Robert Koch
A Life
in Medicine
and Bacteriology
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Ten years have passed since Dr. Thomas Brock gave us this first major English-language biography of Robert Koch in 1988. Written by a distinguished microbiologist rather than a professional historian or biographer, the book has nonetheless stood the test of time. Indeed, though Brock did not work with unpublished archival sources or dissertations, his command of the published sources is extremely impressive, especially his use of the German-language biographies of Koch that are otherwise inaccessible to most English-speaking readers. Thus, although Brock did not make use of archival materials (many of which, in East Berlin, may have become available to scholars only after the fall of the Berlin Wall), his analysis in many ways opens up or foreshadows important themes that historians have since developed in more depth. His bibliography and notes are very well arranged so that this biography is useful to the average reader and an essential starting point for any historians studying Koch from this point on.

In his lifetime, Robert Koch did more to singlehandedly advance the world's understanding of microbes as causes of disease than any other man, with the exception only of his great French rival Louis Pasteur. Koch and his students created almost from scratch the majority of the techniques necessary for any modern study of bacteria, including microphotography of the organisms, staining procedures, and solid culture media, which allowed reproducible pure cultures and quantification of bacterial numbers for the first time. They also identified the microorganisms that cause anthrax, wound infections, tuberculosis, diphtheria, cholera, and many other major infectious diseases. Koch had the talents of a first-rate researcher: he was a keen observer, an ingenious technical innovator, and extremely persistent and single-minded in pursuit of his goal. Yet, being trained from the outset of his career as a practicing medical doctor, Koch never lost sight of the prac-
tical benefit to human health that was implicit in his work, for instance, devoting much time to the study of effective water filtration systems after the 1892 Hamburg cholera epidemic. This is not a mere academic point; Brock reminds us (p. 3) that “water filtration has probably saved more lives than immunization and chemotherapy combined.” Brock is thorough, and his deep admiration for Koch is obvious, as he documents in detail all of Koch’s important contributions, without which it is difficult to imagine the existence of bacteriology as we know it.

Yet this book stands out from a great many hagiographic biographies of the past. With figures of the stature of Koch or Pasteur, one rarely finds a study that can document the awesome contributions of the scientist without setting him up as a hero figure, a giant with no human qualities. Brock, more in tune with the needs of our own time, sees that Koch’s human qualities make an equally fascinating part of the story. An important thread in this narrative of Koch’s career is to show how an “eager amateur” country doctor, experimenting on microbes in his spare time, went on to become “an imperious and authoritarian father figure whose influence on bacteriology and medicine was so strong as to be downright dangerous.” And the point, Brock emphasizes, is not just to understand Koch’s personality, but to better understand science itself, including “the origins of the cult of personality in research” (p. 4). It might be added that, for a new generation of scientists in training, it is much more valuable as a model for study to have a real, complex human being, including his mistakes and excesses. By comparison, a too-perfect hero figure will always leave most of us doubting that we can measure up, especially in the face of the messy enterprise that laboratory science can be on a day-to-day basis.

I shall not attempt an exhaustive summary of the past decade’s historical work on Koch, but I shall briefly indicate some of the most important works and what contribution each has made. For the reader who wants more detail, the bibliographies of those works will provide a more than adequate guide to the literature. Subjects that have received excellent and fascinating treatment include the rivalry between the Koch and Pasteur schools (and the two men personally), the pleomorphism-monomorphism debate, the greater complexity of Koch’s postulates than at first meets the eye, the larger political and cultural resonances of the bacteriological revolution (especially in Germany), the tuberculin discovery and its relation to the creation of Koch’s Institute for Infectious Diseases, and finally the difficulties of the histo-
rian's task in adequately assessing a scientific giant without merely re-creating the mythic hero figure that the scientist often himself began constructing during his own lifetime.

