode rays. The ionization was not spread uniformly throughout the gas, as would have been expected if it were caused by either electromagnetic pulses or waves. As Wilson said, the pictures were "in agreement with Bragg's view that the whole of the ionisation by X-rays may be regarded as being due to β or cathode rays arising from the X-rays."[7] Bragg's particle theory of X-rays fell victim after 1912 to the diffraction experiments, but the cloud-chamber photographs remained. The cathode rays could no longer be viewed as coming from Bragg's neutral pairs and instead were thought to be ejected from an atom when an X-ray was absorbed. Moving at high velocities through a gas, or through living tissue, these relatively few cathode rays collided with atoms and caused the vast bulk of the observed ionization.

Secondary cathode rays from the absorption of X-rays in metal sheets had been observed before Wilson's cloud-chamber photographs of their paths in a gas. Their velocities, like those of electrons produced from metal surfaces by ultraviolet light (the "photoelectric" effect), had been found experimentally to be independent of the intensity of the radiation. The velocity of the secondary cathode rays depended only on the hardness of the X-rays.[8] When in 1912 the diffraction experiments showed that X-rays were electromagnetic waves, it was readily assumed that the velocities of the secondary cathode rays could be calculated from the same "Planck-Einstein" relation that governed the photoelectric effect:

$$h\nu = E_c = 1/2(mv^2)$$

where $\underline{\nu}$ was the frequency of the radiation, $\underline{h}$ was Planck's constant, $\underline{E}_c$ was the kinetic energy of the cathode ray produced, $\underline{m}$ was its mass and $\underline{v}$ was its velocity.[9] Today, the Planck-Einstein relation is usually associated with a particle theory of light and X-rays, which are regarded as "photons" or light quanta of energy $h\underline{\nu}$. Before the early 1920s, however, this relation was not generally viewed as a statement concerning the energy of light particles. The view most important to medical radiology was that the Planck-Einstein relation governed the transformation of the energy of an electromagnetic wave of frequency $\underline{\nu}$ into the kinetic energy of cathode rays. Applied to the secondary cathode rays revealed in the cloud-chamber photographs, the Planck-Einstein relation would, as we shall see, play an important role in the development of X-ray dosimetry.

The secondary cathode rays were not the only "secondary" rays produced by X-rays when they were absorbed in matter. Röntgen himself had been among the discoverers of scattered X-rays.[10] These appeared to be equivalent in hardness to the primary beam, but they radiated in all directions from the place where the primary beam was absorbed. A great deal of experimental research had been conducted on scattered X-rays during the first decade of the twentieth century. Barkla, using the pulse theory as a guide, had been a major contributor to this work. In 1907, he and Sadler discovered that on exposure to a heterogeneous beam of primary X-rays the element nickel produced not only scattered radiation but also homogeneous secondary rays softer than the primary beam.

Further investigation showed that these homogeneous X-rays were characteristic of the element exposed to the primary beam, that they were softer the lower the atomic weight of the material, and that they could only be excited by a primary beam containing X-rays of equal or greater degree of hardness.[11]

By 1914, Barkla's characteristic X-rays had become a major focus of attention in atomic physics, and during the next decade they would prove critical to the development of a quantum theory of the atom.[12] For our purposes, however, the most important aspects of the characteristic rays were experimental. Corresponding to the emission of characteristic secondary X-rays were absorption edges: as the hardness of the primary beam was increased, its absorption in a given material increased sharply at the same degree of hardness as the characteristic secondary X-rays and then declined gradually. As a result of this selective absorption, materials absorbed X-rays of different quality differently, and the exponential law for the decrease in X-ray intensity did not hold in the region of the absorption "edges." Moreover, every material appeared to be especially transparent to X-rays corresponding to the hardness of its own characteristic X-rays, since these would be selectively absorbed and re-emitted. By the beginning of World War I, characteristic secondary X-rays had been observed in all elements down to the atomic weight of aluminum (27).

With these details in the background, let us summarize the immediate pre-War situation in physics from the point of view of someone interested in the interaction of radiation with matter.

The simple picture of exponential absorption of X-rays or gamma rays of any given hardness that was held ten years earlier had changed considerably. X-rays produced several different kinds of secondary radiation: the scattered X-rays of approximately the same quality as the primary X-rays; the characteristic X-rays of equal or lesser hardness than the primary beam; and the cathode rays that were directly responsible for the ionization caused by exposure to the primary beam. Gamma rays were also scattered, and they produced secondary cathode rays as well. With increasing hardness, X-rays showed selective absorption edges characteristic of a given element. The proportion of X-rays or gamma rays that was scattered increased with the hardness of the rays, and this proportion was also greater for elements of lower atomic weight. The velocity of the secondary cathode rays produced in the absorption of X-rays or gamma rays increased with the hardness of the radiation in accordance with the Planck-Einstein relation, and this velocity was independent of the intensity of the radiation. The interaction of X-rays with matter was a complex process, and none of these phenomena was readily explicable on the generally accepted theory that X-rays and gamma rays were electromagnetic waves.

While this anomalous situation was developing in physics, the use of radiation as a research tool in biology was expanding rapidly. Their interest aroused by the effects of radiation on germ cells and by radiation-induced carcinoma, biologists and research-oriented physicians working on embryology, heredity and cell development began in the decade before World War I to regard radiation as an elegant means of disturbing the normal course of development and reproduction. If the primary lesion in such a disturbance could be identified, the biologists would have a hint of what structures in the cell controlled these processes. Work in the biological laboratory used radium more than X-rays, but this preference was due to the constant attention required in operating an X-ray tube rather than to scientific considerations. If radium could be obtained at all, it was easier to use than an X-ray tube, and in general the gamma rays of radium were assumed to produce biological effects similar to those of X-rays. Exposing a variety of experimental samples--molds, fertilized eggs of sea urchins and of worms found in horse saliva, tadpoles, and plant cells as well as various tissues from higher species--the biological investigations produced a significant volume of descriptive material concerning radiation damage on the cellular and sub-cellular levels.[13] In 1905, two French physicians summarized their investigations of the differential effects of X-rays on the various cells of the rat testicle: "X-rays act with greater intensity on cells the greater their reproductive activity, the longer they take in cell division, and the less their morphology and functions are definitively determined."[14] This "law" of Bergonié-Tribondeau, which was discussed briefly in Chapter 1, stood up remarkably well during the next ten years. There were exceptions to the generalization, but it nevertheless summarized a vast amount of experience and it came to be regarded as the cornerstone of radiation biology.

To biologists who believed the chromosomes were essential to cell reproduction and heredity, the law of Bergonié-Tribondeau suggested that the strings of highly-staining material in the nucleus might be the cite of the primary lesion in radiation effects. The chromosome theory of heredity was not, however, a dominant view before World War I. Radiation had other biochemical effects that might account for the damage to tissue: it affected a number of enzymes, and it also cleaved the phospholipid lecithin, which seemed to be present in all cells, and this chemical reaction produced the toxic substance cholin.[15] Foremost among the advocates of the chromosomes as the cite of the primary lesion in radiation effects before the War was Oscar Hertwig, who with his daughter Paula produced extensive experiments to demonstrate that radiation affected the chromatin directly (that is, not indirectly as a result of other biochemical effects) and that the effects on the unfertilized sperm or egg manifested themselves after fertilization in the development of the individual.[16] Indeed, there were several claims to the experimental demonstration that characteristics acquired by exposure to radiation were inherited, but the validity of these claims did not gain general recognition.[17]

Beginning around 1910, and accelerating rapidly thereafter, the chromosomal view of radiation damage gained ground, becoming during and after World War I a plurality if not a majority view. There were two important factors in this change: the chromosome theory of heredity was gaining wider acceptance from 1910 on, and the colloidal aggregate theory of proteins appeared to offer a general account of how radiation might affect the chromosomes, which were thought to consist primarily of protein.[18] According to the colloidal aggregate theory, proteins were colloids held in suspension by electric charges on their surfaces. Discharge of the colloidal particles would cause precipitation. Indeed, β-rays from radium had been shown to cause the precipitation of inorganic colloids as early as 1904.[19] X-rays apparently also caused the precipitation of organic colloidal protein in causing the formation of cataracts in the eye, an effect that had been observed in laboratory animals as early as 1905.[20] A French physician reviewed the relevant medical and biological literature and drew the straightforward conclusion before World War I: radiation precipitated the colloidal proteins that made up the chromosomes and thus affected the hereditary material.[21] In some cases, the cells might be so damaged that they could not reproduce at all, thus resulting in the destruction of tissue after a latency period that depended on the life-span of the cells.[22] With smaller doses, the precipitation of colloidal protein stimulated cell growth, leading to neoplasia.

In addition to its links to the chromosome theory of heredity and to the colloidal aggregate theory of proteins, this view of radiation effects had the advantage of being comprehensible from the point of view of the physicist. Ionization was precisely the physical process required to discharge the colloidal proteins. Not until the early 1920s would the "point-heat" theory provide a mathematical link between the secondary cathode rays in Wilson's cloud-chamber photographs and biological effects, but on the eve of World War I there already appeared to be a loose articulation between the physicist's view of the interaction of radiation with matter, a view he could admittedly not account for on the electromagnetic wave theory of X-rays, and the biologist's view of the interaction of radiation with colloidal proteins. The science of radiology appeared to be more unified, and as we shall see this appearance would prove important. We shall also, however, see that the contributions of scientific radiology to medical radiology did not come through understanding of the basic mechanism of radiation effects, as one might expect. Although the "point-heat" theory would in the 1930s prove of importance to the study of genetic effects, this theory was not the basis on which scientific and medical radiology were unified in the 1920s.

The invention just before World War I of a high-vacuum, hot-cathode X-ray tube by William David Coolidge would be a major factor in bringing about that post-War integration of scientific and medical radiology.[23] At least since Rutherford's comment that the Coolidge tube was "a triumph of the application of the latest scientific knowledge and technique," it has been traditional to stress the scientific component of this invention, and even to suggest that it stemmed from a program of "pure," as opposed to "applied," research. Coolidge was an American who was working at the newly founded General Electric Research Laboratory in Schenectady. With a bachelors degree from MIT in electrical engineering and a doctorate from the University of Leipzig in physics, Coolidge belonged to a growing group of academically trained physicists doing industrial research, and the scientific component of his invention was unquestionably strong. Coolidge knew the relationship between the temperature of a filament and the charge it emitted from the work of the English physicist O. W. Richardson.[25] Irving Langmuir, Coolidge's colleague at General Electric, had worked with these "thermionic" currents in high-vacuum tubes.[28] To evacuate his tubes, Coolidge used a so-called "molecular" pump designed in 1913 by the German physicist W. Gaede. This pump worked on physical principles that had been discovered in the previous decade.[27] Neither the heated cathode nor the high vacuum were entirely new to X-ray tube technology, but the combination was a unique one. Coolidge understood from Langmuir's work that the residual gas in the ordinary X-ray tube was the source of its rapid variations in hardness, and that a tube could operate without this gas if a sufficient flow of electrons could be generated from the cathode.

The scientific input was not, however, the entire story of the Coolidge tube. Coolidge began his research in the mundane world of product improvement by working on the production of ductile tungsten for use as a filament in electric light bulbs. With a melting point of 3370° centigrade, tungsten was well-suited to this use, but because it was hard and brittle at ordinary temperatures it could not be easily "worked," as required in a mass production process. By mechanically manipulating tungsten at temperatures well below its melting point and removing very small traces of impurities, Coolidge managed to produce flexible tungsten wire.[28]

Having succeeded in making this refractory element workable, Coolidge looked for other applications. General Electric was a major producer of X-ray tubes. Use of alternating current transformers to produce high-voltage discharges had, as I have mentioned, increased the amount of current that could be passed through an X-ray tube. The anticathodes in use could not, however, endure the heat generated by the increased bombardment of cathode rays. For this and other reasons, the problem of choosing a material for the anticathode was a common subject of discussion among manufacturers around 1910.[29] In his 1912 patent application, Coolidge noted the use of ductile tungsten not only as a filament in incandescent lamps, but also as a covering for the anticathodes of X-ray tubes.[30]

This part of Coolidge's work cannot be described as "pure" research. Far from transforming technology by the application of scientific principles, Coolidge reported that his work on ductile tungsten required

> ...twenty trained research chemists, with a large body of assistants, in the research laboratory. These men were of course given, from the factory organization, all of the mechanical and electrical assistance they could use, and were assisted in no small measure by the staff of the incandescent lamp factory.[31]

The methods were essentially trial and error, especially in the purification of tungsten, and there were in any case no fundamental principles of physics involved. There were even reasons to believe, as Coolidge pointed out, that tungsten, because of its relationship to other metals in the periodic table, would not become more ductile with further purification. Coolidge's success in producing ductile tungsten cannot, then, be viewed solely as a scientific contribution to technology. When in 1913 he used his ductile tungsten as a hot cathode in a high-vacuum X-ray tube, Coolidge was again responding to a commercial imperative, not a scientific one. Rapid variations in hardness made it difficult to use X-rays for either diagnostic or therapeutic purposes. A great deal of ingenuity had gone into inventing regulators that would automatically adjust the gas pressure in the tube, and easy control of X-ray quantity and quality had been a major selling point for General Electric and other tube manufacturers.

The Coolidge tube did have significant advantages over earlier models when it came to controlling X-ray output. Because cathode rays (or as Coolidge said, electrons) rather than ions of the gas in the tube carried the bulk of the current, the quantity of X-rays produced was more nearly proportional to the current through the tube and their quality was more nearly proportional to the voltage across the tube. These two parameters could be adjusted independently, with the current depending on the temperature of the cathode. Easy regulation of output, along with spectacular reliability in continuous use, were to become major factors in the rapid adoption of the Coolidge tube in medical practice. Less obvious to Coolidge in 1913, but just as important for medical radiology in the next decade, were the high voltages that could be used across a Coolidge tube. Before World War I, the voltages in use were almost entirely in the range of 20,000 to 50,000 volts. By the end of the War, up to 200,000 volts were in use in a few clinics. Higher voltages meant more penetrating X-rays. Medical practitioners would double and triple the thickness of the aluminum filters they used, and they would then turn to copper filters. In the physicist's terms, the wavelengths of the X-rays available decreased from a minimum of around .4 angstrom to a minimum of around .04 angstrom. Medical radiology, and especially deep therapy, had a new and powerful tool at its disposal.

Thus, on the eve of World War I, X-ray technology, radiation biology and radiation physics were able to offer contributions to medical radiology. Before August 1914, however, these contributions were just beginning to be absorbed. The first reports on the clinical use of Coolidge tubes had just appeared, secondary rays were occasionally being mentioned in the medical radiological literature, and ionization was slowly becoming a term more familiar to physicians. No major impact was yet detectable. Coolidge tubes were only one of several sorts being tested, including the hot-cathode but low-vacuum Lilienfeld tube.[32] The importance of secondary rays in X-ray protection and dosimetry was still uncertain. Kienböck's silver bromide paper and the barium platinocyanide pastilles were the most commonly used clinical dosage devices, with ionization methods still limited to the physics laboratory and to clinical work with radium.

The War disrupted many professional institutions, and it may seem plausible to suggest that it simply delayed inevitable developments. Thus Price's notion that war simply shifts the exponential growth curves of a scientific field might well be true for both scientific and medical radiology.[33] International conferences of the scientific radiologists (scheduled to be held in Vienna in 1915) and of the medical radiologists (which would normally have been held in 1916) were postponed.[34] The German Röntgen Society did not hold its annual congress between 1914 and 1920. Journals on both sides suffered delays in publication.[35] A leading French practitioner completed a report on the radiotherapy of uterine fibromas that he had begun in 1913 with a continuation published in 1918.[36] British radiologists, who had been assured by their government that X-ray tubes would not "be considered as contraband in the event of a war with a Continental Power" because they "would be used for the relief of the wounded," found themselves cut off from essential German suppliers of both the tubes and the glass used in their manufacture.[37] The Prussian Ministry of War in 1915 prohibited the export of medical journals, even to Neutral Powers.[38] Even informal communications between the German medical radiological community and non-German counterparts were disrupted, and it was not until the early 1920s, as we shall see in Chapter 6, that communication was re-established. The War, in short, redirected resources and personnel, limited supplies of equipment, and cut professional communities off from their colleagues in other countries.

The disruptions affected medical research work with X-rays and with radium differently, and the net results varied markedly from country to country. In the short term, radium research work suffered more than X-ray research work, though as we shall see the long-term effect was highly favorable to expanding the use of radium in medicine. In France, where work with radium had been relatively more important than in other countries, research was seriously disrupted. The volume of the medical radiological literature fell dramatically, and a large portion of the papers published were devoted to localization of bullets and shrapnel by X-rays and other work directly related to the War.[39] In England, medical radiological research and, for reasons I shall mention below, the number of original contributions to the _Archives of Radiology and Electrotherapy_, increased dramatically between 1914 and 1917.[40] There was, however, a severe shortage of X-ray tubes in Britain that hampered research work. Only four small British firms were making tubes in 1914, and all of them depended on glass imported from Germany.[41] About half the pre-War tubes used in England were manufactured abroad, mostly in Germany, and part of the remaining half was manufactured by German firms in England.[42] The German medical X-ray community, which for most practical purposes included the German-speaking medical radiologists of Austria-Hungary, was the largest in the world before the War, and it was also considered by non-Germans to be the most advanced.[43] Dependent almost entirely on domestic manufacture, with a large part of the market dominated by subsidiaries of large electrical firms, German research thrived without the Röntgen Society congresses. Only after the War, in the midst of political and economic upheavals, was there a noticeable effect on the volume of the medical radiological literature, and even then the effect was slight.[44] Research in the United States continued at something like its pre-War level, with American radium and X-ray equipment manufacturers filling the gap left by the Germans. In addition, there was a striking rise in the quality of work done in the United States, so that by the early 1920s what had been a provincial community that reacted to European developments was increasingly contributing to major advances in both X-ray technology and medical applications.

Delay of inevitable developments was not, then, the only, or even the primary effect of the War on medical radiological research. We would miss several effects of major importance if we looked only at the shift in Price's exponential growth curves. Looking beyond research to the impact of the War on medical radiological practice and on the social institutions that supported it, there were other important changes. While medical radiology was suffering in some respects the inevitable disruption of civilian activities in wartime economies, the profession was also benefitting enormously from the rechanneling of resources to meet military needs. X-rays had proved their usefulness in military medicine even before 1900. Used primarily to locate bullets and shrapnel, X-ray machines had seen service in the British army's 1897 Sudan expedition, in the Graeco-Turkish War of the same year, in the Spanish-American War, and in the Boer War. Meeting military demands for mobility and reliability had called for innovations in basic equipment, simplified and accelerated techniques, and intensive training of both physicians and nonphysicians.[45] In all these respects, the demands of World

War I far exceeded those of the previous campaigns in which X-rays had been used. The vast armies of the Allies and the Central Powers by the end of the War included among their support operations thousands of diagnostic X-ray installations.

The most important direct effect on medical radiology was the demand for personnel to staff these units. In 1914, the French Army had less than two dozen X-ray installations. By 1918, it had 400. Staffing these installations were 840 physicians and almost 1200 nonphysician operators, including 175 women trained by Marie Curie.[46] The United States Army, which before the War had only five mobile X-ray units mounted on four-mule escort wagons, sent more than 700 installations, many of them automobile-mounted, to Europe before the Armistice.[47] In the second half of 1917, Army schools in Boston, New York, Philadelphia, Pittsburgh, Baltimore, Richmond, Chicago, Kansas City and Los Angeles trained 200 physicians in X-ray diagnosis. Radiology was second only to surgery in getting the pick of the physicians available to the United States Army.[48] In Britain, there was also extensive training of physicians and nonphysicians for the Army. One estimate placed the number of X-ray operators in 1916 at six times the pre-War level, and it was presumably this increase that led to the increased number of original contributions to the Archives.[49] Despite the disruptions of War, medical radiology grew as it had never grown before.

This greatly enlarged profession had to be supplied with X-ray tubes, high-voltage generators, protective devices, and other auxiliary equipment. Before 1914 X-ray tubes had been hand-blown, but by 1918, they were being mass-produced.[50] However new and untried, Coolidge tubes were so much more regular in their output and reliable in their operation that they were greatly preferable for use under military conditions. Coolidge himself designed an automobile-mounted model for the United States Army.[51] By the end of the War, Coolidge tubes were common in both the United States and in Germany. They seem to have come into use more slowly in France and England.[52] Even cut off from a large portion of their export market, German manufacturers of X-ray tubes appear to have done especially well during the War. The firm of Siemens and Halske, which had patented the tungsten anticathode before the invention of the Coolidge tube, arranged with AEG, the German firm licensed to produce Coolidge tubes by General Electric, to exchange rights and to produce the high-vacuum, hot-cathode tube jointly. Siemens and Halske, which had previously assigned X-ray tubes and other medical electrical equipment to its Division for Measuring Instruments, established a separate Sales Division for Electromedical Apparatus. In 1916, the firm of Reiniger, Gebbert and Schall acquired control of Veifawerke by buying the shares of Friedrich Dessauer, one of the pioneer tube manufacturers.[53] At about the same time, Reiniger, Gebbert and Schall, which two decades earlier had provided Röntgen with his Ruhmkorff coil, brought Christen from his university post in Bern to lead an enlarged research unit at Munich.[54]

The War also increased the demand for radium. Medical use was not the major factor, though after the War medicine benefitted from the increased supply. During the War, radium was needed primarily in self-luminescent paints, which were used for gunsights and dials (especially in airplanes) and also for warning signs on the backs of military vehicles. Dial-painting, as I shall have occasion to mention, was to cause a major incident in the history of radiation injuries. In addition, radium recovered from gunsights and dials was distributed to British hospitals for therapeutic trials after the War.[55] The United States was also successful in increasing its supplies of radium, and by the end of the War it had more than any other country: a total of about 50 grams.[56]

With the rapid increases in personnel and equipment came a need for controlling quality and standardizing procedures. Quality control of military purchases fell in part to national standards laboratories. The German Royal Physical-Technical Institute at Charlottenburg, the British National Physical Laboratory, and the United States Bureau of Standards were to be especially important for radiation protection and measurement. Created as a result of nineteenth-century increases in trade and manufacturing, these laboratories were responsible for maintaining reference standards and comparing them with commerical standards or products. Much of this work was routine. The National Physical Laboratory in the early years tested a large number of clinical thermometers.[57] Before the War, the standards laboratories had played only peripheral roles in radiation work. They acted as depositories for radium standards, but they had played little or no role in their development. The standards laboratories had, however, one important feature that repeatedly led them beyond routine testing activities: they used academically trained scientists to work on technological problems, a practice that was still very new in industry. During the War, the national standards laboratories were called on to test X-ray equipment, protection devices and self-luminescent paints.[58] After the War, the staff were turned in part to work on radiation protection and measurement. Physicists interested in these problems thus gained a much stronger institutional basis than they had had before, and medical radiology was provided with a level of expertise that had not been so readily available to it previously.

In addition to quality control of equipment, there were problems in controlling the quality of the procedures used in military radiology, and especially in ensuring that the newly trained operators were adquately protected. Civilian radiological practice might permit wide variations in clinical technique, but military social behavior required standardization, especially when hundreds of new X-ray operators had to be trained and sent immediately into the field. The single most successful effort in this regard was the United States Army X-ray Manual, which was prepared by a Cornell physics professor.[59] Without being innovative, it gave considerable weight to both protection and measurement techniques. The British Röntgen Society, following the pre-War lead of its German counterpart, prepared "Recommendations for the Protection of X-ray Operators."[60] The War Office was sent 250 copies for distribution to military hospitals.[61] The 2 millimeters of lead protection that the Germans had decided on in 1913 was included in the original British draft, but it was eliminated in the final version.[62] The British recommendations emphasized enclosing both the tube and the operator in protective boxes, but unlike the U. S. Army manual they failed to recommend dosage measurements (beyond the use of a penetrometer to test hardness).

The War, then, strengthened the medical radiological community in a number of ways: the body of practitioners was enlarged, the capacity to produce both X-ray tubes and radium was increased, the national standards laboratories became involved, and protection procedures were standardized. In addition to these institutional impacts, the War also affected the career patterns of individuals. Many physicians entered radiology who would not otherwise have done so. The status of the profession within medicine rose, and the first efforts at making radiology a separate specialty requiring post-graduate training began during the War.[63] The War also brought to medical radiology an influx of academically trained physicists beyond those in the national standards laboratories. Most famous of these was Marie Curie, who deposited her radium with the physician Bergonié in Bordeaux at the beginning of the War and devoted herself and her daughter, the future Nobelist Irène, to medical X-ray work for the duration. Marie and Irène Curie initially went into the field as radiographers, but they later returned to Paris to train women as X-ray operators.[64]

More important than the Curies for later developments in medical radiology were a number of German physicists who turned to research in medical radiology during the War.[65] Walter Friedrich, who had performed the diffraction experiments with Knipping in 1912, went during the War to the University of Freiburg, where he collaborated with the gynecologist Bernhard Krönig on research that was critical to the development of deep therapy. In 1923, Friedrich, as Ordinarius for medical physics and scientific radiology, founded the Institute for Radiation Research at Berlin, where he remained for the rest of his long career.[66] At Freiburg, Friedrich trained the physicist Otto Glasser, who emigrated to the United States in the early 1920s and in 1924 settled at the Cleveland Clinic Foundation. Leonard Grebe, an experimental physicist who had failed to obtain a professorship after habilitating at Bonn in 1910, began working in medical radiology shortly after the War with Heinrich Martius, a younger Privatdozent. They both made successful careers in medical radiological research. Richard Glocker, who had been a student of Röntgen's at Munich with Friedrich, began working on radiation dosimetry during the War, as did Gustav Grossmann, who had obtained a doctorate from the Zürich Polytechnique in 1903 and who later made a career in the X-ray industry. Hermann Behnken, who had received his doctorate in physics at Berlin in 1913, was by the end of the War in charge of X-ray work at the Royal Physical-Technical Institute.[67]

The precise reasons for these shifts of interest among German physicists are obscure, and lacking documentary evidence I can only offer speculation. In part, the physicists may have wanted to contribute toward work that they saw as beneficial to the War effort. This was an important part of Curie's motives. The German physicists with whom we are concerned did not, however, go into the field, as she did. Their motives may have been less patriotic and more personal. Even before the War, there had been signs of worsening career prospects for academic physicists. Despite substantial increases in the resources available to the academic physics community as a whole, the age at which physicists entered their first professorship in the decade before 1910 had risen throughout Europe compared to the 1880s. A significant percentage of the German physicists who habilitated between 1890 and 1910 had never received a professorship.[69] The War probably aggravated this pre-existing condition. With research funds and academic posts in short supply, recent doctoral recipients and Privatdozenten found themselves unable to continue the relatively basic research in which they had been trained. A desire to stay out of the army may also have been a factor. Whatever their individual motives, Friedrich, Glasser, Glocker, Grebe, Martius, Grossmann, and Behnken were only a few of the German physicists whose careers took turns toward more practical work during the War. Not only medical radiology, but also fields like metallurgy, chemical and electrical engineering, shipbuilding, and airplane design benefitted from increased interest among academically trained physicists and chemists. For German physicsts, the founding of the Society, and Journal, for Technical Physics in 1920 institutionalized these new interests.[70] By 1924, the Society for Technical Physics had 1600 members.[71] The physicists who entered medical radiology did not play a major role in the new Journal and Society, preferring to participate in the German Röntgen Society (which continued to permit participation by nonphysicians) and to publish primarily in the medical radiological journals. The influx of physicists into medical radiology should, however, be viewed as parallel to a broader movement among German physicists and German scientists in general.

In addition to the flow from physics into medical radiology, there was a small, but highly significant, flow in the other direction, from medical radiology into physics. Friedrich Dessauer, the manufacturer of X-ray tubes who had been involved in medical radiology for over a decade, sold his interest in Veifawerke, retired from industry, and obtained a doctorate in physics from the University of Franfurt in 1917 at the age of thirty-six. In 1922, Dessauer became director of the newly founded Institute for the Physical Foundations of Medicine at Frankfurt, where both physicists and biologists found facilities for research. There were also two physicians who obtained advanced training in physics. Hermann Holthusen, who even before the War had studied with Philipp Lenard at Heidelberg, habilitated there in 1918. He then began a research career in medical radiology that relied heavily on both use of the physician's laboratory tools and competence in contemporary physical theory.[72] Another young physician, Hermann Wintz, began his work in medical radiology at Erlangen during the War in collaboration with Ludwig Seitz, Ordinarius and director of the University Women's Hospital, and with assistance from the firm of Reiniger, Gebbert and Schall. In 1920, Wintz obtained a doctorate in physics from the University of Erlangen, and when Seitz went to Frankfurt in the same year Wintz succeeded to his posts, becoming at age thirty-three the youngest Ordinarius in gynecology in the country.[73]

Theophil Christen, the physician with a doctorate in mathematics who worked for Reiniger, Gebbert and Schall during the War, would probably have proved another important contributor to later developments, but he committed suicide in 1920 after an unsuccessful electoral campaign as a Social Democratic candidate for the Swiss National Council.

Dessauer, Holthusen, Wintz and Christen, along with the physicists who were orienting their research towards medical radiology, belonged to a small but growing group of German research workers who would be difficult to classify as belonging exclusively to either the scientific or the medical radiological communities. From their efforts during and after the War to apply contemporary physics to medical radiological problems would develop a new style of deep therapy, a much more thorough understanding of radiation dosimetry, and a physical theory of radiation effects, all of which I shall describe in Chapter 5.

1. For an account of the Bragg-Barkla controversy, see Roger H. Stuewer, "William H. Bragg's Corpuscular Theory of X-rays and γ-rays," Brit. J. Hist. Sci., 5 (1971) 258-81.

