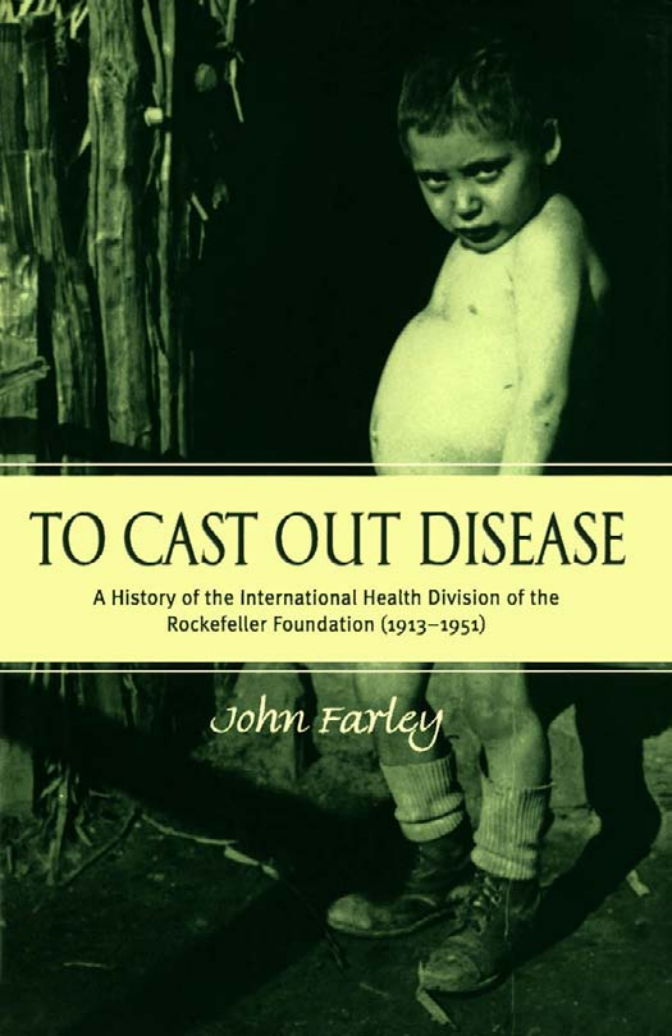

# TO CAST OUT DISEASE

A History of the International Health Division of the Rockefeller Foundation (1913–1951)

John Farley

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## A HISTORY OF THE INTERNATIONAL HEALTH DIVISION OF THE ROCKEFELLER FOUNDATION (1913–1951)

JOHN FARLEY

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*To Grace,*
*our family,*
*and our*
*perfect grandchildren*

*And, of course, to*
*Colonel Bob*

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# Acknowledgments

Living a few hundred yards from a shoreline washed by the frigid waters of the Labrador Current has always presented a barrier to my interest in parasites, tropical diseases, and their histories. I was therefore pleasantly surprised to discover, years ago, that an archive which catered to my interests was situated in Tarrytown, New York, only two hours by air to the southwest; I no longer needed to take that awful night flight to the British archives in London. The Rockefeller Archive Center thus became the focus of my world for the long drawn out slog to uncover the records of the International Health Division which, in the beginning, I believed to be more or less the American equivalent of the British Colonial Medical Service. Both organizations provided an outlet for young men who wished to combine professional satisfaction with a life of adventure and sometimes danger.

I cannot begin to express my deep appreciation for the help I received from Tom Rosenbaum and the other archivists at the center. I know with certainty that had it not been for Tom, I would have given up in disgust many years ago, overwhelmed as I often was by the sheer bulk of material in the archive. I would also like to thank Dr. Darwin Stapleton, the archive's director, for his considerable support, kindness, prompt answers to any queries, and for his multiyear loan of the Health Division's annual reports. I need to thank also those with whom I stayed during my many visits to the archive—particularly the lady, whose name I can no longer remember, who introduced me to the historical and walking delights of

the Croton Aqueduct. Above all, I am indebted to the good ladies of Marymount College, whose guest room became a second home to me and where the college pool allowed me to indulge my passion for a hard morning swim; the only way I could survive the rest of each day without falling asleep.

Most academic authors, if they are honest enough to admit it, will attest to the fact that writing a manuscript and finding a publisher for it are vastly different projects. This manuscript has had an extraordinarily long gestation period. Indeed, had it not been for one very special person it never would have seen the light of day. On hearing that I was about to toss the whole kit and caboodle into the Atlantic Ocean, I received an order from Colonel Robert Joy, who had already read some of the earlier drafts, to get back to work, although not in such polite terms. As a former national serviceman in the British army I had no option but to obey an order from a superior officer. The degree of thanks I owe "Colonel Bob" is incalculable, and when, following his suggestion, I located the patient and professional Jeffrey House of Oxford University Press, the tunnel suddenly began to grow much shorter. I would especially like to acknowledge the extraordinarily detailed and thoughtful critique I received from an anonymous reviewer at Oxford University Press. I have since discovered that the reviewer was John Hutchinson of Simon Fraser University in British Columbia, who sadly died suddenly before he was able to read how I had reacted to his many suggestions. I would like to acknowledge, too, the help asked of, and always given by, the physician and historian, Dr. Margaret Humphreys of Duke University. I very much enjoyed working with my production editor, Lynda Crawford, particularly as I tried to make sense of the American rules of capitalization. Americans are, as H. W. Fowler's *Modern English Usage* makes clear, "anti-capitalists."

I am naturally grateful to the Social Science and Humanities Research Council of Canada (who must be wondering where the book got to) for providing funding for research and travel between Halifax and New York, which I sometimes did by indulging my passion for trains, taking the 32-hour ride to Croton via Montreal. I would also like to thank the Rockefeller Foundation for their generous grant which will be used to allay publication costs.

While writing this book I took early retirement from the Biology Department of Dalhousie University. I owe Grace, my wife, "she who must be obeyed," a heap of thanks for constantly reminding me that retirement is not a time to retire, to stop, but a time to increase the intensity of training for competitive masters swimming, and to continue reading and writing.

*Halifax, Nova Scotia* J.F.

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# 1

# Introduction

In 1955 Paul Russell published his magnum opus, *Man's Mastery of Malaria*, which told the story of how malaria was mastered.[1] "The point is," he wrote, "that malaria, after centuries of effort, is now universally controllable." And if with malaria, why not with other communicable diseases? Yellow fever, for example, was no longer the threat it had been and tuberculosis was continuing its long slide, perhaps into oblivion. In Geneva members of the newly-formed World Health Organization (WHO) had set up expert committees to declare a war to the finish on malaria, venereal disease and tuberculosis, with other diseases close behind. The medical world was awash with optimism.

Few doubted that these spectacular declines reflected the impact of modern medicine, although malariologist Russell took care to point out that many other factors had played a role. A growing choice of tools—chemical drugs, antibiotics, vaccines and insecticides, none more famous than DDT—had become the modern handmaidens of cure and prevention. Of course, there were problems on the horizon that, if taken seriously, would undercut this optimism. Russell himself was aware of mounting insect resistance to DDT and of the malaria parasite's ability to withstand the effects of antimalarial drugs. Others worried about the impact that success in "death control" would have on mankind's lot—fewer people dying, more mouths to feed; fewer dying of malaria, more dying of starvation. Still, though Russell took pains to temper his optimism, one can appreciate how a general reader would react to these words in the book's preface:

While keeping in mind the realities one can nevertheless be confident that malaria is well on its way towards oblivion. Already as a malariologist, I feel premonitory twinges of lonesomeness, and in my own organization I am now a sort of 'last survivor'.

*To Cast out Disease* is a history of the Rockefeller Foundation's International Health Division (IHD), in which Russell spent most of his working life and where he developed his ideas and his optimism. He came to believe that the expertise, tools, and methods the organization had helped develop could, in the long term, lead to the disappearance of many of the world's most feared diseases. Before the founding of the WHO in 1948, it was arguably the world's most important agency of public health work.[2] The International Health Division went by slightly different names at the beginning of its life: *International Health Commission* (IHC) from 1913 to 1916, and the *International Health Board* (IHB) from 1916 to 1927. From 1927 until it closed down in 1951, it was the *International Health Division* (IHD). In this book, it is referred to by its name at the time of the event described, usually with the word "International" omitted.[3] By the time it closed its doors in 1951, its staff had worked in over 80 countries: the United States and Canada, 25 nations in Europe, 15 in the Caribbean, every country in South and Central America, 19 in the Far East, and several in Africa and the Middle East. They began with an effort to control or eradicate hookworm as part of an attempt to build up what today would be called a public health infrastructure; but almost from the beginning they took on malaria and yellow fever and even engaged in a high-cost tuberculosis campaign in France. For many years the control or eradication of hookworm, malaria, and yellow fever was the IHD's main focus of concern.

Similar efforts were directed toward the training of public health physicians and, to a lesser extent, public health nurses. These physicians and nurses were destined to become members of a new cadre of public health professionals, building up public health services where none existed or wrestling control of public health matters from incompetent part-time physicians, untrained in public health, who often owed their positions to political patronage rather than professional expertise. Toward this end, the IHD endowed institutes of public health and public health nursing, offering fellowships to study at these schools, located mostly in North America and Europe. The IHD built several research laboratories, including a very important yellow fever laboratory in New York City, supported laboratory research, developed the yellow fever vaccine, and attempted to develop vaccines for other diseases.

The IHD and its predecessors grew out of the Rockefeller Sanitary Commission, an earlier example of Rockefeller philanthropy, which had been founded in 1909 to strengthen up public health agencies and eliminate hookworm from the American South. Hookworm was where it all started.

John D. Rockefeller Sr.'s Standard Oil Company, founded in 1870, had risen to dominate the world oil markets with its control of wells, refining and oil trans-

portation.[4] In 1891, he appointed as his principal aide in philanthropy 38-year-old Frederick Gates, who had played a major role in Rockefeller's endowment of the University of Chicago. An astute businessman and former Baptist minister, Gates turned the Rockefeller fortunes towards medicine. He had been pulled in that direction after plowing through William Osler's 1892 text, *Principles and Practice of Medicine*. To the surprised Gates, while the book was filled with diagnostic details, little was known about causes—and cures still rested on traditional medicines of questionable value. "When I laid down this book," he wrote, "I had begun to realize how woefully neglected had been the scientific study of medicine in the United States." What was needed, he concluded, was a research institute given to "uninterrupted study and investigation, on ample salary, entirely independent of practice."[5] Rockefeller made his first move towards medicine by endowing the Rockefeller Institute for Medical Research (after 1965 the Rockefeller University). The institute was incorporated in 1901; Simon Flexner was appointed director in 1902; work began in some converted houses in 1904; and in 1906 the first laboratory building was erected at its present site in Manhattan.[6] By 1970 it had generated 16 Nobel Prizes. But it was only the initial step toward medicine and public health.

In June 1909 John D. Rockefeller Sr. signed over a deed of trust to his son John D. Rockefeller Jr., his son-in-law Harold McCormick, and Frederick Gates. Under the trust, over 72,000 shares of the Standard Oil Company, worth more than $700 million in 1990 dollars, were to be given to a newly-created body, the Rockefeller Foundation, whose mandate was "to promote the well-being and to advance the civilization of the peoples of the United States and its territories and possessions and of foreign lands in the acquisition and dissemination of knowledge, in the prevention and relief of suffering, and in the promotion of any and all of the elements of human progress."[7]

In October of that year, Gates, John D. Rockefeller Jr., and 10 others met at John D. Rockefeller Sr.'s offices in New York City, when it was agreed to found the Rockefeller Sanitary Commission for the Eradication of Hookworm Disease in the U.S. South, to be funded for five years with up to 1 million dollars—nearly 14 million in 1990 dollars.[8] Gates and Rockefeller Jr. realized that success depended on cooperation across a broad spectrum in the South: the medical profession, public health officials, businessmen, schools, churches, and the press—and that much effort would need to be expended on public health education. To this end, they offered the post of executive secretary to Wickliffe Rose, a Tennessee-born academic, dean of the Peabody Normal School and the University of Nashville, who may never even have heard of hookworm.[9] As Lewis Hackett, one of the IHD's most eminent field workers, wrote, "many of those who got to know Rose well have found it . . . difficult to fathom why he should have been offered the job, and why he should have accepted it."[10] But accept it he did, opening the Sanitary Commission's office in Washington on January 3, 1910.[11] Three years later, he

was to become the first director of the newly-formed International Health Commission (IHC).

During the four long difficult years it took to set up the Rockefeller Foundation, hookworm and public health became priorities of the Foundation.[12] In January 1913, just before the act to establish the Rockefeller Foundation was successfully presented to the New York State Senate, the *New York Times* published an article by Jerome Greene, secretary of the foundation, that suggested how important the fight against hookworm was to become. The hookworm campaign needed to extend beyond the South, he wrote, "for its eradication must . . . be made a world campaign—not for altruistic motives merely but because no one country can be safe until all have been cleared of this pest.[13]

At the first meeting of the Rockefeller Foundation Trustees, held on May 23, 1913, Rockefeller Jr., the foundation's president, asked for opinions on possible lines of work. The expansion of the work of the Rockefeller Sanitary Commission, which by then was well underway, beyond the South into other countries was one obvious possibility. Indeed, in anticipation of this move, the Sanitary Commission had ascertained that hookworm disease was "an international problem of serious proportions," prevalent in an equatorial belt from 36° north to 30° south, and that hookworm carriers were a danger to the United States. "Every Indian coolie already in California," the report noted, "was a center from which the infection continued to spread throughout the state."[14]

Rose, Greene, and attorney Starr J. Murphy were asked to formulate a plan for extending hookworm work beyond the South, and present it at the next meeting, one month later. During this and a subsequent meeting at which the International Health Commission received its formal name, the foundation agreed "to extend the work of eradicating Hookworm Disease to other countries and other nations." To that end, it divided the world into six geopolitical areas in each of which would exist a distinct service. It was envisaged that the Sanitary Commission would continue its work in the American South, while the Health Commission would divide its work among Latin America, the Orient, and the empires of Britain, France, and the Netherlands.[15] In the end, they undertook work in only four areas: the South, Latin America, and in British colonies in the Far East and in the West Indies. And, in a final link to the Sanitary Commission, Rose became director of the International Health Commission on June 27, 1913.

From the start, members of the Health Commission agreed on two premises. First, philanthropy should not be confused with charity. They followed to the letter the philanthropic principles of Gates and Rockefeller Sr. Philanthropy was an investment. It should be offered to government agencies, not individuals; it should be of limited duration so as to stimulate self-help, not dependence; and it should be withheld unless recipients showed promise of continuing to work after aid had ended. This was a golden rule that was rarely broken; charity and emergency relief were alien concepts.

Second, the Health Division and its precursors held to one overriding concept from which they rarely diverged: disease was the determining factor of ill health and health could be attained only by the control or elimination of communicable diseases. Fred Gates's so-called Rockefeller Creed has become a cliché but still warrants quoting: "Disease is the supreme ill of human life, and it is the main source of almost all other human ills—poverty, crime, ignorance, vice, inefficiency, hereditary taints, and many other evils."[16] In time, officers of the IHD believed they had found the solution to these disease problems. It would involve the elimination of the disease-causing organisms by drugs or by vector control. Where possible, it would prevent reinfection by means of vaccines—a totally biomedical view of public health from which they rarely diverged and which carried all before it. When the Division closed in 1951, it did so in an era of hope and optimism generated by widespread belief in the efficacy of the new biomedical tools, an era illustrated in Russell's book.