Koch disagreed with Pasteur on a number of important issues in bacteriology. Brock tells us that in the Pasteur-Koch controversy, Koch could at times be so personally vicious as to be shocking. We learn, however, that the controversy "was certainly rooted, in part, in the French-German antagonism that still festered as an aftermath of the Franco-German war." This has also been documented for Pasteur in the most recent work. Brock points out (p. 176–177) that differences in style between the Koch and Pasteur schools, also "rooted in national characteristics," but with "more deep-seated significance" scientifically, contributed to the animosity and misunderstanding as well. These themes have been explored and developed in great depth by Andrew Mendelsohn (originally in his Princeton Ph.D. dissertation, which is soon to appear as a book), who characterizes the French and German schools as two fundamentally different "cultures of bacteriology." He argues that Pasteur's more ecological approach to microorganisms was rooted in an agricultural French context in which the "economy of nature" (and hence the role of microorganisms in that big picture) was a primary cultural motif. Indeed, Pasteur's entire work with microbes began through fermentation, their positive functions such as winemaking suggesting the ecological necessity of microbes. By contrast, Koch's view of bacteria was medically rooted from the beginning, his surgical experience in the Franco-Prussian war; microbes were pathogens, exclusively negative agents to be eliminated if at all possible. This fundamental difference in "culture" produced different methods of working with bacteria, e.g., Koch's early insistence on working with pure cultures as opposed to the Pastorians' preference for working with liquid cultures that, by nature, were mixed populations. But Mendelsohn shows that the deep philosophical difference continued to be manifested in the French and German "schools" and as the new science of bacteriology was transformed over several decades.

Another important contribution to this aspect of Koch's work has come from University of Toronto historian Pauline Mazumdar. She shows convincingly that an important early source of Koch's basic opposition to the Pastorians was his deep epistemological commitment to monomorphism, the idea that microbes, like all other organisms, come only in discrete species. Carl von Nägeli in Munich and his students
championed the opposite theory, pleomorphism, that microbes can undergo such a wide range of mutability under different environmental conditions that the morphologically different types are almost all interconvertible. Mazumdar shows that these views on microorganisms were manifestations of very long-standing opposed views of nature that she calls the "Linnaeans" and the "Unitarians." Koch emulated the work of the botanist Ferdinand Cohn, with whom he shared the view that bacteria came in true Linnaean species, and thus he was opposed to Nägeli's ideas as soon as he heard them. It was Koch's deep a priori belief in this view of nature that led him to think that photography of the organisms would be an important contribution to their study. Mazumdar suggests that Koch's insistence, as early as 1878 (long before there was any proof of such an idea), that one bacterial species must be the cause of one and only one disease also grew out of this basic difference in beliefs about nature. In his 1878 paper on wound infections, he used this as a new dimension to his definition of bacterial species: the disease that an organism caused was one of the chief features that could be used to define which species of microbe it must be. Brock pointed out many years ago in his book *Milestones in Microbiology* (p. 100–101) that Koch was exhibiting theory-laden observation when he emphasized tiny differences between, e.g., micrococci and indeed that the researcher was begging the question at issue in imagining "that the minor differences he saw were significant. . . . He wanted these organisms to be different" so that his theory would be verified. Brock concludes, "Fortunately Koch was right on this point, but there was no a priori reason why he should have been right, and so we must conclude that he was lucky." Thus, the widespread opposition Koch faced from other doctors and students of microbes was not at first due to their being unaware of his evidence. Rather, it was precisely because they saw that he did not have the evidence for his strong one bacterium-one disease claim that his theory was opposed and the theories of Nägeli and Max von Pettenkofer retained wide support into the 1890s.

So why should this contribute to antagonism between Koch and the Pasteur school as well? Mazumdar points out that the pleomorphist camp used exclusively liquid cultures, as did everybody else prior to Koch's invention of solid media. But Koch was convinced that it was that fact above all else that caused confusion between (what he was sure were) different separate species in a mixed culture and (what his opponents interpreted as) the different stages in the interconverting
life cycle of pleomorphic microbes. When Pasteur first announced the discovery of attenuated virulence in a microbe in 1880, this reminded Koch all too much of the pleomorphists’ claims that such a fundamental defining property of the organism as its ability to cause a disease was a mutable thing. And since the Pastorians, like Nägeli and his followers, still used liquid media for their cultures, this clinched the case in Koch’s mind: the Pasteur school was highly suspect of being as fundamentally misguided about stable, unchanging bacterial species as were the pleomorphists.