2. On the lead-up to the diffraction experiments, see Paul Forman, "The Discovery of the Diffraction of X-rays by Crystals; a Critique of the Myths," Arch. Hist. Ex. Sci., 6 (1969) 38-81. On the different treatments of the results, see Chapter 5 of J. L. Heilbron, H. G. J. Moseley: the Life and Letters of an English Physicist, 1887-1915 (Berkeley: University of California, 1974).

3. M. Siegbahn, "Über den Zusammenhang zwischen Absorption und Wellenlänge bei Röntgenstrahlen," Phys. Z., 15 (1914) 753-56, eingegangen 11 Juli 1911.

4. For references to spectrometers as scientific instruments unsuitable in the clinic, see R. Glocker (D. phil.), "Eine neue Methode zur Intensitäts- und Härtebestimmung von Röntgenstrahlen (besonders für Zwecke der Tiefentherapie," Fortschr. Röntgenstr., 24 (1916-17) 91-101.

5. William Duane and D. L. Hunt, "On X-ray Wave-Length," Phys. Rev., 6 (1915) 166-77. This relationship was readily explicable on Einstein's light quantum hypothesis, which was not however generally accepted, see Roger H. Stuewer, The Compton Effect: Turning Point in Physics (New York: Science History Publications, 1975), especially Chapter 2.

6. W. H. Bragg (M. A., F. R. S., Elder Professor Mathematics and Physics in the University of Adelaide), "On the Properties and Natures of various Electric Radiations," Phil. Mag., 14 (1907) 429-49, read before the Royal Society of South Australia, 7 May 1907 and 4 June 1907, and "The Consequences of the Corpuscular Hypothesis of the γ and X Rays, and the Range of Rays," Phil. Mag., 20 (1910) 381-416.

7. C. T. R. Wilson, "On A Method of making Visible the Paths of Ionising Particles through a Gas," Proc. Roy. Soc., 85A (1911) 285-88, at 287.

8. C. D. Cooksey (Sheffield Scientific School of Yale University), "On the Corpuscular Rays produced in different Metals by Röntgen Rays," Amer. J. Sci., 24 (1907) 285-304 and P. D. Innes, (M. A., B. Sc., 1851 Exhibition Scholar of the University of Edinburgh, Trinity College, Cambridge), "On the Velocity of the the Cathode Particles emitted by Various Metals under the Influence of Röntgen Rays, and its Bearing on the Theory of Atomic Disintegration," Proc. Roy. Soc., 79A (1907) 442-62.

9. On the origins in Einstein's work of the "Planck-Einstein" relation, see Martin J. Klein, "Einstein's First Paper on Quanta," Nat. Phil., 2 (1963) 59-86. See also Stuewer, note 5 above.

10. Even in his "preliminary communication," Röntgen concluded that solid bodies scatter X-rays just as turbid media scatter light, see paragraph 8 of "Eine neue Art von Strahlen (Vorläufige Mitteilung)," Sitzungsber. Phys.-Med. Ges. (Würzburg) (1895) 132-41, at 137-8. In his third (1897) communication, Röntgen demonstrated the scattering of X-rays by air, see paragraph 1 of "Weitere Beobachtunger über die Eigenschaften der X-Strahlen," Math. Naturwiss. Mitt. Sitzungsber. Preuss. Acad. Wiss., Physik.-math. Kl., 1897 as translated in O. Glasser, Wilhelm Conrad Röntgen and the Early History of the Röntgen Rays (Springfield, Illinois: Charles C. Thomas, 1934), pp. 401-18. Stuewer, note 5 above, mistakenly calls J. J. Thomson's 1903 suggestion that X-rays would be scattered "an extremely bold conjecture, since no scattering experiments of any kind had been carried out as yet," at 3. In fact, however, scattered X-rays, or S (for secondary) rays as they were sometimes called, were well known before Thomson's theoretical explanation for them on the basis of the pulse theory.

11. C. G. Barkla and C. A. Sadler, "Secondary X-Rays and the Atomic Weight of Nickel," Phil. Mag., 14 (September 1907) 408-22 and the general review, "Homogeneous Secondary Radiations," Phil. Mag., 16 (October 1908) 550-84.

12. John L. Heilbron, "The Kossel-Sommerfeld Theory and the Ring Atom," Isis, 58 (1967) 450-85 and Paul Forman, "The Doublet Riddle and Atomic Physics circa 1924," Isis, 59 (1968) 156-74.

13. O. Hertwig, "Das Radium als Hilfsmittel für Entwicklungs-physiologische Experimente," Deut. Med. Wschr., 37[2] (1911) 2209-12; also G. Bohn, "Influence des rayons du radium sur les animaux en voie de croissance," C. R. Acad. Sci. (Paris), 136 (1903) 1012-13, presentée par Alfred Giard, séance du 27 avril 1903 and "Influence des rayons du radium sur les oeufs vièrges et fécondés, et sur les premiers stades du développement," ibid., 1085-86, presentée par Alfred Giard, séance du 4 mai 1903; J. Dauphin, "Influence des rayons du radium sur le développement et la croissance des champignons inférieurs," C. R. Acad. Sci. (Paris), 138 (1904) 154-6; Perthes (aus dem Chirurgisch-poliklinischen Institut der Univ. Leipzig),

"Versuche über den Einfluss der Röntgenstrahlen auf die Zellteilung," Deut. Med. Wschr., 30^{1} (1904) 632-35 and 668-70; and C. S. Gager (New York Botanical Garden), "Effects of Exposing Germ Cells to the Rays of Radium" and "Effects of Radium Rays on Mitoses," Science, 27 (1908) 335-36.

14. J. Bergonié amd L. Tribondeau, "Interprétation de quelques résultats de la radiothérapie et essai de fixation d'une technique rationelle," C. R. Acad. Sci. (Paris), 143 (1906) 983-85, presentée par M. d'Arsonval, at 983: "les rayons X agissent avec d'autant plus d'intensité sur les cellules que l'activité reproductice de ces cellules est plus grandes, que leur devenir karyokinétique est plus long, que leur morphologie et leurs fonctions sont moins définitivement fixées."

15. The lecithin hypothesis was originally proposed by Gottwald Schwarz (cand. med., aus dem Röntgen-Laboratorium des k. k. Wiener allg. Krankenhauses), "Ueber die Wirkung der Radiumstrahlen (Eine physiologisch-chemische Studie am Hühnerei)," (Pflügers) Arch. Physiol. Mensch. Thier., 100 (1903) 532-46. See also A. Schaper (Prof., aus der Entwicklungsgeschichtlichen Abteilung des Anatomischen Institutes in Breslau), "Experimentelle Untersuchungen über die Wirkung des Radiums auf embryonale und regnerative Entwicklungsvorgänge," Deut. Med. Wschr., 30^{2} (1904) 1434-37 and 1465-68 and Richard Werner (Assistenten der Chirurgischen Klinik der Universität Heidelberg. Direktor: Geh. Rat Professor Dr. V. Czerny Exzellenz), "Zur Kenntnis und Verwertung der Rolle des Lecithins bei der biologischen Wirkung der Radium- und Röntgenstrahlen," Deut. Med. Wschr., 31^{1} (1905) 61-3. A report to the tenth Röntgen Congress favored the lecithin hypothesis, see Paul Krause (Prof. Dr., Bonn), "Die biologischen Wirkungen der Röntgenstrahlen auf tierisches un menschliches Gewebe," Verh. Deut. Rönt. Ges., 10 (1914) 24-45. For effects on enzymes, see the summary in S. Loewenthal, Grundriss der Radiumtherapie und der biologischen Radiumforschung (Wiesbaden: J. F. Bergmann, 1912), pp. 45 and 49-52.

16. Oscar Hertwig (aus dem anatomisch-biologischen Institut zu Berlin), "Die Radiumkrankheit tierischer Keimzellen. Ein Beitrag zur experimentellen Zeugungs- und Vererbungslehre," Arch. Mikro. Anat., 77, Abt. II (1911) 1-164; Paula Hertwig, "Durch Radiumbestrahlung hervorgerufene Veränderungen in den Kernteilungsfiguren der Eier von *Ascaris megalocephala*," ibid., 301-11; and Oscar Hertwig, "Versuche an Tritoneiern über die Einwirkung bestrahlter Samenfäden auf die tierische Entwicklung. Zweiter Beitrag zur experimentellen Zeugungs- und Vererbungslehre," Arch. Mikro. Anat., 82, Abt. II (1913) 1-63. For Oscar Hertwig's advocacy of the chromosome theory of heredity, see Der Kampf um Kernfragen der Entwicklungs- und Vererbungslehre (Jena: Gustav Fischer, 1909).

17. Manfred Fraenkel (Charlottenburg), "Die 'Vererbung erworbener Eigenschaften' mittels Röntgenbestrahlung nachgewiesen," Verh. Deut. Rönt. Ges., 7 (1911) 91-99, with discussion, and Pierre Marie, Jean Clunet and Gaston Raulot-Lapointe, "Hérédité des caractères acquis, chez les cellules néoplastiques," Bull. Ass. Franc. Cancer, 4 (1911) 166-72.

18. On the acceptance of the chromosome theory of heredity, see the brief discussion in Garland Allen, Life Science in the Twentieth Century (New York: Wiley, 1975), pp. 56-68. For the colloidal aggregate theory of proteins, there appears to be no good, comprehensive secondary account, but see the tendentious denigration in M. Florkin and E. M. Stotz, "The Dark Age of Biocolloidology," Chapter 14 of A History of Biochemistry (Amsterdam: Elsevier, 1972), the brief description in Robert Olby, The Path to the Double Helix (London: Macmillan, 1974), pp. 6-7, and the references to the important role of the colloidal aggregate theory in studies of the antigen-antibody reaction between 1900 and 1920 in Pauline M. H. Mazumdar, "The Antigen-Antibody Reaction and the Physics and Chemistry of Life," Bull. Hist. Med., 48 (1974) 1-21.

19. Victor Henri and André Mayer (laboratoire de physiologie de la Sorbonne), "Action des radiations du radium sur les colloïdes," C. R. Soc. Biol. (Paris), 56 (1904) 229-30 and "Précipitation des colloïdes positifs par les radiations β du radium," C. R. Soc. Biol. (Paris), 57 (1904) 33-34.

20. Tribondeau and Récamier, "Altérations des yeux et du squelette facial d'un chat nouveau-né par röntgenisation," C. R. Soc. Biol. (Paris), 58 (Réunion biologique de Bordeaux, séance du 6 juin 1905) 1031-32.

21. H. Bordier, "Actions biochimiques des radiations et en particulier des radiations de Röntgen," Arch. Elec. Med., 22 (1913) 289-314, presented to the Fourth International Congress of Physiotherapy (Berlin, 23-30 March 1913), and in German "Biochemische Wirkung der Strahlen, insbesondere der Röntgenstrahlen," Strahlenth., 2 (1913) 367-95. Bordier had suggested earlier that radiation precipitated colloidal proteins in causing damage to the fingernails of radiologists, see H. Bordier (Professeur agrégé à la Faculté de médicine de Lyon), "Actions des rayons X sur les ongles: essai d'interprétation des effets de ces rayons sur les tissus vivants," Congrès International pour l'Étude de la Radiologie et de l'Ionisation (tenu à Liège de 12 au 14 septembre 1905), Comptes rendus (Paris: H. Dunod, 1906) 86-88.

22. H. Heineke (Leipzig), "Zur Theorie der Strahlenwirkung, insbesondere über die Latenzzeit," Munchen. Med. Wschr., 61 (1914) 807-14.

23. W. D. Coolidge (Research Laboratory of the General Electric Company, Schenectady, New York), "A Powerful Röntgen Ray Tube with a Pure Electron Discharge," Phys. Rev., 2 (1913) 409-30. For a biography of Coolidge, see John Anderson Miller, William David Coolidge: Yankee Scientist (Schenectady, New York: Mohawk Development Service, 1963).

24. For Rutherford's comment, see his Silvanus P. Thomson Memorial Lecture, J. Rönt. Soc., 14 (1918) 75-86, at 83. For the notion that the General Electric research program was "pure," see E. Dale Trout, "Tubes and Generators," in A. J. Bruwer, Classic Descripitons in Diagnostic Röntgenology (Springfield, Illinois: Charles C. Thomas, 1964) pp. 213-23, at 217: "From a program of pure research, three men, Coolidge, Langmuir and Dushman reported on work that would bring significant changes in X-ray technology."

25. O. W. Richardson (B. A., Coutts Trotter Student, Trinity College), "On the Negative Radiation from Hot Platinum," Proc. Camb. Phil. Soc., 11 (1902, read 25 November 1901 and "The Electrical Conductivity imparted to a Vacuum by Hot Conductors," Proc. Roy. Soc., 71 (1903) 415-18, communicated by Professor J. J. Thomson (F. R. S.), received 28 February and read 26 March 1903. Thomas Edison was the discoverer of the emission of "negative radiation" from hot filaments, which was generally known as the Edison effect.

26. I. Langmuir, "The Effect of Space Charge and Residual Gases on Thermionic Currents in High Vacuum," Phys. Rev., 2 (1913) 450-86.

27. W. Gaede (Tech.-Physik. Inst. d. Universität, Freiburg i. Br.), "Die Molekularluftpumpe," Ann. Phys., 41 (1913) 337-80. For this and other developments, see Saul Dushman, Scientific Foundations of Vacuum Technique (New York: Wiley, 1949), pp. 126-7, 136-59 and 176-90.

28. W. D. Coolidge, "Ductile Tungsten," Trans. Amer. Inst. Elec. Eng., 29 (1910) 961-65, as reproduced in A. J. Bruwer, note 24 above, 247-49, at 249.

29. See, for example, J. H. Gardiner, "Quantitative Measurements of the Conversion of Kathode Rays into Röntgen Rays by Anti-kathodes of Different Metals," J. Rönt. Soc., 6 (July 1910) 83-97, with discussion.

30. W. D. Coolidge, "Tungsten and Method of Making Same for Use as Filaments of Incandescent Lamps and for Other Purposes," U. S. Patent 1,082,933, filed 19 June 1912 and issued 30 December 1913, as described by Trout, note 24 above.

31. Coolidge, note 28 above, at 249.

32. J. E. Lilienfeld and W. J. Rosenthal (aus dem physikalischen Institut (Direktor: Prof. Dr. O. Wiener) und dem chirurgisch-poliklinischen Insitut (Direktor: Prof. Dr. H. Heineke) der Universität Leipzig), "Eine Röntgenröhre von beliebig und momentan einstellbarem, vom Vakuum unabhängigem Härtegrad," Fortschr. Röntgenstr., 18 (1912) 256-63. Lilienfeld and Rosenthal used an auxiliary hot cathode to increase the flow of current through the tube.

33. Derek J. de Solla Price, Science Since Babylon (New Haven: Yale University Press, 1961), pp. 102-104.

34. For the plans for the Vienna "Congress for Radioactivity and Electronics," see the letters between E. Rutherford and Stefan Meyer (Rutherford Correspondence microfilms), 25 October 1913 to 29 June 1914. I have been unable to find a clear reference to a 1916 International Conference of Medical Electrology and Radiology, but such conferences had been held every two years from 1906 to 1914, see Antoinette Béclère, Antoine Béclère (Paris: J. B. Baillière, 1972) at 420.

35. For mention of war-time delays, see Fortschr. Röntgenstr., 24 (1916-17) 516 and J. Rönt. Soc., 15 (1919) 94.

36. Béclère (Physician to the Hospital of Saint Antoine, Member of the Academy of Medicine), "Communication on the Radiotherapy of Uterine Fibroids, With the Results of 400 Cases, personally observed, the Mode of Action of the Treatment, and the Indications for its Adoption," Arch. Radiol. Electroth., 24 (1919-20) 254-61, translated from J. Radiol. Electroth., 3 (1918-19) 433-39. Béclère regarded this as a sequel to his August 1913 report to the 27th International Congress of Medicine (London), "Le traitement des fibro-myomes utérins par les rayons de Röntgen."

37. Mr. Haldane (Haddington) gave this reply in response to a question raised in the House of Commons by Mr. Weir (Ross and Cromarty), see "Notes," J. Rönt. Soc., 3 (February 1907) 78.

38. "Interdiction de sortie des publications médicales," Arch. Elec. Med., 25 (1915) 88 (feuilles de garde).

39. The Archives d'Electricité médicale, a large portion of which was devoted to radiological research, published over 1200 pages in 1913 and shrank to a mere 391 pages in 1915. The Journal de Radiologie et d'Electrologie (which was founded in 1914) published 751 pages in the year 1914-15, the same number of pages in the two-year period 1915-17 and only 576 pages in 1918 and 1919 combined. The Journal had still not recovered its initial size in 1920.

40. "Editorial," Arch. Radiol. Electroth., 23 (June 1918) 1-2.

41. "The Shortage of X-Ray Tubes," Arch. Rönt. Ray., 19 (October 1914) 159-60 and the letter of A. E. Dean at p. 197.

42. Geoffrey Pearce, "The Future of the British X-Ray Industry," J. Rönt. Soc., 13 (1917) 60-87 and 91-106.

43. In 1913, the German Röntgen Society had 655 members and the British Röntgen Society had 188 members, see Verh. Deut. Ges. Rönt., 9 (1913) and J. Rönt. Soc., 9 (1913) 114-18; in 1914 the American Röntgen Ray Society had 170 members, see Amer. J. Rönt. (1913-14) 394-97. See also Arch. Rönt. Ray., 18 (1913-14) 317, where Strahlentherapie is described as "by far the most important periodical devoted to Röntgen- and Radio-therapy."

44. Volumes 23 (1915-16), 24 (1916-17) and 25 (1917-18) of the leading German medical radiological journal, Fortschritte auf dem Gebiete der Röntgenstrahlen, averaged 567 pages. Volume 25 was printed on noticeably lower quality paper, and volume 26 (1918-19) was 482 pages, smaller than each of the previous three volumes. But by the early 1920s, the journal had grown beyond its previous size, despite the resumption of the German Röntgen Society congresses and the publication of their Verhandlungen in 1920.

45. J. Battersby (M. B., Major, R. A. M. C.), "The Present Position of the Röntgen Rays in Military Surgery," Arch. Rönt. Ray., 3 (1899) 74-80; H. Küttner (Assistenarzt der Tübinger Chirurgischen Klinik), "Ueber die Bedeutung der Röntgenstrahlen für die Kriegschirurgie nach Erfahrungen im Griechisch-Türkischen Krieg 1897," in Beit. Klin. Chir., 20 (1898) 167-230; F. C. Abbott, "Surgery in the Graeco-Turkish War," Lancet (1899) 30 and 152; W. C. Borden (Captain and Assistant Surgeon, U. S. Army), The Use of the Röntgen Ray by the Medical Department of the United States Army in the War with Spain, a report to Brigadier General

G. M. Sternberg, Surgeon-General, U. S. Army (Washington: U. S. Government Printing Office, 1900); and J. Hall-Edwards, "Bullets and their Billets: Experiences with X-Rays in South Africa," Arch. Rönt. Ray., 6 (January 1902) 31-39.

46. Haret (Paris), "La radiologie dans le Service de Santé de l'armée française pendant la guerre 1914-18," Bull. Acad. Med. (Paris), 81 (27 mai 1919) 718-20.

47. William A. Duncan (Captain, Medical Corps, U. S. Army), "The X-ray Equipment and Work in the Army at the Present Time," Amer. J. Rönt., 1 (1913-14) 275-85, read by invitation at the Boston meeting of the American Röntgan Ray Society and P. M. Ashburn, A History of the Medical Department of the United States Army (Boston: Houghton-Mifflin, 1929).

48. Willis F. Manges, "Military Röntgenology," in O. Glasser, The Science of Radiology (Springfield, Illinois: Charles C. Thomas, 1933) 187-97. Physicians who claimed to be röntgenologists had a relatively low rejection rate (4.6%, compared to 70% for otolaryngology) on United States Army tests, see Edward L. Munson (M. D., Brigadier-General, U. S. Army), "The Needs of Medical Education as Revealed by the War," J. Amer. Med. Ass., 72[2] (1919) 1050-55, read before the 15th Annual Conference of the Council on Medical Education of the American Medical Association, Chicago, 3 March 1919.

49. G. B. Batten (M. D.) in a discussion on "The Injurious Effects Produced by X-Rays" held at the Röntgen Society on 1 February 1916, J. Rönt. Soc., 12 (1916) 38-56, at 53.

50. R. C. Robinson and C. N. Moore (Research Laboratory, General Elecgric Company), "Manufacture of the Coolidge X-Ray Tube," Amer. J. Rönt., 7 (1920) 254-60. A few glass-blowing operations still had to be done by hand, but Robinson and Moore do not say which ones.

51. W. D. Coolidge and C. N. Moore (General Electric Research Laboratory), "A Portable Röntgen-ray Generating Outfit," Gen. Elec. Rev., 21 (1918) 60-67.

52. Coolidge had first demonstrated his tube in December 1913, when there was reportedly "...no arrangements...yet...made for its commercial manufacture," see the "Editorial," Amer. J. Rönt., 1 (1913-14) 90-91, at 91. But in April 1914, the Coolidge tube was the sensation of the 10th congress of the German Röntgen Society, see Fortsch. Röntgenstr. (1914-15) 142, where a German-made model was described, see Levy-Dorn (Berlin), "Über die neue Coolidgeröhre der A. E. G.," Verh. Deut. Ges. Rönt., 10 (1914) 156-57, with discussion 158-60. In France, the Coolidge tube was described by J. Belot and W. Vignal, "À propos d'un nouveau tube de Röntgen à vide de Hittorf (tube Coolidge)," J. Radiol. Electroth., 1 (1914-15) 227-32, but Béclère, note 36 above, was just beginning to use a Coolidge tube in 1918. The British Thomson-Houston Company owned the British patents ("Notes," J. Rönt. Soc., 11 (1914-15) 50), but an English visitor to the 1923 congress of the German Röntgen Society reported, "One was struck by the state of hot cathode tube production in Germany and Austria , which appears to be far in advance of England," see W. E. Schall (B. Sc.), "The German Röntgen Society," J. Rönt. Soc., 19 (1923) 172-74.

53. George Siemens, Der Weg der Elektrotechnik, 2 volumes (Freiburg/Müchen: Karl Alber, 1966), II, 112ff.

54. See the obituary of Christen by Bernhard Walter, Fortschr. Röntgenstr., 28 (1921-22) 391-92.

55. "Radium and Clinical Research," J. Rönt. Soc., 16 (July 1920) 83, where the initial loan of five grams to the Middlesex Hospital was reported. For a later report on the use of this war-generated supply of radium, see the U. K. Medical Research Council, Medical Uses of Radium, Summary of Reports from Research Centres for 1923, Special Report Series no. 60 (London: His Majesty's Stationery Office, 1924). An earlier report received only limited circulation and is not readily available.

56. The figure is from Robert Reid, Marie Curie (New York: New American Library, 1974) at 216. See also "Radium by the Gramme," J. Rönt. Soc., 16 (July 1920) 83, where it was reported that three new radium factories had been established in the United States since 1913. The Standard Chemical Company of Pittsburgh alone produced 18 grams per year, which was said to be as much as all the French producers combined.

57. National Physical Laboratory, Report for the Year 1902 (London: Harrison, 1903), p. 7.

58. Bureau of Standards, War Work of the Bureau of Standards, Miscellaneous Publication 46 (Washington: Government Printing Office, 1921) pp. 251-55 and 298-99. The National Physical Laboratory in 1916 began issuing certificates specifying the percentage of hard, medium and soft X-rays absorbed by protective materials, see J. H. Gardiner in the discussion at the Röntgen Society, note 49 above, at 56. A proposal that protection materials be tested was made at the April 1914 German Röntgen Society Congress, see David (Halle), Verh. Deut. Ges. Rönt., 10 (1914) 19. The Royal Physical-Technical Institute began work with X-rays at the beginning of the War and established a special Röntgenstrahlenlaboratorium in 1919, at the behest of industry (especially the Osram-Kommanditgesellschaft), see Behnken (Berlin), "Die Eichung von Dosismessern in absolutem Masse in der Physikalisch-technischen Reichsanstalt," Verh. Deut. Ges. Rönt., 15 (1924) 92-94.

59. John Sandford Shearer, see the obituary in Amer. J. Rönt., 9 (1922) 520-24.

60. The proposal for these protection recommendations originated in a "Discussion on Protective Devices for X-ray Operators," at the 1 June 1915 Annual General Meeting of the Röntgen Society, J. Rönt. Soc., 11 (1914-15) 110-13, at 113: "...in view of the recent large increase in the number of X-ray installations, this Society considers it a matter of the greatest importance that the personal safety of the operators conducting the X-ray examinations should be secured by the universal adoption of stringent rules...." In response to this resolution, the Council of the Röntgen Society decided in September 1915 "that a set of rules should be prepared for the protection of X ray operators," see the circular letter dated 18 September 1915 signed by Robert Knox (M. D., Honorary Secretary) in a box of Röntgen Society documents in the library of the British Institute of Radiology referred to below as RS (BIR). The rules, ibid., were dated November 1915. A proposal to purchase for the War Office a model X-ray installation apparently went unfulfilled, see the "Annual Report," J. Rönt. Soc., 11 (1914-15) 92-102.

61. See the letter of acknowledgment dated 7 February 1916 on War Office stationery from a Lieutenant Colonel whose signature is illegible, RS (BIR), ibid.

62. See the draft attached to Knox's letter, note 60 above.

63. The British Association for the Advancement of Radiology and Physiotherapy, whose primary object was the establishment of a post-graduate diploma, was established in 1917. See also J. S. Shearer (Ph. D., Cornell University), "Graduate Instruction in Röntgenology," Amer. J. Rönt., 9 (1922) 459-64.

64. For a description of Marie Curie's work during the War, see Chapter 19 of Reid, note 56 above. For Curie's own story, see Mme Pierre Curie, La Radiologie et la guerre (Paris: Librairie Félix Alcan, 1921). Curie's figures on the numbers of X-ray installations, physicians and operators differ from those of Haret, note 46 above.

65. Unless otherwise noted below, biographical details are taken from various volumes of J. C. Poggendorff, Biographisch-literarisches Handwörterbuch.

66. The obituary in Strahlenth., 136 (1968) 765-66.

67. R. G. Jaeger, "In memoriam Hermann Behnken," Strahlenth., 139 (1970) 113-15.

68. Paul Forman, John L. Heilbron, and Spencer Weart, "Physics circa 1900: Personnel, Funding, and Productivity of the Academic Establishments," Hist. Stud. Phys. Sci., 5 (1975), at 54.

69. Ibid., at 53.

70. On the founding of the Society for Technical Physics, see Paul Forman, The Environment and Practice of Atomic Physics in Weimar Germany: a Study in the History of Science, Ph. D. thesis, University of California at Berkeley, 1967 (Ann Arbor, Michigan: University Microfilms, 1968) 139 ff.

71. George Gehloff, "Funf Jahre Deutsche Gesellschaft für technische Physik," Z. Tech. Phys., 5 (1924) 201-204

72. For some autobiographical comments by Holthusen, see the 23 January 1971 lecture he gave at Würzburg reproduced in Hans v. Braunbehres and Walter Frommhold, "Hermann Holthusen," Fortschr. Röntgenstr., 115[1] (1971) 141-46. Holthusen's contact with physics appears to have begun not with X-rays but with a study of the absorption of radium emanation by blood, see C. Ramsauer and H. Holthusen (aus dem Radiologischen Institut und der Medizinischen Klinik der Universität Heidelberg), "Über die Aufnahme der Radiumenataion durch das Blut," Sitzungsber. Akad. Wiss. (Heidelberg), Math.-naturwiss. Kl., Abt. B (Biol. Wiss.), Jahrgang 1913, 2. Abh. vorgelegt von P. Lenard, eingegangen 25. Februar 1913.

73. The obituary by Wilhelm Flaskamp, Strahlenth., 79 (1949) 3-10.

Chapter 5: X-ray Measurements and Radium Protection, 1914-24

> In the question of practical dosimetry the physicist has for the present given the word; here it is the duty of the clinician to carry the work further on the available basis.
>
> --W. Friedrich (Ph. D.) and O. A. Glasser (Ph. D.), 1922[1]

> The question of radium protection is now in the midst of development, and comparable to the state in which the question of X-ray protection was fifteen years ago.
>
> --Thibonneau (M. D.), 1921[2]

Two contrasts between medical work with X-rays and with radium that I have highlighted in previous chapters would fade during World War I and in the early 1920s. First, ionization measurements would become widely recognized as desirable in X-ray clinics, thus closing the gap between clinical measurements of X-rays and radium. Second, radium protection, which had been of little professional concern before the War, would grow suddenly in importance after the War and would become, like X-ray protection, the subject of special studies and recommendations.

The first of these developments, which took place initially in Germany among the small group of research workers I described in Chapter 4, will raise once again the issue of the relationship between practical concerns in medical radiology and contemporary scientific knowledge. It would be reasonable to suppose that the shift to ionization measurements was based on theoretical understanding of the mechanisms underlying the biological effects of radiation. I have discussed in Chapter 4 the early development of an ionization theory of radiation effects and I shall continue that discussion below. As it turned out, however, even within the small group of research workers directly involved there would be no agreement before 1925 on the physical and chemical mechanisms of radiation effects. Instead, the reason for the shift to ionization measurements in clinical X-ray work would be the practical requirements of a new style of deep therapy. Despite many advances, scientific theory still failed to offer a firm basis for medical practice.