The key to improving public health lay in the eradication of disease, all agreed; but there was a great deal of conflict over how best to achieve that goal. Was the Sanitary Commission set up simply to eradicate hookworm and other diseases, or was it, as Rose believed, a means to stimulate state and local effort to develop permanent public health agencies. The former policy seems to have been promoted by Gates. From Rose's perspective, however, the Sanitary Commission had a much broader mandate than that. It was a means of awakening interest in public health and there was no better place to do that than in the hookworm dispensaries. Here, Rose explained, people "lingered, gathered in groups around the table of exhibits, exchanged experiences, listened to stories of improvement of persons who had been treated, and returned home to tell their neighbors what they had seen or heard."[17] Reaffirming this vision some years later, Rose exclaimed: "The purpose of our work in any country is not to bring hookworm disease under control, but to make demonstrations which will lead ultimately to the enlistment of local agencies in the work."[18] In other words, Rose saw hookworm work as a wedge for making people aware that medicine had the potential to identify the cause of disease and cure it, and at the same time point to hygiene and sanitation as the vehicles of disease prevention. State authorities and local physicians could be stimulated to assume responsibility for development of statewide public health services. "Following the treatment and cure of this disease," the original resolutions state, "an intelligent public interest is awakened in hygiene and in modern scientific medicine and in practical measures for permanent public sanitation," and so, the document continued, the Health Commission would "follow up the treatment and cure of this disease with agencies for the promotion of public sanitation."[19] The eradication of hookworm was a means to an end, not an end in itself.

Rose's clear mandate did not outlive his tenure. It might have made a great deal of sense to an educator like Rose, but the three directors who followed him and the field and New York staffs were not educators: they were physicians, and a

handful of nurses. Physicians could not be expected to attack a disease without believing that its eradication or control was the main objective. To be told that the elimination of hookworm was not their primary objective must have been incomprehensible to many. Not surprisingly, Rose's ideas created tensions within the organization. When the Health Board shifted from hookworm to malaria and yellow fever and realized that an organization crafted in the American South could not simply be picked up and transferred holus-bolus to the British colonies, it moved away from Rose's vision and took on disease eradication as an end in itself, although Rose's views constantly reappeared throughout the years.

In 1928, a further point of controversy was introduced by Rose's successor, Fred Russell, who pressed for more basic laboratory research and called for the Health Division to set up a central laboratory in New York City—where the Rockefeller Institute for Medical Research already existed. To Rose and to most field officers of his time, the challenge lay in the delivery of knowledge already known and not in the discovery of new knowledge. Over the years many agreed with Rose, who, in the words of Hackett, "refused to allow the IHD to lose headway in channels of unpredictable investigation . . . The well-being of mankind meant reaching the maximum number of people in the shortest possible time with the equipment of knowledge and methods we already possessed."[20] But Russell and the man who would succeed him at the IHD, Wilbur Sawyer, disagreed. From their experience as laboratory men, and in light of the successes with yellow fever vaccines and with virus work, they saw laboratory-based research as fundamentally important. It became a major point of contention within the organization, setting field officers against laboratory personnel. Why should the Health Division fund basic laboratory research when the Rockefeller Institute had been built to do that very thing?

There was little disagreement, however, over the type of public health institution they wished to build and where they wished to build it. They would have no truck with the idea of minimally-trained "barefoot doctors" or with the Soviet Union's "Feldshers"—doctors' assistants qualified by practical training to perform certain tasks. After some bad experiences in Brazil, they sought to build only "model" schools of hygiene and schools of nursing in the civilized climes of Toronto, London, other European cities, and, to their later embarrassment, Tokyo. Leadership was always a key word in their vocabulary.

## The Medical Barons

Leadership was the key to understanding how the Health Division worked. Wickliffe Rose, the first director, unlike his successors, did not have a medical degree (Fig. 1.1a). Although given free rein at his job, he was more vulnerable to the dictates of the Rockefeller Foundation's Board of Trustees. Fosdick described Rose as a "mouse-like man, self-effacing," although others commented

on his impulsiveness. All seemed to agree, however, that while a clear thinker and a good judge of plans and programs, he was incapable of judging men and ended up appointing a heterogeneous crowd of infighters. Nevertheless, most of the staff seemed to have developed a warm feeling for Rose, perhaps fostered by the many letters of encouragement they received from him.

After a rocky start, the International Health Board developed a momentum of its own, becoming the most powerful branch of the Rockefeller Foundation, increasingly independent of the foundation's trustees—and thereby creating jealousies. George Vincent, president of the Rockefeller Foundation from 1917 to 1929, disliked the organization's growing independence and may have managed in the end to force Rose's retirement. But such successful interference by outsiders was rare. Power accumulated in the hands of those whom Robert Kohler aptly

**Figure 1.1a.** The first director, Wickliffe Rose (courtesy of the Rockefeller Archive Center).

called the "medical barons."[21] None was more baronial than Frederick Fuller Russell (Fig. 1.1b), who took over from Rose in 1923. The medical barons, all of whom held medical degrees, included leading field officers and laboratory staff, as well as the director, associate directors and assistant directors in the New York office. Attempts to rein them in failed in the late 1920s and, in fact, resulted in even greater independence. In 1929, a new body was called into being, a group of medical experts called the Scientific Directors who were interposed between the trustees and the Health Division as part of a general reorganization of the Rockefeller Foundation (see Table 1.1).

Russell developed into the division's most formidable director, building up the organization to a new level of professionalism and respect and gaining maximum funding. His training and background were in contrast to those of Rose. A native of Auburn, New York, he graduated in medicine in 1893 from the Columbia College of Physicians and Surgeons. Five years later, after a year at the University of Berlin, he was commissioned in the U.S. Army Medical Corps to become part of Surgeon General George Sternberg's team. In 1907, he was named cura-

**FIGURE 1.1b.** The second director, Frederick Russell (courtesy of the Rockefeller Archive Center).

**TABLE 1.1.** Presidents of the Rockefeller Foundation and directors of the International Health Commission, Board, and Division.

| ROCKEFELLER FOUNDATION PRESIDENTS | HEALTH ORGANIZATION | DIRECTORS |
|---|---|---|
| J. D. Rockefeller Jr. (1913–1917) | International Health Commission (1913–1916) | Wickliffe Rose (1913–1923) |
| George Vincent (1917–1929) | International Health Board (1916–1927) | Frederick Russell (1923–1935) |
| Max Mason (1929–1936) | International Health Division (1927–1951) | |
| Raymond Fosdick (1936–1948) | | Wilbur Sawyer (1935–1944) |
| Chester Barnard (1948–) | | George Strode (1944–1951) |

tor of the Army Medical Museum and instructor in bacteriology and clinical microscopy at the Army Medical School. A year later he was sent to the Royal Army Medical College in London and the Institute of Infectious Diseases in Berlin to investigate the experiences of the British and German armies with typhoid vaccines. On his return he recommended that vaccinations against typhoid be given to members of the U.S. army who were likely to be exposed to the disease; a special room at the Medical Museum was fitted up as a vaccine laboratory under Russell's direction.[22] Its success was such that the U.S. army became the first to make typhoid vaccination compulsory, for which Russell took much justified credit.[23] In 1913 he became professor of bacteriology in the Army Medical School, and during World War I he directed the army's Division of Laboratories and Infectious Disease. Before joining the IHD, he spent two years as chief of the Board of Health Laboratory in the Panama Canal Zone.[24]

This was the impressive figure whom Rose engaged, in 1919, to develop a laboratory service, an act which to some extent counters the criticism that he was not a good judge of character. The following year Col. Russell resigned from the army to become a full-time member of the Health Board's staff and was given the rank of Brigadier General in the U.S. Army Reserve. After three years as director of the Health Board's public-health laboratory service, he stepped up to succeed Rose as director.

Until his retirement in 1935, Russell dominated the Health Division to an extraordinary degree despite ongoing hostility from George Vincent, and from some of the scientific directors, who resented his quick promotion, and who opposed his attempts to strengthen the emphasis on laboratory research. Russell was far too confident and powerful to worry about what was going on behind his back. With his authoritative bearing and military manner, Russell, unlike Rose, established few personal relationships with his colleagues. But he was clearly a leader. Under his direction the Health Division's morale remained high and it reached the pinnacle of its power and influence. He was certainly not, as Hector Howard tried to paint him, "an old broken down army doctor, fiddling with laboratory work."[25]

On Russell's retirement in 1935, the position passed to Wilbur Sawyer, whom Russell had groomed for the job, fearing that his numerous critics would try to reverse the changes Russell had made (Fig. 1.1c). Born in Wisconsin in 1879, Saw-

**FIGURE 1.1c.** The third director, Wilbur Sawyer (courtesy of the Rockefeller Archive Center).

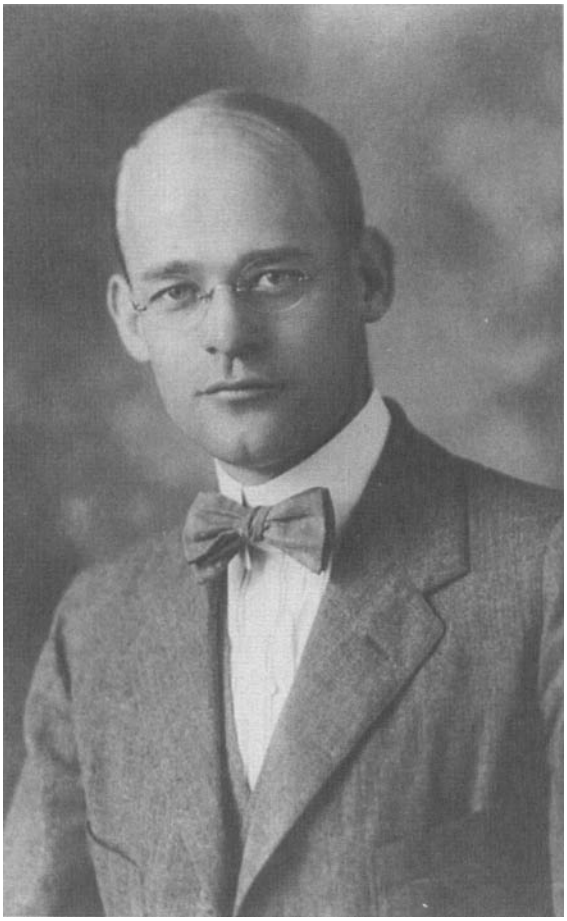

**Figure 1.1d.** The final director, George Strode (courtesy of the Rockefeller Archive Center).

yer graduated with a degree in medicine from Harvard in 1906. He joined the Health Board's staff in 1919 after war service and a teaching position at the University of California. He ran the Australian hookworm campaign before transferring to the board's laboratory service in 1924, and later taking charge of the yellow fever laboratory in New York City. Sawyer was described by his colleagues as "cold, uninspiring and meticulous, conscientious," and even as "ruthlessly ambitious." He, more than Russell, according to Hackett, tilted the Health Division in the direction of research.

The division's last director, George Strode, took over in 1944, when Sawyer left to become director of health in the United Nations Relief and Rehabilitation Administration (UNRRA) (Fig. 1.1d). Strode held a medical degree from the

University of Pennsylvania, and he joined the Health Board in 1920 following war service in France. After three years in charge of the Health Board's Brazil program, he ran the International Health Division's Paris office until World War II. Described as "well-balanced with good judgment," he was an administrator rather than a laboratory man. He seems to have been far more popular than Sawyer, lifting the morale of the field staff considerably after Sawyer's dark days. But he was not another Russell when the Health Division badly needed one; in the end, he failed to stand up to critics of the IHD, and to lead the staff in new directions.

The directors were served by a group of associate and assistant directors in New York, responsible, among other things, for the preparation of reports and site visits to many of the areas in which the organization worked. These men included John Ferrell, who with a doctoral degree in public health from Johns Hopkins had joined the Sanitary Commission as state director for North Carolina. When the Health Commission formed, Rose picked him to be his second-in-command. Although much despised by Russell, who, despite Ferrell's Hopkins degree, regarded him as a minimally trained physician from the deep-South, Ferrell remained an associate director until retirement in 1944. Andrew Warren was another long serving officer in New York. He joined the Health Board's field staff in 1921, became an assistant director in 1936 and remained as an associate director from 1945 until the division closed in 1951. In those years, he became one of the most traveled officers, visiting over 30 countries.

The medical barons were not simply those occupying the New York office. Many in the field staff deserved that label too, none more so than Victor Heiser (Fig. 1.2a). Rose had met Heiser on his eastern tour and made him the wonderfully-named "Director of the East," for the IHD. Outspoken, tough, and opinionated, with a considerable ego, Heiser saw himself as the Potentate of the East. He ran his fiefdom with little control from New York, and with a fine eye for public relations. A robust, domineering, vigorous, suave man of the world, who enjoyed life to the full, he may be regarded more as a medical duke than a medical baron and, as such, he clashed head-on with Russell, who may possibly have fired him in 1934. Russell certainly found him difficult to deal with and impossible to command. Heiser was born in Pennsylvania and orphaned by the Johnstown floods; he worked his way through Jefferson Medical College, graduating in 1897. Before Rose enticed him away, he spent 17 years with the U.S. Public Health Service (USPHS), including 10 years as chief quarantine officer in the Philippines. There, he told Rose, he became disenchanted as control passed into the hands of Filipinos; as a passionate advocate of the white man's burden he no longer wished to spend his time "watching and defeating personal political schemes."[26]

If there is room for debate over whether Heiser should be included among Rose-appointed misfits, there can be no disagreement over Hector Howard, whom Rose chose to direct the first hookworm campaign in British Guiana, and later to direct

**FIGURE 1.2a.** Victor Heiser (courtesy of the Rockefeller Archive Center).

the West Indies program (Fig. 1.2b). Howard was a poorly trained, Bible-thumping, fundamentalist Christian physician from Mississippi with a one-year medical degree from Memphis. His colleagues regarded him as a sharp-tongued pessimistic hypochondriac, with a deep inferiority complex, particularly when faced by people of Frederick Russell's quality. He took a childish delight in stirring up trouble and was completely intolerant of new ideas, originality or deviation from those under him. Also, much to the amusement of his more liberal-minded colleagues, his bible-thumping rhetoric was constantly interposed with a steady stream of crude profanities. Loathed by Russell, with whom he had a long and acrid row over the Scopes trial in Tennessee, Howard was unable to understand those aspects of medicine which Russell stood for: laboratory work, epidemiology, vi-

**FIGURE 1.2b.** Hector Howard (courtesy of the Rockefeller Archive Center).

ruses and vaccines. So, like Heiser, he, too, was fired in 1934. Rose had great confidence in Howard but why it took Brig. Gen. Russell so long to fire him remains a mystery.

The enigmatic John Black Grant, arguably the most intellectual figure on the Health Division staff, worked closely with Heiser and was much influenced by him (Fig. 1.2c). This self-styled "Rockefeller bolshevik" was the China-born son of Canadian Baptist medical missionaries. He had returned to Canada to attend Acadia University, a small Baptist institution in Nova Scotia, from which he graduated in 1912, after an undistinguished and unhappy time. Described by classmates as dour, humorless, rude and cynical, he was suspended for one year after accompanying the college ice-hockey team to an away game when forbidden to do so because of low grades. Nevertheless, he easily graduated from the University of

Michigan's School of Medicine in 1917 and, as a Chinese-speaker was eagerly recruited by the Health Board.[27]

After a period of training in the county health departments of North Carolina, Grant was posted to Puerto Rico and Santo Domingo for hookworm and malaria surveys. In 1921, like others in the organization, he spent time at the Johns Hopkins School of Hygiene, where he came into contact with Sir Arthur Newsholme, visiting professor of public health administration from England, and began reading the literature of the Fabians, a British socialist organization. Somewhat radicalized, he was loaned by the Health Division to Peking Union Medical College—the Johns Hopkins of China—where he became associate professor of hygiene and public health, before moving on to become director of the All-India Institute of Hygiene in Calcutta.[28] Grant certainly was not a bolshevik, but he was one of the

**FIGURE 1.2c.** John Black Grant (courtesy of the Rockefeller Archive Center).

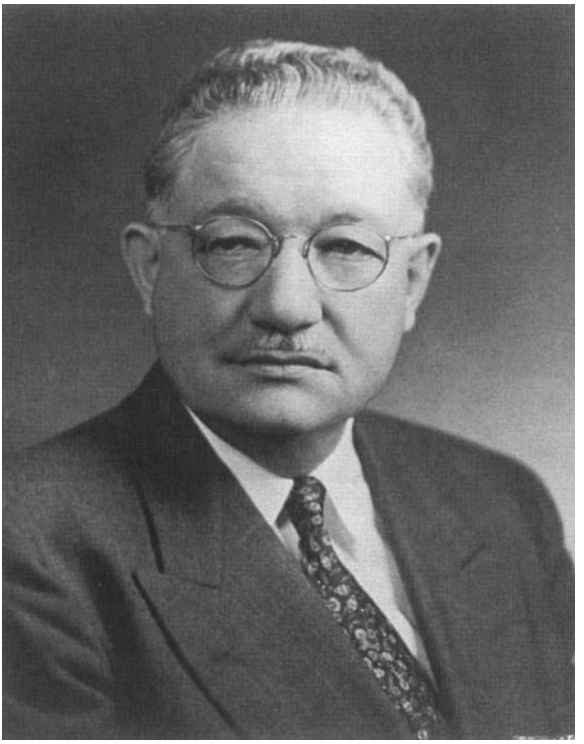

**FIGURE 1.2d.** Fred Soper (courtesy of the Rockefeller Archive Center).

few in the Health Division who believed that health care went beyond medical care, and believed that good health rested on an equitable social and economic system.