Of course, we now know that Koch was wrong in thinking that stable species were incompatible with quite significant genetic mutability within a species of microorganisms. Indeed, some would argue that the discovery of such important phenomena as the variation among “smooth” and “rough” forms of pneumococci (and the resultant path to the double helix) may have been held up for decades by the extent to which Koch’s monomorphist dogma held sway, once he triumphed with the discovery of the causes of spectacular diseases such as cholera and tuberculosis. Bacteriologist Ludwik Fleck made such an argument in the 1930s. Brock also cites Philip Hadley and shows that disagreement over this point had surfaced among American bacteriologists even in the last years of Koch’s life. Yet the slow waning of Koch’s dogmatically extreme monomorphist view was still to create trouble, even for American researchers, as Harris Coulter has shown in the case of Arthur Isaac Kendall. Clearly, this is a prime example of what Brock intimated when he warned of the excessive influence of the “cult of personality” that grew up around Koch. While his insights about the use of pure cultures were brilliant, his a priori bias led him to mistakenly think this must mean that Pasteur’s discovery of variability among microbes was illusory. For the next several decades, this necessitated the creation of epistemological wastebasket categories such as “involution forms,” into which observations could be banished when they seemed at odds with the monomorphist paradigm. If an overriding commitment to his belief had the benefit of driving Koch through the years of hard work necessary to isolate the causes of major human woes, it simultaneously had the effect, proportional to his success, of calcifying the research and preserving Koch’s mistakes, at least for several decades. It also, as much as nationalistic feelings, may have contributed to driving a deep wedge of suspicion between him and the only other group of workers with whom he could have collaborated as equals. Such is a common feature of research that should give us pause.
One of the single most important new historical contributions on Koch is Richard Evans's masterly analysis of the 1892 Hamburg cholera epidemic, in which Koch and his new bacteriology clashed with the sanitarian theory of Max von Pettenkofer. Pettenkofer’s theory was based on exhaustive study of local conditions of soil and climate and emphasized that the cholera germ was only one ingredient needed to produce the disease. Only when the bacterium came in contact with the soil under specific conditions related to the underground water table could the actual cholera poison be generated. Thus, in Pettenkofer’s theory, which had enjoyed wide respect in medical circles for 20 years by 1892, the bacteria getting into drinking water could in no way transmit the disease. In the epidemic of 1892, it was noticed that the rate of cholera in the immediately adjacent, downstream city of Altona was negligible compared to the rate in Hamburg. Altona had a sand filtration system for its water supply, so to Koch and his supporters it seemed obvious that drinking water must be the main means of transmission of the disease. Historically, one of the things calling out for explanation has been how anyone in Hamburg could have resisted such compelling epidemiological evidence and not immediately thrown out the Pettenkofer theory in favor of the Koch theory that bacterium equals disease. Yet the epidemic reached very serious proportions indeed, claiming nearly 10,000 lives in 6 weeks before Koch’s views came to dominate. The power of Evans’s close-up history is in explaining which forces in the medical community and government of Hamburg lent support to Pettenkofer’s theory and why. Furthermore, Evans shows that larger political tensions between Hamburg and the German Empire (with Prussia the dominant state), over “federal” intervention in local matters, exacerbated the reasons why local Hamburg officials would be opposed to Koch, an official of the Prussian bureaucracy.