The second development with which we shall be concerned in this chapter, the development of radium protection, will raise again the issue of the relationship between professional "self-regulation" and public pressure. Outside Germany, the risks of radium exposure would become a subject of concern in the early 1920s. As with the previous development of X-ray protection, radium protection would, as we shall see, be a professional response to public outcry, this time expressed primarily in the newspapers rather than in the courts.

In tracing the development of ionization measurements for clinical X-ray work, we must first return to developments in deep therapy with X-rays where we left them in Chapter 3. Deep therapists at Freiburg had, as I mentioned there, reported delivering very high doses to the skin without causing harm. The doses had been measured with Kienböck's silver bromide strips. The Freiburg claims were greeted with disbelief at other clinics, where much lower doses had often been observed to cause severe skin reactions. The practical problem of comparing the Freiburg results with results in other gynecological clinics inspired in the German medical radiological community a practical proposal: to compare the most commonly used dosimeters and to draw up tables that would enable their readings to be interconverted. Responding to an appeal by its President, the German Röntgen Society established a new subsidiary group, the Special Commission for Comparison of Dosimeters.[3] Despite the War and the suspension of the Röntgen Society congresses, the Commission remained nominally active until 1918, with nine of its sixteen members indicating that they could participate.[4] The most important conclusion of the Special Commission was reached, however, in 1915, after eight of the members had replied to a request for suggestions as to how to proceed. Their views on the heterogeneity of the X-rays in use and on the usefulness of the available clinical dosimeters in comparing quantities of X-rays of different qualities varied widely. Guido Holzknecht, who had introduced the "chromoradiometer" more than a decade earlier, reported in his capacity as Chairman of the Commission:

> The simple comparison of the dosimeters was expressly or implicitly characterized by most of the committee members as impossible. It is therefore necessary that, first of all, some sort of exact measurement procedure be created, and that would be a very complicated laboratory task....For this, all those who have done and can do physics and mathematics should be called upon.[5]

Many interesting papers were published as <u>Proceedings</u> of the Commission, but its name was changed to the Special Committee for Röntgen Ray Measurement and its initial purpose remained unfulfilled.[6] A similar British group, created by the Röntgen Society, also failed in 1915 to come to definitive recommendations on X-ray measurements, and in France it was concluded that the problem was "too complex for the construction of measuring instruments giving precise readings that were always intercomparable."[7]

Within Germany, the response to Holzknecht's call for "all those who have done and can do physics and mathematics" was more immediate than it would have been in peacetime. Already in 1914 the physicist Gustav Grossmann had pointed out to the medical radiological community that the silver bromide in Kienböck's quantimeter was not suitable for measurements of X-ray quantity over a wide range of qualities because silver had a selective absorption edge within the range of X-ray wavelengths used for deep therapy. Selective absorption would cause the Kienböck strips to darken more rapidly at some wavelengths than at others, leading to dosage readings that were much higher than the doses actually delivered to the tissue being irradiated. Barium and platinum, constituents of the widely used Sabouraud-Noiré and Bordier pastilles, were also known to have selective absorption edges, but for the moment they lay beyond the range of X-ray wavelengths commonly used for therapy, at voltages above 50,000. Outside areas of selective absorption, the absorption coefficients for all materials were parallel as hardness changed, so Grossmann suggested that the ideal dosimeter was one that had an atomic weight less than aluminum, the lightest element in which selective absorption had been observed. The obvious choice for the "test body" was air, and the obvious method to a physicist was the familiar technique of measuring the saturation current due to ionization.[8]

The medical radiological community was not won over immediately. Grossmann's work seemed "theoretical," and the question of the so-called "silver error" was regarded as unsolved.[9] At the same time, the Freiburg gynecologists who had been reporting extremely high doses on the basis of their measurements with Kienböck's strips could not afford to ignore Grossmann's suggestions about the selective absorption of silver. One of them, Bernhard Krönig, brought Walter Friedrich, the Munich physicist who had worked with Lane and Knipping, to Freiburg to work on the dosimetry problem as well as on the problem of determining the biological effectiveness of X-rays of different quality. In 1918, Krönig and Friedrich published an extensive series of experiments undertaken in 1915 and 1916 at the Women's Hospital using a Coolidge tube.[10] These experiments confirmed Grossmann's suspicions concerning the importance of selective absorption. The percentage absorption of X-rays of different wavelengths, and of gamma rays, in silver and in platinum was not parallel to the absorption in water, which Krönig and Friedrich demonstrated was equivalent to soft tissue. The absorption of X-rays and gamma rays in air was, however, closely parallel to their absorption in water and thus in tissue. As a practical matter, then, Krönig and Friedrich concluded that ionization measurements in air would give doses that were comparable over a wide range of wavelengths. They explicitly aimed to measure dose in Christen's sense (the energy absorbed in a given volume) rather than X-ray quantity (the energy passing through a surface).

In order to make ionization measurements comparable among clinics, Krönig and Friedrich defined an absolute unit of radiation dosage e as "that amount of radiation which by ionization in one cubic centimeter of air transfers one electrostatic unit of charge in a saturation current." This unit is similar to a unit that had been used by a

French physicist a decade earlier as well as to the "röntgen," the unit of X-ray dosage that a decade later would be adopted on the international level.[11] The recurrence of this unit is not surprising. To the physicist, saturation current was the usual method of measuirng X-ray intensity, and it was a short step to measure dosage in terms of the charge transported by the saturation current. There were, however, other possibilities for an ionization unit of radiation dosage. One possibility was the "megamegaion" (the number of ions, in million millions, produced in air). This unit required the same sorts of measurements as Krönig and Friedrich's e, but it also required an additional caclulation after the amount of charge transported had been determined, or the calibration of the electrometer in number of ions rather than in electrostatic units. The "megamegaion" never came into general use.[12] Another possibility was to use the ionization produced by a given amount of radium as a unit of X-ray dosage, a proposal made even before the War in Britain. This procedure did not gain favor in Germany, but radium was used there to verify the constancy of measuring instruments and, as we shall see in the next chapter, the French in the post-War period would favor using radium as the basis of a unit of X-ray dosage.

More important than the wording of the definition of Krönig and Friedrich's e unit were their procedures for measuring ionization. For the first time in the medical X-ray literature, they presented an extensive discussion of the sources of error in ionization measurements, including inadequate insulation, the dielectric polarization and de-polarization of the insulating material, protection of the ionization chamber from electrostatic forces and from radiation other than the radiation in the primary beam, and the nature of the materials of which the ionization chamber was constructed. This last point concerning materials was especially important. Ionization chambers for clinical use had been introduced in Germany just before the War. They were small, usually one cubic centimeter in volume, so that they could be easily handled and even introduced into the patient's body; rectal insertion during irradiation of the ovaries was one method of measuring the dose actually delivered near the target. The commercially available ionization chambers had metal walls. As Krönig and Friedrich pointed out, metal walls would give off secondary radiation when X-rays were absorbed in them. The ionization measured would then not be entirely from the air in the chamber, but would include ionization by both characteristic X-rays and secondary cathode rays from the walls. To avoid the characteristic wall radiation, Krönig and Friedrich used a cow's horn ionization chamber (whose walls were made conducting by a coating of graphite). This small chamber with walls of low atomic weight material, which did not emit secondary X-rays, Krönig and Friedrich calibrated in their e unit using an ionization chamber that they believed sufficiently large to measure the total ionization due to the secondary cathode rays arising from the absorption of X-rays in the air.

Using the cow's horn chamber, or an aluminum chamber whose readings were corrected for wall radiation, Krönig and Friedrich went on to answer two important questions for deep therapy: how did deep doses calculated from the surface dose and the absorption coefficient compare with experimentally determined deep doses? and how did the biological effectiveness of X-rays vary with quality? The former question was not one that others in the medical radiological community had asked, and it seems likely that Friedrich expected that the calculated doses and the measured doses would not agree. The exponential law of absorption, on which the calculations depended, did not take into account scattered X-rays, which would add to the ionization a dose that came to be called the "Streuzusatzdosis." This added scattering dose would increase with the hardness of the primary beam, and because of scattering the dose delivered at the center of the "field" of the primary beam would be greater the larger the size of the field. Krönig and Friedrich readily confirmed these phenomena with experiments conducted in a basin of water (a "phantom"). The practical problem of delivering a specified dose to deep-lying tissue thus became more complex than before. Numerous experimenters over the next decade would work on determining doses in water phantoms with different combinations of filters, focal distance, field size, and other parameters.[13]

Krönig and Friedrich also answered the question of whether X-rays of different quality had the same effects, and with their answer came a simplification of therapeutic concepts. Using frog larvae (Rana temporaria or Rana esculenta) that were known from Hertwig's work to suffer readily visible malformations as a result of exposure to radiation, they showed that the biological effects were independent of the quality of the rays. The effects depended only on the absorbed dose measured by their ionization chamber, a quantity that they assumed to be identical to, or at least proportional to, the energy of the absorbed radiation. These experiments with frog larvae were confirmed by less extensive experiments on garden peas (Vicea fava) and on human ovaries. The dependence of biological effects on the absorbed dose alone suggested that it might be possible to measure the doses required to bring about specific effects, regardless of the quality of the radiation used. Krönig and Friedrich defined, and made some preliminary attempts to measure, an "ovarian" dose (the dose required to produce cessation of ovulation and menstruation), an "erythema" dose (the dose required to produce a distinctly visible reaction on the skin), and a "carcinoma" dose (the dose required to produce a palpable decrease in the size of a carcinoma).

It remained for Ludwig Seitz, director of the University Women's Hospital at Erlangen, and Hermann Wintz, the young physician who obtained a doctorate in physics, to apply these concepts in extensive clinical trials.[14] Seitz and Wintz worked initially with pre-War X-ray tubes desiged specially for deep therapy, but they turned eventually to Coolidge tubes excited with up to 200,000 volts. From 1914 on, they treated dozens of cases of carcinoma and sarcoma that came into the military clinic housed in the Erlangen Women's Hospital during the War. They also treated five hundred cases of uterine fibroma and of functional uterine bleeding. The Erlangen gynecologists measured their doses with an "iontoquantimeter" produced by Reiniger, Gebbert and Schall, and with similar instruments designed in cooperation with an electrical engineer. In accordance with Krönig and Friedrich's suggestions, Seitz and Wintz tried to avoid characteristic radiation from the walls of the ionization chamber, but they did not use the physically-oriented e unit for X-ray dosage. They had no large ionization chamber and thus lacked a way to standardize their ionization chambers in absolute units. Instead, Seitz and Wintz turned to what they called a "biological system of measurements" to ensure compatibility among different clinics. The basis of this system was Krönig and Friedrich's "erythema" dose, which Seitz and Wintz recast as the "unit skin dose" (Hauteinheitdosis). The unit skin dose was the dose of filtered X-rays required to produce a reddening of the skin within a specified time period. For healthy, previously unirradiated skin on a particular part of the body, Seitz and Wintz determined experimentally that the variations in the unit skin dose were relatively small, on the order of 10 or 15 per cent, and they therefore thought themselves justified in expressing dosage in terms of the unit skin dose.

In their clinical trials, the Erlangen gynecologists found that the doses required to cause permanent cessation of ovulation, or to cure carcinoma or sarcoma, were also remarkably constant, as Krönig and Friedrich had suggested. The "castration" dose was 34 per cent of the unit skin dose, the "carcinoma" dose was 110 per cent of the unit skin dose and the "sarcoma" dose was 60-70 per cent of the unit skin dose. These doses Seitz and Wintz delivered, as far as possible, in a single sitting by increasing the focal distance, by irradiating from several different directions, and by using fields that were as large as possible without overlap. They thus avoided what they termed, again following Krönig and Friedrich, the "dissipation" (Verzettlung) of the dose by the usual procedure of delivering it over the course of several sittings spread out over days or weeks. The objective in this intensive deep therapy was to kill the cancer cells, or in the case of ovarian treatments to kill the oogonia and ova. The success rates were extremely high, with 100 per cent success reported in 500 cases of at least temporary female sterilization produced in one or more sittings. Of 170 cases in which sterilization had been produced in a single sitting, 77 per cent had remained sterile for at least one year. Problems did sometimes arise from exposure of the intestines during treatment and the resulting "Röntgen hangover" (Röntgenkater), several instances of "late" effects (dermatitis developing long after irradiation) were observed, and there were generally changes in the patient's blood. On the whole, however, Seitz and Wintz regarded serious harm as rare.

As we shall see in the next chapter, the intensive deep therapy of Seitz and Wintz rapidly became known abroad as the "German" style of therapy. Within Germany, however, the Erlangen technique faced a good deal of opposition. To many practitioners, the technique had the same disadvantages as the single-sitting technique that Kienböck had introduced for superficial therapy twenty years earlier: it was overly schematic and did not permit adjusting the treatment to the progress of individual cases. The margins of safety seemed exceedingly small to professionals who were convinced that protection was essential. In the many-sittings approach, the skin and blood of the patient were given an opportunity to recover between irradiations, the Röntgen hangover was less frequent, and in treatment of uterine fibromas the artificially-induced menopause came about more slowly and the fibromas disintegrated more gradually.[15] Opposition also came from practitioners who disagreed with the basic objective of the Erlangen technique, which was to kill cells. It was possible that radiation cured by stimulating the body's natural defense mechanisms. This notion had been discussed earlier, and as we shall see by 1920 it was a dominant view in England. In Germany, the introduction of intensive deep therapy raised the issue anew, since the stimulative effect of radiation was believed to exist at small doses rather than at the high doses Seitz and Wintz were using.[16] Repeated applications of the "stimulation" dose (Reizdosis) posed a sharp contrast to the application of large doses in a single setting.

The Erlangen technique also ran into opposition because of the reliance on ionization measurements. Opposition among practitioners was to be expected, but more important for the moment was opposition among research workers to ionization as the basic mechanism of radiation effects. On the biological side, investigations of the precipitation of colloids by radiation had shown that denaturation preceded precipitation. The primary effect of the radiation was then probably a chemical change, not a physical discharge of the colloidal particles.[17] On the physical side, the assumption that ionization in air measured the absorbed energy was coming into question. This assumption, as I have mentioned above, was implicit in most pre-War laboratory work with radiation. Ionization was not, however, the only effect observed when radiation was absorbed. Heating and chemical changes might also occur, and there was no guarantee that the proportion of these different physical effects remained the same over a wide range of wavelengths. Christen, from his post at the Radiation Research Unit of Reiniger, Gebbert and Schall, raised this question of the relationship between ionization and absorbed energy in 1916, and Richard Glocker, in the first review of medical radiological dosage techniques published in a physical journal, reiterated the underlying assumption and emphasized its importance: "All ionization measurements of the energy of radiation rest on the assumption that the number of ions produced per unit volume of gas is, independent of the hardness of the rays, directly proportional to the radiation energy absorbed in the unit volume."[18]

Beginning in 1919, Hermann Holthusen, the physician who had worked with Lenard and habilitated at Heidelberg, denied this vulnerable assumption of proportionality between ionization and absorbed energy, attacked the ionization theory of radiation effects and put forward an alternate, though equally reductionist, view of the underlying mechanism. Krönig and Friedrich had ostensibly chosen ionization as the basis for their X-ray measurements on practical grounds. Their finding that biological effects were proportional to ionization independent of X-ray quality neverthless strengthened the notion that ionization was the fundamental mechanism. Holthusen, by contrast, was convinced that excitation of biologically important molecules (without ionization) could bring about chemical changes that caused the observed radiation effects. To decide the issue, Holthusen proposed measuring biological effects resulting from exposures to X-rays of different quanlities while maintaining, in one series, equal doses measured by ionization and, in another series, equal doses measured by absorbed energy. If the biological effects were the same when ionization was the same even though the quality of the X-rays had changed, Holthusen would conclude that ionization was the underlying mechanism. If the biological effects were the same when the absorbed energy was the same even though the quality of the X-rays had changed, Holthusen believed he would have *prima facie* evidence against ionization as the underlying mechanism and for his own "chemical" view.

In his assumption of proportionality between cause in the physical world and effect in the biological world, Holthusen betrayed a remarkable innocence that many of his colleagues did not share. Moreover, the experimental evidence Holthusen would offer was subject to interpretations different from his own. In some respects, however, Holthusen's analysis was sophisticated, and it had the merit of being the first substantial effort to work out a reductionist view in detail. A firm believer in the electromagnetic theory of X-rays, Holthusen assumed that the Planck-Einstein relation held for the conversion in air of X-ray energy into the energy of secondary cathode particles, just as the relation held for the conversion of the energy of ultraviolet light into the energy of photoelectrons. Using this relation, Holthusen calculated from the frequency of an X-ray of a given hardness the initial velocity of the secondary cathode ray it produced. Holthusen's mentor in physics, Lenard, had collected extensive data on cathode rays, including the number of ions produced by cathode rays of different velocities. This ionization was not proportional to the square of the velocity of the cathode rays, as would be required by the Planck-Einstein relation if ionization was to be regarded as a measure of absorbed energy. Instead, the ratio of the number of ions produced to the square of the velocity of the cathode rays producing them increased with increasing velocity. To obtain a true measure of absorbed energy from the ionization produced by X-rays, Holthusen corrected the measured ionization with this "energy transformation factor." This procedure said nothing about the number of secondary cathode rays produced by X-rays of varying qualities, and it tacitly assumed that X-rays of different qualities produced the same number of cathode rays.

In fact, the results of Holthusen's experiments with equal absorbed energies could have been interpreted as reflecting a decrease in the number of secondary cathode rays produced with increasing hardness. Holthusen, however, ignored this complication and tried to decide on experimental grounds only between ionization and the total energy of the secondary cathode rays as a measure of the biological effects. For this purpose, he used eggs of <u>Ascaris megalocephala</u>, a worm found in horse saliva that was known to suffer radiation-induced malformations.[20] Contrary to Krönig and Friedrich's claim from their work with frog larvae, Holthusen found that when he varied the hardness of his X-rays the effects on his worms were proportional not to ionization but to the total energy of the secondary cathode rays. So far as fundamental mechanisms were concerned, this result left Holthusen short of demonstrating that chemical changes independent of ionization were the cause of the biological effects, but the result unquestionably cast doubt on the use of ionization measurements for dosage.

Also upsetting to the advocates of ionization measurements was Holthusen' demonstration that the chambers in use were inadequate to the task. Holthusen knew from Lenard's data that secondary cathode rays with a range of 10 centimeters in air were obtainable from X-rays considered "medium hard." Krönig and Friedrich, however, had used a larger chamber only 6.5 centimeters long to standardize the smaller chamber actually used in their experiments. In defining their _e_ unit, therefore, Krönig and Friedrich had failed to use the entire ionization due to the secondary cathode rays, as Holthusen demonstrated by showing that the ionization measured decreased if the hardness of the X-rays increased sufficiently to cause the range of the secondary rays to exceed the length of the chamber. Holthusen believed that the failure of Krönig and Friedrich to use all the secondary cathode rays accounted for their different results on the variation of biological effects with hardness. Had they measured the total ionization, Krönig and Friedrich would have found that the biological effects were not proportional to ionization but instead decreased with increasing hardness.

Following up his attack on ionization as the fundamental mechanism of radiation effects, Holthusen put forward an important, though ultimately fruitless, proposal concerning dosage measurements and units. Instead of the ionization unit e, Holthusen preferred a biological system for measuring dosage and a biological unit, in part because he believed that variation in biological effect with changes in radiation quality would be parallel for all biological objects and that therefore a biological method would eliminate the need for considering quality separately.[21] The notion of a biological unit had considerable appeal. Two physicians who had proposed a "mouse" dose, which they defined as the quantity of radiation required to cause death in mice by damage to lymphoid tissue, argued in favor of biological measurements as follows:

> ...because in therapy it is always a matter of producing an effect on a biological process, be it of normal or pathological character, so it would naturally be the best if one could undertake the graduation of the effect, that is the measurement of the dose, by means of a biological measurement method.[22]

A unit based on the effect of radiation on the roots of pea plants was proposed in 1920.[23] In England, a physicist who was unaware of the controversy in Germany had proposed in 1918 a unit called the "rad," which he defined as the quantity of radiation which, when absorbed by sarcoma cells, caused their eventual destruction on implantation into rats.[24] Holthusen picked up this suggestion and also proposed that Krönig and Friedrich's frog larvae, his own horse saliva worms, or the fruit flies used in the United States for genetics experiments might serve as the basis for a biological unit of dosage.

In practice, the only biological unit that was used extensively was Seitz and Wintz's unit skin dose, and it became common throughout Germany in the early 1920s. The shift to the unit skin dose from earlier units was an easy one, since most of them, like Holzknecht's H, were fractions of the dose that caused a skin reaction. The use of biological units did not, however, bring use of biological measurements. The Sabouraud-Noiré and other pastilles as well as the Kienböck strips gave way not to mice or pea plants but to ionization chambers. Reiniger, Gebbert and Schall, which was the only firm manufacturing the Kienböck strips, by 1923 was explicitly denying responsibility for any harm to patients caused when using the strips, and it was by then clear that in a law suit their use would not be considered adequate to avoid a penalty for negligence.[25] Using whatever ionization chamber it chose, each clinic determined for itself the amount of ionization required to produce a skin reaction and then reported its dosages as percentages of this unit skin dose. The method was, above all, convenient: commercially available ionization chambers could be used, no standardization with a large ionization chamber was required, and each clinic could work independently and yet compare its results with those of other clinics.

To the physicists who had entered medical radiology in the previous decade, the reliance among the practitioners on the unit skin dose was intolerable. In the first place, the unit skin dose did not meet the physicists' standard of precision. There were variations in the skin reaction used to define the dose in different clinics, and the small, commercially available chambers gave readings that depended on the hardness of the X-rays used. In 1922, a comparative study of the unit skin doses in different clinics yielded variations of around 50 per cent.[26] In 1924, a more extensive study of fourteen clinics using twenty-seven different ionization chambers was undertaken by Leonard Grebe and Heinrich Martius, the Bonn physicists.[27] They found that unit skin doses within Germany varied by as much as a factor of four. Secondly, the physicists found the combination of a physical method of measurement with a biological unit anomalous. Ionization, in their view, belonged to physics and should be measured in physical units.

Friedrich, working with his student Otto Glasser, strengthened the physicists' hand by demonstrating in 1922 that the discrepancy between the results he had obtained with Krönig and Holthusen's results on the question of the variation of biological effects with hardness was the result of a difference in their experimental arrangements.[28] While admitting Holthusen's claim that the chamber used in Friedrich's earlier work with Krönig was not sufficiently large relative to the range of the secondary cathode rays, Friedrich and Glasser believed that the discrepancy came from another source. Holthusen had irradiated his worms in a thin layer, but Krönig and Friedrich had irradiated their frog larvae submerged in a tank of water. Holthusen, then, had avoided signficant scattering, but Krönig and Friedrich had measured a dose that included scattering from the water. The added scattering dose in their small chamber increased with increasing hardness in such a way that the ionization Krönig and Friedrich actually measured was proportional to the energy of the secondary cathode rays. The Krönig and Friedrich results, correctly interpreted, therefore agreed with Holthusen's and confirmed the notion that biological effect was proportional not to ionization but to the absorbed energy.

With this difficulty resolved, Friedrich and Glasser attacked Holthusen on another front. Holthusen had wanted to decide the dosimetry question by determining the mechanism of the radiation effects. Friedrich and Glasser proposed separating these issues. Admitting that not enough was known of the mechanism of radiation effects to be sure that the processes that occurred in an ionization chamber were the same as those that occurred in biological materials, it was still possible to design sufficiently sensitive and reliable instruments, and units, to give reproducible results. From this point of view, ionization measurements and the e unit, measured in a large chamber and avoiding radiation from the walls, were still the best methods from the physicists' point of view, regardless of the mechanism of radiation effects. The physicists pressed this point of view in a new Commission for the Creation of a Standard Instrument for X-ray Measurement, created by the German Röntgen Society in 1923.[29] Moreover, Hermann Behnken, who had moved from Friedrich's laboratory to the Physical-Technical Institute, went ahead without agreement from the biological side.[30] Before the Commission had reached agreement, Behnken had already begun standardizing small ionization chambers using an "air pressure" chamber that he had designed and that the firm of Siemens and Halske had produced.[31] In this chamber, the range of the secondary cathode rays was shortened by increasing the air pressure to up to 10 atmospheres. It was Behnken who relabelled the e unit R, for röntgen, following an earlier French proposal that I shall mention in Chapter 6. The practical dosage problem, so far as the German physicists were concerned, appeared to be solved by 1924.

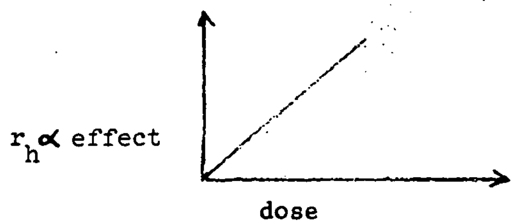
It is ironic that, just as the physicists were separating the practical issues in X-ray dosimetry from the problem of understanding the mechanism of radiation effects, a theory of radiation effects was being proposed that would provide an account of how the physical process of ionization, as observed in Wilson's cloud-chamber photographs, caused biological effects. Friedrich Dessauer, the German X-ray tube manufacturer who obtained a doctorate in physics during the War, proposed in 1922 the "point heat" or, as it later became known, "hit" theory.[32] Noting that the total amount of energy transferred in the absorption of X-rays was no more than the energy transferred in a couple of swallows of warm water, Dessauer accounted for the difference in the biological effects caused by the way in which the energy was delivered. The hit theory treated the biological effects of radiation as the result of hits by one or more secondary cathode rays on specially sensitive structures or targets in the cell. A hit caused rapid motion of the molecules within a target, an event Dessauer thought of as "point heat." The nature of the targets was a matter of continuing debate, but the mathematical part of the theory did not require their specificaion. It was well known to physicists that hitting a target of a given size with randomly thrown projectiles was a process governed by a probability law usually attributed to Poisson. If only a single hit were required to bring about a given effect, this Poisson distribution yielded as the fraction r of targets unhit after time t

$$r = e^{-\delta t}$$

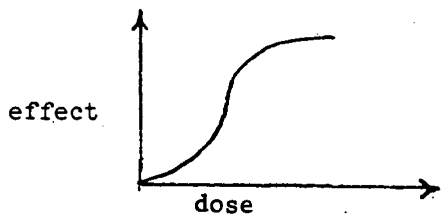
or, as the fraction of targets hit in the same time,

$$r_h = 1 - e^{-\delta t}$$

where δ is the time average number of hits. At low doses (small δt), there would therefore be a linear relationship between dose and effect:



If more than one hit were required to bring about a given effect, the relationship between dose and effect would in general be sigmoid:



The hit theory would later in the 1920s and in the 1930s prove fruitful for radiation biology and provide, as Dessauer had hoped it would, a physical basis for radiation therapy and radiation genetics, but in the early 1920s it was a controversial theory that did not attract much immediate support in the medical radiological community. The mathematics used was beyond most practitioners, and in addition the evidence was not persuasive to biologists. The primary support came from experiments showing a sigmoid relationship between dose and effect, and such a relationship could derive from random variability in the biological materials as well as from random hits on cellular targets.[33] Holthusen summarized the initial attitude of physicians and biologists when he said, "The many ways in which the rays can affect the body do not allow themselves to be reduced to a formula."[34] Holthusen was more than Dessauer's equal in physics and mathematics, so this statement should not be read as hostility arising from incomprehension. Holthusen and others believed that this physical theory did not take adequately into account the complexity of biological phenomena. Thus, for the moment, the hit theory did not provide significant support for the shift to ionization measurements. The creator of the hit theory, Dessauer, was a diehard advocate of the Kienböck strips rather than ionization chambers for clinical use, provided that the practitioner followed his recommendation to use homogeneous X-rays.[35] Thus, even in Germany significant gaps between scientific understanding and medical practice remained in the mid 1920s: ionization measurements were accepted in the clinic but without a firm theoretical basis and with continuing use of biological units.

Outside Germany, intensive deep therapy and ionization measurements were still novelties in the early 1920s. I shall discuss the reception of these developments in the next chapter. While the Germans were settling the problems of X-ray dosage, professional organizations in other countries had become concerned with protection against effects on the blood that resulted from exposure to radium as well as X-rays. By 1924, professional committees in Britain, France and the United States would make recommendations on radium protection, and action was being taken in other countries as well. I shall now turn to these developments outside Germany and focus in particular on the role of public concern in forcing medical radiological communities to put radium protection on a par with X-ray protection.

Effects of radiation on the constituents of the blood and on the blood-forming organs had been known, as we mentioned in Chapter 2, since 1903. In addition to clinical trials in curing diseases like pseudo-leukemia and leukemia, there had been during the next decade extensive experiments undertaken to sort out whether the clinical manifestations were the result of effects on the white and red blood cells during circulation, or whether it was the lymphoid and myeloid tissues responsible for forming these blood cells that were affected. The issue was a complex one that was not satisfactorily settled until the mid-1920s, but in the meanwhile it became clear that there were substantial dangers for both X-ray operators and radium workers.[36] In 1911, several Viennese physicians reported the deaths of four X-ray operators and one radium worker from what they termed leukemia. In blood samples from ten healthy X-ray workers, they found reduced numbers of polynuclear leucocytes and increased numbers of lymphocytes.[37]

In 1914, the death of an Italian physician who had worked with X-rays for fourteen years was reported prominently in the Archives of the Röntgen Ray.[38] The diagnosis was pernicious anemia, a condition that was thought to be secondary to destruction of other tissues, especially in the testicles, bone marrow and spleen.