Then there was Fred Soper who became closely linked to yellow fever and malaria work following his appointment as regional director of the IHD for South America, based in Rio de Janeiro (Fig. 1.2d). Described at one time as "perhaps the most successful field general the Rockefeller Foundation ever had,"[29] he was a 1918 graduate of the Rush Medical College in Chicago, later gaining a doctorate in public health from Johns Hopkins. Fred Soper, Malcolm Gladwell correctly noted, "was the General Patton of entomology."[30] Like Patton, he evoked strong reactions in others, both positive and negative, particularly during his campaigns against typhus and malaria during World War II. He resigned from the Division in 1947, perhaps somewhat under a cloud, to become director of the Pan American Sanitary Bureau which had been formed by the U.S. government in 1924. He later became the driving force behind the WHO's Global Malaria Eradication Program.

Paul Russell, author of *Man's Mastery of Malaria*, who became one of the division's leading experts on malaria, was born in Boston in 1894 (Fig. 1.2e). He obtained his medical degree in 1921 from Cornell and joined the Health Board in 1923, straight out of a two-year internship at the Bellevue Hospital in New York. After working on the malaria problem in the South, he spent three years in the Singapore region, and earned a master's degree from the Harvard School of Public Health, before being assigned to malaria work in the Philippines and India. He joined the U.S. Army Medical Corps in 1942 and served under General MacArthur in the Phillippines, one of the few Allied generals who understood the danger of malaria to fighting troops. In 1943 he was transferred to the Mediterranean region to take over malaria control in Italy, and then became head of the department of parasitology in the Army Medical School. He returned to the Health Division in 1947 where he served as an expert advisor on malaria and, like Soper, played a major role in the WHO's malaria program.

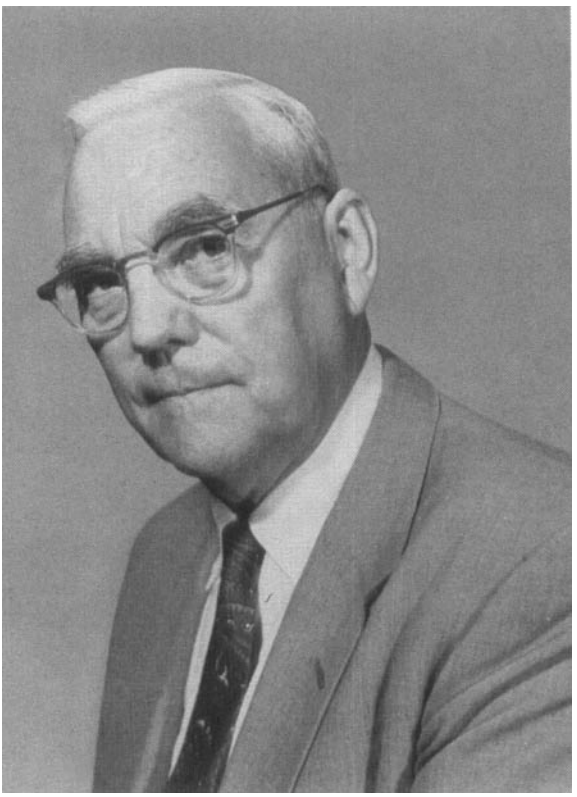

**FIGURE 1.2e.** Paul Russell (courtesy of the Rockefeller Archive Center).

Lewis Hackett, another of the Health Division's major field officers, was born in California (Fig. 1.2f). He graduated from the Harvard Medical School in 1912 and a year later gained that university's first diploma in public health. Snatched up by the Health Commission, he had his first experience working on hookworm in Central America before taking over the Brazilian program in 1916. Subsequently, he was posted to Rome to run the Italian malaria campaign and, in 1940, to Buenos Aires as director of the Rio de la Plata and Andean Region. Hackett and his much beloved wife, Hazel (née Woods), were perhaps the best-liked couple in the organization. He had a wonderful sense of humor, which best displays itself in the many hundreds of letters he wrote. In writing to a friend Ira about their

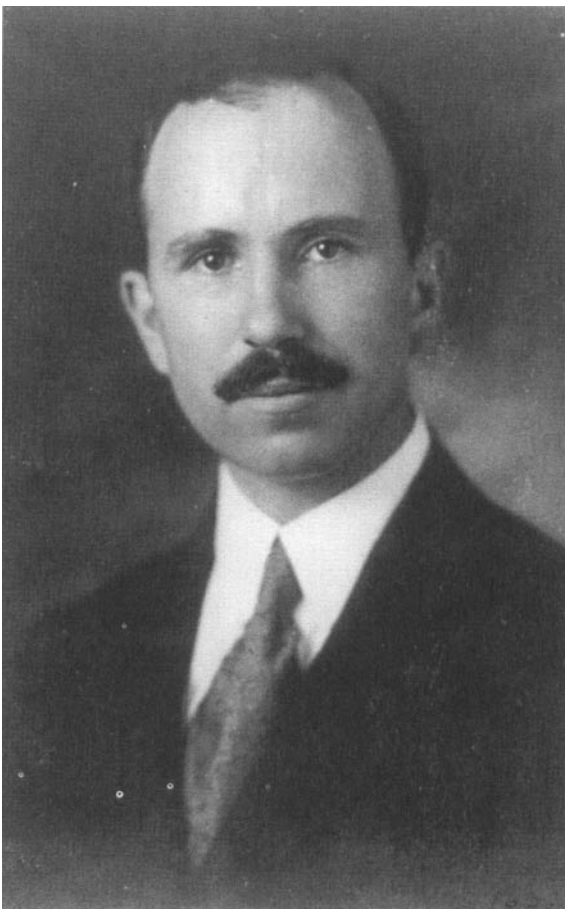

**FIGURE 1.2f.** Lewis Hackett (courtesy of the Rockefeller Archive Center).

experiences in Brazil during the First World War when, at least according to the Hacketts, the police routinely arrested blondes because of anti-German feelings, he told the story of a poor blond Dutch couple who were arrested while camping in the woods near to Rio and accused of being spies. They got off only with difficulty, Hackett relates, "as the prosecutor very reasonably argued, no one would leave a comfortable house in the city to carry food and utensils into an uninhabited forest full of snakes, and there to suffer the inclemencies of the weather and eat badly cooked food, and sleep on a most uncomfortable bed on the ground unprotected absolutely from the night air unless he was a spy."[31]

These, then, were some of the more important medical barons who ran the Health Division. And run it they did. Their strengths were to be found in smoothly-operating and well-organized medical campaigns. Whether it was the "intensive method" hookworm campaigns, the use of the chemical Paris green against malaria vectors, or the well-orchestrated searches for *Aedes aegypti*, the vector of yellow fever, the same techniques were used wherever the field officers operated. The discovery of jungle yellow fever and their work on the yellow fever vaccines can also be seen as part of this general medical competence. But they had problems, one of them being the recurrent inability to measure the success or failure of their disease campaigns.

Because of the decentralized nature of the Division's operations, there seems to have been little long term planning; in retrospect, many of their decisions appear ad hoc and haphazard. What was apparently a decision to build schools of hygiene in poorer countries was suddenly reversed and schools were opened instead in Europe; only one of these—in London—had any real link with the poorer countries. Heiser accidentally ran into a Japanese official in Geneva; that meeting led to a school of hygiene being built in a country with little need of Rockefeller support. Russell thought highly of Eugene Opie and decided once again to support work on tuberculosis, despite having vowed only a few years before never to do so again. With few exceptions one does not gain the impression that the officers in New York sat down to develop a long-range program in any field. A strong-willed person like Soper could push the division, with relative ease, to undertake tasks which he was personally enthusiastic about.

How much did all this cost? In total, the Health Division and its predecessors spent the equivalent of $664,968,000 in 1990 U.S. dollars, an average of nearly $18 million per year, with the peak amounts of over $25 million being spent yearly between 1925 and 1935. Given that this money was spread over 80 countries, one can argue that the Health Division spread its resources too thinly and tried to do too much.

As Table 1.2 shows, when the Health Division closed in 1951, it had spent 31% of its total budget on attempts to control or eliminate diseases,[32] 19% on the endowment and maintenance of schools of hygiene and nursing, and 8% on fellow-

**TABLE 1.2.** The budget distribution in percentages

| | ALL YEARS | ROSE (1914–23) | RUSSELL (1924–34) | SAWYER (1935–44) | STRODE (1945–50) |
|---|---|---|---|---|---|
| Diseases | 31% | 70% | 27% | 25% | 16% |
| Administration | 25% | 14% | 20% | 31% | 35% |
| Health Education | 19% | 4% | 26% | 19% | 10% |
| Fellowships | 8% | 4% | 9% | 8% | 8% |
| Health Units | 7% | 5% | 11% | 4% | 7% |
| Laboratories | 4% | 1% | 2% | 7% | 10% |
| Health Commission | 1% | 0% | 0% | 3% | 10% |
| Other | 4% | 2% | 5% | 3% | 4% |

Source: Annual reports of the International Health Commission, Board, and Division.[32]

ships for public health physicians and nurses. Another 12% went to county health units, laboratories and the so-called Health Commission (not to be confused with the original International Health Commission) that was formed in 1940 to fund wartime activities.

Ninety percent of the disease budget was spent on the big four—hookworm, yellow fever, malaria, and tuberculosis—although the amount spent on these individually varied widely between directors (Table 1.3).

After World War II the Health Division attempted to continue as if the world had not changed in any fundamental way. But the world *had* changed. Public health, for example, had been drawn into debates over social, economic and cultural problems as many countries began to put in place a welfare state. At the same time the birth of important new public health agencies, such as the WHO, indicated that action to solve public health problems would move beyond national boundaries. The Health Division was unable to adapt to these changes and opted for the status quo. While many officers of the Rockefeller Foundation were aware of the Health Division's inertia, they were equally unable to suggest a new model. In 1951, the Rockefeller Foundation was reorganized in a most unimaginative way and the Health Division closed. Nevertheless, many of its ideas lived on and found expression during the early years of the WHO as it took on the task of eradicating disease.

**TABLE 1.3.** Percentage distribution of disease budget

| YEARS | HOOKWORM | YELLOW FEVER | MALARIA | TUBERCULOSIS | OTHER |
|---|---|---|---|---|---|
| Rose | 51% | 14% | 10% | 25% | 0% |
| Russell | 15% | 53% | 24% | 5% | 4% |
| Sawyer | 1% | 40% | 27% | 7% | 26% |
| Strode | 0% | 35% | 30% | 4% | 32% |
| TOTAL | 22% | 37% | 20% | 12% | 9% |

## Notes

*Throughout the notes, the Rockefeller Archive Center, site of many of the documents, has been indicated by RAC.*

1. P. Russell, *Man's Mastery of Malaria* (London: Oxford University Press, 1955).
2. This is not the first attempt to write a history of the International Health Division. In 1950, Lewis Hackett, who had recently retired after a lifetime of field service in the organization, was commissioned by the Rockefeller Foundation to write the division's history. On his death in 1962 he left behind an incomplete 600-page, 10-chapter manuscript, *History of the International Health Division.* Anxious to see the work finished, the foundation approached Greer Williams, then director of development at the Boston Children's Hospital Medical Center, who, by 1966 had completed an eight-part, manuscript entitled *The Friendly Americans: The Story of the Rockefeller Foundation in World Health.* Little, Brown and Company rejected the manuscript as being fit only for specialist readers and not a viable publishing project for them.

   What was the next step, the foundation wondered? Anxious to buttress the Rockefeller name and bring their work to the attention of the public, they opted for a "drastically shortened version," and Greer Williams agreed "to take the choice cuts . . . and throw the carcass away." The shortened version, which Scribner's had agreed to publish as *The Plague Killers*, was restricted to a popular account of the campaigns against hookworm, malaria and yellow fever. Although described by the Book-of-the Month Club News as "a monument to the Rockefeller Foundation," it no longer bore any resemblance to a history of the Health Division. Indeed the chapters on hookworm had been savaged by Scribner's with a comment that the original version had "too many references to privies and to the Rockefeller Foundation."

   Neither Hackett's nor Greer Williams's manuscripts are based on the copious correspondence and quarterly field reports now housed in the Rockefeller Archives. Hackett, whose manuscript is far superior to that of Williams, relied on the organization's annual reports and his own intimate knowledge of what had gone on; Williams relied to a large extent on personal interviews. In both cases their early chapters have now been rendered obsolete by the work of John Ettling on the Sanitary Commission and of Elizabeth Fee on the Johns Hopkins School of Hygiene and Public Health.
3. The name change in 1916 was of little consequence. It was done merely to indicate a broad and continuous mandate; the name "commission" had implied a precise and limited function, namely, the eradication of hookworm. The name change in 1927, however, was of great significance; it was tied to a change in the way the organization was governed.
4. There are many biographies of the Rockefellers, none better than Ron Chernow, *Titan: The Life of John D. Rockefeller, Sr.* (New York: Random House, 1998); and J. Harr and P. Johnson, *The Rockefeller Century* (New York: Scribner, 1988).
5. F. Gates, "The Memoirs of Frederick T. Gates," *American Heritage* 6 (1955): 71–86.
6. G. Corner, *A History of the Rockefeller Institute. 1901–1953. Origins and Growth* (New York: Rockefeller Institute Press, 1964).
7. Original Deed of Trust. RAC. RG.2 B.24 f.241, 242. For details of founding see Raymond Fosdick, *The Story of the Rockefeller Foundation* (New York: Harper, 1952).
8. Minutes of a Meeting of The Rockefeller Commission for the Eradication of Hookworm Disease, October 26, 1909. RAC. RG.2 B.53 f.546. The history of the Sanitary Commission is told in John Ettling, *The Germ of Laziness: Rockefeller Philanthropy and Public Health in the New South* (Cambridge, Mass: Harvard University Press,

1981). W. Link, *The Paradox of Southern Progressivism 1880–1930* (Chapel Hill: University of North Carolina Press, 1992) puts the history of the commission within a broader framework of southern history. My discussions on this organization are taken basically from these books.