The famous historian of medicine Erwin Ackerknecht first suggested 50 years ago that anticontagionist, sanitarian theories of epidemic disease would tend to be supported by 19th century classical liberals (including free-trade advocates) while contagionist theories would be more likely to be supported by political conservatives. Why? Because contagionist theory implied the need for more centralized government authority and interference in local affairs to enforce quarantines and disinfection measures. Free traders, especially merchants and businessmen whose livelihood depended upon the free and cheap movement of goods across state and national boundaries, stood to lose most if
quarantines were imposed because of epidemics. In the 19th century, this group tended toward reformist, liberal politics. More extreme liberals, such as the famous pathologist Rudolf Virchow, insisted that social reforms for the underfed, overworked poor who lived in unclean conditions were the only real cure for epidemic diseases. Virchow and his supporters would always be highly suspicious of germs as any kind of true causative agents, recognizing that the easiest way for a conservative government (such as that of Prussia or, after 1871, the Prussian-dominated Empire) to avoid expensive and democratizing social reforms was to blame epidemics entirely upon a germ from without, thus avoiding issues of poverty and inequality altogether and insisting that all that was needed was quarantine and disinfection. These far-left liberals were not surprised that the imperial government in Berlin supported Koch. And they were skeptical of Pettenkofer's theory for allowing any role for a germ at all. Yet, compared to Koch's Prussian state antigerm bureaucracy, which gave the germ total causative blame, Pettenkofer's theory still appealed to liberals because it did at least emphasize the importance of local conditions in creating the actual poison that caused disease. Thus, it implied that local medical officials, not far-off Prussian bureaucrats trying to pass sweeping uniform policies and force them on all German cities under all conditions, were by far the most appropriate people to decide how best to deal with epidemics. Practically speaking, sanitary theories did actually greatly reduce overall mortality from epidemics because of their emphasis on building sewers and public water supplies (though not necessarily with filtration) and on improving nutrition and general living conditions. Thus, in England and in many areas of Germany, sanitary theory was credited with actually solving the problems, without the need for germs as central players.

If all this were not enough to make Pettenkofer's theory more popular with most local medical officials, Evans shows that Hamburg was an even more special case and proves that this political context was a very important reason why Hamburg, alone among German cities by 1892, experienced a severe cholera outbreak that year. Hamburg's government had been run for centuries by the mercantile class, as the merchants had basically made the wealth of this trading port since medieval times, when it first became a free and independent city-state within the Hanseatic League. In its liberal, free-trade policies and culture, the city had long been known as the "most English city on the
Continental," let alone in Bismarck's conservative German Empire. The mercantile ruling class selected the medical officials, and the doctors most likely to become public servants were those who saw their interests most closely tied to those of the wealthy merchants. Thus, the entire history and culture of the city militated against quarantine policies and the havoc they caused in disrupting trade, and Hamburg held out longer than any other German city against centralized control of local medical policies by Berlin. In 1892, huge numbers of eastern Europeans were passing through Hamburg to board ships for emigration abroad, especially to America. At that time, if ever, city officials would be loath to impose a lengthy quarantine, stopping the flow of this highly profitable cargo. Yet it was just the flow of these poor emigrants that was bringing the cholera bacillus from the east and depositing it in the sewers, the river, and the harbor of Hamburg. Because one of the most pointed differences between the Koch and Pettenkofer approaches was in whether epidemic disease poisons could be spread by drinking water supplies, the kind of epidemic Hamburg (or Pettenkofer's Munich) was most unprepared for, despite other intelligent sanitary measures, was an epidemic of a water-borne disease such as cholera or typhoid. Since centralized Berlin policy on germs, as dictated from Koch's lab, was enforced through almost the entire remainder of the Empire, only Hamburg fell victim to cholera that year. Needless to say, Koch was sent to investigate, and his eventual triumph over the 1892 epidemic was a crippling blow to Hamburg's continued economic and political independence from Prussian domination. It is not possible to predict simply and unequivocally that any given doctor would support or oppose contagious theory and policy based solely on his basic political views. Nevertheless, the integral nature of political history in understanding the fortunes of the germ theory of disease never came through more clearly than in Evans's story of *Death in Hamburg*.