These blood-related incidents were, however, isolated cases that did not alarm the bulk of practitioners. The weight of the evidence was again on the side of no effect, or an idiosyncratic one that could be ignored. When an English physicist tried in 1916 to interest the Röntgen Society in further efforts in the area of radiation protection because of cumulative effects on blood, the reception from his physician colleagues was cool:

> If the effects of X-rays are steady and cumulative, workers like Sir MacKenzie Davidson [who in 1912 had been knighted for his contributions to radiology] and myself would have withered away long since.[39]

> Less than a pastille dose, especially if spread over a long period, is not going to do anyone any harm. Therefore, if the operator adopts the precaution of merely standing three yards from his apparatus, he is going to be pretty safe in any X-ray treatment.[40]

At the same meeting at which these comments were made, there was considerable concern with dermatitis being reported among newly trained operators in the field. The lack of concern was therefore not a general lack of concern with protection but was limited to effects supposedly caused over a long period by repeated exposures when no acute symptoms were apparent. The practitioners were convinced that such effects either did not occur or were so rare that they did not merit the attention of the profession. Another discussion of protection at the Röntgen Society in 1919 that was concerned primarily with the harder X-rays available from Coolidge tubes led to " a hopeless divergence of opinion on the degree of screening required for the protection of the operator."[41]

This situation changed rapidly in late 1920 and in 1921. J. C. Mottram, a physician who had been appointed head of the Research Department at the London Radium Institute, reported in August 1920 five cases of aplastic pernicious anemia occurring among clinical and laboratory workers at the Institute during the preceding several years. Three of the workers had died.[42] Mottram had been working for several years on effects on the blood, and one of his collaborators had been the physicist who raised the question of protection from these effects at the Röntgen Society during the War.[43] By aplastic pernicious anemia, Mottram meant a severe decrease in the red blood cell count that was secondary to damage to the hematopoietic tissue in the bone marrow. He readily confirmed the effects on the bone marrow under laboratory conditions, and he also showed that with the introduction of protection measures in the clinic the red blood cell counts of the radium workers returned to normal.[44] These protection measures included better ventilation (to remove radium emanation); the use of forceps, of lead-lined boxes carried close to the floor in slings, and of lead rubber gloves in handling radium applicators; placement of filters on these applicators by temporary workers changed every three months; and the use of five-centimeter thick lead screens and similarly lined tables during manipulation of the applicators.

Mottram's reports from the London Radium Institute were, as we shall see, already having an impact in the winter of 1921 on the French medical radiological community, but in Britain it was the death in March 1921 of a leading X-ray physician, Ironside Bruce, from acute aplastic anemia that galvanized public reaction and thereby generated a professional response.[45] The moment was an especially delicate one. A public fund-raising campaign to establish an Institute of Radiology had been launched in 1920.[46] The British Association for the Advancement of Radiology and Physiotherapy (BARP), which had been founded in 1917, was trying to maintain the war-time increase in the importance of radiology and raise the status of radiology within medicine. BARP had in 1919 convinced the University of Cambridge to offer the first postgraduate examination and diploma in medical radiology and electrology.[47] It would hardly do to have the practitioners of a field that was asking for public support and specialty status dying of injuries caused by the tools of their trade. The need for protection had, in fact, been used as a justification of specialty status.[48] A series of newspaper reports on Bruce's death and the dangers of X-rays led Robert Knox, a physician who had worked with G. W. C. Kaye on the examination of aircraft timbers for faults using X-rays during the War, to propose in a letter to the London Times the establishment of a committee of physicists, physiologists and radiologists to report on X-ray effects, especially effects on the blood, and on protective measures.[49]

The intense pressures that members of the medical radiological community were feeling as a result of newspaper reports in the spring of 1921 are reflected in a statement by an X-ray tube manufacturer at the Röntgen Society in April:

> I would remind those present that almost the only publicity we get is when someone dies, or swallows the radium....listen to these headings from the papers of the past two days. 'X-ray Dangers, a peril to people in rooms above the operating chamber'; 'Perilous X-rays, leaden screens to protect next door neighbors.' Not a word about the tens of thousands of people whose lives have been saved by X-rays, or whose sufferings have been relieved. I have reason to think that certain papers are deliberately putting forward the danger side of X-rays and radium, and that they will not publish re-assuring matter. Such a position wold be scarcely possible if there were a Röntgen Society propoaganda committee.[50]

Pressure from the newspapers was also cited repeatedly in retrospective accounts as a reason for professional action.[51]

The committee that was established soon thereafter under the chairmanship of Sir Humphrey Rolleston, President of the Royal Society of Medicine, was far from being merely a propaganda committee, but it was nevertheless an attempt to defend medical radiology from what the profession regarded as undue public criticism. It issued its first report in June 1921 and did not dissolve until the 1950s. Mottram's concern for radium protection was incorporated along with X-ray protection. An informal cooperative venture of the leading medical radiological organizations in Britain, this British X-ray and Radium Protection Committee became an important forum for discussion among physicians and nonphysicians of protection measures and a model that other countries would imitate. The organizations involved included BARP, the Röntgen Society and the Electrotherapeutic Section of the Royal Society of Medicine, whose formation in 1903 I had occasion to mention above. These organizations, as we shall see in the next chapter, had not been on good terms, and their cooperation in the Protection Committee should be viewed as a temporary show of unity in response to a common threat.

In France, as in Britain, the effects of radiation on the blood had been known and largely discounted by practitioners as a serious source of concern. French biologists had done extensive experimental work on these effects before the War.[52] When, however, a French physician in 1918 reminded his colleagues of the earlier death of the Italian radiologist of pernicious anemia, a leading radiologist replied that the case was an isolated one and may not have been caused by X-rays.[53] In late March 1921, H. Bordier, a physician and professor of medicine at Lyons who had done research with both X-rays and radium, reported on Mottram's work to the French Acadmy of Medicine, claiming that radium was more dangerous so far as effects on the blood were concerned than X-rays.[54] The Paris Radium Institute had only recently opened its new buildings, the completion of which had been delayed by the War. There was a good deal of talk about curing cancer with radium, and Curie had only recently succeeded in convincing the American public to buy her a gram of radium partly with the promise of progress in the fight against cancer.[55] It is not surprising then that the report on Mottram's work brought public concern, and in response the appointment of a special commission of the Academy of Medicine to advise on the need for protection measurs. The _rapporteur_ of the commission was clear about the importance of public pressure in its creation: "The well known dangers of radioactive materials and X-rays have for some time drawn the attention of the public at large in a remarkable way, and have provoked unjustified fears."[56] After the death of Ironside Bruce, Bordier made an attempt to interest the French Society for Medical Radiology, a group that excluded nonphysicians, in undertaking blood examinations of X-ray workers, but here the weight of the evidence and the interests of the profession seemed to the physicians to be against taking action.[57] The reply to Bordier was negative: "this observation [that the protection being used is insufficient] is perhaps true for his service, but it cannot be generalized without supporting evidence, under penalty of discouraging young radiologists."[58]

In addition to the direct public pressure, there were other mechanisms at work in encouraging the French medical community to take radium protection measures: insurance companies and the laws governing industrial health and workmen's copmensation. At least one company that insured X-ray and radium clinics for liability for harm to their own personnel and to visitors was concerned with the reports of effects on the blood, and a physician hastened to ensure the insurers that there was no risk to patients and little risk to the personnel.[59] At the same time, inquiries from insurance companies made it clear that it would be desirable if the medical radiological community could agree on appropriate protection measures and thereby avoid the impositio of even more burdensome procedures.[60] So far as existing laws were concerned, France had in 1919 extended the protection of its industrial accident law to most occupational diseases. The medical radiological community was concerned that radium institutes not be classified among the "dirty industries" that were excluded from the workmen's compensation scheme.[61]

The British and French recommendations for radium protection were in many respects similar.[62] As the Röntgen Society noted, "The attitude of the [French] Commission towards these dangers is identical with that of the British Committee; it is held that the dangers may be avoided by the adoption of well-recognized precautions."[63] These precautions included those that Mottram had suggested. The Paris Radium Institute, even before the discussion at the Academy of Medicine, had indicated its willingness to adopt similar measures, except that the French preferred lead screens two centimeters (rather than five centimeters) thick for the tables at which manipulations were carried out on the radium applicators.[64] The British and French recommendations also included blood examinations for the personnel, and limitations on working hours or provisions for additional vacation time. The philosophy underlying the British recommendations was not explicitly stated, but it was implied that with proper precautions no harm would occur.[65] The French were more explicit:

> ...there is, as always, a threshold. We are sure of the existence of this threshold because there is often, perhaps always, some emanation in the air that we breath, especially near certain mineral springs, and these areas are inhabited by thriving populations; many sick people even go there for their health.[66]

> There is surely a threhold for the effect of penetrating radiation, as for all kinds of energy, and this notion is confirmed by the fact that we live continuously in a very weak penetrating radiation.[67]
>
> We cannot in fact effect a complete suppression of penetrating radiation in order to ensure protection; it suffices to reduce it below the threshold for the harmful effect, and that is easily done.[68]

No attempt, however, was made to verify experimentally that no effect occurred below a quantitatively determined threshold or to design protection measures that would assure that it was not exceeded. These steps, as we shall see in the next chapter, would be taken by physicists, not physicians.

The British and French initiatives for radium protection had counterparts in other countries as well. The deaths at the London Radium Institute raised concern about the insurability of workers in radiology and inspired a survey of 1500 radiologists in the United States by the President of the American Radium Society, who also proposed that the Society appoint a protection committee to cooperate with the Safety Committee of the American Röntgen Ray Society and the Bureau of Standards.[69] This particular proposal was not acted on, but the survey of radiologists produced results showing few effects on the blood of radium and X-ray workers, an outcome that the organizer regarded as helpful to radiologists in obtaining insurance at reasonable rates.[70] The Safety Committee of the American Röntgen Ray Society, which had been established in 1920 to recommend electrical safety precautions following the electrocution of a French physician during an X-ray examination, refocussed its attention on X-ray protection.[71] In the meanwhile, an investigation of workers engaged in measuring samples against the radium standard at the Bureau of Standards was undertaken by the United States Public Health Service.[72] In Norway, the "alarming news" from Britain and the death of a Norwegian radiologist in 1923 from pernicious anemia led to X-ray and radium protection measures at the Royal Hospital.[73] In Holland, the Ministry of Health set up a committee concerned with radium as well as with X-ray protection, and in the Soviet Union the Radiological Congress estabished a protection committee.[74]

Germany is conspicuously absent from this list of countries in which action was taken on radium protection. X-ray protection continued to receive the attention of the German Röntgen Society, which in 1922 began an extensive survey of cases of injury.[75] In 1924, the Society adopted several "guidelines" for work with X-rays that reiterated the physician's obligation to provide for protection, suggested the use of a .5 mm aluminum filter in radiography, and emphasized that dose measurements were indispensable.[76] The Röntgen Society had explicitly extended its purview to medical uses of radioactive substances, which in any case had long appeared as a subject at the annual congresses. Radium did not, however, have the public visibility in Germany that it had in other countries, and its prospects as a therapeutic agent do not appear to have excited popular interest. The deaths at the London Radium Institute did not excite interest either, and Germany thus lagged in professional activities concerned with radium protection.

1. "Untersuchungen und Betrachtungen über das Problem der Dosimetrie," Strahlenth., 14 (1922) 362-88, at 388.

2. In the discussion following Laquerrière, "Des dangers des installations de Radio et de Radiumologie. Rapport à une compagnie d'assurance sur le 'risque' du personnel et des visiteurs dans un institut de Radio et de Radiumologie," Bull. Off. Soc. Franc. Electroth. Radiol. (Mai 1921) 132-37.

3. For the President's appeal, see Levy-Dorn's opening address to the Society's congress, Verh. Deut. Rönt. Ges., 10 (1914), at 16, and for the creation of the Commission see the business meeting, at 18: "Der Zweck dieser Kommission soll sein, die Dosimeter, die landläufig sind und sich irgendwie bewährt haben, untereinander zu vergleichen, damit so feste Daten bekannt werden, auf die Sie sich einigermassen verlassen können; denn wie Sie ja wissen, geben die verschiedenen Dosimeter ganz verschiedene Auskünfte." Of the sixteen original members, seven are readily identifiable as physicians (including Christen), five as nonphysicians and the remaining four are not readily classifiable.

4. For the decision to continue the work of the Commission, see the "II. Rundschreiben des Vorsitzenden," Fortschr. Röntgenstr., 23 (1915-16) 72: "Es wäre recht erfreulich, wenn die Dosimeterkommission nach dem Kriege mit den erreichbaren Resultaten hervortreten würde, die wir Daheimgebliebenen unter dem Schutze unserer mächtigen Heeresorganisation fast wie im Frieden leisten könnten."

5. Fortschr. Röntgenstr., 23 (1915-16), at 213: "Der einfache Vergleich der Dosimeter ist von den meisten Arbeitern ausdrücklich oder implizite als unlösbar bezeichnet worden und es hat sich die Notwendigkeit ergeben, zuerst irgendein exaktes Verfahren der Messung der Röntgenstrahlen zu schaffen, und wäre es eine noch so komplizierte Laboratoriumanordnung....Dazu sollte alles, was Physik und Mathematik geleistet haben und leisten können, herangezogen werden."

6. For these papers, which were never actually read to the group but were published together, see "Arbetien und Verhandlungen der Sonderkommission für Dosimetervergleich der Deutschen Röntgengesellschaft," II. Gruppe, Fortschr. Röntgenstr., 23 (1915-16) 213-300, abgeschlossen im Juli 1915; "Arbeiten und Verhandlungen des Sonderauschusses für Röntgenstrahlenmessung der Deutschen Röntgengesellschaft," III. Gruppe, ibid., 509-32, abgeschlossen am 22 XI. 1915; IV. Gruppe, Fortschr. Röntgenstr., 24 (1916-17) 373-423, abgeschlossen am 15. VI. 1916; V. Gruppe, Fortschr. Röntgenstr., 25 (1917-18) 55-71; and Fortschr. Röntgenstr., 26 (1918-19) 38-41.

7. The British "Measurement" or "Dosimetry" Committee was formed late in 1913, see the "Annual Report," J. Rönt. Soc., 10 (1914) 88-97 and reported inconclusively in 1915, see the "Interim Report on the Standardisation of X-ray Dosage," J. Rönt Soc., 11 (1914-15) 102-10, authorized to be printed in full by the Council at its meeting held on 1 June 1915. The Committee had ten members when it reported, of whom seven were nonphysicians, two were practicing physicians and one was a retired physician. For the French conclusion, see R. Ledoux-Lebard and A. Dauvillier, "Principes rationnels de dosimétrie radiologique. Considération théoriques et pratiques," J. Radiol. Electrol., 2 (1916-17) 153-62, at 153: "...il est impossible d'arriver à une solution satisfaisante du problème du dosage des rayons X parce qu'il se présente sous une forme beaucoup trop complexe pour que soient réalisables des appareils de mesure donnant des indications précises et toujours comparables à elles-memes."

8. G. Grossmann (Charlottenburg), "Grundprinzipien der Dosimetrie," Fortschr. Röntgenstr., 22 (1914-15) 101-42.

9. For experiments that indicated that the Kienböck strips and the Sabouraud-Noiré pastilles gave parallel results over a wide range of hardnesses, thus demonstrating that Grossmann's "theoretical" considerations were incorrect, see H. Meyer, Fortschr. Röntgenstr., 23 (1915-16) 75-76. It was later recognized that these experiments merely demonstrated that the strips and the pastilles were equally insensitive to changes in X-ray dosage.

10. Bernhard Krönig (o. ö. Prof. der Geburtschilfe und Gynäkologie an der Universität Freiburg i. Br., Direktor der Univ.-Frauenklinik) and Walter Friedrich (Privatdozent für Physik an der Universität Freiburg i. Br., wissenschaftlicher Assistent an der Univ.-Frauenklinik), Physikalische und Biologische Grundlagen der Strahlentherapie, III. Sonderband zu Strahlentherapie (Berlin: Urban and Schwarzenberg, 1918). There is a clumsy but readable translation by Henry Schmitz, The Principles of Physics and Biology of Radiation Therapy (New York: Rebman, 1922). For the sponsors (who included industry, government, private patrons and a local scientific society), see the Forward to the German edition.

11. P. Villard, "Instruments de mesure à lecture directe pour les rayons X," Arch. Elec. Med., 16 (1908) 692-99.

12. For the "megamegaion," see B. Szilard, "Appareil pour la mesure de la quantité de rayons X," Radium, 7 (1910) 223-24 and "On the Absolute Measurement of the Biological Action of the X-rays and Gamma Rays," Arch. Rönt. Ray, 19 (1914-15) 3-20.

13. For references to this literature, see Hugo Fricke (Ph. D.) and Otto Glasser (Ph. D.), "Studies on the Physical Foundations of Röntgen-Ray Therapy I," Amer. J. Rönt., 11 (1924) 435-42, from the Biophysics Department of the Cleveland Clinic Foundation.

14. The full report of this work is Unsere Methode der Röntgen-Tiefentherapie und ihre Erfolge, V. Sonderband zu Strahlentherapie (Berlin: Urban and Schwarzenberg, 1920). For a summary account, see Hermann Wintz (Erlangen), "Die Grundlagen einer erfolgreichen Röntgentiefentherapie," Verh. Deut. Rönt. Ges., 11 (1920) 64-68, auf Einladung der D. R.-G., with discussion at 92-98.

15. See the comments of Albers-Schönberg in the discussion following Wintz, ibid.

16. See, for example, Manfred Fränkel (Charlottenburg), "Die Reizdosenanwendung, ihre Bedeutung für die Röntgentherapie," Verh. Deut. Rönt. Ges., 11 (1920) 89-92 or the full text "Die Bedeutung der Röntgen-Reizstrahlen in der Medizin mit besonderer Einwirkung auf das endokrine System und seiner Beeinflussung des Karzinoms," Strahlenth., 12 (1921) 603-38 and 850-99.

17. A. Fernau and W. Pauli (aus der k. k. Radiumstation im allgemeinen Krankenhause und dem Laboratorium für physikalisch-chemische Biologie der k. k. Universität Wien, mit Unterstützung der Fürst Liechtenstein-Spende), "Über die Einwirkung der durchdringenden Radiumstrahlung auf anorganische und Biokollide. I," Biochemische Zeitschrift, 70 (1915) 426-41, eingegangen am 4 Juni 1915. This W. Pauli was the father of the well-known physicist of the same name.

18. Th. Christen (aus der Strahlenforschungsstelle der Reiniger, Gebbert und Schall-A.-G., München), "Energiemessung von ionisierenden Strahlen insbesondere von Röntgenstrahlen," Phys. Z., 17 (1916) 23-25, eingegangen 17 Januar 1916, at 25: "...ein Zweifel auftaucht ob denn auch wirklich der Sättigungsstrom der in der Luft absorbierten Energie streng proportional sei oder ob nicht am Ende das als Proportionalitätskonstante aufzufassende Umsetzungsverhältnis zwischen der absorbierten Energie und der Ionisation

eine Funktion der Wellenlänge sei, etwa auf Kosten von gleichzeitig enstehender Erwärmung der Luft"; R. Glocker, "Die Messmethoden der Röntgenstrahlen," Phys. Z., 18 (1917) 302-15 and 330-38, eingegangen 14 Mai 1917, at 306: "Alle Ionisationsmessungen von Strahlungsenergien beruhen auf der Voraussetzung, dass die pro Volumeinheit des Gases erzeugte Ionenzahl, unabhängig von der Härte der Strahlen, direkt proportional der in der Volumeinheit absorbierten Strahlungsenergie ist."

19. Hermann Holthusen (aus der Medizinischen Klinik Heidelberg), "Über die Bedingungen der Röntgestrahlenenergiemessung bei verschiedenen Impulsbreiten auf luftelektrischem Wege," Fortschr. Röntgenstr., 26 (1918-19) 211-31.

20. Hermann Holthusen (aus der medizinische Klinik Heidelberg), "Über die biologische Wirksamkeit von Röntgenstrahlen verschiedener Wellenlänge," Fortschr. Röntgenstr., 27 (1919-21) 213-44.

21. Holthusen (Hamburg), "Über die Beziehungen zwischen physikalischer und biologischer Dosierung," Verh. Deut. Rönt. Ges., 15 (1924) or Fortschr. Röntgenstr., 32 (1. Kongressheft 1924) 73-79.

22. Hans Meyer (Privatdozent) and Hans Ritter, "Experimentelle Studien zur Feststellung eines biologischen Normalmasses für Röntgenstrahlen," Strahlenth., 1 (1912) 183-88, aus dem Institut für Strahlenbehandlung der Königl. Dermatol. Klinik zu Kiel (Direktor: Prof. Klingmüller), at 183: "...da es sich in der Therapie stets darum handelt, eine Wirkung auf biologische Prozesse hervorzurufen, seien sie nun normaler oder pathologischer Natur, so wäre es natürlich das beste, wenn man die Abstufung der Wirkung, d. h. also die Dosierung an der Hand eines biologischen Messverfahrens vornehmen könnte."

23. Otto Jüngling (Priv.-Dozent, Assistenzarzt der Chirurgischen Universitätsklinik Tübingen, Vorstand: Prof. Dr. Perthes), "Die praktische Verwendbarkeit der Wurzelreaktion von Vicia faba equina zur Bestimmung der biologischen Wertigkeit der Röntgenstrahlung," Munchen. Med. Wschr., 67^2 (1920) 1141-44.

24. S. Russ (D. Sc., Cancer Research Laboratories, Middlesex Hospital), "A Suggestion for a New X-Ray Unit in Radiotherapy," Arch. Radiol. Electroth. (December 1918) 226-32.

25. Hans Küstner (Göttingen), "Vorarbeitung zur Schaffung eines Standardgeräts zur Dosierung der Röntgenstrahlen," from the report of the "Sitzung der von der Deutschen Röntgengesellschaft eingesetzten Kommission zwecks Schaffung eines Standard-instrumentes für die Röntgenstrahlenmessung," am 21 Oktober 1923 in Göttingen, Fortschr. Röntgenstr., 31 (1923-24) 483-87, at 485: "Seine Anwendung kann den Arzt keinesfalls vor Schadenersatzansprüchen des Patienten schützen. Die Firma Reiniger, Gebbert and Schall, Erlangen, von der alle Streifen, Entwickeln und Geräte bezogen waren, lehnt diese Verantwortung auf Entschädigungsansprüche in ihrer Gebrauchsanweisung ebenfalls ab."

26. Albert Bachem (Priv.-Doz., aus dem Institut für physikalische Grundlagen der Medizin in Frankfurt a. M., zurzeit Chicago), "Zur praktischen Dosierung der Röntgenstrahlen verschiedener Härte," Strahlenth., 13 (1922) 605-10.

27. L. Grebe (Röntgen-Forschungs u. Unterrichtsinstitut der Universität Bonn) and H. Martius (Universitäts-Frauenklinik in Bonn), "Vergleichende Messungen über der Grösse der zur Erreichung der Hauterythems gebräuchlichen Röntgenstrahlenmenge," Strahlenth., 18 (1924) 395-409.

28. W. Friedrich and O. A. Glasser, "Untersuchungen und Betrachtungen über das Problem der Dosimetrie," Strahlenth., 14 (1922) 362-88.

29. I have been unable to find a clear reference to the creation of the Commission, but see "Sitzung der von der Deutschen Röntgengesellschaft eingesetzten Kommission zwecks Schaffung eines Standard-instrumentes für die Röntgenstrahlenmessung," am 21 Oktober 1923 in Göttingen, Fortschr. Röntgenstr., 31 (1923-24) 483-87.

30. "Bekanntmachung betreffend die Eichung von Röntgenstrahlen-Dosismessern in der Physikalisch-Technischen Reichsanstalt," Fortschr. Röntgenstr., 31 (1923-24) 565-66 and Behnken (Berlin), "Die Eichung von Dosismessern in absolutem Masse in der Physikalisch-technischen Reichsanstalt," Verh. Deut. Rönt. Ges., 15 (1924) 92-94.

31. H. Behnken, "Die Vereinheitlichung der Röntgenstrahlen-Dosismessung und die Eichung von Dosismessern," Z. Tech. Phys., 5 (1924) 3-16, eingegangen 4 September 1923.

32. Fr. Dessauer, "Über einige Wirkungen von Strahlen. I.," Z. Phys., 12 (1922) 38-47, Mitteilung aus dem Universitäts-institut für physikalische Grundlagen der Medizin in Frankfurt a. Main, eingegangen am 30 September 1922; and Marietta Blau and Kamillo Altenburger, "Über einige Wirkungen von Strahlen. II.," Z. Phys., 12 (1922) 315-29, Mitteilung aus dem Universitäts-institut für physikalische Grundlagen der Medizin, eingegangen am 2 November 1922. See also Fr. Dessauer (Direktor des Instituts für physikalische Grundlagen der Medizin an der Universität Frankfurt a. M.), Dosierung und Wesen der Röntgen-strahlenwirkung in der Tiefentherapie vom physilaischen Stand-punkt, Strahlentherapeutische Monographien Band II (Dresden and Leipzig: Theodor Steinkopff, 1923), especially Teil II. Apparently independent of Dessauer, an English physicist also proposed the hit theory and used it to calculate the size of the targets, see J. A. Crowther (University Lecturer in Physics Applied to Medical Radiology, University of Cambridge), "Some Considerations relative to the Action of X-rays on Tissue Cells," Proc. Roy. Soc., 96B (1924) 207-11, received 31 January 1924.

33. See, for example, Charles Packard (Columbia University, Institute of Cancer Research, F. C. Wood, Director) "The Measurement of Quantitative Biological Effects of X-Rays," J. Cancer Res., 10 (1926) 319-39 and J. C. Mottram, "The Survival Curves of Cells Under Radiation," J. Cancer Res., 11 (1927) 130-34.

34. H. Holthusen (Hamburg), "Die Wirkung der Röntgenstrahlen in biologischen Hinsicht," Verh. Deut. Rönt. Ges., 15 (2. Teil 1924) 3-4, from the Zwischentagung der Deutschen Röntgen-Gesellschaft als Abteilung 22 der 88. Versammlung der Gesellschaft Deutscher Naturforscher und Artze in Innsbruck, 24-26 September 1924, at 4: "Die vielfältigen Wege, auf denen sich die Strahlen im Körper auswirken können, lassen sich nicht auf eine Formal bringen." See also the other papers and the discussion, ibid., pp. 4-13.

35. Dessauer, Dosierung und Wesen..., note 32 above.

36. As it turned out, the site of the primary lesion was the bone marrow, not the components of the blood in circulation, which are relatively resistant to radiation damage, see J. Jolly and A. Laccasagne, "De la resistance des leucocytes du sang vis-a-vis rayons X," C. R. Soc. Biol. (Paris), 89 (1923) 379 and A. Laccasagne and J. Lavedan, "Les modifications histologiques du sang consécutives aux irradiations expérimentales," Paris Med., 51 (2 fevrier 1924) 97-103.

37. Nikolaus v. Jagić, Gottwald Schwarz and Leo von Siebenrock (aus der I. Medizinischen Universitätsklinik in Wien, Vorstand: Prof. C. v. Noorden), "Blutbefunde bei Röntgenologen," Berlin. Klin. Wschr., 48[2] (1911) 1220-22.

38. "The Autopsy of a Radiologist," Arch. Rönt. Ray, 18 (April 1914) 393-94. The radiologist was Emilio Tiraboschi, who worked at the Ospidale Maggiore in Bergamo. The original report was published in S. Gavazzeni and S. Minelli (Bergamo), "L'Autopsia d'un radiologo," Radiol. Med., 1 (February 1914) 66-71.

39. "The Injurious Effects Produced by X-rays," a discussion at the Röntgen Society on 1 February 1916, J. Rönt. Soc., 12 (1916) 38-56. Sidney Russ (D. Sc.) opened the discussion. The comment quoted here was by Reginald Morton, at 40.

40. The comment was by N. S. Finzi, ibid., at 44.

41. "Editorial Notes," J. Rönt. Soc., 15 (July 1919) 66.

42. J. C. Mottram (from the Research Department, Radium Institute, London), "The Red Cell Blood Content of Those Handling Radium for Therapeutic Purposes," Arch. Radiol. Electroth., 25 (December 1920) 194-97, read before the Pathological Society of Great Britain and Ireland, 3 August 1920. Mottram had already reported abnormalities in the blood of these workers, see J. C. Mottram (M. B.) and J. R. Clarke, "The Leucocytic Blood-Content of those Handling Radium for Therapeutic Purposes," Proc. Roy. Soc. Med., 13 (1920) 25-30, reprinted in Arch. Radiol. Electroth., 24 (1919-20) 345-50. It turned out later that the immediate cause of death in two of these three cases was not pernicious anemia, but Mottram thought that the anemia had weakened the resistance of the workers, see "Foreign Letters," J. Amer. Med. Ass., 76[2], (21 May 1921) 1412-13.

43. For one product of this collaboration, see S. Russ (D. Sc.), Helen Chambers (M. D. Lond.), Gladwys Scott and J. C. Mottram (M. B. Lond.), "Experimental Studies with Small Doses of X Rays," Lancet, 196 (26 April 1919) 692-95, undertaken at the request of the Medical Research Council and funded by the Cancer Investigation Fund of Middlesex Hospital.

44. J. C. Mottram, "Histological Changes in the Bone Marrow of Rats Exposed to the γ Radiations from Radium," Arch. Radiol. Electroth., 25 (1920-21) 197-99 and "The Effect of Increased Protection from Radiation upon the Blood Condition of Radium Workers," ibid., 368-72, dated May 1921.

45. For Bruce's obituary, which did not mention the cause of death but identified him as a "martyr," see Arch. Radiol. Electroth., 25 (1920-21) 338. The case was later described, without identifying the victim, in F. E. Larkin, "A Case of Acute Aplastic Anemia," ibid., 380-82. Such were the sensitivities of a profession that had chosen to ignore this particular risk, but the lay press had already identified the victim and his disease.