9. Details of his appointment in R. Acheson, *Wickliffe Rose of the Rockefeller Foundation: 1896–1914 The Formative Years* (Cambridge: Killycarn Press, 1992). Rose was their second choice; the first, J. G Joyner of North Carolina, declined the offer.
10. Hackett MS, *History*, Chap. 3, p. 31. RAC. RG.3 S.908 B.5 f.30.
11. Details of Rose's life can be gleaned from Ettling's *The Germ of Laziness*, the Hackett MS and, more recently, Roy Acheson, *Wickliffe Rose*.
12. The struggle to incorporate the Rockefeller Foundation is told in Fosdick, *The Story of the Rockefeller Foundation*.
13. *New York Times*, January 18, 1913.
14. *Hookworm Infection in Foreign Countries* (Washington: Rockefeller Sanitary Commission, 1911).
15. The original plan was presented in "Outline Plan for Cooperating in the Work of Relief and Control of Uncinariasis" RAC. RG.3 S.908 B.12 f.128. The original resolutions setting up the International Health Commission were approved at the foundation's meeting of June 27th, 1913. RG.3 S.908 B.11 f.123.
16. Gates, quoted in Fosdick, *History of the Rockefeller Foundation*, p. 23.
17. Quotation from W. Link, *The Paradox of Southern Progressivism*, p. 148.
18. W. Rose to L. Hackett, April 25, 1917. RAC. RG.1.1 S.305 B.15 f.133.
19. Original document, January 27, 1913. RAC. RG.3 S.908 B.11 f.123.
20. Hackett, MS. *History*. Chap. 9, p. 23.
21. R. Kohler, *Partners in Science: Foundations and Natural Scientists 1900–1945* (Chicago: University of Chicago Press, 1991).
22. In 1904, the first attempt to vaccinate American troops against typhoid with a dead typhoid bacilli oral vaccine had ended in disaster; publication of the results was prohibited by the U.S. Army for reasons of "public policy." In 1959 it was finally revealed that 7 of the 12 volunteers involved in the test became ill with typhoid, some of them severely; three suffered a fever. The flask containing the bacilli had not been sterilized correctly, and, according to Edward Vedder, who was James Carroll's assistant, Carroll had refused to test the flask by plating before use and instead tried to put the blame on Vedder. W. D. Tigertt, "The Initial Effort to Immunize American Soldier Volunteers with Typhoid Vaccine." *Military Medicine* 124 (1959): 342–349.
23. Details of this program from *Report of the Surgeon General U.S. Army*, for 1909, 1910, 1911 and 1912 ; F. Russell, "Antityphoid vaccination." *Amer. J. Med. Sci.* 1913: 803–833; "Immunization to Typhoid Fever." *Amer. J. Hyg. Monograph Series 17* (Baltimore: Johns Hopkins, 1941); R. Henry, *The Armed Forces Institute of Pathology. Its First Century 1862–1962* (Washington: Office of the Surgeon General, 1964). I would like to thank Colonel Robert Joy for drawing my attention to this and for sending copies of these articles to me.
24. T. Whayne, Memoir of Frederick Fuller Russell (1870–1960), *Trans. Coll. Physicians Philadelphia* 30 July, 1962.
25. Quoted in Greer William's Notes on Fred Russell. RAC. RG.3.1 S.908 B.7 f.8694.
26. W. Rose. Diary, June, 1914. RAC. Heiser tells his story in V. Heiser, *An American Doctor's Odyssey. Adventures in Forty-Five Countries*. (New York: W. Norton, 1936). Fred Russell's name is conspicuously absent from this book.
27. J. Farley. "John Black Grant '12: The Rockefeller Bolshevik," *Acadia University*

*Alumni Bulletin*, 74 (1990): 10–12; Reminiscences of Dr. John B. Grant. Transcript of interviews conducted by Saul Benison for the Oral History Research Office, Columbia University. Copies also in RAC. S.900 Hist. Grant, Vols. 1–7. Many Grant memorabilia exist in the archives of Acadia University, Nova Scotia. This includes a family history including his own life and a MS entitled "Around the World in 81 Days," written by his second wife, Denise Grant.

28. The history of PUMC has been extensively covered. Mary Ferguson, *China Medical Board and Peking Union Medical College* (New York: China Medical Board, 1970); John Bowers, *Western Medicine in a Chinese Palace: Peking Union Medical College, 1917–51* (New York: John Macy Foundation, 1972); and especially by Mary Bullock, *An American Transplant: The Rockefeller Foundation and Peking Union Medical College* (Berkeley: University of California Press, 1980).
29. *Rockefeller Foundation Illustrated.* Vol. 2, 1974. Details of Soper's life and work can be found in the Fred Soper Manuscripts, NLM Bethesda. Details in J. A. Kerr (ed.), *Building the Health Bridge. Selections from the works of Fred Soper* (Bloomington: Indiana University Press, 1970) ; J. Duffy (ed.), *Ventures in World Health. The Memoirs of Fred Soper* (Pan American Health Organization, 1977).
30. M. Gladwell, "The Mosquito Killer." *New Yorker*, July 2, 2001, 43–51.
31. L. Hackett to Ira, May 26, 1918. Hackett Correspondence 1902–1919. RAC. Acc 33 B.1 f.1.
32. All budget figures have been taken from the yearly expenditures published annually in the Annual Reports of the International Health Commission, Board, and Division. Unfortunately I had to manipulate these figures to some extent because over the years changes were made in the way the budget statements were prepared. Also, endowment money for schools of hygiene was often charged directly to the Rockefeller Foundation rather than to the Health Division.

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# I

# ROSE'S VISION

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# 2

## The First Stage: Means to an End

When the International Health Commission was formed in 1913, the Rockefeller Sanitary Commission had been in operation for three years. All seemed set for it to continue its work in the South while the Health Commission took hookworm campaigns to other countries. But that was not to occur; the Sanitary Commission was suddenly closed down in December 1914 and the Health Commission left to pick up the pieces in the South.

In the early years, in the South, the Health Commission operated as originally intended by its first director, Wickliffe Rose, as a copy of the Sanitary Commission. These early campaigns, whether directed by the Sanitary Commission or by the Health Commission, were not directed at the eradication of hookworm per se. Instead, hookworm was a means to an end, a way to educate the public, physicians, and politicians in public health with the hope that professionally-run public health services would develop. But there were those, such as Frederick Gates, Rockefeller's advisor on philanthropy, who believed that its goal was indeed the eradication of hookworm. These two ideas, one focused on education and public health, the other on the eradication of disease, were to coexist uncomfortably during the entire life of the organization. The story begins with hookworm. What was the nature of the disease which first drew Rockefeller and his son into public health work?

## Hookworm

Ancylostomiasis, uncinariasis, or hookworm disease, is caused by parasitic nematode worms, either *Ancylostoma duodenale* or *Necator americanus*. These worms, which are about 1 cm. long, attach to the gut wall by means of teeth (in *duodenale*) or cutting ridges (in *americana*) on the inside of their mouths, and were known to be the cause of severe anemia, circulatory disorders, weakness, emaciation, and mental retardation. Worm infestation also brought on a perverted appetite, which often led to such acts as "dirt eating." First described in the mid-nineteenth century, the worms' pathological effects had been dramatically displayed to Europeans when, between 1872 and 1882, Italian miners became infected during the digging of the 15-km.-long St Gotthard tunnel between Göschenen and Airolo on the Milan to Arth-Goldau rail line.[1] But the disease was best known in the tropics. It was, the governor of British Guiana noted in a 1901 memorandum to the British Colonial Office, "an important disease from the standpoint of the employer of native labour," for it imposed financial hardships on the employers because of the sickness and inefficiency of coolies, wrought by the hookworm.[2]

The parasite's life cycle, and a chemical cure, were known, and the disease seemed both preventable and curable. The hookworm's larvae, which hatch from the worm eggs extruded in an infected person's feces, go through various moults if deposited in warm, damp, sandy soil. After the third moult the larvae re-enter the human body through the mouth, or, most often, by boring through the skin of a barefooted individual. They travel to the lungs, pass up the windpipe, and are finally swallowed to mature in the gut.[3] Two drugs were routinely used against the worms: an extract of male fern, or a rather ferocious combination of the potentially dangerous poison, thymol, and the purgative, Epsom salts.[4]

The parasite remained virtually unknown to North Americans before the Spanish-American War. But with the occupation of Puerto Rico in 1898, the army surgeon, Bailey Ashford, discovered that many anemic farm workers were infected with hookworm.[5] He sent specimens to his former teacher Charles Stiles, a parasitologist with USPHS, who, after a search, found hookworm in the District of Columbia, North and South Carolina, Georgia, and Florida. "There is, in fact, not the slightest room for doubt," Stiles noted in a preliminary report of 1902, "that much of the trouble popularly attributed to 'dirt-eating' and 'resin-chewing' and even some of the proverbial laziness of the poorer classes of the white population are in reality manifestations of uncinariasis." He added, "remove the disease and they can develop ambition."[6]

Stiles set out on a rather futile one–man campaign to cure Southerners of hookworm, and to prevent reinfection by the construction of sanitary privies and the wearing of shoes. In the process, Stiles' work came to the attention of Frederick Gates who, with his strong interest in medical matters, arranged for Stiles to give him a private lecture and demonstration on hookworm.[7] Gates realized, as Stiles

did not, that to succeed they would need to woo southern leaders. An invitation to meet in Rockefeller Sr.'s office on that October day in 1909 went out not only to William Welch, Professor of Pathology at Johns Hopkins Medical School, and Simon Flexner, director of the Rockefeller Institute for Medical Research, but to many Southern educators. Assembled in New York, they agreed that a commission should be formed, and optimistically concluded that hookworm "can be easily recognized, readily and effectively treated, and by simple and proper sanitary precautions successfully prevented." It all seemed relatively simple, and five years a satisfactory time scale in which to complete their mission.[8]

## The Sanitary Commission

The Rockefeller Sanitary Commission began its operations in Virginia, in 1910, and proceeded in three stages: first, conduct a survey; second, cure the disease in mobile county–dispensaries (see Fig. 2.1); and third, provide illustrated lectures, in an effort to cajole householders into building sanitary privies to stop soil pollution (the delicate name given defecation on the ground), and awaken interest in public health. The Sanitary Commission hoped by these means to encourage the development of government-run scientific public health systems for the cure and prevention of all preventable diseases. By 1914, over 1 million people had been examined in these dispensaries and 440,000 of them had been found to harbor the hookworm parasites. But before the disease could be conquered, Lewis Hackett noted, "every last person in the community had to appreciate the danger and be enlisted for the battle."[9]

This was to be the education-centered model for Rose's vision of the International Health Commission and, by the time it was formed, Rose seemed satisfied with the progress already made by the Sanitary Commission. From his perspective as an educator, southern politicians, physicians, and press seemed to be aware of the hookworm problem. Schools across the South were teaching students about the dangers of soil pollution; medical students at Tulane University in New Orleans, perhaps the finest medical school in the South, were being taught about the disease. More significantly, Rose noted, twelve counties in three states had appointed full-time salaried health officers. Hopefully, such professionals would slowly replace current part-time physicians, untrained in public health work, who generally failed to report births, deaths, and the occurrence of contagious diseases.[10] All seemed set for the Health Commission to extend to other lands what had seemed so successful in the South.

## The End of the Sanitary Commission

But it did not happen that way. With Rose en route to Egypt, Ceylon, and points east to prepare for a second wave of hookworm campaigns in countries that were part of the British Empire, Fred Gates called together the executive committee of


**FIGURE 2.1.** Mobile county dispensary, Sanitary Commission (courtesy of the Rockefeller Archive Center).

the Health Commission and announced that the Sanitary Commission would be closed down in 1914. He was strictly within his rights; at the original meeting of the Sanitary Commission John D. Rockefeller Sr. had promised up to 1 million dollars for an aggressive five-year campaign, and the five years were about to end. But the timing was bad and the method clumsy.

Later in the year, Rockefeller informed the executive of the Sanitary Commission, in a letter that was probably dictated by Gates, that "the chief purpose of the Commission may . . . be deemed to have been accomplished" and ordered transfer of its records and supplies to the foundation by the end of the year.[11] Rose, who had returned from his overseas mission, agreed that much had been accomplished, but suggested that much more needed to be done.[12]

Gates had written to Rockefeller outlining his reasons for wanting the Sanitary Commission closed.[13] Rose he said, had not only reduced hookworm to "one of the minor infections of the south, perhaps the most easily and universally recognized and cured of all," but had awakened the boards of health in all the southern states, had shown them models of effective state organization, and had enlisted local physicians in every county of every state. Gates grossly overstated the situation; indeed Rose had ruefully complained in his 1914 report that only 8000 out of 27,000 physicians had agreed to report hookworm cases.

Overstated as it was, this letter left Rose exposed. The Sanitary Commission had, according to Gates, aroused state boards of health and that, according to Rose,

had been its goal all along. Whether there was still a hookworm problem or not was beside the point; the disease was to be passed into the hands of awakened state authorities. How could Rose complain?

But a far more contentious issue pushed its way into the letter, which illustrated just how distant Gates and Rose were in their thinking. "Not to be confounded with this local, temporary mastery of a single curable disease, even in thought," Gates wrote, "is the mighty question of national health, sanitation, preventive medicine." Not to be confounded—what must Rose have thought? To Gates, the very essence of Rose's two commissions—that they existed as a means to educate the public into the necessity of public health—was dismissed as "this greatest and most difficult of enterprises, a thousand years will not accomplish." Gates believed that the foundation should, with circumspection, become involved with public health, but that required a different organization. As far as Gates was concerned the Rockefeller organization had already taken its first major step towards the improvement of public health in the country by opening the Rockefeller Institute of Medical Research; the Sanitary Commission, in contrast, was concerned only with hookworm.

Gates's letter should perhaps have initiated a debate over the role of the International Health Commission, then in its very early stages, and one would perhaps have expected Rose to demand one. But the ever-retiring Rose backed away, and the Sanitary Commission closed on December 31, 1914, leaving the crucial issue unresolved. The work in the South against hookworm, however, did not end with the closure of the Sanitary Commission. In 1916 the International Health Board simply took over where the Sanitary Commission had left off, and began to fund county health units whose major concern was with hookworm.

## County Health Units

In 1916, the Health Board established its first county health unit in North Carolina, and it became the opening wedge in the Health Board's long-term goal for the South. One sees in these southern county health units Rose's vision of the Health Board at work. Although directed primarily at hookworm, they were educational in scope, awakening people to the importance of public health and hygiene.[14]

North Carolina was not a healthy place in those days. Its population, 85% of which was rural, suffered from "low-ebb vitality" brought about by chronic, devitalizing and crippling diseases such as malaria and hookworm, and by constipation, gum problems, childhood adenoids and bad teeth. These are not killers, a 1918 report noted, but "like our financial losses, are in driblets, pennies, nickels, dimes, and an occasional quarter—in little drafts, long continued."[15] And the state had an annual death rate exceeded only by that of Kentucky. As a typhoid pamphlet noted: "Thank the Lord for Kentucky."[16]

The Health Board–supported hookworm campaigns in North Carolina used the "intensive method" developed a few years earlier on Knott's Island, an isolated

village community situated off the coast of the state. This was an attempt to measure the prevalence of hookworm in an isolated community and to ascertain whether it could actually be freed from the parasite. On a small island, the entire population could, in theory, be examined, treated and reexamined to determine the percentage cured. Thus, in 1913, 560 of the 567 people on the island were examined; 93 were found with hookworm eggs in their feces and given repeated treatments with thymol until cured. Of the 93 treated, 20 remained positive after one treatment, nine after two and only one after three.[17] The authors of the report felt that they had practically eliminated hookworm from the island and that the intensive method could be used elsewhere.

The cure was unpleasant. Patients were required to fast the previous evening and to purge themselves with a large dose of Epsom salts. Early the following morning, again without eating, they were required to take 20 to 30 grains of thymol at 6 am and again at 8 am. At 11 am they consumed a large dose of Epsom salts, which fully flushed out their bowels and the contained hookworms. After a second cure, one week later, their feces were reexamined; if eggs were found they were required to repeat the cure until their feces cleared.