Brock's biography makes eminently clear that Koch understood how crucial the support of the imperial and Prussian state politicians in Berlin was for the advancement of his career and the spread of his ideas. And the relationship was a true symbiosis. For, in the wake of worldwide fame for French science that came after Pasteur's triumphs with anthrax vaccine in 1881, the prestige of German science was at stake. If the memory of the Franco-Prussian war 10 years before were not still clearly in everyone's mind, Pasteur was deeply embittered and publicly campaigned all through these years for more support for
French science, insisting that France had fallen behind the state support the Germans gave to science and that this was an important reason for her defeat by the "Prussian chancre." Thus, supporting Koch and trumpeting his triumphs as the German answer to Pasteur were priorities for Berlin. The famed "race" between the German and French teams to find the germ of cholera in 1883 and the declaration of Koch's *Vibrio cholerae* in 1884 as a triumph for German science must be seen in this context. There are two reasons. First, several other investigators had observed the cholera bacillus before Koch (the Italian Pacini is officially credited with having first seen it in 1854), so presumably more conclusive proof of a causative link should account for the contemporary trumpeting of credit for Koch. But the second point is exactly this: Koch was *unable* to fulfill the criteria for proof of causation that were to be announced in that same year and later became enshrined in our microbiology textbooks as Koch's postulates. The most crucial missing link was the inability to infect an animal model with the bacterium and cause the disease. Thus, the Pettenkofer school, and many outside Germany as well, was highly skeptical of whether Koch's bacillus was any more proven to cause cholera than any of the other numerous intestinal bacteria for which the claim had been made before. Anticontagionists recalled in particular an episode in 1849 when British researchers claimed to have shown that a fungus was the cause of cholera, only to have it shown within the year that the organism in question was a common mold contaminant. Why, in this context, Koch's *Vibrio* came to be so widely celebrated makes much more sense if we recall that the Berlin government was the most vocal advocate of that view, treating Koch and his team as national heroes upon their return from India and arranging for Koch to be publicly greeted by the German emperor.

The French were not the only intended targets of this orchestrated propaganda for the superiority of German science. As Evans points out, 1884 was the year that saw the beginning of the imperial powers' "scramble for Africa." And the furious competition to conquer disease in the name of science ran neck-and-neck over the next 3 decades with the race among the Germans, French, British, and others to colonize territory in the name of civilization. The link was twofold: a propaganda war to justify imperialist expansion on one hand, combined with the need for science to control aggressive tropical diseases so that large numbers of Europeans, especially troops, could live in Africa on the other. By 1896, Koch had begun to shift his major research interest to
the tropical diseases of Africa. That Koch’s scientific interest moved in this direction is not to be doubted. But again we must recall that it was only the support of the German government that made possible Koch’s intensive full-time work in bacteriology after 1880. So perhaps it is not so surprising, and even may be instructive for modern high-budget science, to study the degree to which he who paid the piper chose in this case to call the tune. The racist and political roots of this, along with the science, are explored in depth in recent work by Heidelberg historian Wolfgang Eckart. Eckart shows, for instance, that trials of chemotherapy agents against the diseases were much easier to accomplish in the African colonial setting, with nonwhites as experimental subjects, than would ever have been possible in European labs.

To return to the issue of Koch’s postulates, this is an area in which very interesting work has been done recently as well. While Koch was willing to deploy these rules in a more rigid form for publicity purposes, we learn from Brock’s discussion (p. 180–182) that, from the very beginning, Koch understood that the situation was more problematic. He was convinced that his vibrio was the cause of cholera, for example, and thus that it might still not be possible in every case to fulfill the requirement for reinfecting an animal with the pure culture and reproducing the disease. Thus, from before they were even announced, in the mind of Koch the researcher these rules were not the kind of dogmatic requirements that they went on to become in microbiology textbooks. Historian Victoria Harden of the National Institutes of Health has studied the ongoing conflict in research, especially after the discovery of the viruses (which can almost never be cultivated on nonliving media), between the postulates as a helpful guideline for seeking new disease agents and simultaneously as obstacles to new fundamental breakthroughs. Virus researchers have insisted from the earliest days of their work that new versions of the rules must be continually reinvented to take into account the new properties of pathogens that differ from those of the bacteria worked with in the 1880s. Basic disagreements can be caused when two researchers insist on different versions of these postulates as bottom-line criteria, and in no case has this come out more clearly than in the objections of virologist Peter Duesberg that the epidemiological data for human immunodeficiency virus (HIV) are insufficient to prove that it causes AIDS. Harden looks at cases up to and including this one and tries to evaluate the validity of Duesberg’s arguments and those of his opponents, such as William Blattner and
Robert Gallo, in light of past historical disputes over which form of Koch's postulates is most reliable. In light of such a history, it is fascinating to reflect on the process by which such a scientific idea, though more flexible in the mind of its creator, can become an obstacle to new discoveries, especially if propagated in too rigid a form in science textbooks. Of course, in allowing the German state to use simplified notions of his work as propaganda tools, Koch himself must have realized his own participation in this process from the beginning.