46. "The MacKenzie Davidson Memorial Fund," Arch. Radiol. Electroth., 24 (1919-20) 306-7, where the original appeal and list of sponsoring luminaries (including A. Bonar Law, Stanley Baldwin, J. J. Thomson, Coolidge, and leading lights of the British medical radiological community) is reproduced. Failing an institute, plans called for a university chair.

47. "Report of the Special Board of Medicine upon a proposal to establish a Diploma in Medical Radiology and Electrology in the University [of Cambridge]," dated 20 May 1919 and reprinted under the heading "British Association of Radiology and Physiotherapy," Arch. Radiol. Electroth., 24 (1919-20) 31-34. This report to the Vice-Chancellor was communicated to the Senate, which on 17 June 1919 promulgated detailed plans for the syllabus of subjects to be covered by the examination, see the account in "The Work of the British Association of Radiology and Physiotherapy," ibid., 209-16. In the first of these reports, it is noted that a physician had contributed £1000 to cover the University's initial expenses in setting the examination, which would eventually become self-supporting from fees charged the candidates. The examination was given for the first time in July 1920, and the physicians were relieved that "no question on higher mathematics was asked, and a knowledge of only simple calculations would be required...," see "The Diploma in Medical Radiology and Electrology," Arch. Radiol. Electroth., 25 (1920-21) 164-68, where the entire examination is reproduced.

48. "Report of the Special Board," ibid., at 32: "...only medical men who have received special training in Physics and Practical Radiology, Electrotherapy and Electrology generally, are in a position to understand and foresee not only the development of their application to diagnosis and treatment, but also their limitations and dangers."

49. Knox's letter to the Times was printed in 29 March 1921. It has been reprinted in J. D. Nauman, "Pioneer Descriptions in the Story of X-ray Protection," in A. J. Bruwer, Classic Descriptions in Diagnostic Röntgenology (Springfield, Illinois: Charles C. Thomas, 1964), pp. 311-39. For Knox's war-time work with Kaye, see Captain R. Knox (M. D., R. A. M. C.) and Major G. W. C. Kaye, (O. B. E., M. A., D. Sc., R. A. F.), "The Examination of Aircraft Timber by X Rays," a contribution to a "General Discussion on the Examination of Materials by X Rays," held jointly by the Faraday Society and the Röntgen Society, 29 April 1919 and abstracted in Arch. Radiol. Electroth., 24 (1919-20) 295-97.

50. Cuthbert Andrews, "X-rays and Propaganda," J. Rönt. Soc., 17 (1921) 129-32, read 21 April 1921, at 131-2.

51. The most detailed of these retrospective accounts is by Stanley Melville (M. D.) in "A Discussion on the International Protection Recommendations," 19 November 1931 and 14 January 1932, Brit. J. Radiol., 5 (1932) 215-33, at 218: "In the spring of 1921 radiology was very near to what might have been a terrific onslaught by the Press. On my way home from a Memorial Service for our old friend Ironside Bruce, whose untimely death caused much concern, I discussed with the Secretary to the Medical Society of London the many references that had been made in the public Press to his death. To my amazement, he informed me that he had been discussing the matter with a member of one of our most powerful papers, and that they had every intention of launching into a warning to the public against the dangers of X rays. I found on enquiry at the office of the paper that my information was correct." See also, G. W. C. Kaye, Röntgenology: it's Early History, some Basic Physical Principles and the Protective Measures (London: William Heinemann, 1928), p. 69.

52. For one of many articles, see Ch. Aubertin and E. Beaujard, "Actions des rayons X sur le sang et la moelle osseuse. I. Action d'une dose unique d'intensite moyenne en irradiation totale," Arch. Med. Exp., 20 (1908) 273-88.

53. Mignon (M. le Docteur), "La Protection en radiologie," J. Radiol. Electrol., 3 (1918-19) 165-172, communication faite à la Réunion des radiologistes de la XIII^{e} Région, with discussion. It was Belot who emphasized that this was an isolated case.

54. H. Bordier (Professeur à la Faculté de Médicine de Lyon), "Les dangers du radium. Utilité des mesures de protection," Bull. Acad. Med. (Paris), 85 (1921) 416-17, séance du 29 mars.

55. Robert Reid, Marie Curie (New York: New American Library, 1974) gives an extensive account of this post-war period in France and Curie's trip to the United States in Chapter 20 and 21. Reid is, however, wrong in saying (at p. 240) that there was no committee concerned with radium protection in France as late as 1922.

56. Broca, "Sur les dangers des radiation pénétrantes et les moyens de les éviter," au nom de la Commission du Radium, Brit. Acad. Med. (Paris), 85 (1921) 651-60, séance du 7 juin, at 651: "Les dangers bien connus des corps radioactifs et des rayons X ont attiré depuis quelque temps d'une manière spéciale l'attention du grand public, et ont provoqué des craintes injustifiées." See also the retrospective account of the 1921 events in Bouchacourt and Morel-Kahn (les docteurs), "De quelques point fondamentaux concernant la protection des personnes utilisant les R. X," Bull. Soc. Radiol. Med. (Paris), 16 (14 February 1928) 59-65, at 60.

57. H. Bordier (le docteur, professeur agregé à la Faculté de Medicine de Lyon), "Sur un cas d'anemie mortelle due aux rayons X," Bull. Soc. Radiol. Med. (Paris), 9 (8 November 1921) 158-60, with discussion.

58. Haret, in the discussion, ibid., at 160: "cette observation est peut-être vraie pour son service, mais elle ne peut-être generalisée, sans preuve à l'appui, sous peine de décourager les jeunes radiologistes." Béclère added: "Nous pouvons tous un jour ou l'autre être atteints d'une affection grave sans qu'il soit absolument besoin de mettre en cause les rayons de Röntgen. Il s'ensuit pas, d'ailleurs, que nous ne devions pas nous entourer du maximum de tous les moyen de protection."

59. Laquerriere, note 2 above.

60. See Cl. Regaud, "Sur les dangers du radium, " Bull. Acad. Med. (Paris), 85 (1921) 608-12, séance du 24 mai, where Regaud argued against the lead protection surrounding patients undergoing radium treatments suggested by H. Bordier (Professor à la Faculté de médicine de Lyon), "Dangers du radium et mesures à prendre pour les éviter," ibid., 512-13, séance du 26 avril.

61. For mention of the extension of the French law on workmen's compensation to occupational diseases, see Wilhelm Flaskamp (Dr. med., aus der Universitäts-Frauenklinik Erlangen), "Röntgenschädigungen als Unfälle und Gewerbekrankheiten," Fortschr. Röntgenstr., 32 (1924) 641-47. For the concern with being classified among the "industries insalubres," see A. Broca, note 56 above, at 654.

62. For the first recommendations of the British X-ray and Radium Protection Committee, dated June 1921, see J. Rönt. Soc., 17 (July 1921) 100-103. For the report of the French commission, see Broca, note 56 above. The British committee had ten members, including the chairman and honorary secretaries, of whom only three appear to have been nonphysicians (two physicists and one X-ray tube manufacturer). The French committee had five members, all presumably physicians since it was appointed by the Academy of Medicine.

63. J. Rönt. Soc., 17 (1921) 99.

64. A. Felix (Institut de Radium de l'Université de Paris), "Dispositifs de protection contre les rayons du radium, à l'usage des radiumologistes-manipulateurs," J. Radiol. Electrol., 5 (February 1921) 61-66.

65. See the report, note 62 above, at 100: "The danger of over-exposure to X-rays and radium can be avoided by the provision of efficient protection and suitable working conditions..."

66. Broca, note 56 above, at 654: "...il y a, comme partout, un seuil. Nous sommes certains de l'existence de ce seuil, car il y a souvent, peut-être toujours, de l'emanation dans l'air que nous respirons, en particulier au voisinage de certaines sources minérales, et ces contrées sont habitées par des populations florissantes; beaucoup de malades meme y vont rétablir leur santé."

67. Ibid., at 657: "Il y a certainement un seuil d'action pour les radiations pénétrantes, comme pour toutes les formes d'énergie, et cette vue de l'ésprit est confirmée par le fait que nous vivons constamment dans une radiation pénétrante très faible..."

68. Ibid., at 659: "Il n'y a pas lieu, en effet, pour assurer la protection, de réaliser la suppresion complète des radiations pénétrantes; il suffit de les amener au-dessous du seuil d'action nocive, et celà est aisé."

69. George E. Pfahler (M. D., Philadelphia), "Protection in Radiology," Presidential Address, read at the 7th Annual Meeting of the American Radium Society, St. Louis, Missouri, 22-23 May 1922, Amer. J. Rönt., 9 (1922) 803-808.

70. George E. Pfahler (M. D., Professor of Radiology, Graduate School of Medicine, University of Pennsylvania), "The Effects of the X-rays and Radium on the Blood and General Health of Radiologists," Amer. J. Rönt., 9 (1922) 647-56, read at the 23rd Annual Meeting of the American Röntgen Ray Society, Los Angeles, 12-16 September 1922, with discussion at 771-74.

71. For reports on the electrocution of Dr. Auguste Jaugeas at Béclère's X-ray clinic, see "Electrocution of a Radiologist," Arch. Radiol. Electroth., 24 (1919-20) 267-69 and Amer. J. Rönt., 7 (1921) 167-68. For the creation of the Safety Committee, see Amer. J. Rönt., 8 (1921) 204, and for its report on electrical dangers, delayed by the death of its chairman, see "Report of the Safety Committee," presented at the Los Angeles meeting of the American Röntgen Ray Society, Amer. J. Rönt., 10 (1923) 246-47. I have been unable to find the Safety Committee's first report (1923) on X-ray protection, but for a follow-up report see "Report of the Safety Committee of the American Röntgen Ray Society," presented at the 25th Annual Meeting of the American Röntgen Ray Society, Swampscott, Massachusetts, 3-6 September 1924, Amer. J. Rönt., 12 (1924) 566-71. All of the members are mistakenly identified there as M. D.'s, but at least two (William D. Coolidge and William Duane) were physicists and not physicians.

72. R. C. Williams (Passed Assistant Surgeon, Office of Industrial Hygiene and Sanitation, U. S. Public Health Service), "Preliminary Note on Observation Made on Physical Condition of Persons Engaged in Measuring Radium Preparations," Pub. Health Repts., 38 (21 December 1923) 3007-28.

73. P. Amundsen (Christiana), "Blood Anomalies in Radiologists and in Persons Employed in Radiological Service," Acta Radiol., 3 (1924) 1-7.

74. Kaye, note 51 above.

75. Groedel, (Frankfurt a. M.-Bad Nauheim), "Einleitung. Sammelreferat über Röntgenschädigungen," Verh. Deut. Rönt. Ges., 13 (1922) 75. The survey was to be conducted by the Sonderausschuss für die Beurteilung von Röntgenschädigungen und zum Studium ihrer Verhütung.

76. Max Levy-Dorn (Prof. Dr., Vorsitzendem des Sonderausschusses für die Beurteilungen von Röntgenschädigungen und zum Studium ihrer Verhütung), "Leitsätze für das Arbeiten mit Röntgenstrahlen gemäss Beschluss der Deutschen Röntgengesellschaft vom 28 April 1924," Deut. Zeit. Ges. Gericht. Med., 4 (1924) 288-89.

Chapter 6: International Standards: the Röntgen and the Tolerance Dose, 1925-34

The race is to the swift and the Hun will take the hindmost.

--G. W. C. Kaye (D. Sc.), in his Presidential Address to the Röntgen Society, November 1917[1]

This Congress was not only a congress of scientists, no, it was an assembly of nations.

--Gosta Forsell (M. D.), at the closing of the First International Congress of Radiology, London, July 1925[2]

Before and during World War I, medical radiology had managed without recourse to collective decisions taken on the international level. The seven International Conferences of Medical Radiology and Electrology held between 1900 and 1914 were simply meetings at which individual speakers delivered papers to audiences organized in sections according to their interests. Such conferences unquestionably influence professional standards and behavior, but they do so through the give and take among individuals. There is another kind of conference at which collective decisions as well as scholarly meetings of the sections are a major activity. At these conferences, committees are appointed to study particular problems, plenary sessions pass formal resolutions, and an executive committee is often required to manage the flow of decisions. Before the War, this latter sort of professional activity had been limited to the national medical radiological orgnizations. I have had occasion in previous chapters to mention some of the decisions taken, especially in the German and British Röntgen Societies.

Formal decisionmaking of this type on the international level might be viewed as an entirely natural development growing from the inherently international character of science and medicine and the belief in the objective character of knowledge. In this view, science and medicine are, except for minor aberrations, immune to nationalist appeals. Recent studies suggest, however, that the internationalist ethic of science often does not account for the actual behavior of scientists vis-à-vis their colleagues in other countries.[3] Instead, it has been suggested that cooperative efforts grow from competitive, and often nationalistic, motives. The function of international cooperation in this newer view is to provide a basis for further competition. In the account I shall offer here of international cooperation on radium and X-ray units and standards, we shall see that, as Forman has put it in another context, "in many cases 'cooperation' and 'competition' are not behaviorally antithetic, and therefore need not be...motivationally antithetic."[4] The point can also be stated positively, as Forman has also suggested: cooperation can depend on competition, and competition can in turn depend on cooperation.[5]

In addition to illustrating the causal relationship between nationalistic competition and international cooperation, the development of international standards in medical radiology will reveal a causal relationship between conflict within national communities and international cooperation. Even when inspired by nationalist goals, a professional community does not necessarily find itself unified in all respects. In medical radiology, differences between physicians and nonphysicians had long been evident, and as we shall see these groups split further once physicists and physician-specialists started emerging as small but aggressive subgroups. As minorities, such subgroups face serious difficulties in having their claims to special status recognized; their superior academic credentials may hinder more than help. The majority in a professional community often resents efforts to establish subgroups with higher status and with power to impose standards of behavior that many members may find it difficult to meet. Appealing to internationl cooperation is one of the strategies the minority subgroups use in outflanking domestic opposition. Better represented at international meetings and more aggressive in pursuing agreement with their colleagues in other countries, the physicists and physician-specialists could achieve influence on the international level that was not so readily available to them within national medical radiological organizations.

Before considering in detail the development of international cooperation after World War I, let us turn briefly to the pre-War establishment of an international radium standard, which I mentioned briefly in Chapter 3. The story of this standard illustrates the close relationship between competition and cooperation, and thus foreshadows the later development of X-ray standards.

For a decade after the discovery of radium, physicists and chemists managed without any formal international cooperation in standardizing measurements. Each laboratory kept a small radium standard of its own in order to determine precisely the activity of its radioactive samples in terms of the activity of a given weight of radium. If problems arose, standards were exchanged among laboratories and checked against each other. If a new research group needed to establish a standard, a member of the group went to a leading laboratory and compared a highly purified sample of radium salt with the sample kept as a standard there. In Britain, this decentralized system had become more formal than elsewhere after an effort to establish an X-ray standard by comparison with a given amount of radium led a Standards' Committee of the Röntgen Society in 1908 to define the ionization produced by the gamma rays from a pure one milligram sample of radium bromide after passage through a one centimeter thick lead shield as the "Unit of Radioactivity."[6] Though this unit never served the initial purpose of X-ray standardization, but was used only in radium work, radium standards were prepared, compared with the standard of Rutherford and Boltwood (which had informally served the purposes of the physical laboratories until then) and deposited in the National Physical Laboratory. There was, however, still no formal means of establishing whether the British standards were the same as those used in other countries.

The informal system of radium standardization started coming apart in 1909 when Otto Brill, an Austrian working in the laboratory of Sir William Ramsay, one of the leading English radium chemists, challenged Marie Curie's 1907 determination of the atomic weight of radium. Criticizing Curie for not checking for impurities other than barium, Brill claimed that with improved purification he had determined an atomic weight of 228.5 rather than Curie's 226.2.[7]

Rutherford, who had participated in the Röntgen Society Standards' Committee, was sufficiently concerned to compare his own standard with samples obtained from other countries. He found a sample sent by Curie to be 9 per cent lower in activity than his own, and other samples were low by as much as 20 per cent.[8] The purity of a radium sample reflected directly on the skills of its producer, so at stake was the national and professional pride of radium research workers in England, France, Austria, and probably other countries as well.

Also at stake was the capacity to continue scientific and medical research with radium on an international basis. The purity and atomic weight of radium had a direct impact on scientific questions like determining the charge of an alpha particle, on medical questions like the amount of radium to use in reproducing the therapeutic results of other clinics, and on commercial questions like the value of radium from different sources. Physicists had faced difficulties of this sort before. They had been solved by establishing international standards like the meter and kilogram kept at the International Bureau of Weights and Measures at Sèvres or by carefully defining the precise conditions under which a physical unit should be measured, as in the international definitions of the unit of electrical resistance (the ohm) adopted in the 1880s and 1890s.[9] Taking the lead in promoting international standardization, Rutherford was careful to avoid any reference to Curie's sample and any identification of the others that had not agreed with his own. Publicly, he agreed with the view "that no one else should be called on for the important work of furnishing a radium standard than Mme. Curie, the discoverer of radium."[10] Rutherford also emphasized that the needs of science, medicine and commerce converged, leaving to the imagination of his readers the difficulties in continuing competitive efforts in all three fields if agreement was not reached on a radium standard.[11]

Agreement was reached by 1912, though not without manifestation of the strong national feelings that the question of standardization aroused. At the International Congress of Radiology and Electricity, a meeting of the scientific radiological community in Brussels in 1910, there were proposals for standards and comparison methods from England, France and Germany.[12] Amid the chaos of the poorly organized and high spirited meeting, there emerged under Rutherford's guidance an agreement that Curie would prepare a radium standard to be certified by an International Radium Standards Committee with a maximum of two physicists representing each country. The amount of radium emanation in equilibrium with one gram of radium was defined as the "curie."[13] Unsure of the health and the competence of Curie, Rutherford encouraged the Viennese physicist Stefan Meyer to prepare an additional standard. Curie's work was delayed, but less by her chronic illness than by the scandal over her alleged affair with the married physicist Paul Langevin.[14] Early in 1912, however, the standardization was completed to everyone's satisfaction: Curie's standard and Meyer's were found to be in very close agreement using a comparison method invented by Rutherford and his student James Chadwick.[15] Competition in scientific and medical work with radium could continue on firm ground.

By the end of the War, nationalist feelings were much higher than they had been at the time of the radium standardization, and nationalism would play a major role in achieving X-ray standardization. In addition to the material disruptions discussed above, the War brought intense anti-German feelings to the radiological communities in Allied countries. In this respect, both physicians and physicists shared in the popular resentment of Germany.

Any restraint they may have felt because of the internationalist ethic of science and medicine was short-lived. The organizers of the seventh International Congress of Medical Radiology and Electrology, which had taken place in Lyons on the eve of the pre-War mobilization in July 1914, delayed distribution of the Proceedings for nationalist reasons:

> ...inspired by a thought which each of us will find echoed in himself, the Minister of War has asked...that the distribution of the volume be postponed until the end of the hostilities in order to avoid the ill-timed publication of several German papers contributed to the Congress, which are in any case of little consequence.[16]

Allied scientists and physicians often justified anti-German comments and acts by reference to German nationalism. Physicians in Allied countries strongly resented the Prussian Ministry of War order prohibiting the distribution of medical journals beyond Germany, even though it was not strictly enforced. In France, a war-time review of the German medical radiological literature based on materials obtained from neutral countries referred to the Germans as "boches," denounced Christen as a "despicable renegade" for leaving Switzerland to work in Germany, and concluded--after quoting anti-French comments from a German journal--that "the German mentality is furthermore something very extraordinary and remains incomprehensible for us in the baseness and crudeness of its processes and its conceptions."[17] In Britain, the physician-controlled Archives of the Röntgen Ray changed its name in 1915 to the Archives of Radiology and Electrotherapy to rid itself of the taint of the German professor who had signed a nationlist Proclamation in 1914 and who had donated his gold Rumford Medal, given to him by the Royal Society in 1896, to what the Allies considered a nationalist cause, the German Red Cross.[18] The Röntgen Society removed Röntgen, and two other Germans, from its list of honorary members and considered a change in name in order to remove "all taint of Germanism from the Society," as G. W. C. Kaye put it. Even the arguments against the proposed change were couched in strongly anti-German terms. Röntgen, it was noted, was "of Dutch origin" (his mother was Dutch), and changing the name of the Society was "rather too closely characteristic of learned German professors....to imitate."[19] In Italy, France and England, committees were organized during the War to fight against post-War domination of the scientific and medical instruments industry by German manufacturers.[20]

Boycotts of German manufacturers were generally unsuccessful after the Armistice, but other anti-German efforts continued. The few small British X-ray equipment manufacturers failed to amalgamate so that they could compete with the larger German firms, and German products once again began to dominate the British market despite a one-third tax imposed by the Safeguarding of Industries Act in 1921.[21]

In the privacy of their order forms, and with the quality of their equipment important to the success of their practices and the safety of their bodies, British radiologists apparently failed to heed the demands of nationalism. These demands, however, continued to influence the more public activities of the medical radiological community. Germans were excluded from many scientific and medical meetings in Allied countries in the early 1920s.[22] Medical radiologists participated in this so-called "boycott," which has been described elsewhere.[23] In 1920 and in 1922, "bilingual" conferences, conducted in English and French, were held in Antwerp and in London. The exclusion of the Germans was not justified by a denial of the international character of science and medicine, but rather on personal grounds:

> We frequently hear the remark that science is international in its aims and objects, and there are few who would dissent from this view, but we never interpreted this as meaning that scientists are devoid of personal feeling. We can read any scientific publications, including those of German authors, without memories of the war being thereby evoked, but a Congress is essentially a personal and friendly matter; we think the time will come for international greetings, but that it has not yet arrived.[24]

The post-war exclusion of the Germans from professional meetings did not require government action, and insofar as government rules did affect these meetings the Allies were not exempt. The 1922 bilingual conference in London had originally been scheduled for 1921, but "the adverse exchange, and the absurd regulations as to passports and costly visas" made it difficult for the French and Belgians to attend and caused a postponement.[25]

Despite the disavowal of prejudice in evaluating the technical literature, reception of German innovations in Britain and in France was cool, and in the case of the Erlangen technique the Allies were often outwardly hostile and nationalist. News of the Erlangen technique reached the British public in mid-1921 through newspaper reports inspired by the West London Hospital, which had purchased German apparatus for deep therapy. Provoked by a favorable editorial in The Lancet (Britain's leading medical weekly), BARP issued a warning against the Erlangen technique and its exaggerated claims.[26] BARP feared that "public disappointment" from "unfulfilled promises" would discredit radiotherapy. In France, the Erlangen technique was quickly identified as the "German" method and contrasted with the superior, "French" method of less intense exposures and more sittings.[27] In the treatment of uterine fibromas, the French method also differed in irradiating not only the ovaries, but also the uterus itself because Antoine Béclère, the leading French advocate of radiotherapy in gynecology, believed that irradiation had a direct effect on the tumor.[28] Some of the British and French objections were similar to those that German critics had put forward: the Erlangen technique was more dangerous because it could not be discontinued when the first sign of an adverse reaction appeared, and it was overly schematic. As Béclère put it: "The judicious employment of a pliable method which can be adapted to the exigencies of each particular case is preferable to the blind acceptance of a uniform formula; but after all, we must insist that the radiotherapist be not only an able technician, but also an excellent clinician."[29]

In addition to their objections to the Erlangen technique as excessively dangerous and schematic, the British thought that the Germans misunderstood the mechanism of the therapeutic action of radiation against cancer. The Germans aimed to kill the cancer cells. The British thought this goal unattainable and perhaps even counter-productive, as well as dangerous because it required such high doses. A British physician had suggested in 1907 that X-rays and radium could stimulate the body's natural defenses against cancer.[30] This notion gained support with the discovery in 1911 in the United States of a transmissible sarcoma of fowls.[31] Cancer appeared to be a transmissible disease. X-rays, however, did not have any marked effect on known disease micro-organisms. It is known today that radiation suppresses the immune response, but most British radiologists by the end of the War probably believed that the purpose of radiation therapy was not to kill the cancer cells or microorganisms directly, but rather to stimulate immunity with short, repeated doses.[32] The intensive therapy used at Erlangen was, from this point of view, entirely misconceived.

British radiologists could be openly hostile toward the Erlangen technique, but once brought to public attention the German claims were difficult to ignore. At stake appeared to be no less than a cure for cancer. There was public dissatisfaction in Britain with the twenty-year-old Cancer Research Fund, and a more activist Anti-Cancer League had been established to promote early diagnosis and treatment.[33] Hospitals found it relatively easy to raise funds for the sort of high-voltage X-ray equipment that the Erlangen technique required. The Bradford Royal Infirmary received a 1000-pound donation from a "yarn and stuff" merchant in the summer of 1921 for its equipment, and BARP itself received an anonymous donation of 4000 pounds, intended in part to send someone to Erlangen.[34] By the end of 1921, Wintz found that the large number of foreign visitors was hampering the work of this clinic.[35] Many of the visitors returned from Erlangen with favorable reports. Especially impressive, and unknown in clinics outside Germany, were the precise ionization measurements. One British physician was impressed with the "scientific method," and another reported that "the whole process is mathematical and accurate."[36]

The earlier hostility toward the Erlangen technique and the continuing enmity toward the Germans heightened the impact of these favorable reports and excited competitive efforts. The French adopted small, graphite-lined chambers like those Krönig and Friedrich had used, but instead of standardizing against a larger ionization chamber the French adopted a simpler procedure. Iser Solomon, a physician with a degree in physics, proposed in 1921 using the radiation from one gram of radium filtered through one-half millimeter of platinum and placed at a distance of two centimeters from the ionization chamber. Ignoring the details of the German research on ionization methods that I have discussed above, French physicians regarded the amount of ionization produced under these standard conditions as a unit of dosage, which Solomon dubbed the "röntgen." The French assumed that this unit could be used to compare X-rays of different quality.[37] In Britain, the image of foreigners moving ahead was used to goad the profession, and to justify buying German apparatus:

> Our friends and rivals are wide enough awake to the necessities of the work and are working strenuosly with more powerful apparatus than we employ--such a state of things cannot be allowed to continue--we must have the apparatus wherever it is made.[38]

The campaign for an Institute of Radiology, which we have mentioned above, was directly linked to the effort to catch up with the Germans in deep therapy, but here there was an even more effective goad available:

> Let us take an example from Russia...two new Institutes have been inaugurated at Petrograd. One of these is an Institute of Röntgenology and Radiology....In the midst of the upheaval--the like of which the world has never seen--Bolshevik Russia can erect an Institute of Radiology. Are we in Britain going to be outdone in a matter of scientific research by Russia?[39]

Competition with the Germans and the Russians was a strong stimulus.

In addition to this external competition with foreigners, there was an internal conflict among British medical radiological organizations that eventually came to bear on the question of international cooperation. The setting of the Cambridge examination in 1920, and the organization in the same year of a Society of Radiographers that pledged its membership to practice only under the supervision of physicians, fulfilled BARP's initial goals of physician control and specialty status. BARP's leadership feared that the organization might drift into competition with the Electrotherapeutic Section of the Royal Society of Medicine (an organization under the exclusive control of physicians) and with the Röntgen Society (where physicians and nonphysicians participated on an equal footing). Amalgamation with the Electrotherapeutic Section was out of the question because the Royal Society of Medicine could not permit "activities of a medico-political nature," which were BARP's primary interest. The Council of BARP, which was dominated by physicians interested in radiology as a specialty, therefore decided that, provided an independent medical section could be maintained, amalgamation with the Röntgen Society would be desirable. The rank and file membership of BARP, which included many general practitioners opposed to joining with a society composed in large part of "laymen," defeated the Council's proposal. Discredited, the Council was enlarged, and BARP incorporated as a legal entity for the first time in 1921.[40]

The British Röntgen Society, in the meanwhile, was undergoing important changes and began as a result to bid for a stronger role in medical radiology. As in Germany, the War had brought more academically trained physicists into what came to be called "applied" research, and into the Röntgen Society. Though their numbers were still small relative to the total membership, which increased dramatically as the X-ray operators trained for military service joined, the physicists were well-represented on the Council of the Röntgen Society; after 1916 the post of President alternated between physicians and academically trained physicists.[41] Some of these physicists had only tenuous connections with medical radiology and participated in the Röntgen Society sporadically, but there was a small group whose professional interests focussed increasingly on the medical applications of X-rays and radium. G. W. C. Kaye, who had been trained under J. J. Thompson, became head of the radiological unit at the National Physical Laboratory, where activities had been expanded beyond custodianship of the British radium standard to include testing of X-ray protection materials and standardization of X-ray measurements. Sidney Russ, another physicist trained under J. J. Thomson, occupied a newly endowed position at the Middlesex Cancer Hospital. J. A. Crowther, University Lecturer in Physics Applied to Medical Radiology at Cambridge, taught the physics section of the course for the Diploma in Medical Radiology and Electrology. These academically trained physicists did not make their livings practicing medical radiology, and they were therefore not in direct competition with physicians. On the whole, the physicists supported the physicians in their efforts to gain supervisory control over radiological practice and in their campaign for specialty status. The physicist J. W. Nicholson, who as a student in 1901 and 1902 had worked as a medical radiologist at the Cancer Hospital in Manchester, in his Presidential Address to the Röntgen Society in 1922 referred to

> ...the anxiety we share with the medical profession that operators entrusted with such work must have a medical qualification...I can only say that, as a physicist, I am in the most complete sympathy with my medical colleagues in this matter...[42]

While supporting physician control of radiological practice, the physicists were also acutely aware of the shortcomings of their physician colleagues in maintaining adequate X-ray and radium protection. Kaye had become aware of the difficulties in protection during the War, when his unit at the National Physical Laboratory checked lead glass and other materials and found them very variable in quality. In its second memorandum, issued in December 1921, the British X-ray and Radium Protection Committee, of which Kaye was a member, recommended that the National Physical Laboratory check the physical layout of X-ray clinics, their protection devices and their electrical measuring instruments.[43] Nicholson reported the disappointing results to the Röntgen Society:

> the NPL...has examined a large number of X-ray departments in various hospitals, and almost invariably found their equipment as regards protective appliances is by no means satisfactory...the question is essentially an international one...I cannot say too strongly that though the investigation of X-ray phenomena from the point of view of the patient is of necessity the fundamental activity of our medical members, it is vital, in view of the new dangers which arise from radiations now in use, that operators should have some concern for their own welfare.[44]

Nicholson, Kaye, Russ and other physicists in the Röntgen Society saw the solution to protection problems, and also the solution to the problem of competing with the Germans, in closer cooperation between the scientific and medical sides of the radiological community. The Journal of the Röntgen Society had deplored the founding of the Society of Radiographers in 1920 as a step in the wrong direction, and it had pleaded for amalgamation of all the radiological organizations into a single national entity:

> If unreasoning prejudice could be swept away, a powerful British Society of Radiology (with a truly representative journal) could well look after the combined interests of the Röntgen Society, the Electrotherapeutic Section of the Royal Society of Medicine, the B. A. R. P., and this new Society of Radiographers.[45]

This amalgamation would eventually be achieved, but only after a complex series of negotiations that culminated in 1927.