As a result of this work, Watson Rankin, secretary of the North Carolina State board of health, made a formal request to the International Health Board to share the cost, on a gradually decreasing basis, of 10 county health units for three years. During the first year these units would aim to prevent soil pollution, and in subsequent years would gradually broaden the scope of work so as to eventually develop a "well-rounded county department of health." In the memorandum of agreement, signed on April 17, 1917, the long-term goal of the work against soil pollution was made clear; public education was at the forefront.

> The ultimate purpose of the work against soil pollution of the International Health Board has been and is to demonstrate disease preventability to the people of local governments, in order that they may be influenced to organize and maintain effective local agencies for disease prevention in general.[18]

## The Northampton County Health Department

Northampton County, on the northern boundary of the state within the coastal plain, was the site of one of these health units. It opened its doors in August 1917 to deal primarily with hookworm, soil pollution and typhoid vaccinations. After a few months it expanded to include a hookworm unit, a soil pollution unit, a school inspection unit, a quarantine unit, an influenza unit, an antityphoid vaccination unit, an infant hygiene unit, and even a life extension unit, which promised every adult a free medical examination in order to "prolong life, and to make the quality of living better."[19]

Typical of county health units throughout the South, Northampton County operated mainly through the schools. Each school was expected to have fly-proof

privies, screening against mosquitoes, adequate ventilation, and no communal drinking-cups. In addition, pupils would be taught personal hygiene, and the causes and prevention of disease.

The hookworm unit required distribution of labeled specimen boxes to pupils to be returned with fecal contents wrapped in a piece of paper. This material was then mailed (!) to the Health Department's laboratory in Raleigh and the parents of those infected notified and asked to allow their children to be treated. According to the report, over 90% of those with infected children brought them in for treatment.

Other stratagems were also employed. Children's competitive urges were encouraged by contests in which prizes were awarded for the sanitary improvements in school and at home, and for the best essays on sanitation. "We are putting on this contest," a circular informed the teacher, "in hopes that it will better the sanitary condition of your school and your school community, as well as help the child in a literary way." Contesting schools were graded according to the number of sanitary privies (400 points each), quality of the water supply, and the number of pupils examined for hookworm (100 points each). Fewer points went for school ventilation, hand-washing arrangements, the size and use made of playgrounds, and the number of children vaccinated against typhoid and smallpox. Children were encouraged to enter essay contests, in which they could accumulate up to 500 points. They could gain an extra 500 points for having a sanitary privy and sanitary well in the home, for using a toothbrush daily, for receiving typhoid and smallpox vaccinations, and for being examined for hookworm

As part of the interschool competition, children were asked to complete a fairly intimate survey about their home life. "These questions," the survey form noted, "are not asked for idle curiosity, but simply to teach the children practical sanitation." Is your house screened, the questionnaire asked, and do you have a privy and a well, and how far away is the hog pen? How many members of your family have been vaccinated, or examined for hookworm, do they have tuberculosis, and what measures are taken against flies, mosquitoes, and malaria? Where do you sleep , how many of your family use toothbrushes, and do you believe flies carry typhoid and mosquitoes carry malaria?[20]

Three five-dollar U.S. government thrift bonds were awarded for the two best essays from white pupils in each grade; third- and fourth-place winners each received a year's subscription to the *Roanoke-Chowan Times* and the *Northampton Progress*. Black students were prohibited from entering the essay competition although thrift bonds were given to black schools and black households that had made the most sanitary improvements. The whole operation appealed to Mr. P. J. Long, the school superintendent, "as a splendid educational enterprise both for the school and the community," hinting broadly that he could not see "how any right-spirited teacher can be indifferent to this generous offer," and warning that failure to cooperate would indicate "a poor professional attitude."

On April 18, 1917, the governor of North Carolina and health and educational officials gathered for the first time to honor the prize winners at a ceremony advertised as WAR PROGRAM on GERMS and GERMANS (Our Two Greatest Enemies) where they heard Miss Ella Outland and Miss Bettie Long (one wonders whether she was related to the school superintendent) read their essays on "Sanitation in Home and School," and the governor address the crowd on "War Savings and Sanitation."[21]

The quarantine unit was set up to protect the community against contagious diseases. People were told it was "a religious duty and a duty to their neighbors to have their houses placarded when they have contagious diseases." Physicians were encouraged to report such cases. The schoolteacher was an important element in this unit as well. Apart from teaching her pupils about contagious diseases, she was asked to appoint two girls and two boys as health officers for the week. Each Saturday the officers would forward a completed questionnaire about their school to the county health department and, once again, the best report would win a prize. Teachers in the early grades were required to organize a Better Health Company with a captain elected for each week. Every morning the captain would call the roll and ask each student whether they had brushed their teeth night and morning, washed their hands before each meal, cleaned their finger nails before coming to school and slept with their windows open.

Those found to have a contagious disease would receive a set of rules and regulations as per chapter 263 of the 1917 Public Laws of North Carolina. "I wish to appeal to you as a law-abiding citizen and as one who has due consideration for the health and possibly the lives of your neighbors and your community," the covering letter from the quarantine officer said, "to give the county and the state . . . your full cooperation." And then in a final flourish, the officer reminded them: "Whatsoever ye would that men should do unto you, do ye even so to them: for this is the law."[22]

Weekly newspaper articles endorsed the healthy life; "Little Journeys Along the Byways and Highways of Disease and Health," the Northampton county health officer called his series of articles. Obey the Sanitary Privy Ordinance Law, one article intoned, and no more lives would be lost in vain.[23] This law, passed in 1918, required each house to have a sewage closet or a "decent, properly located and fly proof sanitary privey (*sic*)" with the threat of a minimum five-dollar fine for the first offense, and up to $50 or 30 days imprisonment for repeated violations. Once built, of course, these privies had to be inspected by a board of health representative and have a seal of approval attached (see Figs. 2.2a, b). "It is a duty," the approval seal noted, "to keep this privy sanitary in order to protect yourself and your family, and your community from filth diseases."[24]

How successful were these campaigns and how was success to be measured? In public health campaigns, success comes over the long haul, but the authorities

**FIGURE 2.2a.** Unsatisfactory privies (courtesy of the Rockefeller Archive Center).

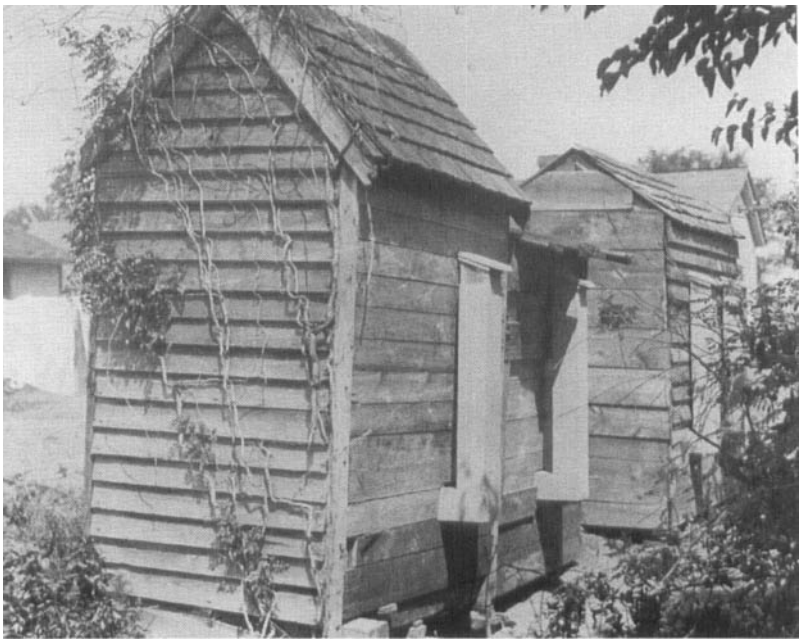

**FIGURE 2.2b.** Satisfactory privies (courtesy of the Rockefeller Archive Center).

must have felt satisfied to learn that 12,525 people showed up for 144 lectures and 17,525 publicity notices and 3963 letters were mailed out. (Who actually made these bizarre counts remains a mystery) The erection of privies, perhaps the most tangible sign of progress, was also encouraging if not somewhat suspect. In Seaboard Township, for example, there was only one sanitary privy shown on the map drawn for August, 1917, whereas only 17 months later the map indicates approximately 150 had been built, mostly of the pit-privy variety. But if success was to be measured by the numbers cured of hookworm, then the campaign was less successful. Of the 25,000 people surveyed, fully 22,480 refused even a first examination. Of the 2452 examined, 969 carried hookworm eggs but only 747 of them accepted the first treatment, 699 of whom were reported to have been cured. Clearly there was still much to be done in Northampton County.[25]

Despite these poor hookworm figures, the authorities seemed satisfied. We have become the leading state in the fight for health conservation, the *Raleigh News and Observer* noted in March 1919. By the mere payment of only 40 cents per head "privy-tax," the Privy Law, the article claimed, will save 300 to 500 lives and improve the health of 6000 to 7000 per year.[26] That seemed like a good investment.

But how good an investment was it? Between 1920 and 1924 attempts to answer such questions peaked when Watson Rankin, encouraged by John Ferrell of the Health Board, began to calculate "unit costs" and "cost equivalents" to calculate the financial return from each dollar invested in a health unit. Thus, if the state's tuberculosis work had cost $100,000 and if Rankin had assumed that this work had saved 100 lives at an economic value of $4000 per life, giving an equivalent cost of $400,000, then the rate of return would have been four; four dollars for every dollar invested. By such bizarre means Rankin actually worked out that the county health unit of Northampton County, which had cost $64,036.64 to run, actually returned $277,026.30; a return of $4.33 per dollar invested. This "cost equivalent" rested on a whole set of questionable assumptions. Every lecture returned 10 cents per head, every newspaper article $5.00, every sanitary privy $5.00. Typhoid vaccinations were assumed to have saved 17 lives at $4000 each and to have been responsible for 154 recoveries at $200 each. No capitalist would have been disappointed at such a return on his investment.[27]

In 1923 the health forms were altered in such a way that county could be compared with county, and thus listed in order of "earnings per dollar invested." The extraordinary faith that many placed in such manufactured numbers was made very clear. "These earnings on the dollar expended," Rankin proclaimed, "furnishes a common denominator, a uniform basis of comparison, for local health work regardless of variations in size, population, budget, or health problems of the different counties." More worrying to those involved in the actual work, it was said to provide an index "to the energy of the personnel."[28] The following year the intense intercounty competition generated by such numbers decreased somewhat when the state was divided into east and west districts. As a result the

west, which showed a rate of return of $1.69, must have felt itself to be more energetic, efficient and industrious than the east, which returned only $1.60. By 1925, when Rankin resigned, these heinous comparisons ceased although rates of return were still calculated through to 1926. Among those in the Health Board, Ferrell, at least, supported these forays into a mathematical never-never land. As Ferrell noted of county health work, " the time will come when they [the people] will demand clear-cut evidence of its value just as stockholders in a company demand dividends on investments."[29] However Rankin's attempt to assign the job of calculating rates of return to the Health Board was refused, which is perhaps a little surprising given their belief in the value of hard numbers.[30]

Perhaps Mr. A. C. Whitley's letter to the health officer best indicates that attitudes might be changing as a result of all this activity. "Mr. Cola Martin has thrown the dead body of a cow within 75 yards of my dwelling house," he complained, "and has refused to remove cow after I requested him to do so." The said cow, he continued "has remained offensive and dangerous to my family's health [it would be, the episode took place in August!]. Would you please notify me what steps to take or notify Mr. Martin to make some disposition of the dead cow." Two days later the offending Mr. Martin was requested by the health officer to remove or bury the cow. However trivial, this small mirror into the life and times of North Carolina indicates that by this time the state health department had become a recognized arm of government to whom one could turn in times of emergency

These early county health units of the South illustrate the goals of the original Health Commission as set up by Rose, a direct continuation of the Sanitary Commission for the Eradication of Hookworm. In contrast to what was to follow, the attack against hookworm was a means to a greater and more important end.

Although such health units were not exported in any great numbers, they retained an important place in the work of the Health Division, a reminder of the original mandate. Throughout its history, the IHD responded in the same way when called upon to assist in improving the public health situation in various countries. First it would undertake a survey and make recommendations. Then it would offer to pay for professional training of key personnel, usually in schools of hygiene which the IHD itself had funded. Finally it would finance development of a public health service, often by means of demonstration health units. By such means health units were opened in many European countries, Brazil, Chile, Sri Lanka, India and also in Canada.[31]

## Latin America

Work in Brazil would be modelled on that in the South, Rose noted in 1917. "The purpose of our work in any country is not to bring hookworm disease under control, but to make a demonstration which will lead ultimately to the enlistment of local agencies in the work." Such demonstrations stimulate public interest, he

continued, and enlist state and local agencies in the work.[32] Thus, a hookworm survey in Brazil led to an intensive campaign against the disease in the state of Rio de Janeiro beginning May 1, 1917. By October, Hackett was able to report to Rose that an important first step had been taken by passage of a new law mandating sanitary latrines in all buildings.[33] By 1921 when the board handed over to the Federal Sanitary Commission, hookworm campaigns and health units were in operation in 10 Brazilian states, and, although problems still existed, Hackett could boast that the idea of health units had been "planted in soil cleared and made ready by intensive campaigning."[34]

Similarly, in its short-lived hookworm campaign in Mexico (1924–1928), the Health Board held to its original mandate. Once again, the parasite became the opening wedge for building a public health system. "Ultimately," Anne-Emanuelle Birn concluded, "the elimination of hookworm disease was not the goal of the hookworm campaign. Instead, it offered an effective means of igniting interest in public health among the Mexican political establishment, physicians, and rural inhabitants, though a dramatic demonstration of hookworm control."[35]

Rose's vision was still visible many years later when, in 1940, Lewis Hackett was transferred to Buenos Aires to become director of what was rather grandly called the Rio de la Plata and Andean Region. In his first report, Hackett indicated that he, unlike many others, still remembered the original goals of the organization. The overriding aim of the Health Division, he wrote, was to form polyvalent public health services, and in poor countries that might be best achieved by attacking malaria, hookworm and yellow fever. But in countries with more advanced social programs, that aim could best be achieved by demonstrating public health services on a small scale using full-time specifically trained health professionals rather than, as was usually the case, relying on part-time physicians who lacked the preventive approach.[36]

Chile, Hackett discovered, was the only country that had anything resembling progressive social programs and the time seemed ripe for a fundamental change, Hackett told Sawyer eight months after his arrival. The Chileans had set up a system of socialized medicine, and believed they had done all they could do for the country's health. But because health indexes had not changed as a result, there was a feeling of something fundamentally wrong. As a result Eugenio Suarez, director of Chile's Bacteriological Institute and Director of Public Health turned to Hackett for advice.[37]

Aware that Suarez's attempt to take public health out of the political arena had not met with political approval, and unsure whether Suarez would survive that opposition, Hackett delayed action until after the 1942 presidential election.[38] What Hackett hoped for came about. The Radical Party under a new chief, Don Juan Rios, won the election; the former dictator and military strong man, General Carlos Ibañez, did not intervene; Suarez kept his job and Dr John Janney was posted to Santiago to take charge of the Chilean program. "I have never been in a country

where the prospects for IHD cooperation were so promising as they are in Chile today," Janney enthused a month after his arrival.[39] Janney, after surviving a German gas attack in World War I, gained his medical degree from Johns Hopkins and remained in Chile until the IHD closed in 1951.