We see this kind of double-edged nature of patronage again when Brock shows how (p. 198–199) Koch's German government superiors forced him to announce his discovery of tuberculin and its possible curative role for tuberculosis before he thought it scientifically appropriate. The premature announcement was forced on Koch because of the publicity opportunity of making the announcement at the Tenth International Medical Congress. It almost certainly also resulted from the enormous international prestige that came to the Pasteur group in the first years after the development of the rabies vaccine, which led to donations of an enormous sum of money that was used to create the Institut Pasteur in 1888. In the wake of this, Koch's government patrons were planning to create an institute for him in Berlin that would have comparable prestige for cutting-edge research. As soon as his work on tuberculin made it mistakenly seem that it would be an effective therapy for tuberculosis, the negotiations for Koch's new institute became bound up immediately with the potential fame and profit associated with that remedy.

In this area too, recent historical work has also brought new and interesting details of Koch's negotiations with the state bureaucracy to light. Heidelberg scholar Christoph Gradmann has found, in East Berlin archives, detailed government documents showing that the negotiations bogged down in late 1890, but not because Koch was digging in his heels about being forced to announce the discovery prematurely. Koch was trying to strike a deal that would guarantee him a large share of the profits that would accrue from tuberculin sales for the first 6 years. This is in striking contrast with the image of Koch the selfless researcher, whom biographers have been convinced had no real interest in fame or profit. Over the ensuing months, when large-scale trials brought out the fact that tuberculin really had very little therapeutic effect, Koch was forced to back down from his tough stance and accept the creation of the Institute for Infectious Diseases on terms mostly
dictated by the German government, since he feared losing all in the public relations debacle over tuberculin. It never became public that, as many tuberculin critics had charged during the months of controversy, Koch hoped to personally profit from the discovery, in addition to getting his institute for the good of humanity.

Here we are faced with something harder to accept into our previous heroic vision of Koch. Despite Brock's thoughtful comments on the larger context of science, politics, and bureaucracy in which Koch worked, here is one area where Brock the microbiologist generously views his subject with the basic faith that Koch was "strongly motivated to excel without regard to fame and fortune," at least in his early years. And yet, working out the role of personal profit in this incredible new field, so important for humanity, is surely an important part of the history of work on human disease. The controversy that followed Selman Waksman and Albert Schatz's discovery of streptomycin, which eventually led to a lawsuit and a court settlement over allocation of profits from that drug, shows that this tension did not go away after the early days of giants like Koch and Pasteur. Surely the recent dispute between Robert Gallo and Luc Montagnier over patent rights resulting from the discovery of HIV shows that it is still a matter very relevant in research. We must realistically include these matters in our picture of Koch to see the full human being and to understand the full relevance of his story for our own times, as well as for the future of scientific research. Paraphrasing from Gerald Geison's recent scholarly and provocative biography of Pasteur, we need a Koch for our times, not only the Koch who has inspired generations of young people to become scientists, but also the more complex person that we know he must have been. This can be done while simultaneously keeping in view the important contributions to science that Koch made so brilliantly. It is a tribute to this book that it has gone so far in that direction without sacrificing the details that make the science itself so compelling, indeed world-changing.