The major issue in this conflict over amalgamation was physician control over radiological practice, and the discussions on amalgamation that began in 1924 between the Röntgen Society and BARP, which in that year changed its name to the British Institute of Radiology, stalled repeatedly on this issue. To see the conflict as a simple two-part conflict between physicians and nonphysicians would, however, be a mistake. There were actually four groups involved. Among the physicians, one group included those who were trying to practice radiology as a specialty as well as those who were engaged in research; a second group consisted of general practitioners who did not want, or could not afford, to practice only radiology, and who contributed relatively little to research. Among the nonphysicians, there were the so-called "lay" radiographers who actually operated X-ray machines as well as tube manufacturers and interested amateurs; and there were also the academic physicists, who did not practice radiology. The major conflict occurred between the physician general practitioners and the nonphysician radiographers, especially those lay practitioners of the older generation who had refused to join the Society of Radiographers. The physicists supported this older generation of lay practitioners more strongly than might be supposed, partly out of respect for elder colleagues who formed an important part of the membership of the Röntgen Society and partly because some of the proposals put forward for amalgamation would have lumped all the nonphysicians together in a lower category of membership. Similarly, the physician-specialists supported the general practitioners more strongly than might be supposed, partly because the general practitioners formed an important part of the membership of BARP and partly because as physicians the specialists had an interest in maintaining strict provisions for physician control.[46]

Despite this conflict, there were substantial areas of common interest to the physicists and the physician-specialists. Research was one such common interest. In 1923, the Electrotherapeutic Section of the Royal Society of Medicine and the Röntgen Society began to hold annual joint meetings at which a joint prize was awarded, alternately to a physician and a nonphysician.[47] In 1924, the Journal of the Röntgen Society and the Archives of Radiology and Electrotherapy, which had become the official organ of the British Institute of Radiology, joined forces as separate but equal "sections" of a newly-founded British Journal of Radiology. Deep therapy and the ionization measurements of X-ray doses associated with it were one of the areas of research that attracted the interest of both the specialists and the physicists. Another important common interest was in X-ray and radium protection. Despite some grumbling about its excessive requirements among general practitioners, the X-ray and Radium Protection Committee continued its work. The press campaign on the dangers of X-rays had been avoided, and the Committee in 1924 convinced the Home Secretary to include X-ray and radium work in the Schedule of Dangerous Occupations covered under the Workmen's Compensation Act.[48] The physician-specialists were pleased that the emphasis on protection confirmed their view that specialized knowledge was required to practice radiology. In addition, the specialists found the strictness of the requirements beneficial in terms of professional advancement as well as in terms of health: they could afford the costly shielding and elaborate protection procedures simply because a greater proportion of their incomes came from radiological work. The general practitioner who did only a few diagnostic exposures per week was much less ready to accept such encumbrances, and he may well have thought it unfair that the Protection Committee required the same precautions to protect X-ray operators regardless of the extent of their exposure.[49] Protection requirements thus increased the competitive edge of the specialist over the general practitioner, and interest in protection brought the specialist into closer alignment with the physicist.

Thus, while the British Institute of Radiology and the Röntgen Society continued at odds over the precise wording of a clause on physician control over radiological practice, the common interests of the specialists and the physicists developed into an effort at international cooperation. BARP had acquired a building at 32 Welbeck Street, not far from the Royal Society of Medicine. The British Institute of Radiology, as BARP became shortly thereafter, planned to open this permanent headquarters in 1924 and invited non-German foreign radiological societies to send representatives to the event. Economic conditions on the Continent had improved greatly since the postponement of the bilingual conference in 1921, and the response to this invitation was much greater than expected.[50]

As a result, the Röntgen Society in the summer of 1924 joined the Electrotherapeutic Section of the Royal Society of Medicine and the British Institute of Radiology in nominating representatives to a Provisional Committee, which polled radiological societies, journals and individuals, including some German radiologists, on the question of calling an international conference for July 1925 in London.[51] The response was sufficiently positive for the Provisional Committee to be converted to an Organizing Committee that sent out announcements early in 1925 of what was still cautiously regarded as a "preliminary" meeting.[52]

Not all of the German medical radiological community was keen on the notion of participating in the conference. Though they had suffered little tangible harm by exclusion from the two post-War bilingual conferences, which did not equal in either quality or quantity the research presented at the annual German Röntgen Society Congresses resumed in 1920, the boycott had been galling to the Germans. When word of the projected London conference reached them in late 1924, probably by means of the Provisional Committee's poll, the question of participation was brought before the Council of the German Röntgen Society and discussed during the annual meeting of the German Society of Scientists and Physicians at Innsbruck. The discussion has not been preserved, and no decision was recorded in the proceedings of the meeting. Only after the official announcement emphasized that all nations would be invited did the Council recommend that members of the German Röntgen Society participate.[53] About forty Germans attended out of a total of 500 participants. This percentage was much smaller than might have been expected from the relative sizes of the radiological communities and also much smaller than the percentage of German participation in the subsequent international conferences held before World War II.

In both Britain and Germany, it is likely that it was the physicists, perhaps supported by the physician-specialists, who advocated German participation in the 1925 London conference. More than anyone else associated with the medical radiological community, the physicists appreciated the importance of an internationally agreed unit of X-ray dosage, and once the work of Krönig and Friedrich, Holthusen, and Seitz and Wintz had become known it was difficult to consider the dosage problem without the Germans. At a joint meeting of the Physical Society and the Röntgen Society in 1923, the Middlesex Hospital physicist Russ had called for international standardization, and the two Societies had soon thereafter appointed a joint committee to consider the dosage problem.[54] On the German side, the initiation of standardization activities at the Physical-Technical Institute in 1924 seemed to the physicists to be a preliminary step toward international standardization. As the Charlottenburg physicist Behnken put it, "international standardization is then the next goal to keep an eye on."[55] With the official adoption of the röntgen by the German Röntgen Society in April 1925, the stage was set for the London conference, where the physicists, as we shall see, would press for ionization measurements, international standardization of X-ray units, and radiation protection.

The London conference was successful in re-establishing formal communication between the German and non-German radiological communities. The participants declared it the first International Congress of Radiology and decided that the second would meet in Stockholm in 1928. An International Commission on X-ray Units was established and the groundwork was laid for international cooperation on radiation protection. The Germans returned home praising British hospitality, boasting that radiology was the first medical discipline to return to true international cooperation, and pleased that Röntgen had been given his due in the speeches at the conference dinner.[56] The British were delighted with the decision to make the conference the first of a series, and the French, though anxious to have pictures of the Curies and of Becquerel appear on the dinner program at the next Congress, appear to have been relieved that the Germans behaved themselves in a civilized manner.[57] The sudden emergence of cooperation from conflict should not be surprising. If the British physician-specialists and their Allied colleagues were to compete with the Germans in doing intensive deep therapy, or if they were to deny the Erlangen claims and put forward their own, their results would have to be comparable with the results of the German clinics. Making the results comparable required cooperation in standardizing doses. The physicists, as we shall see, pushed this necessary cooperation on dosage standardization a step farther to a less necessary, but highly significant, cooperation on radiation protection. In doing so, they again found supporters among the physician-specialists.

The pre-1925 nationalism continued at the Conference and fueled its most important decision: to establish an International Commission on X-ray Units. After opening speeches emphasizing common interests and international friendship, a joint meeting of the sections on physics and radiology under the chairmanship of William Bragg, who had become Britain's leading X-ray physicist, debated the problem of X-ray dosage measurements.[58] Béclère presented the case for the French, radium-based "röntgen."[59] Behnken presented the case for the German "röntgen" defined in terms of the charge produced by the ionization of air.[60] Grebe and Martius presented the case against the unit skin dose by showing that it varied, even within Germany, by as much as a factor of four.[61] No one defended the unit skin dose openly, but the medical practitioners had certainly not abandoned it. Using the unit skin dose and defending it to an audience that was half physicists were two quite different activities. The unit skin dose remained in use until the 1930s, when it only gradually lost its hold. There were other proposals as well, including a last-ditch effort by Dessauer to defend photographic measurements, a proposal by a French physicist to measure dose in energy (ergs) absorbed per gram of tissue (this unit is today known as the "rad" and is in common use), and also a proposal to adopt the common clinical procedure of specifying the voltage across the tube and the current through it. The discussion brought the major conflict, between the French and German röntgens, into the open.[62] The British X-ray

Unit Committee that had been formed by the Röntgen Society and the Physical Society in late 1923 refrained from offering its own proposal. The Congress rewarded this restraint by giving the British the task of calling together an International X-ray Unit Commission to decide the issue at the second International Congress three years hence.[63]

The physicists were less successful in pressing the issue of protection. G. W. C. Kaye took the lead, proposing "...international agreement on, at any rate, the main questions of protective measures." He claimed that "such a step would have obvious advantages."[64] In making this proposal, Kaye reviewed the history of the British X-ray and Radium Protection Committee and mentioned its counterparts in other countries, but the tangible advantages of his proposal were by no means obvious. Intercomparability of therapeutic results did not depend on international agreement on protection requirements, as they depended on international agreement on an X-ray unit. One might be tempted to assign to Kaye idealistic motives, but they would hardly account for the success of his proposal within the Physics Section of the conference, which adopted a resolution placing "on record the desirability of adopting a standard scheme of X-ray and Radium protection throughout the world."[65] There were still strong nationalist feelings in this group, and Kaye himself had been a vehement anti-German nationalist only a few years before. Kaye was, I believe, appealing for agreement on the international level to strengthen the hand of the physicists, and their physician-specialist supporters, on the national level. Had there been a significant number of non-specialist physicians in the Physics Section (which naturally there were not), or had the Kaye proposal been submitted to the Congress as a whole, the outcome might have been different.

Within the Physics Section, however, Kaye found a good deal of sympathy. The receptivity of the physicists to strengthening radiation protection should not be regarded as entirely disinterested. Their role in medical radiology was still being defined, and they stood to gain in status and security if they could demonstrate their usefulness in designing and checking protection measures. There was, however, evidence to support the physicists' view that physicians, left to themselves, would not institute and maintain adequate protection measures, even though it was the physicians themselves who often suffered most from laxity. The National Physical Laboratory inspections had revealed many shortcomings, and by 1925 similar investigations undertaken at the four largest X-ray clinics in Stockholm and at the Saint Antoine Hospital in Paris had shown significant quantities of so-called "stray" radiation arising from inadequate shielding and from scattering in the body of the patient and in the walls, ceiling and floor.[65] Many physicians appear to have been unaware of the increasing proportion of scattered relative to absorbed radiation with increasing X-ray hardness. In Germany, the Röntgen Society survey of X-ray injuries to patients was not yet published, but it was already known that the survey included a significant proportion of injuries due to negligence or ignorance on the part of X-ray operators, including physicians and technicians under physician supervision.[67]

Kaye's proposal for international protection recommendations was not formally discussed among the physicians at the 1925 Congress, an omission that confirms the leading role of the physicists in pressing for international protection standards. The position of the physicians can, however, be inferred from later developments. As we shall see, radiation protection became among physicians part of a dual strategy for promoting specialization: on the one hand, there was a need for higher educational standards; on the other hand, there was a need for improving the apparatus used in radiology, including protection devices. Both raising educational standards and improving apparatus would tend to place radiology in the hands of those who practiced it full time. The general practitioner, who at worst might rely on nothing more than a short course offered by an X-ray tube salesman for his knowledge of radiology and who at best had a few weeks of instruction during medical school, would be the eventual victim of this dual strategy. The physician-specialists had much to gain from the physicists' initiative in favor of radiation protection.

The common interests of the physicists and the physician-specialists would become increasingly evident in the decade after 1925. The London Congress had a broad impact, and in a number of important respects it set the agenda for international cooperation in the radiological community for the next decade. The physicists would have their way, achieving both international standardization of X-ray dosage and international recommendations for X-ray and radium protection, though not without unanticipated difficulties.

The dosage problem posed technical difficulties, which were solved largely within the physicists' part of the radiological community. The protection problem posed difficulties of a different sort involving the relationship of the physicists to the physicians, and of both to the issue of specialization.

When the Physical-Technical Institute began its standardization program in 1924, Behnken and other physicists were convinced that the technical difficulties had been overcome with the introduction of the air-pressure chamber. German physicians, however, found maintaining the standardization in röntgens under clinical conditions difficult, and even physician-specialists who used ionization chambers often continued to express doses in terms of the unit skin dose rather than in terms of röntgens.[68] This practice seemed eminently sensible after the physicists discovered in 1926 and 1927 that the dose in röntgens required to produce an erythema was different in Germany and in the United States. The Bonn physicists Grebe and Martius, using ionization chambers standardized by Behnken's air-pressure chamber, had found an average erythema dose of 600 röntgens in their survey of German clinics. Glasser, the student of Friedrich who had emigrated to the United States, found an erythema dose of 1400 röntgens using clinical ionization chambers standardized against a large chamber of atmospheric pressure.[70] Part of the problem arose because Glasser placed his ionization chambers directly on the skin, thus including in his measurements the dose due to back scattering from the body; Grebe and Martius positioned their ionization chambers in the air at the same distance from the tube as the exposed skin. Even after this difference was recognized, however, there remained a significant discrepancy that was traced to a difference in the size of the röntgen unit measured in Germany and in the United States. Glasser found that his own röntgen unit agreed with those of two other United States-based investigators (one of whom, like Glasser, was a recent immigrant from Germany) to within ±4 per cent. The röntgen units measured by Behnken, by Grebe and Martius, and by Friedrich also agreed well, but they were 50 per cent smaller than the "American" röntgen.[71] The fact that two of the three "American" investigators were German-born and German-trained physicists was irrelevant: the competition was cast in terms of a rivalry between the United States and Germany.

The French, gleeful at the discrepancy, leapt into the fray and tried to re-assert the claims of the French, radium-based röntgen. Béclère put it this way:

> It is necessary that an impartial arbitrator intervene between the physicists of Germany and of the United States....I hope that a French physicist has this ambition and that the honor will come to him to settle [the question].[72]

Béclère had in mind his own laboratory chief, Solomon, who had proposed the French röntgen in 1921. Solomon was aware of the arbitrary character of his unit and suggested that the distance at which the radium was placed might be altered to make the unit agree with the German röntgen.[73] Glasser welcomed this suggestion by showing that the French röntgen bore a constant relationship to the American röntgen (with a variation of less than ±5 per cent.)[74] At the same time, Glasser found that seven German ionization chambers calibrated in German röntgens varied by $\pm$13.5 per cent among themselves. Such a variation might at the time have been tolerable in X-ray clinics, but it reflected badly on the laboratory skills of the German physicists, and Behnken replied with a detailed description of the German equipment and procedures.[75] With the second International Congress of Radiology only a few months away, a major battle among the physicists over X-ray units appeared to be in the making.

The conflict was averted at the last minute, when Behnken travelled to the United States and checked the German ionization chambers that Glasser was using in Cleveland. Unfortunately, the sources of error and Behnken's means of correcting them are not clear. Glasser merely cited in general terms "faulty construction and lack of proper control of the instruments used in the transportation of the German R unit."[76] After Behnken's repairs, the German and American instruments agreed. The British, in the meanwhile, had carried out their mandate from the 1925 Congress and had invited national physical and radiological societies to send representatives to the International Commission on X-ray Units, which met for the first time in Stockholm during the second International Congress of Radiology in July 1928. This group adopted the German definition of the röntgen, which both the Americans and the Germans had used, and the Congress as a whole endorsed this decision. The French were given their due in a recommendation that the constancy of ionization chambers standardized in röntgens be checked using radium.[77] At the third International Congress (Paris, 1931), the "international röntgen" was declared satisfactory, and with the physicists at last agreed among themselves this unit gradually entered clinical practice, becoming well-established by the mid-1930s.[78] There would be a variety of new difficulties before World War II arising from the increasing voltages used in generating X-rays and from the suggestion that the dosage of gamma rays from radium be measured in röntgens, but by 1934 doses of X-rays from tubes excited with several hundred thousand volts were comparable to within ±1 per cent.[79] The problem of X-ray dosage was at long last solved.

As for radiation protection, the resolution passed at the 1925 Congress by the Physics Section called for an international scheme but failed to specify a procedure for reaching an agreement, and the Congress took no action on the question. It can be inferred from later developments, however, that Kaye had proposed to the physicists that the recommendations of the British X-ray and Radium Protection Committee be adopted as international recommendations at the Stockholm Congress in 1928. The British recommendations included specific thicknesses of lead shielding for X-ray tubes to be used in diagnostic work, in superficial therapy and in deep therapy. The shielding requirements were to prove a major item of contention. The British Committee had aimed to reduce the dose to the operator as much as possible without interfering with radiological practice.[80]

This goal was a reasonable one, but the specification of shielding requirements failed to allow for the variety of tube designs, for the increasing voltages becoming available, or for the possibility of adequate shielding but inadequate protection because of scattering in the patient's body and in the room.

The way around these difficulties in the physicists' view was to specify a dose limit to the operator of the X-ray tube, or to the manipulator of radium applicators, and to calculate the shielding required from this dose limit. Equipped with their ionization chambers, the physicists could then check whether the dose limit was being exceeded. The question that neither the physicists nor the physicians could answer was how large the dose limit should be. If it were too large, then the operators would suffer harm, but if it were too small the shielding required would hinder radiological practice. One way to decide the size of the dose limit would have been to weigh the risk to the operators against the benefits of radiological practice, a procedure that at least in principle is used today in radiation protection for nonmedical sources of exposure and is often advocated for controlling other sorts of technological risks. Even before World War I, it had been common in replying to public fears concerning the risks of radiation exposure for members of the radiological community to emphasize the benefits of X-rays and radium in medicine, but explicit efforts to determine a dose limit by risk-benefit analysis were still a long way off. The strategy used was based, instead, on the assumption that there was a threshold for the biological effects of radiation. The assumption did not have, and did not seem to require, experimental confirmation. To both physicists and physicians, experience suggested that many people had been exposed to radiation, some for many years, without suffering harm.

The threshold assumption had been common previously, but it was only in the mid-1920s that anyone made a serious effort to determine the threshold quantitatively. Late in 1924, an American physicist, A. Mutscheller, initiated these efforts by attempting to measure the "tolerance dose," which he defined as "the dose which an operator can, for a prolonged period of time, tolerate, without ultimately suffering injury."[81] Mutscheller's procedure was to measure the dose actually delivered to the X-ray operators in "several typical good installations" and on the basis of these figures and "fair averages" to calculate the dose to the operators over the period of a month.[82] Mutscheller in this way arrived at 1/100 unit skin dose (which he termed the erythema dose) received over a period of a month as the tolerance dose. He put forward the figure tentatively and called for "close cooperation between physicists and biologists and a systematic cooperation of röntgenologists, and careful examination of the blood and other organs of röntgen-ray operators [to] decide the point."[83]

In the wake of the 1925 Congress, Mutscheller's proposal received an enthusiastic response among physicists. The German physicist Glocker, in commenting on the recommendations of the

British X-ray and Radium Protection Committee to the 1926 Röntgen Congress, concluded by pointing toward the importance of the tolerance dose for future decisions on radiation protection:

> To get an exact basis for the drafting of radiation protection measures it is indispensable that, through as many statistical contributions as possible from the membership of the German Röntgen Society, a more precise value of the tolerance dose be obtained. Cooperation to this end thus lies in the röntgenologists very own interest! [84]

Glocker preferred to express the tolerance dose on an hourly basis rather than on Mutscheller's monthly basis, but he accepted Mutscheller's figure of 1/100 unit skin dose per month in calculating his own tolerance dose of 1/20,000 unit skin dose per/hour. Behnken also accepted Mutscheller's figure, and the Swedish physicist Rolf Sievert, in the study of protection in Stockholm's four largest X-ray clinics referred to above, assumed a tolerance dose of 1/10 unit skin dose per year, which with one month vacation would be approximately the same as Mutscheller's tolerance dose.[85] In Britain, a physician and a physicist working together confirmed Mutscheller's figure with data on two workers at the Manchester Royal Infirmary and showed that this tolerance dose could be readily achieved in diagnostic work.[86] Kaye showed that the shielding thicknesses recommended by the British X-ray and Radium Protection Committee were consistent with a tolerance dose about 40 per cent smaller than Mutscheller's.[87]

Thus the time duration for which the tolerance dose was specified varied, but the physicists were generally agreed on the approach. They were also prepared to compromise with the physicians in their continued adherence to the unit skin dose, which seemed to be appropriate to protection questions. Kaye in late 1927 outlined the ideal procedure for determining protection measures from the physicists' point of view as follows:

> ...a scheme of X-ray protection which rests on a sound physical and biological basis involves:
>
> a) Measuring under specified conditions the intensity of X-rays in terms of a specifiable and reproducible physical standard expressed, if possible, in absolute units.
>
> b) Establishing a maximum tolerance dose in terms of a specifiable and reproducible biological standard, and if possible, expressing this biological standard in physical units.
>
> c) Establishing reliable figures for the transmission of X-rays of specified quality by lead and other absorbents.
>
> d) Calculating the thickness of the absorbent necessary to reduce the intensity of a given beam of X-rays to that corresponding to the tolerance dose at some specified point.[88]

In carrying out this dose-limiting procedure, it was more important to have a single agreed number for the tolerance dose than to have precisely the correct number, and the physicists argued little over variations as great as 50 per cent in estimates of the tolerance dose. Just before the 1928 Stockholm Conference, Mutscheller let it be known that, with the approval of Kaye and Glocker, he intended to propose international adoption of a tolerance dose of 1/100 erythema dose per month.[89] The proposal was well-received by the physicists present, who included Glasser and Lauriston Taylor, an American who had begun to play a major role in radiological work at the National Bureau of Standards.

While the physicists were occupied with the tolerance dose in the aftermath of the 1925 Congress, physician-specialists outside Britain welcomed Kaye's proposal for international protection recommendations. In Austria, Holzknecht began to press the government, through a report written for the Technical Testing Bureau, to issue rules governing both the equipment used in X-ray clinics for protection and the training required of radiologists and X-ray operators.[90] Holzknecht claimed that the worst injuries to patients were caused by forgetting to place a filter in the primary beam to remove the softest X-rays. An automatic device that prevented operation of the X-ray tube if the filter was not in place cost .1 per cent of the 12,000 marks required to buy an X-ray installation, and yet he believed as many as 90 per cent of the X-ray tubes lacked the device. In Germany, the Röntgen Society in 1926 decided to revise its 1913 Instruction Sheet to take into account the higher voltage X-ray tubes that had come into use.[91] At the same time, its Special Committee for the Judgment of Röntgen Injuries and the Study of their Prevention reported on "Foreign Legal Prescriptions for the Exercise of the Röntgen Procedure."[92] This report cited approvingly a French requirement of two or three years study, plus a year of practical experience, before being examined for recognition as a "specialist." It also advocated licensing of X-ray installations by the state, a procedure that had been adopted in Denmark and in New York City. In France, the educational requirements for recognition as an X-ray specialist had, as the German report noted, been greatly expanded, and in addition consideration was again being given to rules for X-ray and radium protection.[93] In Sweden, an X-ray and Radium Protection Committee modeled after the British Committee adopted similar recommendations.[94] At the same time, the Swedes chose "Instruction and Training in Medical Radiology" as a theme for the 1928 Congress in Stockholm, a theme that necessarily raised questions about specialization.[95] In the Soviet Union, the People's Commissariat for Work issued rules for X-ray protection in the fall of 1925 that required upgrading or closing down X-ray installations that could not comply within three months, a requirement that at least in principle would eliminate the smaller, less busy X-ray installations run by non-specialists.[96]

Notably lacking from these efforts by physicians to promote, in tandem, protection and specialization was much consideration of the tolerance dose. Some medical objections to the tolerance dose arose from the necessary variations in clinical conditions and practices: idiosyncratic reactions of individual patients, though recognized as less frequent than had been thought before the use of ionization chambers, were still regarded as a possibility; and reaction to radiation varied with the time in which a given dose was delivered, so that four exposures over a period of a month would not have the same effect as the same dose delivered in a single sitting. Though from a physical point of view it made no difference for what time period the tolerance dose was specified, from the biological point of view the time factor was critical. In addition to these medical arguments and probably more important in accounting for lack of physician interest in the tolerance dose, the calculation of shielding thicknesses was a mysterious mathematical procedure to most physicians, however routine it had become for the physicist in medical radiology. Behnken, in proposing in 1926 that the first protection rule always be "the tolerance dose should, in the places protected, nowhere and

and never be exceeded," tried to allay the physicians' fears: "A röntgenologist need hardly get the creeps from 'higher' mathematics that enter..., especially since the practitioner needs to use only the condensed table corresponding to average [voltage] requirements." The reply was that protection measures had to be phrased "so that everyone will be able to understand them."[97]

Just as in their continued use of the unit skin dose, the physicians did not openly confront the physicists on the issue of the tolerance dose, and they were content to let it be used to calculate recommended shielding thicknesses. At the same time, however, the tolerance dose went unspecified in the protection recommendations, which then read like compendia of good clinical practice based on the collective experience of physicians. If the notion that protection lay within the prerogatives of the physician were to be maintained, it would hardly do to have the tolerance dose cited as the basis for the rest of the protection recommendations. Thus, when the British X-ray and Radium Protection Committee extended its recommendations to higher voltages in 1927 in a way that was consistent with a tolerance dose of 1/100 unit skin dose per month, this figure was not mentioned in the recommendations.[98] The second International Congress of Radiology, meeting in Stockholm in 1928, adopted at the behest of the Physics Section simplified and abridged international protection recommendations that for most practical purposes followed the British recommendations, but Mutscheller's effort to have the tolerance dose adopted apparently failed.[99]

In addition to adopting international protection recommendations, the Stockholm Congress created the International X-ray and Radium Protection Commission, with five of the initial seven members physicists (including Solomon, who was also a physician).[100] The creation of this Commission raised the issue of protection to a new level of visibility and generated a wave of activities on the national level. When the League of Nations Health Organization asked the German physician-physicist Wintz in 1931 to review national protection measures, he was able to cite detailed rules under consideration or adopted in Austria, Czechoslovakia, Denmark, Britain, Germany, Greece, Hungary, the Soviety Union, Sweden, and Switzerland.[101] In the United States, the creation of the International Protection Commission led to the formation of an Advisory Committee on X-ray and Radium Protection with representatives from the American Röntgen Ray Society, the less restrictive Radiological Society of North America, the American Medical Association, the National Bureau of Standards, and the equipment manufacturers.[102] The British X-ray and Radium Protection Committee continued its work, and in Germany the Röntgen Society, which in 1927 had formed a Standards Bureau that affiliated with the German Standards Committee, promulgated radiation protection recommendations in 1930 after two years of discussion and redrafting.[103]

The International Protection Commission met in Paris in 1931 during the third International Congress of Radiology and made a number of changes in the international recommendations. Among the most important were the extension of the table of recommended shielding thicknesses for X-rays from 225,000 volts to 400,000 volts and the addition of a table of recommended shielding thicknesses for radium that replaced an earlier requirement of 5 cm of lead shielding for each 100 mg.[104] The first table was determined from a tolerance dose of 10^{-5} röntgens per second, which was the equivalent (assuming 200 working hours per month and a unit skin dose of 600 röntgens) of Mutscheller's original 1/100 unit skin dose per month. The recommended shieldings for radium were approximately those that would have been derived from a tolerance dose one third the size of Mutscheller's, an added precaution taken because of the continuous emission of radiation from radium while X-ray tubes were assumed to be used only eight hours per day.[105] In neither case did the international recommendations specify the tolerance dose.