Janney's first priority was to establish a health unit. Quinta Normal, one of the poorest areas of Santiago with a population of 65,000 and the usual characteristics of high death rates, poor sanitation, poor housing, high rates of tuberculosis and malnutrition, was chosen as the site for the unit. It opened in July 1942, with a great deal of hope and optimism. "Rarely has the IHD been able to count on so much good will and support, in advance," Hackett was thrilled to report.[40] In a similar vein, Hackett concluded his first report of 1942 with the view that "Chile may offer the best opportunity for fruitful cooperation which we may find in South America."[41]

The Quinto Normal Health Unit seems to have fulfilled the Health Division's wishes. By 1945, 65% of the houses had been connected to sewer lines; over 500 functional pit latrines had been built; steady progress had been made in the provision of running water; and a public laundry, showers and baths had been installed. On the strictly medical side, clinics were being offered in maternal and child health, venereal disease, nutrition and dental hygiene, while tuberculosis was being tackled with new X-Ray units; plans were in hand to begin artificial pneumothoraces and a BCG vaccine program. The center's scope of activities had extended far beyond what had been thought possible only a few years before, Janney reported. The center and a new school of hygiene "with their staffs of young professionals and well-equipped classrooms, labs, clinics and offices, are a source of pride to everyone who has had any part in their creation."[42]

## Canada

Similarly, and somewhat surprisingly, surveys and health units became relatively common in Canada where hookworm was not an issue. Indeed a lack of long-term planning and the habit of ad hoc decisions is nowhere better illustrated than in the Health Board's decision to undertake health surveys in Canada and, perhaps, to open health units there. The first opportunity took place in New Brunswick. During the summer of 1921, Rose, on one of his fishing trips to the northern woods, met Dr William Roberts, Canada's first provincial Minister of Health, who no doubt beguiled Rose with stories of his bill to establish the province's first department of public health.

It had stirred up a political hornet's nest in the legislative assembly. The opposition, Roberts wrote, although unable to block the bill's passage, "could not have fought harder if it had been 26 Germans they were facing."[43] Dr Crocket, a Fredericton physician and main Tory critic of the bill, complained about the cost of the new department; he claimed, quoting from a tourist map of the province, that people there lived longer and were more free of disease than anywhere else

on the globe. "We now behold a minister who has to look after our birth, our marriage, and when life's fitful fever closes, record the dissolution and keep our memory clean," he told members during a heated debate. Another member compared the minister to the hated Kaiser, complaining that he was being given powers "almost as great as those of the man who was attempting to take away the salvation of the world."[44]

But the Health Board's plans broke down when the province's health districts refused to assume responsibility once the Health Board had withdrawn. Delay followed delay as Roberts found it frustratingly difficult to convince local politicians to assume any such responsibility, so when the Liberals were defeated in the 1925 election and Roberts lost his seat, the Health Board withdrew from New Brunswick—it had not been a successful visit.[45]

A quite different reception awaited the Health Board across the provincial border, in Québec. There, the willingness of local officials to accept some financial responsibilities for much-desired county health units, and the enthusiastic support of Québec's Director of Health, Dr A. Lessard, as well as the clergy, newspaper editors and many government officials led to an 18-year association between the Health Board and that province. Indeed the County of Lac St. Jean, well to the north of Québec City, received kudos from the Health Board when it raised $2500 through a special public health tax. "The fact that the community through its officials accepts responsibility for public health work to the extent of imposing a special tax for it," wrote Read, "marks an important step in advance."[46]

The health units, wherever they were located, carried out much the same work, modified by local conditions. They were what Rose was attempting to build as a means to improve the public's health. The Beauce County unit in Québec, for example, carried out sanitary inspections and educational work; reported on contagious diseases especially tuberculosis; took responsibility for child hygiene and carried out vaccinations against smallpox, typhoid, diphtheria and tetanus. It also carried out prenatal and preschool visits and school health inspections.[47]

This was the type of organization which Rose had envisaged for the Health Commission and which persisted whenever the organization was called in to suggest ways in which one public health system or another could be improved. But when they moved into the British colonies, and when Rose decided to extend the organization's work to include malaria and yellow fever, and when Rose was superseded by directors who were all members of the medical profession, the control and even eradication of disease became the prime objective—diseases were no longer means to an end.

But Rose's vision for the Health Division and its predecessors never completely died. In 1928, even Frederick Russell, the Health Division's second director, felt compelled to remind Michael Connor, who was running the yellow fever campaign in Brazil, that "sometimes we are apt to forget the real objectives of our work—that is, that what we want to do is to help each country establish a health

organization suitable to the needs of the country." The various diseases are important, he reminded Connor, "but they are only partial objectives."[48] Indeed, one can argue that few forgot this as much as Russell himself. Rose's vision also remained in place when, in 1914, he was sent to France to develop the Rockefeller war relief program.

## Notes

1. E. Perronicito, "Helminthological observations upon the endemic disease developed among the labourers in the tunnel of Mount St. Gothard," *J. Quekett Micro. Club.* 6 (1879–81): 141–150. A monument to these victims now stands in the village of Airolo at the south end of the tunnel.
2. British Colonial Office. Public Record Office. Miscellaneous Papers, CO 885/20/238.
3. The papers of Arthur Looss, discoverer of the parasite's life-cycle, are translated and reproduced in B. H. Kean, K. Mott, and A. J. Russell, *Tropical Medicine and Parasitology. Classical Investigations* (Ithaca: Cornell University Press, 1978), Vol. II: 287–324.
4. That thymol could "cure" hookworm was discovered in the 1890s. Before that time the chemical was used externally on wounds as a substitute for carbolic acid, and in night commodes to deodorize urine. It had been given internally as an antipyretic, noted the *United States Dispensatory* in 1896, but ringing in the ears, deafness, sweating, and "alarming collapse" often followed. Furthermore, injecting thymol into the veins of dogs was reported to have caused death by respiratory failure. By the end of the century, however, it had come into favor as an antihelminthic, but one that caused "a serious constitutional disturbance." To prevent such disturbances a purge such as Epsom salts came to be used in any internal treatment using thymol. In theory, the Epsom salts swept out the excess thymol as well as the dead and dying worms before the side-effects of nausea, weakness, and other malfunctions took place. G. B. Wood and B. Bache, *The Dispensatory of the United States of America*, 17th ed. (Philadelphia: J.B. Lippincott, 1896); *Squire's Companion to the British Pharmacopoeia* (London: Churchill, 1916).
5. Bailey Ashford, *A Soldier in Science* (New York: W. Morrow, 1934); *Report of the Surgeon-General of the Army for the fiscal year ended June 30, 1900* (Washington: Government Printing Office, 1900).
6. C. Wardell Stiles, *Report upon the Prevalence and Geographical Distribution of Hookworm Disease in the United States* (Washington: Hygienic Laboratory, Bulletin No. 10, 1903). Stiles' preliminary report (1902) is included in this large report.
7. The long convoluted series of events which led to this meeting is told in J. Ettling, *The Germ of Laziness. Rockefeller Philanthropy and Public Health in the New South* (Cambridge, Mass: Harvard University Press, 1981) and in L. Hackett's MS, *History of the International Health Division*, Chap 3. RAC. RG.3 S.908 B.5 f.30.
8. Minutes of a Meeting of The Rockefeller Commission for the Eradication of Hookworm Disease, October 26, 1909. RAC. RG.2 B. 53 f.546.
9. L. Hackett, *MS, History,* Chap. 3, p. 40.
10. W. Rose, "Work of Rockefeller Sanitary Commission." August 12, 1914. RAC. San. Comm. III. RG.2 B.52 f.545. For public health in the old South, see W. Link, *The Paradox of Southern Progressivism 1880–1930* (Chapel Hill: University of North Carolina Press, 1992).

11. J. D. Rockefeller Sr. to Sanitary Commission. August 12, 1914. RAC. San. Comm. III. RG.2 B.52. f.545.
12. Rose, "Work of Rockefeller Sanitary Commission." August 12, 1914. Ibid.
13. Gates to John Rockefeller, August 10, 1914. Ibid.
14. Discussed in W. Link, *Southern Progressivism*.
15. B. Washburn, "Report of the Bureau of County Health Work of the North Carolina Board of Health for 1918." RAC. RG.5.3 S.236 B.64.
16. "Gist of the North Carolina Typhoid Problem." RAC. RG.5.3 S. 236 B.62.
17. G. Leonard & C. Pridgen, "Report of Health Work on Knott's Island (Oct–Dec, 1913)." RAC. San Comm. S.2 B.7 f.129.
18. J. Ferrell, "Proposition from the North Carolina State Board of Health for the Control of Hookworm and other Soil Pollution Diseases;" Rankin to Ferrell, April 2, 1917. RAC. RG.5.1.2 B.43 f.654.
19. "A General History and Summary of 17 months of Health Work in Northampton County, North Carolina. August 1, 1917 to January 1, 1919." RAC. RG.53 S.236 B.64. Much of my account of the Northampton work is taken from this wonderful report which includes reports from the Hookworm Unit, the Soil Pollution Unit, the School Inspection Unit, the Infant Hygiene Unit, the Anti-Typhoid Vaccination Unit, the Quarantine Unit, and the Influenza Unit.
20. Report of Hookworm Unit, Northampton County. Ibid.
21. Ibid.
22. Report of Quarantine Unit, Northampton County. Ibid.
23. F.Register, "Little Journeys along the Byways and Highways of Disease and Health." Editorial, *Roanoke-Chowan Times*. 1918. Ibid.
24. *Ordinance. Requiring Sanitary Privies in Northampton County*, December 15, 1917; Approval Seal. In Report of Soil Pollution Unit, Northampton County. Ibid.
25. Report of Hookworm Unit, Northampton County. Ibid.
26. "Development of Health Work in North Carolina." *Raleigh News and Observer*, March 16, 1919.
27. W. Rankin, "Annual Report of the Secretary of the North Carolina Board of Health," April 1919 to April, 1920. RAC. RG.5.2 S.236 B.17 f.100.
28. W. Rankin, "Report of the Secretary of the North Carolina State Board of Health for the Fourth Quarter of 1923." RAC. RG.5.3 S.236 B.64.
29. Ferrell to Leach, February 26, 1925. RAC. RG.5.1.2 B.208 f.2667.
30. The Rankin and Ferrell correspondence in RAC. RG.5.1.2 S.236 B. 91 f.1275.
31. In particular see "Health Units and Public Health, 1928–1945," in S. Kavadi, *The Rockefeller Foundation and Public Health in Colonial India 1916–1945* (Mumbai: Foundation for Research in Community Health, 1999).
32. Rose to L. Hackett, April 25, 1917. RAC. RG.5.3 S.305 B.107.
33. L. Hackett to Rose, October 25, 1917. RAC. RG.1.1 S.305 B.15 f.133.
34. L. Hackett, "Hookworm Infection Survey of the State of Rio de Janeiro, Brazil. Nov 1916–March. 1917." RAC. RG.5.2 S.305 B.111; L. Hackett, "Annual Report for Brazil. January 1 to December 31, 1921." RG.5.3 S.305 B.112.
35. A-E Birn & A. Solórzano, "The hook of hookworm: Public health and the politics of eradication in Mexico," in *A. Cunningham & B. Andrews (eds), Western Medicine as Contested Knowledge* (Manchester: Manchester Univ. Press, 1997), p. 166. Anne-Emanuelle Birn, in her D.Sc. dissertation and numerous articles has given us an in depth analysis of the Health Board's work in Mexico. A-E Birn, *Local Health and Foreign Wealth: The Rockefeller Foundation's Public Health Programs in Mexico,*

*1924–1951*. D.Sc. Disseration, Johns Hopkins School of Hygiene, 1993, quotation also from p. 132. Mexico was always somewhat of an anomaly; along with Canada and the U.S., the Foundation classified it as part of North America, not Central America (although I have not done so in this book).

36. L. Hackett, The Rio de la Plata and Andean Region. A review of IHD activities from October 1940 to June 1942. RAC RG.5.3 S.300 B.102.
37. Hackett to Sawyer, April 23, October 1,1941. RAC. RG.1.1 S.309 B.1 f.5.
38. Hackett to Sawyer, December 11, 1941. Ibid.
39. J. Janney, "Project for Quinta Normal Health Centre." RAC. RG.1.1 S.309 B.6 f.62.
40. Hackett to Sawyer, June 10, 1942. Ibid.
41. L. Hackett. The Rio de la Plata and Andean Region. October 1940 to June 1942 RAC. RG.5.3 S.300 B.102.
42. Janney, The Rio de la Plata and Andean Region, Annual Report, 1944." RAC. RG5.3 S.300 B.103.
43. Roberts to J. Hall. April 27, 1918. Public Archives of New Brunswick (PANB), Fredericton, New Brunswick. Canada. RS 136 J2.
44. *Synoptic Report of the Proceedings of the Legislative Assembly of New Brunswick.* April 5 and 23, 1918. PANB.
45. Series of letters between Russell, Florence Read and Roberts. RAC. RG.5.1.2 S.427 B.168 f.2163.
46. Read to Lessard, August 5, 1926. RAC. RG.5.1.2 S.427 B.263 f.3334.
47. Reports of the Beauce County Health Unit 1929. RAC RG.5.3 S.427 B.177.
48. F. Russell to Connor, February 14, 1928. RAC. RG.1.1 S.305 B.20 f.158.

# 3

## Tuberculosis in France (1917–1924)

In November 1914, before the Health Board's work on hookworm got underway, the trustees of the Rockefeller Foundation sent the peripatetic Rose to France as chairman of the Rockefeller Foundation's first War Relief Commission to Europe.[1] As a result, three years later, the Health Board became engaged in a French tuberculosis campaign.

Tuberculosis is a disease quite unlike hookworm, malaria and yellow fever. There were no magic bullets, no cure other than good food and sunlight, and no seemingly weak links which could be attacked by pesticides and larvicides. As Rose must have realized, tuberculosis was associated with "debilitating, draining labor; little or no job security; meagre wages; poor diets; slum housing; filthy bodies and surroundings; and heavy drinking."[2] Nevertheless, the campaign was in line with Rose's early view of the mission of the Health Board; tuberculosis was to be a means to an end. When the French campaign came to an end, however, both Rose and Russell vowed never again to become involved with a social disease such as tuberculosis, or to be drawn into emergency relief operations, and never again to allow their policies to be dictated by the trustees of the foundation. They upheld the last two promises but reneged on the first. The campaign was also important in bringing them into Europe for the first time. They were to remain there until the Health Division closed, first as an outpost in Paris but soon as part of the European office of the Rockefeller Foundation situated at 20, rue de la Baume.

## War Relief

The Rockefeller Foundation became involved in World War I by offering assistance to the U.S. Red Cross, and, through the agency of Standard Oil, by chartering ships to carry food to Belgian civilians. Gates saw the provision of such aid as a wonderful public relations gesture at a time when the much maligned Rockefeller name needed some favorable publicity. "Any conspicuous and important act connected with the war," Jerome Greene told John Rockefeller Jr., "would attract great attention and would, in Mr Gates's opinion, do more than anything else in the world to create the right impression of the essential humanity and generosity of your father's Foundation."[3] As a result, the foundation decided to send its first War Relief Commission to examine the problems of postwar rehabilitation (they assumed the war would be a short one). But in August 1915 the commission returned home, reluctant to become involved with more charity relief.[4] After months of doubt and uncertainty over what could be done in Europe, the foundation decided to send a second War Relief Commission. Directed by Warwick Greene, a Harvard University lawyer with experience in the Philippines, its headquarters were set up in Berne in the spring of 1916.