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Further Reading


Preface

Robert Koch was one of the most important figures in medical science and was also the founder of bacteriology. Surprisingly, there has been no serious biography of Koch in English. Indeed, even the German-language biographies of Koch are dated and mostly inaccessible. The present book attempts to correct this deficiency.

This book began as an outgrowth of a larger project on the history of microbiology. When I began my research, I realized that I would have to spend considerable time on Koch because of his major importance. Yet there was no detailed summary of his work in English. There are many books in English about Louis Pasteur, the other leading figure of 19th century microbiology, but Robert Koch has attracted little attention in spite of his well-recognized importance. The biography by Dolman in the Dictionary of Scientific Biography is meritorious but is brief and hence serves primarily as a useful starting point. Most of the German-language biographies suffer from the sin of “hero worship”. The biography by Bochalli is not only brief but rather stilted. The book by Heymann is rather too detailed and only deals with Koch up until his discovery of the tubercle bacillus. The book by Möllers, Koch’s last assistant, is a useful source book of correspondence and dates but is ponderous and much too detailed for the general scientific reader. Both the Heymann and Möllers are out of print. The book by Genschorek, published in the German Democratic Republic, is also fairly brief. There are also a few fictionalized accounts of Koch in German, as well as a German-language movie, but nothing of any use to the student, scientist, or medical researcher.

The present book is based in the first instance on Koch’s published work itself, and on the detailed Koch correspondence published by Heymann and Möllers, but has been greatly supplemented by my own reading in the bacteriological literature of the late 19th century. Prob-
ably at no time in the history of bacteriology did so much happen so quickly as between the year 1876, when Koch published his first work on the life cycle of the anthrax bacillus, and 1900, when Koch went into semi-retirement. In 1876, most physicians did not believe in the germ theory of disease, and medical practice was antiquated and based on incredible personal bias. By 1900, the Koch school of bacteriology was well established, and the disciplines of hygiene and public health had been placed on firm footings. Guided by Koch's postulates, investigators uncovered much of what is now known of the causes of the most important infectious diseases of humans and lower animals. Even today, Koch's postulates are considered in detail whenever a new infectious disease (such as AIDS), arises. To a real extent, we owe our current good health and longevity to discoveries made by Robert Koch and his school.

Koch was one of the true scientific revolutionaries. Beginning as a simple country doctor, he ended up his career as a Noble Prize winner and a dominant figure in 19th century and early 20th century medical research. His story can serve as an example of how a lone doctor, living and working in scientific isolation, can rise above his environment and become a major medical and public figure.

The manuscript for this book was read in its entirety by Professors Hanspeter Mochmann (Berlin-Buch), Werner Köhler (Jena), and Dieter Gröschel (Virginia), all of whom provided invaluable suggestions and corrections. Professor Mochmann also served as a gracious host during my visit to the various Koch sites in East Berlin. Dr. Masao Soekawa provided me with full access to the extensive Koch materials at the Kitasato Institute in Tokyo. Mr. Klaus Gerber of the Robert Koch Institute in West Berlin gave me full access to the Koch archives of that institute, and also provided me with photographs. Other photographs were provided by Dieter Gröschel, Hanspeter Mochmann, and by Dr. W. Presber of the Institute for Medical Microbiology in East Berlin. The Koch bibliography published here is based on a more extensive German-language version kindly prepared for me by Professors Mochmann and Köhler.

In preparing this book, I was fortunate in having access to an excellent library on the history of medicine at the University of Wisconsin-Madison. Librarian Dorothy Whitcomb provided endless amounts of help. Her courtesy in giving me long-term access to some of the most important Koch sources is greatly appreciated. Professor William Coleman gave me important advice and insights as this work progressed.
Jon Bartells did an excellent job as research assistant, digging out from the various library catacombs an amazing amount of valuable material. Kathie Brock copyedited the manuscript and provided many valuable comments. The Graduate School of the University of Wisconsin-Madison provided a modest amount of financial support. I would also like to acknowledge the good grace of my colleagues in the Department of Bacteriology of the University of Wisconsin-Madison for tolerating my decision to do work on the history of science. I hope I have justified their faith.
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