National and international discussions of the protection recommendations could not, however, continue to ignore the tolerance dose, and conflicts over protection measures revolved increasinbly about this notion. In 1930, for example, two physicists working for the Dutch Philips Company, a major X-ray tube manufacturer since its introduction of a tube that had most of the required protection built inside rather than surrounding the glass bulb, challenged the draft German recommendations on the grounds that they did not follow the international recommendations and cited the tolerance dose as the basis for the latter.[106] In 1931, when a British physician attacked the international recommendations as too strict, the reply he received from both physicists and physicians present was that the international recommendations were consistent with the tolerance dose.[107] Wintz, in his 1931 report to the League of Nations outlined in detail the procedure for deriving shielding thicknesses from the tolerance dose and discussed checking the effectiveness of protection measures in terms of verifying that the tolerance dose had not been exceeded.[108]

Thus, the tolerance dose was slowly coming into open circulation and achieving acceptance among physicians as well as physicists. When the International Protection Commission met at the fourth International Congress of Radiology in Zürich in 1934, five of its nine members were physicians (once again including Solomon). The X-ray protection recommendations the Commission approved cited the tolerance dose prominently and explicitly, though still cautiously: "The evidence at present available appears to suggest that under satisfactory working conditions a person in normal health can tolerate exposure to X-rays to an extent of about 0.2 international röntgens (r) per day."[109] This figure was approximately equivalent to 10^{-5} röntgens per second assuming a seven-hour working day, and its adoption should be viewed as the acknowledgment of a long-standing practice rather than as an innovation. The international recommendations also noted that no tolerance dose for exposure to the gamma rays of radium were available, but without citing any reasons.[110]

This omission may have been connected with two newly discovered instances of injuries caused by radium and by radium emanation: to radium dial painters and to arsenic miners. In 1924 and 1925, a peculiar and sometimes fatal necrosis of the jaw had been diagnosed in women working as dial painters in a New Jersey plant.[111] The women had ingested radium while tipping their brushes to a fine point between their lips. The incident first came to the attention of the public through the efforts of the New Jersey Consumers' League, and only after it was highly publicized through the case of the "five women doomed to death" did government authorities and the medical profession beyond the local community become actively interested.[112] An out-of-court settlement was reached in 1928, but shortly thereafter it was discovered that some of the radium dial painters were developing osteogenic sarcomas at the sites of previously observed irritations of their bones.[113] At about the same time as the initial discovery of "radium jaw," a lung disease characteristic of arsenic ore miners in the Schneeberg mountains of Germany was attracting the attention of research workers sponsored by the Saxon Regional Committee for the Investigation and Control of Cancer.[114] By the early 1930s, radium emanation in the Schneeberg mines and in the uranium mines at Joachimstal (Czechoslovakia) had become a prime suspect as the causal agent of the lung cancer manifest in some of the miners.[115] Neither the radium dial painters affair nor the Schneeberg and Joachimstal miners incident had been caused directly by medical uses of radiation, as earlier radiation-induced injuries had been, but both raised questions about the long-term effects of using radium and radium emanation in therapy. Radium protection clearly faced new and poorly understood challenges, and specifying a tolerance dose in the face of these uncertainties may well have seemed unwise.

1. G. W. C. Kaye (M. A., D. Sc., Capt. R. E. (T.)), "X-rays and the War," Presidential Address delivered 6 November 1917, J. Rönt. Soc., 14 (1918) 2-17, at 16.

2. Gosta Forsell, as quoted in "Erster Internationaler Radiologen-Kongress," Fortschr. Röntgenstr., 33 (1925) 797-800, at 800: "Dieser Kongress war nicht nur ein Kongress von Wissenshcaftlern, nein, er war eine Versammlung der Nationen."

3. For a general statement of this view, see J. J. Salomon, Science and Politics, tr. Noel Lindsay (Cambridge, Massachusetts: MIT Press, 1973). For specialized studies, see Brigitte Schröder-Gudehus, Deutsche Wissenschaft und internationale Zusammenarbeit, 1914-28: Ein Beitrag zum Studium kultureller Beziehungen in politischen Krisenzeiten, Thèse presentée à l'université de Genève (Genève: Dumaret et Golay, 1966); Daniel J. Kevles, "Into Hostile Political Camps: the Reorganization of International Science in World War I," Isis, 62 (1970) 47-60; and Paul Forman, "Scientific Internationalism and the Weimar Physicists: the Ideology and Its Manipulation in Germany after World War I," Isis, 64 (1973) 151-80.

4. Paul Forman, The Environment and Practice of Atomic Physics in Weimar Germany: A Study in the History of Science (University of California, Berkeley, Ph. D. 1967; Ann Arbor, Michigan: University Microfilms, 1968), at 139.

5. Forman, note 3 above.

6. "Interim Report of the Standards' Committee" presented in January 1908, J. Rönt. Soc., 4 (February 1908) 27-36. The suggestion that X-rays be standardized by comparison with radium had been made by C. E. S. Phillips, "The Need for a Radio-active Standard," J. Rönt. Soc., 2 (April 1906) 79-90 and 92-102. The Council of the Röntgen Society formed a committee "of men sufficiently well known in the scientific world to carry weight," including ten people from the scientific community, four from the medical community and two who are not readily identifiable, see J. Rönt. Soc., 3 (1906) 16 and 48. The suggestion for the Committee had been made originally by W. Deane Butcher, "The Means of Accurate Measurement in X-ray Work," J. Rönt. Soc., 1 (April 1905) 74-87, with discussion. For the deposit of the standard at the National Physical Laboratory, see J. Rönt. Soc., 5 (January 1909) 20 and W. Deane Butcher's comments during the session "Radiometrie, Terminologie," 13 September 1910 at the Congrès International pour l'Étude de la Radiologie et d'Électricité, Comptes rendus, volume 1.

7. Curie, "Sur le poids atomique du radium," C. R. Acad. Sci. (Paris), 145 (1907) 422-25 and Otto Brill, "Über die Fortschritte der chemischen Forschung auf dem Gebiete der Radioactivität," Verh. Ges. Deut. Naturf. Artz., 81 (1909) 124-49, at 132 ff.

8. The 20 per cent figure is mentioned in E. Rutherford, Radiumnormalmasse und deren Verwendung bei radioaktiven Messungen, tr. B. Finkelstein (Leipzig: Akademische Verlagsgesellschaft, 1911), at 13. This booklet apparently never appeared in English and is not included in James Chadwick, ed., The Collected Papers of Lord Rutherford of Nelson (London: George Allen and Unwin, 1962). Curie's sample is mentioned in a Rutherford letter to Stefan Meyer, 25 April 1911 (Rutherford Correspondence microfilms): "I forgot whether I told you that a small standard which Mme Curie sent me last year was about 9 per cent lower than my standard. She was cautious, however, in committing herself to its correctness on account of the uncertainty of weighing such small quantities." The Rutherford Correspondence microfilms are on deposit with the Sources for the History of Quantum Physics at the American Institute of Physics (New York); the American Philosophical Society (Philadelphia); the University of California (Berkeley); and the Niels Bohr Institute (Copenhagen). The material contained in these microfilms is listed in Lawrence Badash, Rutherford Correspondence Catalogue (New York: American Institute of Physics, 1974).

9. D. Isaachsen, "Introduction Historique," in Ch.-Éd. Guillaume, La Création du Bureau International des Poids et Mesures et Son Oeuvre (Paris: Gauthiers-Villars, 1927), pp. 1-31; British Association for the Advancement of Science, Reports of the Committee on Electrical Standards: A Record of the History of "Absolute Units" and of Lord Kelvin's Work in Connexion with These (Cambridge: University Press, 1913).

10. Rutherford, ibid., at 44: "...dass niemand anders zu der wichtigen Arbeit, ein Radiumnormalmass herzustellen berufen sei, als Mme Curie, die Entdeckerin des Radiums..."

11. Ibid.

12. E. Rutherford (Manchester) "Rapport sur les étalons de Radium"; J. Danne (Paris), "Sur la nécessité de créer un étalon international de Radium"; P. Lenard (Heidelberg), "Sur les mesures et les unités radioactives" and A. Becker (Heidelberg), "Sur l'émanomètre"; all in the "Resumés des communications," Congrès international de Radiologie et d'Electricité, Bruxelles, 13, 14 and 15 September 1910, Radium, 7 (1910) 221-247.

13. "Statuts de la Commission de l'étalon," Radium, 7 (1910) 65 (feuilles de couverture). The members of the International Radium Standards Committee were M. Curie and A. Debierne for France, H. Geitel and O. Hahn for Germany, St. Meyer and E. von Schweidler for Austria, B. Boltwood for the United States, E. Rutherford and F. Soddy for Britain, and A. S. Eve for Canada. Geitel's appeal to include Elster, his constant collaborator, on the basis "dass er sich als Hälfte von Elster und Geitel fühle," was rejected on the grounds that only two from each country were permitted, see Stefan Meyer to Rutherford, 29 September 1910 (Rutherford Correspondence microfilms), note 8 above, though this requirement does not appear in the statutes.

14. See Robert Reid, Marie Curie (New York: New American Library, 1974). The encouragement to Meyer is in the letter quoted at note 8 above: "In case Mme Curie is unable to complete her work, it will be of great importance to have another standard ready to put in its place."

15. O. Hahn, St. Meyer and E. v. Schweidler, "Bericht über die Versammlung der internationalen Radiumstandardkomission in Paris vom 25. bis 28. März 1912," Radium Biol. Heilk., 1 (1912) 354-56 and E. Rutherford (F. R. S.) and J. Chadwick (B. Sc.), "A Balance Method for Comparison of Quantities of Radium and Some of its Applications," Proc. Phys. Soc., 24 (1912) 141-51, received 21 February 1912, read 23 February 1912.

16. "VII[e] Congrès International d'Électrologie et de la Radiologie Médicale (Lyon, 27-31 juillet 1914)," J. Radiol. Electrol., 2 (1916-17) 659-61, at 659: "...s'inspirant d'une pensée dont chacun trouvera l'écho en lui-meme, le Ministre de la guerre a, au mois de novembre dernier, demandé qu'il fût sursis à la distribution du volume jusqu'à la cessation des hostilités, afin d'éviter la publication déplacée pour le moment, des quelques memoires allemands, d'ailleurs peu considérables, communiqués au Congrès."

17. R. Ledoux-Lebard, "Causeries sur les livres. III--La Radiologie chez les austro-allemands depuis la guerre," J. Radiol. Electrol., 2 (1916-17) 520-30, at 528: "La mentalité germanique est d'ailleurs quelque chose de bien particulier qui reste incompréhensible pour nous dans la bassesse et la primitivité de ses moyens d'action et de ses conceptions."

18. The change of name was attributed in an editorial to the need for broader cooperation among all those concerned with medical electricity, and the words "Archives of the Röntgen Ray" were preserved in parentheses on the masthead, see Arch. Radiol. Electroth., 20 (June 1915) 1-2. However, as early as the previous December there had apparently been pressure to change the name of the journal, for W. Deane Butcher, the editor, wrote "It may be noticed that we have not erased from our list of collaborators the names of the German and Austrian radiologists who have done so much to promote the success of this journal, an expression of our lasting belief that the brief frenzy of war should not cause any lasting breach between radiologists of different tongues....We have no intention of retaliating [against Röntgen] by altering the title of our paper, a discourtesy which would rather savour of the religious rancour of two hundred years ago than of the cool, scientific culture of the twentieth century," Arch. Radiol. Electroth., 19 (December 1914) 239-40. Butcher left the editorship in January 1915 because of illness, the Germans and Austrians were dropped from the list of collaborators in February 1915, and by June 1916 Röntgen's name was not to be found on the journal. As late as 1923, the New York Tribune of 2 November reminded its readers on the occasion of Röntgen's death that "because of his hatred for England," he had donated his Rumford medal to the German Red Cross, see BARP Clippings File V (Volume 5 of the BARP Clippings File in the library of the British Institute of Radiology).

19. "Discussion of the Proposed New Rules of the Society," 7 May 1918, J. Rönt. Soc., 14 (October 1918) 107-18, at 109. The members present at this meeting voted in favor of a change to "The Society of Radiology (Röntgen Society)," but the general membership turned down the proposal in a postal vote, ibid., at 115.

20. "Comité mèdical et scientifique d'expansion économique," Arch. Elec. Med., 24 (1916) 23 (feuilles de garde).

21. For amalagamation proposals, see Geoffrey Pearce, "The Future of the British X-ray Industry," J. Rönt. Soc., 13 (1917) 60-87 and 91-106, with discussion, and Kaye, note 1 above. For reference to the Safeguarding of Industries Act, see the 5 November 1921 Yorkshire Observer, at 58, in BARP Clippings File II, note 18 above. For one objection to the post-War resurgence of German equipment in Britain, see the letter from an unidentified British manufacturer, "German-Made X-ray Apparatus," J. Rönt. Soc., 18 (1922) 95.

22. In 1923, Germans were excluded from 70 per cent of the 50 international congresses and other events, and in 1924 from 50 per cent, see W. His, "Mitteilungen der Gesellschaft Deutscher Naturforscher und Ärtze," 2 (February 1925) 1-4, published with Naturwiss., 13 (1925).

23. Schröder-Gudehus, note 3 above.

24. "Editorial," Arch. Radiol. Electroth., 25 (1920-21) 321-25, at 321.

25. "Congress of Radiology and Physiotherapy, 14-16 April 1921," Arch. Radiol. Electroth., 25 (1920-21) 316. For mention of the postponement, see J. Rönt. Soc., 18 (1922) 55. For a report of the meeting finally held in 1922, see "Congress of Radiology and Physiotherapy," Brit. Med. J., 1 (17 June 1922) 958-60.

26. "The X Rays in Malignant Disease," Lancet, 2 (2 July 1921) 25 and for the BARP reply, "X-ray Treatment of Cancer," Med. Press, 2 (24 August 1921) 154-55. See also "X-Ray Treatment of Cancer--Erlangen Claims--Radiologists' Warning," The Times (22 August 1921).

27. A. Gunsett (Strasbourg), "Considérations sur les doses en radiothérapie profonde. Méthodes françaises--méthodes allemandes," J. Radiol. Electrol., 5 (1921) 543-51.

28. Béclère (Physician to the Hospital of Saint Antoine, Member of the Academy of Medicine), "Commuication on the Radiotherapy of Uterine Fibroids, With the Results of 400 Cases, personally observed, the Mode of Action of the Treatment and the Indications for its Adoption," Arch. Radiol. Electroth., 24 (1919-20) 254-61, translated from J. Radiol. Electrol., 3 (1918-19) 433-39.

29. Béclère (M. D., Academy of Medicine, Paris), "What is the Best Method for the Treatment of Uterine Fibromyomata by Means of the Röntgen Rays?" Amer. J. Rönt., 9 (1922) 797-802, read at the 23rd Annual Meeting of the American Röntgen Ray Society, Los Angeles, 12-16 September 1922, at 802.

30. W. Deane Butcher, "The Future of Electricity in Medicine," Proc. Roy. Soc. Med., Electrotherapeutical Section, 1 (1907-08) 1-14, delivered 25 October 1907.

31. Peyton Rous (M. D., N. Y.), "Transmission of a Malignant New Growth by Means of a Cell-Free Filtrate," J. Amer. Med. Ass. 56 (1911) 198, from the Laboratories of the Rockefeller Institute for Medical Research.

32. See, for example, Reginald A. Morrell (M. R. C. S. (Eng.), L. R. C. P. (Lond.), Honorary Radiologist to the Sheffield Royal Hospital, Hon. Radiologist and Medical Officer-in-Charge of the Electrical Department, the Chesterfield and North Derbyshire Royal Hospital), "The Present Position of Radio-Therapy in the Treatment of Malignant Disease. A Critical Note with Special Reference to the Erlangen Technique," Med. Press, 2 (30 August 1922) 177-79. See also F. Hernamen-Johnson, "X Rays in Malignant Disease," Lancet, 2 (16 July 1921) 153. For a discussion of both supression and excitation of immunity, see S. Russ (D. Sc.), Helen Chambers (M. D. Lond.), Gladwys Scott and J. C. Mottram (M. B. Lond.), "Experimental Studies with Small Doses of X Rays," Lancet, 196 (26 April 1919) 692-95, undertaken at the request of the Medical Research Council and funded by the Cancer Investigation Fund of Middlesex Hospital.

33. See the 17 July 1922 letter to the Daily Telegraph, in BARP Clippings File IV, note 18 above.

34. For the donation by W. H. Shaw to the Bradford Royal Infirmary, see the article from the 23 August 1921 Yorkshire Observer, in BARP Clippings File I, note 18 above. For the donation to BARP, see Arch. Radiol. Electroth., 26 (July 1921) 38-39.

35. "The Study of Deep X Ray Therapy," Lancet, 2 (31 December 1921) 1381.

36. The former was H. Kingsley Ward (M. C., M. B. Sydney, D. P. H. Oxford, Department of Pathology, University of Oxford), "Deep X Ray Treatment of Cancer: A Personal Impression of the Erlangen Frauenklinik," Lancet, 1 (25 February 1922) 366-68, at 68; the latter was J. Curtiss Webb, (Hon. Radiologist, Gloucester Royal Infirmary), "The Erlangen Technique in X Ray Therapy," Lancet, 2 (1 October 1921) 729-30, at 729. For favorable American reports, see James T. Case (M. D., F. A. C. S., from the Surgical Department of the Battle Creek Sanitarium), "Technical and Clinical Aspects of the New Deep Therapy," Amer. J. Rönt., 9 (1922) 530-37, read at the Midwinter Meeting of the Eastern Section of the American Röntgen Ray Society, Atlantic City, 28 January 1922; and W. H. Stewart (M. D., New York City), "The Present Status of Deep Röntgen Therapy in Europe," Amer. J. Rönt., 9 (1922) 315-18.

37. Iser Solomon (Chef du laboratoire de M. le D^{r} Béclère, à l'hôpital Saint-Antoine), "Ionomètre radiologique," J. Radiol. Electrol., 5 (1921) 509-12. Solomon, born in Rumania, was licencié ès sciences physiques as well as a physician, see the obtituary by Béclère in Paris Med., 47^{1} (1939) 498.

38. Robert Knox (M. D.), "Presidential Address," J. Rönt. Soc., 17 (1921) 5-22, at 19.

39. Ibid., at 20.

40. "The B. A. R. P.," Arch. Radiol. Electroth., 25 (1920-21) 30-32.

41. Of the six nonphysicians on the Council in 1920, four were physicists or engineers (B. Sc., D. Sc. or F. R. S. E.), see J. Rönt. Soc., 16 (1920) 91. For a list of the Presidents of the Röntgen Society, see the Handbook of the British Institute of Radiology, 3rd edition, 1966, at 42. The physicist Presidents after 1916 were G. W. C. Kaye (1917-18), Sidney Russ (1919-20), J. W. Nicholson (1921-22), Sir Oliver Lodge (1923-24) and F. W. Aston (1925-26).

42. J. W. Nicholson (M. A., D. Sc., F. R. S.), "Presidential Address," J. Rönt. Soc., 18 (1922) 5-14, at 10.

43. X-ray and Radium Protection Committee, "Memorandum No. 2," J. Rönt. Soc., 18 (1922) 3-4.

44. Nicholson, note 42 above, at 6.

45. "Society of Radiographers," J. Rönt. Soc., 16 (1920) 82-83, at 83. The Journal later softened its position on the Society of Radiographers, but it continued to favor amalgamation in principle.

46. See the notes of the Joint Committee of Rontgen (sic) Society and British Institute of Radiology in a box of Röntgen Society documents in the Library of the British Institute of Radiology referred to below as RS(BIR) and also "Amalgamation," Brit. J. Radiol., 23 (January 1927) 1-2. For a sample of the conflict between physicians and nonphysicians, see Francis Hernamen-Johnson (M. D., Radiologist to the French Hospital,

Physician to the X-ray Department, the Margaret Street Hospital for Consumption, etc.; late Consulting Radiologist, Aldershot Command), "The Place of the Radiologist and his Kindred in the World of Medicine," Arch. Radiol. Electroth., 24 (1919-20) 181-87 and C. F. Oddie (Radiographer to North Stafford Infirmary), letter to the editor, Arch. Radiol. Electroth., 25 (1920-21) 149-51. It should be noted that the Röntgen Society already had a provision in its rules that gave physicians veto power over nonphysician memberships and prohibited therapeutic work by nonphysicians, see J. Rönt. Soc., 14 (October 1918) 116: "No person engaged in the practice of medical or surgical radiography shall be eligible for membership unless he or she is proposed and seconded by a medical practitioner, who must have personal knowledge of the candidate, the final decision to rest with the Council. No person engaged in therapeutic work shall be eligible for membership unless duly qualified in medicine." These provisions were not, however, retroactive and several nonphysician diagnostic practitioners remained in the Röntgen Society.

47. The Mackenzie-Davidson Memorial Lecture and Medal, see J. Rönt. Soc., 19 (1923) 151.

48. Sir Humphrey Rolleston (Bart., K. C. B., M. D., Hon. D. Sc., D. C. L., LL. D., President of the British Institute of Radiology, Regius Professor of Physic in the University of Cambridge), "On the Effects of Radiations on Patients and Radiologists, and on Protection," Eighth Mackenzie-Davidson Memorial Lecture, Brit. J. Radiol. (Röntgen Society Section), 23 (1927) 266-91, with discussion.

49. For explicit criticism of the British recommendations on this score, see Walter Altschul (Doz. Dr., Prague), "Internationale Strahlenschutzbestimmungen," Strahlenth., 24 (1926-27) 766-68, Vortrag gehalten auf der V. wissenschaftlichen Tagung der Vereinigung Deutscher Röntgenologen und Radiologen in der tschecholslovakischen Republik in Prag am 23 und 24 Oktober 1926. For traces of general opposition to the Protection Committee, see the discussion following N. S. Finzi, "Research in Radiology," Brit. J. Radiol., 23 (January 1927) 4-18, Presidential Paper read 2 November 1926.

50. For the limitation to non-German radiological societies, see L. Jaches, "Sir Archibald Douglas Reid, K. B. E., C. M. G., D. M. R. E.," Amer. J. Rönt., 11 (1924) 288-89 and for a retrospective account see the report on the third International Congress of Radiology (Paris, 1931) in Brit. J. Radiol., 4 (May 1931) 365-68.

51. The minute book of the Provisional Committee, Organizing Committee and Grand Committee, in RS(BIR), note 46 above.

52. For the official British announcement, see "International Congress of Radiology," Acta. Radiol., 4 (20 March 1925) 81-82.

53. Some Germans may even have received invitations to the conference in late 1924, see "Internationaler Radiologenkongress 1925," Fortsch. Röntgenstr., 32 (1924) 725: "Seitens eines vorbereitenden Komitees englischer Röntgenologen ist an eine Reihe von Mitgliedern der Deutschen Röntgen-Gesellschaft die Aufforderung ergangen, sich an einem im Sommer 1925 stattfindenen internationalen Kongress zu beteiligen. Anlässich der Tagung der Deutschen Röntgen-Gesellschaft während der Naturforscherversammlung in Innsbruck wurde beschlossen, an dem Kongress teilzunehmen." There appears to be no report on this question in the proceedings of the Innsbruck meeting in Fortschr. Röntgenstr., 32 (2. Kongressheft 1924), but after printing the official announcement in "Internationaler Kongress für Radiologie, Vorbereitende Tagung, London, 1. bis 4. Juli 1925," Fortschr. Röntgenstr., 33 (1925) 333-34, Haenisch (Hamburg) reported, "Der Ausschuss der Deutschen Röntgen-Gesellschaft hat beschlossen, den Mitgliedern die Beteiligung an dem Kongress zu empfehlen."

54. Sidney Russ, "The Measurement of X-ray Intensity, and the Necessity for an International Method," in the report on the Joint Meeting with the Physical Society," 23 February 1923 in J. Rönt. Soc., 19 (1923) 163-171 at 166 and "Annual Report of the Council--Session 1922-1923," ibid., 191-94.

55. "Als nächtes Ziel ist dann die internationale Standardisierung der Dosismessung ins Auge zu fassen," at 94 in Behnken (Berlin), "Die Eichung von Dosismessern in absolutem Masse in der Physikalisch-technischen Reichsanstalt," Verh. Deut. Rönt. Ges., 15 (1924) 92-94.

56. See the report on the Congress, note 2 above.

57. "The First International Congress. Radiologists from Twenty-one Countries Meet in London," Brit. J. Radiol. (B. I. R. Section), 30 (August 1925) 284-94, at 92.

58. "Discussion on International Units and Standards for X-Ray Work," in the Proceedings of the Section of Physics, the First International Congress of Radiology (London, 30 June-4 July 1925, Central Hall, Westminster) in Brit. J. Radiol. (Röntgen Society Section), 23 (April 1927) 64-101.

59. A. Béclère (Membre de l'Académie de Medicine à Paris), "On International Standardisation of Measures in Röntgentherapy," ibid., pp. 66-72.

60. H. Behnken (Physikalisch-Technische-Reichanstalt, Charlottenburg), "The German Unit of X-Radiation," ibid., pp. 72-77.

61. L. Grebe and H. Martius (Bonn), "Röntgen-Ray Measurements in Absolute Units and Ray-Doses Necessary for Skin-Erythema," ibid., pp. 78-81.

62. Ibid.

63. Ibid., at 101.

64. Ibid., at 162.

65. Ibid., at 170.

66. Rolf M. Sievert, "Einige Untersuchungen über Vorrichtungen zum Schutz gegen Röntgenstrahlen," Acta Radiol., 4 (1925) 61-75 and I. Solomon, "Recherches sur la valeur des moyens de protection contre l'action à distance de rayons de röntgen," J. Radiol. Electroth., 8 (1924) 62-63, communication presentée à l'Acadmie de Médecine, le 16 octobre 1923.

67. This conclusion was already cited in Groedel, Liniger and Lossen (Frankfurt a. M.), "Schädigungen aus unserer Gutachtersammlung der Röntgenschäden," Fortschr. Röntgenstr., 32 (1. Kongressheft 1924) 160-63. The full survey was published in two parts, "Materialensammlung der Unfälle und Schäden in Röntgenbetrieben," Fortschr. Röntgenstr., Ergänzungsband 36 (1925) and Ergänzungsband 38 (1927). For a summary, see Heinz Lossen (Dr. med., Facharzt für die gesamte Röntgenkunde), "Über Ergebnisse unserer Materialiensammlung der Unfälle und Schäden in Reichsdeutschen Röntgenbetrieben (Groedel-Liniger und Lossen)," Acta Radiol., 8 (1927) 345-62, vorgetragen auf der XIV. ordentlichen Hauptversammlung der Schweizerischen Röntgen-Gesellschaft am 28. Mai 1927 in Luzern.

68. See H. Holthusen (Hamburg), "Über die Standardisierung der Röntgendosismessung," Referat III to the 17th Röntgenkongress, 11-13 April 1926, Verh. Deut. Rönt. Ges., 17 (1926) 156-57 and also the succeeding papers and discussion, pp. 158-74. G. Gabriel summed up the practitioners' view in replying to a paper by Fried (Worms) on the use of a Siemens ionization dosimeter: "Es liegen heute in den physikalischen Dosierungsmethoden noch so viel unbekannte Komponenten, dass wir für die Praxis durchaus an den alten Dosierungsmethoden festhalten müssen. Wenn Herr Fried in seinem Vortrage die HED [Hauteinheitdosis] feierlichst zu Grabe getragen hat, so wollen wir sie schleunigst von ihrem Scheintode erwecken, da wir sie als Grundlage für unser weiteres Arbeiten notwendig brauchen."

69. L. Grebe (Röntgen-Forschungs und Unterrichtsinstitut der Universität Bonn) and H. Martius (Universitäts-Frauenklinik in Bonn), "Vergleichende Messungen über der Grösse der zur Erreichung der Hauterythems gebräuchlichen Röntgenstrahlenmenge," Strahlenth., 18 (1924) 395-409.

70. Otto Glasser, "Erythemdosen in Röntgeneinheiten," Strahlenth., 20 (1925) 141-43.

71. Otto Glasser (Ph. D., Cleveland Clinic Foundation) and U. V. Portmann (M. D., Cleveland Clinic Foundation), "The Standardization of the Röntgen-Ray Dose," Amer. J. Rönt., 19 (1928) 47-61, read at the 28th Annual Meeting of the American Röntgen Ray Society, Montreal, Canada, 20-23 September 1927.

72. A. Béclère, "La discordance des mesures pour l'évaluation de l'unité de dose radiotherapique en Allemagne et aux Etats-Unis," J. Radiol. Electrol., 11^2 (1927) 535-39, at 539: "Entre les physiciens de l'Allemagne et des États-Unis, il est nécessaire qu'intervienne un arbitre impartial. Pour terminer par un voeu, je souhaite qu'un physicien français ait cette ambition et que l'honneur lui advienne de la justifier [sic]."

73. Iser Solomon, "Sur la necessité de la standardisation des chambres d'ionisation utilisées en dosimetrie radiologique," J. Radiol. Electrol., 11^1 (1927) 286-90.

74. Glasser and Portmann, note 71 above.

75. Hermann Behnken, "Die Absolutbestimmung der Dosiseinheit '1' Röntgen in der Physikalisch-Rechnischen-Reichanstalt," Strahlenth., 26 (1927) 78-100.

76. Glasser and Portmann, note 71 above, in a footnote added after the reading of the paper, at 54.

77. "A Report of the Second International Congress of Radiology (Stockholm, 23-27 July 1928) and the Proceedings of the Joint Scientific Meetings of the Congress," Acta Radiol., Supplementum III, Pars I (1929) at 60.

78. Brit. J. Radiol., 4 (1931), at 484.

79. "General Recommendations of the National Laboratories for the Standardisation of the X-Ray Dosemeters," Brit. J. Radiol., 7 (1934) 304-308. These recommendations included the use of a large standardization chamber of the sort Glasser had advocated rather than the Behnken air pressure chamber.

80. "X-ray and Radium Protection Committee. Preliminary Report," J. Rönt. Soc., 17 (1921) 100-103.

81. A. Mutscheller, "Physical Standards of Protection Against Röntgen-Ray Dangers," Amer. J. Rönt., 13 (1925) 65-70, at 67.