By then the foundation had become aware of the tuberculosis problem in France. Many individuals and agencies began to request aid to assist thousands of French troops who, living out their lives amid the unimaginable horrors of the trenches had become victims of tuberculosis. These men would, as one letter writer put it, "scatter the disease over France."[5] But it was probably the Duchesse de Richelieu, a native of Baltimore and a representative of the recently-created *Comité central d'assistance aux militaires tuberculeux*, who brought the matter to a head after meeting with Jerome Greene in March 1916.[6] But could a non-combatant nation assist with diseased French troops? Initially Rockefeller Jr. did not think so, but a few months later officials in New York decided that tuberculous troops fell into the category of the "permanently disabled." Prevented from again entering active service, these troops were distinct from the war-wounded, a group the foundation could not assist. The War Relief Commission became free to assist *Le Comité central* in its fight against tuberculosis.[7]

France had not been inactive in fighting the disease,[8] but deaths from tuberculosis remained far higher than those in Britain and Germany, and they were declining at a lower rate.[9] Thousands of recruits succumbed to the ravages of the disease. Many had been discharged only to return home to infect their families and neighbors.[10] In response, the French brought forth the Léon Bourgeois Law of 1916, agreeing to put their greatest emphasis on *Dispensaires d'hygiène sociale et de préservation anti-tuberculeuse*, one of which was required to be formed in each *Département*, but only 22 of which had been opened by 1917. By the end of 1916, 25 "*stations sanitaires*" had been built in which discharged troops, thought capable of improvement, were examined, nursed, and educated

in anti-tuberculosis measures. But these stations could muster only 2000 beds, a miniscule number given the number of French troops who had been discharged because of tuberculosis. Carrying the disease and incapable of work, these wretched souls were then passed to the *Dispensaires d'hygiène* which, without state funding, were forced to rely on the care of private committees set up for the relief of returning tubercular soldiers. It was this crisis that led the Minister of the Interior, Léon Bourgeois, to form *Le Comité central d'assistance aux militaires tuberculeux*, with a mandate to direct and coordinate all this activity.[11] But in reality there was no plan of action. *Le Comité central* was cursed with a huge bureaucracy. There were 14 honorary presidents, including French President Raymond Poincaré; a council of 44 members under its president, Léon Bourgeois; an honorary membership of 100 with another 110 listed as founding members. Tuberculosis soon took a back seat to an administrative nightmare of personal jealousies and political infighting.

Realizing that "immediate action" was called for, Greene asked Wallace Sabine, dean of the Graduate School of Applied Science at Harvard, who was on his way to the Sorbonne at that time, to report on the situation. Sabine realized that "the number of cases to be treated is wholly beyond the capacity of any conceivable system of elaborately-built hospitals or sanatoriums." Both Sabine and Greene were becoming irritated with a group of wealthy American "do-gooding" women who, said Sabine, were pestering them with suggestions of anything from "a vague vision of an enormous tuberculosis village to a real stone and plaster Chateau."[12]

All French effort, Sabine reported, was now devoted to the military, which had taken over existing sanatoria and dispensaries. The war found the anti-tuberculosis effort, "dans un parfait état de dénûment: une architecture de balivernes, purement buraeucratique; aucun plan d'ensemble pour le pays."[13] But any campaign against this "international menace" must concern itself with the civilian population as well, Sabine argued, noting with horror that no French facility was open to women and children with the disease.[14] Armed with this report, the War Relief Commission recommended the appropriation of $2.4 million (1990) for the establishment of a tuberculosis commission that would work in France and expand to other countries to "save the population of Europe from a threatened scourge."

Ever cautious in their planning, the trustees of the Rockefeller Foundation, after consultation with the French ambassador, arranged for Hermann Biggs, Commissioner of Health for New York City and a leading authority on tuberculosis, to visit France and to discuss with the French government the most effective way in which the foundation could assist their work against the disease.[15] Biggs, accompanied by a Belgian-American physician, Alphonse Dochez, and a Columbia University student-translator cum secretary, arrived in France in January 1917, and spent a mere two weeks visiting various institutions. The Biggs-Dochez report laid out the blueprint for the tuberculosis work that followed. "There now

exists in France a really extraordinary opportunity for the Foundation to undertake a work of the highest and most beneficial character," Biggs told Edwin Embree, secretary to the foundation, in a covering letter to his report, and "a similar opportunity will perhaps not be again presented during a generation."[16] Indeed Murant and Zylberman have argued that it was only because of the war that the Health Board was able to begin a French campaign, the First World War equivalent of the Marshall Plan.[17]

Biggs's report presented a brutal picture of the French situation. An estimated 150,000 troops had been discharged because of active tuberculosis, with 45,000 of them certain to succumb to the disease. There were 20,000 prisoners-of-war in Germany with the disease, 85,000 cases in the refugee population and perhaps over 100,000 civilian cases. The French had no effective answer to this problem, and were maintaining what Biggs called a "profound inertia and indifference to the public health situation." Their public health administration, Biggs judged, was overcentralized and bureaucratic, "lacking in efficiency, intelligence, initiative, authority and personnel," and unable to do anything other than that authorized under the law. The foundation should act in France, according to Biggs, not only to assist in the fight against tuberculosis, but to help reorganize the French public health service—how Rose must have been pleased to read this. In other words they were to repeat in France what the Sanitary Commission had set out to do in the American south; only the disease and the locale had changed.[18]

The ever-busy Rose set to work to devise a five-year working plan for the French operation. Initially, the foundation was to fund four mobile educational units, as well as several dispensaries to serve as a guide and stimulate the French to establish 300 to 400 more across the country. He urged that the foundation set up and maintain four training centers for dispensary personnel, and to establish a central administration in Paris which would run the campaign and encourage the reorganization of public health agencies in France.[19] In May 1917, the foundation approved Biggs's report and Rose's working plan, and appointed Livingstone Farrand, president of the University of Colorado and former executive secretary of the National Association for the Study and Prevention of Tuberculosis, as the first director of the Rockefeller Tuberculosis Commission. Administrative responsibilities were put in the hands of the Health Board, which now found itself in a northern clime, far away from the heat and humidity of hookworm, malaria and yellow fever, and distant from the "primitive races" with whom they had dealt previously. They had taken on part of the health burden of a colonizing country rather than a colonized one and were to face attitudes that were new to them. Far from being viewed as experts delivering the benefits of American know-how and modern medicine, French leaders saw them merely as deliverers of much needed financial aid, with expertise only in the art of advertising, in which they were said to be "past masters."[20] Furthermore, the French had their own ideas on how best to contain tuberculosis. By the time the Rockefeller commission arrived in France,

the *Comité central* had been renamed *Le Comité national de défense contre la tuberculose* in order to deal with the civilian as well as the military problems.[21]

## The Commission

On July 17, 1917 the five-member Commission for the Prevention of Tuberculosis in France sailed out of New York harbor. With Farrand were experts in the field: Homer Folks of the American Red Cross; James Miller, professor of clinical medicine at Columbia and director of tuberculosis at Bellevue hospital in New York; Selskar Gunn, professor at M.I.T., secretary of the American Public Health Association and editor of its journal, and Elizabeth Crowell, of the Association of Tuberculosis Clinics in New York. Miller would take charge of the dispensaries, Gunn would organize the educational campaign, and Crowell would be responsible for home care through the Bureau of Public Health Visiting. It was, as the foundation stressed, a very small American team who were merely "to assist in part in directing the great work which the French people themselves are undertaking to fight this disease."[22] As Farrand told the French president shortly after his arrival, "We are not here to give you instruction but to fight with you against the common enemy."[23]

But in practice cooperation proved different. In his first report, Farrand suggested the commission had to act independently in order to stand outside political, social and religious controversies that were undermining whatever French activity there was, and in order to serve as a model of American ideas and methods.[24] Initially, both Rose and Vincent objected.[25] But they retreated after being told that the War Department and the Department of the Interior were at loggerheads and that support for one would antagonize the other. Thus the commission had no option other than to remain independent.[26]

The educational campaign got off to a flying start. Under Gunn's leadership a traveling exhibit was established during 1917 in Eure et Loir. This exhibit, consisting of a van equipped with a 42-panel exhibit, printed matter and films, also acted as a base from which lectures were given. Not surprisingly, given their experiences in the South, and the work of the National Tuberculosis Association in the United States, these traveling shows proved to be the most visible aspect of the whole campaign, much admired by the French. Using techniques drawn from American advertising, they produced, for example, dramatic multi-color posters (Fig. 3.1) that were new to the French.[27] It was a campaign whose progress could be followed by the hard numbers, so much admired by the Health Board. By the end of 1918, a total of 2,115,708 pieces of printed matter had been distributed; 24 articles about tuberculosis had been published in 33 provincial newspapers, and three traveling vans had visited 141 towns (see Fig. 3.2) where 875 lectures had been presented.[28] "America came to the rescue," noted Gunn in a French journal article.[29]

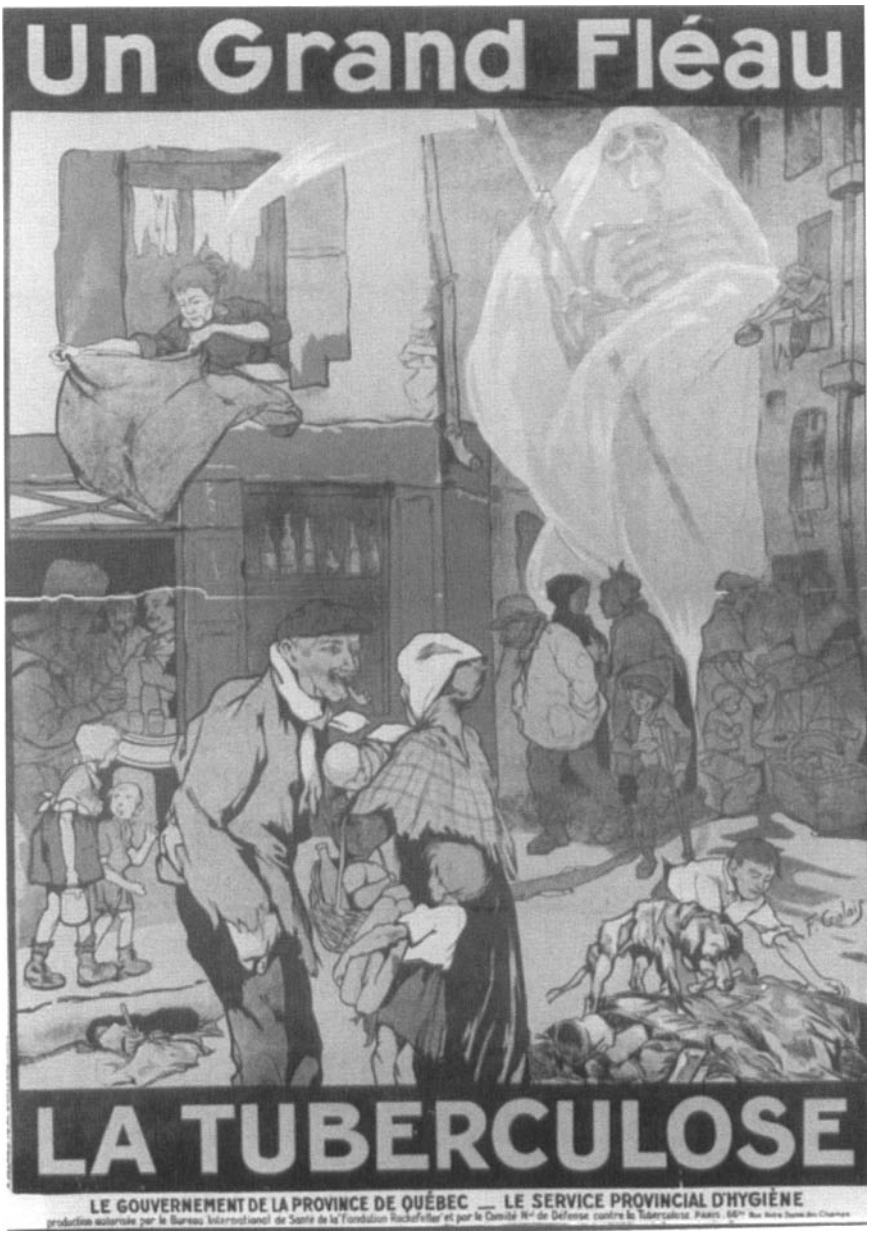


**FIGURE 3.1.** Posters. French TB campaign (courtesy of the Rockefeller Archive Center). (*continued*)


**FIGURE 3.1.** (*continued*)

Mirroring the National Tuberculosis Association's "modern health crusade," schoolchildren became the focus of Gunn's campaign. Children of all ages were required, for example, to write short essays. "What do you know about tuberculosis?" one asked, "What does this disease consist of? How is it transmitted? What are the symptoms? How can one avoid tuberculosis?" Other assignments were more to the point. One asked the students: "What do you think of the American mission sent to France by Mr. Rockefeller to fight against tuberculosis?" Nine-year-old Marthe Chéard's response must have warmed the hearts of Gunn, Rose and American advertisers. This is what she wrote (I have omitted the teacher's corrections).

La mission Américaine est venue en France pour combattre l'ignorance des Français, comment faire pour éviter la tuberculose. Car en France plus de 80,000 Français perissent


**FIGURE 3.2.** French schoolgirls at the TB mobile exhibit (courtesy of the Rockefeller Archive Center).

chaque années. Et c'est pour cela que cette mission Américaine nous a fait beaucoup de conférences et ils nous expliques comment fallait faire pour épargner cette grave maladie. Ils nous l'ont seulement pas expliques mais ils nous l'ont fait voir par le cinéma. Cette maladie se produit souvent parce que on ne prend pas assez de precotions. Cette mission Americaine est venue a vitre pour nous épagner d'une grave maladie. Et si nous suivons les conseils le nombre de morts sera moins nombreaux.[30]

The French looked with less favor upon the American dispensaries. Two were set up in the XIXth *Arrondissement* on the northeast edge of Paris, where the mortality rate from tuberculosis was almost 400 per 100,000. Three more were opened in the most important cities of Eure et Loir southwest of Paris: Chartres, Dreux and Châteaudun.[31] The main reason for this hostility concerned the role of public health nurses and Miss Crowell's Bureau of Public Health Visiting.

Crowell had graduated in 1893 from a hospital school of nursing in Chicago and moved to Pensacola Florida where she became a hugely successful superintendent of the local infirmary. Then, in 1905, she trained as a social worker in New York, a career move much criticized by the nursing elite in the U.S. Before joining the Health Board in 1917, she held the post of executive secretary of the Association of Tuberculosis Clinics in New York for seven years, a position much in tune with her social work-nursing background.[32]

Special tuberculosis dispensaries were pivotal, Miss Crowell said in 1916, and public health nurses were an indispensable part of them. Initially these public health nurses went into the community in an attempt to educate the sick about the disease, hygiene and cleanliness. They were expected also to treat the sick, search for new cases and prevent the illness from spreading to family members. They were, according to a recent article by Jessica Robbins, "viewed as an elite among visiting nurses."[33] But it was essential, according to Crowell, that these nurses be regularly trained hospital nurses, with additional training in public health.[34] Nurses had been crucial to the work of the tuberculosis dispensaries in the United States, and Biggs and Crowell saw them playing an equally significant role in France. Nevertheless, there were hints in Crowell's reports that she was aware of changes taking place in the United States where the emphasis on home care was being replaced by institutional care, such that public health nurses were being marginalized.[35]

But the French had other ideas. French dispensaries, wrote A. Calmette, vice-president of the *Comité national* who was later to develop the BCG vaccine, were set up to discover active cases of tuberculosis, clean up contaminated homes and educate the families. They were not private hospitals, he stressed, "they must not serve to medically treat the tuberculous or to distribute medicines to them. Their role is essentially and exclusively social." Thus, instead of visiting public health nurses, as in the U.S., the French dispensaries employed *visiteuses d'hygiène*, whose role was basically educational. They were expected to visit the homes of the ill and instruct them on precautions to take to avoid spreading the bacilli. But they must "rigorously abstain" from all medical intervention, Calmette wrote, because "they are the peoples' educators, councilors on practical hygiene, and not bedside nurses or guardians of the sick."[36] In other words, the French dispensary had no place for a public health nurse.