82. _Ibid._

83. _Ibid._

84. R. Glocker (Prof. Dr., Suttgart), "Internationale Strahlenschutzbestimmungen," _Strahlenth._, _22_ (1926) 193-204, Referat IV erstattet auf dem Röntgenkongress 1926, at 204: "Um eine exakte Grundlage für die Ausarbeitung von Strahlenschutzbestimmungen zu gewinnen, ist es unerlässlich, dass durch möglichst zahlreiche statistische Beiträge aus dem Kreise der Mitglieder der Deutschen Röntgengesellschaft ein genauerer Wert für die Toleranzdosis gewonnen wird. Eine Mitarbeit an dieser Aufgabe liegt also im eigensten Interesse jede Röntgenlogen!" See also R. Glocker and E. Kaupp (aus dem Röntgenlaboratorium an der Technischen Hochschule Suttgart), "Über den Strahlenschutz und die Toleranzdosis," _Strahlenth._, _20_ (1925) 144-52.

85. Behnken, in the discussion following the oral presentation of Glocker, _ibid._, as reproduced in _Verh. Deut. Rönt. Ges._, _17_ (1926) 177-87, at 183; Sievert, note 66 above.

86. A. E. Barclay (M. D.) and Sydney Cox (B. Sc.) (Manchester) "The Radiation Risks of the Röntgenologist: An Attempt to Measure the Quantity of Röntgen Rays Used in Diagnosis and to Assess the Dangers," _Amer. J. Rönt._, _19_ (1928) 551-61.

87. G. W. C. Kaye (O. B. E., M. A., D. Sc., Superintendant of the Physics Department, the National Physical Laboratory), "Protection and Working Conditions in X-Ray Departments," _Brit. J. Radiol._, _1_ (1928) 295-312, read 18 November 1927 and revised in proof August 1928.

88. _Ibid._

89. A. Mutscheller (Ph. D., New York), "Safety Standards and Protection Against X-Ray Dangers," _Radiology_, _10_ (1928) 468-76, with discussion.

90. G. Holzknecht (Professor für Röntgenkunde an der Wiener Universität), "Zur Frage gesetzlicher Sicherheitsbestimmungen für die Anwendung der Röntgenstrahlen," Wien. Klin. Wschr., 41^{1} (1928) 202-205, which is a 1926 or 1927 report to the medizinische Prüf- und Beratungsstelle (Vorsitzender Prof. Durig) am Technischen Versuchsamt in Wien (Leiter Präsident Ing. Dr. Exner). The report was brought before the Bundesministerium für soziale Verwaltung in November 1927.

91. See Glocker, note 81 above, for the proposal to revise the 1913 Instruction Sheet and for the finished product see "Merkblatt der D. R.-G. über den Gebrauch von Schutzmassnahmen gegen Röntgenstrahlen vom Jahre 1926," Fortschr. Röntgenstr., 34 (Mai 1926) 848.

92. "Gesetzliche Bestimmungen zur Ausübung des Röntgenverfahrens im Auslande," Referat aus dem Sonderausschuss für die Beurteilung von Röntgenschädigungen und zum Studium ihrer Verhütung, 27. April 1927 (Referent: Herr Levy-Dorn, Berlin), Fortschr. Röntgenstr., 36^{1} (1927) 410-11.

93. Jaulin (Orléans), "Rapports sur les dangers des rayons X et des substances radioaktives pour les professionels--moyens de s'en préserver," J. Radiol. Electrol., 11^{1} (avril 1927) 193-98 and Bouchacourt and Morel-Kahn (les docteurs), "De quelques points fondamentaux, concernant la protection des personnes utilisant les R. X," Bull. Soc. Radiol. Med. (Paris), 16 (1928) 59-65.

94. I have unfortunately been unable to find the recommendations of the Swedish committee, but the committee is referred to in note 92 above and it submitted "Proposals from the Swedish X-rays and Radium Protection Committee" to the second International Congress of Radiology (Stockholm, 1928), see the reference in Hermann Wintz (M. D., Ph. D., Director of the University Gynecological Clinic and Röntgen Institute, Erlangen) and Walther Rump (Privatdozent, Ph. D.), Protective Measures Against Dangers Resulting from the Use of Radium, Röntgen and Ultraviolet Rays, prepared for the Health Organization of the League of Nations (Geneva: League of Nations, III. HEALTH. 1931. III. 9) at 73.

95. Note 77 above.

96. "Verfügung des 'Volkskommissariats der Arbeit' der Räterepublik vom 9. September 1925, Nr. 233/389 betreffs des Arbeitsschutzes der in Röntgenkabinetten tätigen Arbeiter," Fortschr. Röntgenstr., 35 (1926-27) 781-83.

97. Behnken, note 85 above, at 183: "Durch die am Anfang vorkommende 'höhere' Mathematik braucht sich wohl kaum ein Röntgenloge gruselig machen zu lassen, zumal da ja der Praktiker nur die dem Durchschnittbedürfnis entsprechend gekürzten Tabellen zu benutzen braucht." The reply was by the physicist Grossmann (Berlin), <u>ibid.</u>, who may have been more sensitive to the practitioner's requirements because he worked in the X-ray industry: "Auch müssen die Vorschriften--worauf ich besonders hinweisen möchte--populär gefasst sein, so dass sie von jedermann verstanden werden können."

98. "Recommendations of the X-ray and Radium Protection Committee," Third Revised Report, May 1927, <u>Brit. J. Radiol.</u> (Archives of Radiology and Electrotherapy), <u>32</u> (1927) 330-36.

99. For the adoption of the international protection recommendations, see note 77 above, pp. 62-5.

100. Note 98 above. The original members of the International X-ray and Radium Protection Commission were the Chairman Rolf Sievert, the Swedish physicist; G. W. C. Kaye, the British physicist; Stanley Melville, a British physician; Guilio Ceresole, an Italian physician; Gustav Grossmann, a German physicist who worked for an X-ray tube manufacturer; Iser Solomon, the French physicist and physician; and Lauriston Taylor, the American Bureau of Standards physicist. Sievert later gave an inaccurate description of the membership and also said Kaye was Chairman, see "The International Commission on Radiological Protection," <u>Inter. Ass.</u>, <u>9</u> (1957) 589-93.

101. Wintz and Rump, note 94 above.

102. Lauriston S. Taylor, "Brief History of the National Committee on Radiation Protection and Measurements (NCRP) Covering the Period 1929-1946," <u>Health Phys.</u>, <u>1</u> (1958) 3-10.

103. For a brief history of the <u>Normenstelle</u> and its affiliation with the <u>Deutsche Normenausschuss</u>, see Herbert Graf, "Die Entwicklung der radiologischen Normung in Deutschland," <u>DIN-Mitt.</u>, <u>54</u> (1975) 531-35.

104. For the changes introduced in 1931, see "Recommendations of the International X-ray and Radium Protection Commission," <u>Brit. J. Radiol.</u>, <u>4</u> (1931) 485-87, and for the recommendations as they stood after these changes were made see "International

Recommendations for X-ray and Radium Protection, Revised by the International X-ray and Radium Protection Commission and Adopted by the Third International Congress of Radiology, Paris, July 1931," *Brit. J. Radiol.*, *5* (1932) 82-85.

105. Wintz and Rump, note 94 above, pp. 19-21.

106. J. H. van der Tuuk and W. Hondius Boldingh (Natuurkundig Laboratorium der N. V. Philips' Gloeilampenfabrieken), "Die Bleischutzdicken in den deutschen Strahlenschutzvorschriften," *Fortschr., Röntgenstr.*, 41^{2} (1930) 965-67 and the reply by R. Glocker, "Zur Frage der 'Bleischutzdicken' in den internationalen und in den deutschen Strahlenschutzvorschriften," *ibid.*, 967-71.

107. "A Discussion on the International Protection Recommendations," 19 November 1931 and 14 January 1932, *Brit. J. Radiol.*, *5* (1932) 215-33, especially the comments of G. E. Bell, W. Binks and the President, A. E. Barclay.

108. Wintz and Rump, note 94 above.

109. "International Recommendations for X-ray and Radium Protection Revised by the International X-ray and Radium Protection Commission at the Fourth International Congress of Radiology, Zürich, July 1934," *Brit. J. Radiol.*, *7* (1934) 695-99, at 695.

110. *Ibid.*

111. Theodore Blum in a footnote to "Osteomyelitis of the Mandible and Maxilla," an address to the American Dental Association, September 1924 was apparently the first report, see Frederick L. Hoffmann (Newark, N. J.0, "Radium (Mesothorium) Necrosis," *J. Amer. Med. Ass.*, 85^{1} (1925) 961-65, read before the Section on Preventive and Industrial Medicine and Public Health at the 76th Annual Session of the American Medical Association, Atlantic City, May 1925.

112. Hoffmann, *ibid.*, credited the New Jersey Consumer League with bringing the case to his attention, and he was the first to report in the medical literature on the dial painters. For one of many newspaper reports, see "Radium and Gas as Death Cause Open New Issue," New York *Times*, 19 May 1925, at 14.

Hoffmann noted that the New Jersey Bureau of Labor had investigated the situation but found nothing, investigators from the Harvard Medical School did not publish a report, and the U. S. Public Health Service considered investigating but did not. Two local physicians and a dentist, however, had been working on the radium dial painters, and Hoffman's publication precipitated the early publication of their more detailed report, see Harrison S. Martland (M. D.), Philip Conlon (M. D.) and Joseph P. Knef (D. D. S.), "Some Unrecognized Dangers in the Use and Handling of Radioactive Substances: With a Special Reference to the Storage of Insoluble Products of Radium and Mesothorium in the Reticulo-endothelial System," J. Amer. Med. Ass., 85 (5 December 1925) 1769-76, from the Medical Service of St. Mary's Hospital, Orange, N. J.; the Pathologic Department of the City Hospital, Newark; and the office of the County Physician of Essex County, N. J. I plan to undertake in cooperation with others a detailed medical, legal and historical investigation of the radium dial painters incident.

113. For reference to the out of court settlement, see Maurice De Laet (Agrégé à l'Université de Bruxelles), "La pathologie professionelle due aux corps radioactifs," Ann. Med. Leg., 8 (1928) 443-52 and also Harrison S. Martland (M. D., the Department of Pathology of the Newark City Hospital and the Office of the Chief Medical Examiner of Essex County, Newark, N. J.), "The Occurence of Malignancies in Radioactive Persons. A General Review of Data Gathered in the Study of the Radium Dial Painters, With Special Reference to the Occurrence of Osteogenic Sarcoma and the Interrelationship of Certain Blood Diseases," Amer. J. Cancer, 15 (1931) 2435-2516. Osteogenic sarcoma was first reported in H. S. Martland and R. E. Humphries, "Osteogenic Sarcoma in Dial Painters Using Luminous Paint," Arch. Pathol., 7 (1929) 406-17.

114. Thiele, Rostoski, Saupe and Schmorl, "Ueber den Schneeberger Lungenkrebs," Munchen Med. Wschr., 71[1] (1924) 24-5, Sitzung vom 8 Oktober 1923 of the Gesellschaft für Natur und Heilkunde in Dresden. This work was sponsored by the Sächsischen Landesausschusses zur Erforschung und Bekämpfung der Krebskrankheit.

115. Aug. Pirshan (Head Physician, State Radium Institute, Jáchymov) and H. Sikl (Extraordinary Professor of Pathology at the Czech University, Prague), "Cancer of the Lung in the Miners of Jáchymov (Joachimstal). Report of Cases Observed 1929-30," Amer. J. Cancer (1932) 681-722.

Chapter 7: Epilogue: Mutation and Politics, 1927-35

With the formal adoption of the tolerance dose into the recommendations of the International Commission for X-ray and Radium Protection in 1934, we have reached the terminal date for the present study, but I have barely begun to touch on radiation protection as it is known and discussed today. For most of the forty odd years since 1934, radiation protection in medicine has been over-shadowed by protection from nonmedical sources of radiation, especially nuclear weapons and nuclear reactors. These more recent concerns have brought radiation protection into the political arena. Public involvement and even governmental intervention were known before 1935, but only after World War II did radiation protection become a global political issue, debated in electoral campaigns and among the representatives of the major powers. Central to this political debate was the production of genetic mutation by radiation, an effect I have barely mentioned. The later, highly political period in the history of radiation protection will be the subject of a second volume. All I can hope to do in this brief epilogue, which will be expanded to a full chapter in the published version of this study, is to raise some questions about the discovery of radiation-induced mutation, about the reception of this discovery before World War II, and about the links between the earlier period of radiation protection that I have discussed above and the later period that I hope to discuss in the future.

Credit for the discovery of radiation-induced mutation is generally given to the American geneticist H. J. Muller, who in 1946 won the Nobel Prize for this research. In 1927, when he offered the decisive evidence that exposure to X-rays caused mutations in the fruit fly Drosophila melanogaster, Muller was a professor at the University of Texas in Austin.[1] Born in Brooklyn, he had been as a student a member of the "Drosophila group" at Columbia University headed by T. H. Morgan. Muller would later leave Texas for Europe, becoming Senior Geneticist at the Soviet Academy of Sciences in Moscow from 1933 to 1937. The association with Morgan and Muller's leftist political views pose a central problem in discussing his discovery of radiation-induced mutation, for Muller himself offered two strikingly different versions of the early intellectual influences to which he was subjected. In an address at Cold Spring Harbor in 1921, Muller emphasized the central importance of Morgan to the Columbia group.[2] In 1934, in an article published in a Soviet tribute to Lenin, Muller denied Morgan's influence and averred that there had been among the Drosophila group at Columbia "a strong direct Marxian influence."[3]

There is a temptation to dismiss the 1934 assertion as left-wing cant, perhaps forced on Muller by the Russians or perhaps produced by his own unfortunate over-enthusiasm for socialism. The 1921 version of what happened at Columbia is more consistent with other accounts, and also more acceptable to current historiography in attributing the primary influence to a figure within science. A second look, however, sheds doubt on this preference for the 1921 version. Muller was often at odds with Morgan over personal and scientific questions.[4] Muller's leftist views had been adopted by the time he worked at Columbia, and political differences might account for the mutual intransigence shown in the disputes between Muller and Morgan. At Cold Spring Harbor, there would have been as much reason for Muller to hide the influence of his leftist political views as there was reason to flaunt that influence in Moscow. Charles Davenport, a right-wing geneticist and eugenicist, headed the Biological Laboratory at Cold Spring Harbor in 1921. Muller was probably delighted with the occasional invitations he received to spend summers at the Laboratory in the 1920s since he intensely disliked Texas for personal, professional and political reasons.[5] To discuss leftist influences before an audience at Cold Spring Harbor in the early 1920s would have been foolhardy, and it would be understandable if Muller omitted this political connection.

Interest in the possibility of a political influence in Muller's scientific work intensifies when we realize that certain leftist influences current around 1910, though not Marxist, could have pointed Muller toward his experiments with radiation. Muller was not, as we have noted above, the first to attempt to produce genetic changes with X-rays or radium. Among those who had tried as early as 1911 was Jacques Loeb, a German biologist who had emigrated to the United States in the 1890s and who had become a close friend of Morgan.[6] Loeb is most commonly remembered for his discovery of artificial parthenogenesis, a procedure in which embryological development of an egg is initiated by treatment with a salt solution or even by the prick of a pin rather than by fertilization. This experimental work was linked to theoretical preoccupations. Loeb was heir to a reductionist tradition in biology and medicine. He believed that all of life was reducible to physical and chemical laws and emphasized that this assertion was a first principle rather than a limited methodological assumption. Like his predecessors, whom Fleming calls the "medical materialists," Loeb's reductionism was combined with leftist political views, the common root of both being an uncompromising materialism.[7] By the time of his death in 1924, Loeb was part of the American socialist scene associated with Thorstein Veblen, H. L. Mencken and Sinclair Lewis.

In promoting reduction, the leftist medical materialists often failed to distinguish between an explanation of biological phenomena in terms of physiochemical laws and a biological effect brought about by physical and chemical means. Loeb's best known work, The Mechanistic Conception of Life can, without much distortion, be described as variations on this confusion. Loeb offered, as evidence for the validity of reductionism, phenomena like parthenogenesis and phototropism for which he had no explanation in physical and chemical terms, but in which physical agents brought about uniquely biological events. Radiation-induced mutation would have been a valuable addition to the armamentarium of the medical materialists.

Could Loeb's materialism have influenced Muller, and could this influence be the one he identified later, inaccurately, as "Marxian"?[8]

Muller's initial efforts in the late 1910s and early 1920s to induce mutation failed to provide conclusive evidence. Muller's success in 1927 was followed quickly by the success of L. J. Stadler, who working independently produced radiation-induced mutations in corn.[9] Plant genetics and Drosophila genetics were related but distinct fields. Was the simultaneous demonstration of radiation-induced mutation accidental, or were there features in common between Muller's work and Stadler's?

Radiation-induced mutation quickly attracted reductionist efforts to explain mutation in terms of physical and chemical events like ionization and chemical reactions. These efforts succeeded in 1935, when three Göttingen scientists applied Dessauer's "point-heat" theory to experimental studies of radiation-induced mutation. N. W. Timoféeff-Ressovsky (a geneticist), K. G. Zimmer (a radiation biologist), and M. Delbrück (a physicist) demonstrated that mutation was a single-hit process, in which only a single ionization event was required to produce the observed linear dose-effect relationship.[10] This work, though no longer considered valid in the form in which it was presented, played a central role in twentieth-century biology, inspiring Erwin Schrödinger's popular reductionist presentation "What is Life?" and leading thereby to James Watson's overweening faith in the molecular character of the gene.[11] For our purposes, however, the most important feature of this Dreimännerwerk was that it directly contradicted the assumption of a tolerance dose. If the "point-heat" theory was correct and mutation was a single-hit process, there was no threshold even at very low doses. The tolerance dose would not provide the absolute protection that it appeared to promise.

This possibility raised two questions: should mutation be considered in radiation protection, and did the linear relationship between dose and mutation found in the laboratory exist as well in the real world? Both of these questions entailed, and to some degree continue to entail, profound difficulties for the radiological community and for the public. No one has ever demonstrated radiation-induced mutation outside the laboratory, and even inside the laboratory the demonstration for low doses, though apparently straightforward, involves numbers of test animals so large that the experiment lies beyond imaginable capabilities. The demonstration of radiation-induced mutation at low doses thus lies in the sphere of what Alvin Weinberg has called "transscience," the sphere of questions that can reasonably be asked of science but which science cannot answer.[12] Here, as in Chapter 2, we see the gap between the laboratory and the clinic opening wide. Experiments can show, in the artificial world of controlled experiments with fruit flies, that mutation is apparently a one-hit process for which there is no threshold. In practice, however, we do not know the relevance of these experiments for mutation in human beings, and the surveys undertaken beginning in the late 1920s concerning genetic damage in human beings exposed to radiation have not shown signficiant results.[13]

Given this uncertainty, it is not surprising that the views of both scientists and nonscientists on the importance of genetic effects has been very varied, often depending more on cultural and political values than on verifiable evidence. As early as 1933, a Joint Committee on the Question of Genetic Damage of the German Society for the Science of Heredity and of the German Röntgen Society, although recognizing that in individual cases it could never be proven whether radiation had caused genetic damage, urged the greatest caution in medical irradiation because of the possibility of damage to the "germinal heritage of our nation."[14] The Nationalist Socialist emphasis on genetic purity and eugenic progress had already had an impact in professional circles. Outside Germany, genetic risks seem to have been largely ignored, despite Muller's pleas, before World War II. The tolerance dose continued in use, and the laboratory experiments showing a linear dose-effect relationship did not enter into the consideration of the protection committees, national or international. Knowing little about genetics, many physicians appear to have regarded genetic damage as a scientific invention: unworthy of the clinician's attention and a threat to the practice of the profession. The public, though occasionally made aware of the fact of genetic risks, did not react strongly to this apparently distant threat.

After World War II, the situation changed rapidly and genetic effects took on major significance. Scientific understanding of genetic effects did not undergo a major transformation, but the political situation did. There was no better evidence for radiation-induced mutation than before, but Muller's pleas were not ignored. In part, this change had to do with the atomic bomb, which raised radiation protection to a level of public interest that it had not known before. The distant threat of genetic damage to future generations became strongly associated in the public eye with the overwhelming destructive capacities demonstrated at Hiroshima and Nagasaki. The change in the signficance accorded genetic effects was especially dramatic in the United States, where domestic politics may have been an important contributing factor. The domestic component of the Cold War, known in the United States as "the McCarthy period," led to a post-War purge of Communists and Communist sympathizers and the harassment of more moderate leftists. These widespread efforts cut off a well-educated and articulate group of Americans from political expression. Disillusioned with the Soviet Union and led by scientists like Muller and Linus Pauling, some of these leftists found an outlet for their political views in antibomb campaigns that strongly emphasized radiation effects, especially the genetic risks from low levels of exposure due to atmospheric weapons testing. If this view is correct, mutation had come full circle, returning to the American leftist tradition from which I have suggested Muller's experiments arose.

With the atomic bomb also came a degree of governmental and industrial involvement in radiation protection and in radiation research that had not been known previously. Both the American and the German bomb projects gave considerable emphasis to biological and medical research. How did this concern come about, and what was its lasting impact on radiation protection and on radiation biology and physics? There is a natural tendency to assume that governmental involvement has been critical, and as a result to indict governmental bureaucracy for today's problems in controlling radiation risks and other modern technological threats. Similarly, there is a tendency to see the scale of modern industry as an important factor in conflicts over radiation protection.[15] To be sure, the risks and benefits associated with radiation have grown enormously since the advent of nuclear weapons and nuclear power. It may, however, be that power utilities and military organizations react today to radiation risks and to organized public pressure in ways that are similar to the ways in which their analogs before World War II, the promoters of X-rays and radium in medicine, reacted to more spontaneous public concern. Radiation protection may continue to depend on the clash of lay fears and professional efforts to allay those fears.

Government regulatory authorities, from this perspective, would be buffers between conflicting interests, and as a result it would be a mistake to read the constitutional mandate of regulatory bodies too literally. The capacity of government on its own to control technological risks may have been vastly over-valued by a public auxious for more protection from radiation and promoters anxious for more protection from the public. Effective regulation even today appears to rely heavily on professional organizations, trade associations, insurance companies, citizen groups, the courts and the news media. Rather than regarding this situation as a temporary and unfortunate state of confusion, as is often done in evolving suggestions for reform, we can begin to regard it as a regulatory system and study its mode of operation for the light it may shed on how society can come to terms with other technological risks. In such a study, the pre-World War II history of radiation protection can help to focus attention on those nongovernmental institutions that may continue to play an important role.

The relevance of pre-World War II experience to post-war events becomes more striking when we recognize that today's international institutions for radiation protection rely heavily on a direct descendant of the International Commission on X-ray and Radium Protection created in 1928. The present-day body is called the International Commission on Radiological Protection (ICRP). A nongovernmental body that is still constitutionally a creature of the International Congress of Radiology, the ICRP is the source of basic protection recommendations that are widely recognized as authoritative. Based in part on the assessments of radiation risks, levels, pathways and sources prepared by the United Nations Scientific Committee on the Effects of Atomic Radiation, the ICRP recommendations are used by both international organizations and national governments in formulating more binding legal provisions. There is disagreement with the recommendations of the ICRP, which now include a limitation on the genetically significant dose to the general population, and in recent years doubts have been expressed both about its right to do what it does and its ability to continue.

The present fact remains, however, that the ICRP and the system of which it is a part is a striking example of how mid-twentieth century society has come to terms with risks posed by science-based technology. How this system operates and why it is effective are key questions for future analysis.

1. H. J. Muller, "The Problem of Genic Modification," Verhandlungen des V. Internationalen Kongresses für Vererbunswissenschaft (Berlin, 1927) in Z. Induk. Abst. Vererb., Supplement Band 1 (1928) 234-60. This article is reprinted in H. J. Muller, Studies in Genetics: the Selected Papers (Bloomington, Indiana: Indiana University Press, 1962). For a more popular version, see his "Artificial Transmutation of the Gene," Science, 66 (1928) 84-87.

2. H. J. Muller, "An Episode in Science," a thirty page typed manuscript with hand-written corrections and the notation "Title and subject chosen by Dr. C. B. Davenport. This lecture was given at the Biological Laboratory of the Brooklyn Institute, Cold Spring Harbor, L. I., the evening of July 25, 1921." This manuscript is on deposit at the Library of the American Philosophical Society (Philadelphia). In a "List of Works by H. J. Muller" kindly provided by Saundra Taylor, Curator of Manuscripts at the Indiana University Libraries, there is a manuscript entitled "A Decade of Drosophila," read at Carnegie Inst., Cold Spr. Harb., Aug. 1921 and published in Russian in 1922. I have not yet consulted these texts.

3. Hermann J. Muller, "Lenin's Doctrines in Relation to Genetics," as reprinted in Appendix II of Loren R. Graham, Science and Philosophy in the Soviet Union (New York: Alfred A. Knopf, 1972), pp. 453-69, at 462. Muller went on to avoid naming names, saying "this is not the place to go into personal details," but it is clear from his description of the influences within the Drosophila group that he meant to be including himself among the Marxists.

4. Elof Axel Carlson, "The Drosophila Group," Genetics, 79 (1975) 15-27 and "The Drosophila Group: the Transition from the Mendelian Unit to the Individual Gene," J. Hist. Biol., 7 (1974) 31-48. Carlson has mentioned the possible relevance of Muller's political views to the disputes with Morgan, but only in passing, see Elof Axel Carson, "An Unacknowledged Founding of Molecular Biology: H. J. Muller's Contributions to Gene Theory, 1910-1936," J. Hist. Biol., 4 (1971) 149-70, at 158.

5. Muller later wrote to L. C. Dunn, à propos of Texas, in a letter of 27 March 1928 (Dunn Collection, American Philosophical Society): "(a) they fired my wife for having a child (before it came and before they knew it was coming--except that she notified them) and won't give her her job back, tho she has a Ph. D. in math. from Illinois, had taught here 4 years and they acknowledged her 'loyal and efficient service,' and intended

to go on indefinitely. (b.) one can't do fly work here in the summer, (c.) there is a dearth of suitable graduate material, even my colleagues are willing to take the work out of my hands for themselves before I can get around to it. (d) there are other objections, connected with it's being in the South." Muller was a staunch anti-racist, and a good part of this letter is concerned with efforts to help a black geneticist who could find neither a suitable post nor research support.

6. J. Loeb and F. W. Bancroft, "Some Experiments on the Production of Mutants in Drosophila," Science, 33 (1911) 781-83.

7. Donald Fleming in the "Introduction" to Jacques Loeb, The Mechanistic Conception of Life (Cambridge, Massachusetts: Harvard University Press 1964), originally published in 1912.

8. Elof Axel Carlson, in a private communication, has kindly informed me that Muller did not correspond with Loeb but that he admired Loeb's early materialist writings, that he had a photograph of Loeb in his office at Indiana University after World War II, and that he mentioned Loeb's influence in a biographical sketch for the National Academy of Sciences (on deposit at the Lilly Library of the University of Indiana).

9. L. J. Stadler, "Genetic Effects of X-rays in Maize," Proc. Nat. Acad. Sci., 14 (1928) 69-75.

10. N. W. Timoféeff-Ressovsky, R. G. Zimmer and M. Delbrück, "Über die Natur der Genmutation und der Genstruktur," Nach. Ges. Wiss. (Göttingen) (1935) 189-245.

11. Erwin Schrödinger, "What is Life? The Physical Aspect of the Living Cell," reprinted with "Mind and Matter," (Cambridge: University Press, 1969). For the connection with Watson, see Donald Fleming, "Emigré Physicists and the Biological Revolution," Pers. Amer. Hist., 2 (1968) 152-89. If the connection between Loeb and Muller suggested above is correct, and if Fleming's analysis of both the medical materialists and Watson is accepted, then the connections between what Paul Cranefield calls "The Organic Physics of 1847 and the Biophysics of Today," J. Hist. Med., 12 (1957) 407-23 are stronger than has been supposed. I hope in the near future to discuss these connections,

which in serial form can be stated as follows: du Bois-Raymond, Helmholtz and Brücke; the leftist medical materialists; Loeb; Muller; Timoféef-Ressovsky, Zimmer and Delbrück; Schrödinger; Watson.

12. Alvin M. Weinberg, "Science and Transscience," in the Ciba Foundation Symposium volume Civilization and Science: in Conflict or Collaboration? (Amsterdam: Associated Scientific Publishers, 1972), pp. 105-22, with discussion.

13. See, for example, L. Loeffler (jetzt I. Assistent des Anthropologischen Instituts der Universität Kiel), "Röntgenschädigungen der männlichen Keimzelle und Nachkommenschaft. Ergebnisse einer Umfrage bei Röntgenärzten und -technikern," Strahlenth., 34 (1929) 735-66, aus dem Kaiser-Wilhelm-Institut für Anthropologie, menschliche Erblehre und Eugenik (Direktor: Prof. E. Fischer), Abteilung für menschliche Erblehre (Leiter: Priv.-Doz. Dr. Frhr. v. Erschuer); and P. M. Hickey (M. D.) and E. W. Hall (M. D.) (Ann Arbor, Michigan), "A Report Analyzing the Results of the Questionnaire Sent Out to Radiologists under the direction of the Sex Committee of the National Research Council," Amer. J. Rönt., 18 (1927) 458-62.

14. "...die Gefährdung des Keimgutes unseres Volkes," in "Zur Erbschädigungsfrage," Verh. Deut. Rönt. Ges., 26 (1933) 111.

15. For an example of this kind of analysis applied to the nuclear industry, see the case study by Dorothy Nelkin, Nuclear Power and its Critics: the Cayuga Lake Controversy (Ithaca: Cornell University Press, 1971).