These *visiteuses d'hygiène* aroused disapproval in Crowell who was anxious to replace them by what she regarded as the more prestigious public health nurse. But to the French these *visiteuses* were front-line troops, "vaillants soldats d'une cause sacrée." Proudly wearing their dark blue uniforms, these single women, 23 to 40 years of age, faced many hazards, visited unhealthy areas of towns and were seen as bringing honor to French women. Neither were they as uneducated as Crowell liked to believe. They were required to take a two-year course in medicine and law, together with practical training in hospitals and dispensaries.[37]

To hire *visiteuses d'hygiène* rather than trained nurses made sense. There was no medical cure at that time beyond fresh air and good food, neither of which was available to the city poor; curtailing the spread of infection was about all that could be done—amounting perhaps to wishful thinking when dealing with overcrowded slum housing. Also, the heavy demands of military hospitals ensured an acute shortage of trained nurses. Perhaps, more to the point, dispensaries posed no threat to the French medical profession. The poor in France could place their names on a "poor list" and receive treatment from "poor-law physicians," who received a

small fee from the government for their efforts. Indeed, in the larger cities, these fees could represent a major slice of physician incomes. Far from constituting a threat, social dispensaries could be helpful to local physicians by referring tuberculous patients to them. But this would be the case only if dispensary staff members refrained from medical treatment.

Crowell saw dispensaries through American eyes. The *visiteuses d'hygiène*, something akin to social welfare officers, had no place in such clinics; their tasks were to be taken on by public health nurses whose mission was both educational and medical. By now, their medical role had become so minimal that one wonders why Crowell was so opposed to them. The commission's popular education effort was supported and praised by French officials, but Crowell's attempt to Americanize the French dispensary by training visiting public health nurses rather than *visiteuses d'hygiène* proved one of the commission's most intractable problems. The public health nurse, in whom the Health Board had shown little interest until then, became the major means by which the commission tried to display to the proud French that American dispensaries were superior to their own. *Infirmières-visiteuse* had to replace *visiteuses d'hygiène*.

Initially this was done by on-the-job training of French Red Cross nurses in the commission's Paris dispensaries. But any long-term solution required that schools for visiting public health nurses be set up to replace those "inferior" schools turning out *visiteuses d'hygiène* which the *Comité national* was already overseeing in Paris and seven other cities. At first Crowell established schools which offered a ten-month training program for those already holding a nursing diploma.[38] But she was not satisfied with the standards of French nursing schools and felt the nurses entering her program were poorly trained. To offset this handicap, she established a two-year diploma program in what came to be called "bifurcated" schools. The first year would be spent in general hospital training, the second in further hospital training or in taking classes in public health, dealing with child services, school hygiene, social work and tuberculosis. By the end of 1921 bifurcated schools had opened in Strasbourg, Bordeaux, Lille, Lyon, Marseilles, Nantes and Paris, while, at the same time, the 10 month course continued to be given to those with nursing diplomas.[39]

## Handing Over

In February 1919 Farrand resigned to become chairman of the American Red Cross, and was replaced by Dr Linsly Williams, formally a senior official in the New York State health department and since 1917 a member of the U.S. army Medical Reserve Corps. According to Farrand, the campaign had helped lift French morale, stimulated their interest in public health and speeded up their work on tuberculosis. It had been a good investment, he wrote, and "if this Commission should do nothing more this work may be regarded as essentially permanent."[40]

On this high note the Health Board decided that the commission should gradually turn over its work to the French.

Rose assumed that the board's funding would peak that year, in accordance with the plan worked out by Farrand and himself. But, to his surprise, the requested budget for 1920 showed an increase over 1919. One can understand Rose's financial concern. In 1919 alone, the budget had reached the equivalent of $4,551,000 (1990), a massive 42% of the total Health Board budget and nearly 50% of the disease budget of that year. Most of the blame was placed on Crowell's Bureau of Public Health Visiting. By 1919 there were seven 10-month schools in operation and the Board was paying the salaries of their instructors, most from the U.S. and Britain. To make matters worse, 37% of their graduates had either resigned or been dismissed.[41] Moreover the Health Board had spent more on tuberculosis than had the French government.[42] The Health Board wanted out of the French tuberculosis problem as quickly as possible.

Under Rose's prodding, commission involvement in France began to decline. At the beginning of 1920, fully 70% of the over 200 dispensaries received commission aid; by the end of 1921 that number had plunged to three.[43] The number of traveling exhibits was reduced in 1920 from four to two, both now run by French staff. In 1921 the commission handed over the 10-month tuberculosis courses for nurses to the French *Comité national*. Finally, in December 1922, the work of the Commission for the Prevention of Tuberculosis in France virtually closed; most of what was left was transferred to the *Comité national*. Only the Bureau of Public Health Visiting and its bifurcated schools remained.

Now came the crunch: how was the success of the commission to be measured? If the goal had been to establish a public-health infrastructure in France, the campaigners should have been satisfied—though they could not be certain how long these changes would last.[44] But, had the prevalence of tuberculosis declined as a result of their activities? The mortality rate continued to decline,[45] but members of the Health Board must have been discouraged to read what Bernard Wyatt, who had assumed command of the commission's medical division, had to say. It was not possible, he wrote, to "prove statistically the efficacy of anti-tuberculosis work because of the difficulty—if not impossibility—of evaluating the different factors involved in the decline of the tuberculosis death rate." This decline, he said, which had been going on in all European countries, depended to a large degree on social conditions, and might not have had much to do with the tuberculosis campaign at all.[46]

Nevertheless, despite an overly-generous budget and the uncertain impact of the campaign on the disease, one would have expected Rose to have felt moderately satisfied. "There exists in France a widespread desire for improving the public health service," Williams announced just before his retirement, and "undoubtedly the work of the commission has brought the subject of tuberculosis, which cannot be divorced from general hygiene, to the attention of many thousands of French citizens." Moreover, he continued, "a deep feeling exists in the minds of the pub-

lic authorities as to the necessity of the physical reconstruction of the French race. . . . It cannot be assumed that the work of the commission is responsible for all this, but it has undoubtedly contributed a large part and been an important factor in giving a stimulus to some of these various activities."[47]

The Health Board had done what Rose had earlier demanded of its programs. In theory, at least, whether tuberculosis had declined or not was irrelevant; it had left, at least according to Williams, a legacy of public health workers and a desire for them, and that is what mattered. As Dr. Risler, vice-president of the *Comité national* wrote in a letter to Gunn in 1924, "Your services were so well organized that we only had to persevere and follow the roads you had mapped out, and the results seem to us so remarkable that we have decided to keep in its entirety the whole organization which you have bequeathed to us."[48]

Both Rose, and later, Russell came to regret this involvement with tuberculosis and vowed never to become directly involved with the disease again. In 1922 Rose turned down a request from Dr A. B. Cook, president of the Canadian Tuberculosis Society, for assistance in a tuberculosis survey.[49] Several years later, the Health Board declined to become involved in a Mexican campaign against tuberculosis.[50] Russell's opposition is easier to understand than Rose's. He was a laboratory man to whom tuberculosis must have been seen as primarily a social disease with which medicine could hardly cope. Rose's hostility makes less sense; the French tuberculosis campaign went a long way towards achieving what had always been his primary goal, educating the public in health matters and building public health agencies.

Perhaps it was the origin of the tuberculosis commission that bothered both men. The decision to take on wartime relief work during World War I was later seen as a major mistake leading up a false alley; the IHD was not a relief agency, and it would not repeat that mistake in World War II. Perhaps, too, Rose and Russell felt the Health Board would not again be pushed into a situation by the trustees of the Rockefeller Foundation. They had pushed Rose into France and decided the Health Board would take charge of the tuberculosis campaign. Henceforth, the medical barons would run the show. But, despite all this hostility, the Health Division was to return to tuberculosis in future years.

## Notes

1. This three-man commission consisted of Rose, E. Bicknell of the American Red Cross and H. James of the Rockefeller Institute.
2. David Barnes, *The Making of a Social Disease. Tuberculosis in Nineteenth Century France* (Berkeley: University of California Press, 1995).
3. J. Greene to Rockefeller Jr. October 30, 1914. RAC. RG.1.1. S.100N B.57. f.567. The Germans had refused to feed Belgian civilians on the grounds that the British had set up a naval blockade of Antwerp.
4. The history of the foundation's war relief activities is outlined in RAC. *Rockefeller Foundation History. Source Material.* Vol. 4 and Memorandum 8011, *On the Rela-*

*tion of the Rockefeller Foundation to War Relief Activities.* December 4, 1916. RG.1.1 S.100 B.57 f.567.

5. Edith Wharton to F. Barlow, 10 March, 1916. RAC. RG.1.1 S.500 B.25 f.247.
6. J. Greene to French Ambassador, 28 March, 1916. Ibid.
7. Letters on the correctness or otherwise of supporting tuberculosis work, in Ibid. In fact some wretched troops with TB were returned to the front after undergoing "cures" in the *Stations sanitaires.*
8. Early work on tuberculosis and poor relief is discussed in J. Hayward, "The official social philosophy of the French Third Republic: Léon Bourgeois and Solidarism." *Int. Rev. Soc. Hist.* 6 (1961): 19–48; C Henderson, "Social Solidarity in France." *Amer. J. Sociology* 11 (1905): 168–182.; J. Weiss "Origins of the French Welfare State: Poor Relief in the Third Republic 1871–1914." *French Hist. Studies* 13 (1983): 47–78; M. Ramsey, "Public Health in France," in D. Porter (ed), *The History of Public Health and the Modern State* (Amsterdam: Rodopi, 1994).
9. The reason for this discrepancy between the French and British/German figures is a matter of debate. While A. Mitchell blames the lack of public health campaigns in France, Barnes argues that the French not only had the lowest standard of living of the three, but were also improving more slowly. A. Mitchell, "An Inexact Science: the Statistics of tuberculosis in late nineteenth century France." *Social History of Med.* 3 (1990): 387–403; "Obsessive questions and faint answers: The French response to tuberculosis in the Belle Epoque." *Bull. Hist. Med.* 62 (1988): 215–235; Barnes, *Making of a Social Disease.*
10. L. Murand & P. Zylberman, "L'autre guerre (1914–1918). La sante publique en France sous l'oeil de l'Amerique," *Revue historique* 276 (1986): 367–398. This is an important paper, but unfortunately the authors do not subscribe to the idea that good writing is almost impossible to misunderstand. Here flowery passages, unusual literary tenses, and convoluted themes render the text dense and obscure, even to those whose first language is French. My criticism can be applied also to their paper in English translation, "Seeds for French health care : Did the Rockefeller Foundation plant the seeds between the two world wars ?" *Stud. Hist. Phil. Biomed. Sci* 31 (2000): 463–475.
11. *Comité central d' assistance aux militaires tuberculeux* (Paris, 1916).
12. W. Sabine to War Relief Commission, August 4, 1916. RAC. RG.1.1 S.500 B.25 f.247.
13. L. Murand & P. Zylberman, "L'autre guerre (1914–1918)," p. 372.
14. Tuberculosis. War Relief Report # 11, September 21, 1916. This report included W. Sabine, "Report to the Rockefeller Foundation in regard to tuberculosis in France and other countries affected by the war." RAC. RG.1.1 S.500 B.28.
15. Hermann Biggs to Starr Murphy, November 29, 1916. RAC. RG.1.1 S.500 B.25 f.247.
16. H. Biggs to Embree, April 2, 1917. RAC. RG.1.1 S.500 B.28.
17. L. Murand & P. Zylberman, "L'autre guerre (1914–1918)," p. 388.
18. H. Biggs and A.R. Dochez, "Tuberculosis in France," March 31, 1917. RAC. RG1.1. S.500 B.28. Dochez was later to receive Health Division funding for his work on influenza. Biggs's work on tuberculosis is discussed in C-E.A Winslow, The *Life of Hermann M. Biggs* (Philadelphia: Lea & Febinger, 1919).
19. W. Rose, "Tentative working plan for control of tuberculosis in France," May 1, 1917. RAC. RG.1.1 S.500 B.25 f.249.
20. C. Nordmann, "La croisade des Américains contre la tuberculose en France," *Revue des Deux Mondes*, September, 1918.
21. Calmette, *L'Effort national de défense contre la tuberculose* (Paris: 1919).

22. E. Embree to Sec. Comité des sanitoriums francaise de grand altitude. July 7, 1917. RAC. RG.1.1 S.500 B.25 f.249.
23. La Mission Americaine pour la lutte antituberculeuse en France," *Bull. comité national d'assistance aux anciens militaires tuberculeux.* 5 Juillet–Sept., 1917.
24. Farrand, "Program of Medical Activities for the Commission for the Prevention of Tuberculosis in France, September to December, 1917." RAC. RG.1.1 S.500 B.25 f.250.
25. Rose to Farrand, November 9, 1917. RAC. RG.1.1 S.500 B.25 f.251.
26. Farrand to Rose, December 21, 1917. Ibid.
27. The use of advertising methods in the National Tuberculosis Association's campaigns in the United States is discussed in Nancy Tomes, *The Gospel of Germs. Men, Women and the Microbe in American Life* (Cambridge: Harvard University Press, 1998), 115–123.
28. Summary Report on the Work of the Commission for the Prevention of Tuberculosis in France for the year 1918. RAC. RG.1.1 S.500 B.29 f.269.
29. S. Gunn, "Une guerre necessaire contre la tuberculose." *Je Sais Tout,* 1919.
30. RAC. RG.1.1 S.500 B.32 f.276.
31. Farrand to Rose, January 1st, 1918. RAC. RG.1.1 S.500 B.25 f.252.
32. E. Vickers, "Frances Elizabeth Crowell and the politics of nursing in Czechoslovakia after the First World War," *Nursing History Review* 7 (1999): 67–96.
33. J. Robbins, "Class struggles in the tubercular world: Nurses, patients and physicians, 1903–1915." *Bull. Hist. Med.* 71 (1997): 412–434.
34. E. Crowell, *Tuberculosis Dispensary, Method and Procedure* (New York: National Association for the Study and Prevention of Tuberculosis, 1916).
35. The gradual marginalization of these dispensaries and of the public health nurse is described in C. Rosenberg, "Social class and medical care in nineteenth century America: The rise and fall of the dispensary." *J. Hist. Med.* 29 (1974): 32–54; K. Buhler-Wilkerson, "False Dawn: The Rise and Decline of Public Health Nursing 1900–1930." in E. C. Lagemann (ed), *Nursing History: New Perspectives, New Possibilities* (New York: Teachers College Press, Columbia University, 1983); B. Bates, *Bargaining for Life. A Social History of Tuberculosis, 1876–1938* (Philadelphia: University of Pennsylvania Press, 1992). For further details of tuberculosis in the United States see P. Jacobs, *The Control of Tuberculosis in the United States* (New York: National T.B. Association, 1940); S.M. Rothman, *Living in the Shadow of Death* (New York: Basic Books, 1994).
36. Calmette, *L'Effort national.*
37. I. Grellet & C. Kruse, *Histoires de la tuberculose. Les fièvres de l'âme 1800–1940.* (Paris: éditions Ramsay, 1983).
38. Farrard to Rose, January 1, 1918. "Report of activities to December 31, 1917." RAC. RG.1.1 S.500 B.25 f.252. Also *Rockefeller Foundation History, Source Material.* Vol. 8: 1931–48.
39. E. Crowell. *Annual Report 1921 Bureau of Public Health Visiting.* RAC. RG.1.1 S.500 B.31 f.273.
40. Conference between Farrard and Rose, January 29, 1919. RAC. RG.1.1 S.500 B.26 f.257.
41. L. Williams, Report of Commission for the Prevention of Tuberculosis in France, 1919. RAC. RG.1.1 S.500 B.29 f.270.
42. L. Murand & P. Zylberman, "L'autre guerre (1914–1918)," p. 393.
43. Maps showing the distribution of these dispensaries from December 1918 to December 1922 are shown in RAC. RG.1.1 S.500 B.31 f.274–75.

44. As far as I can tell, L. Murand & P. Zylberman, in their "L'autre guerre (1914–1918)," seem far more positive about the American campaign than in their "Seeds for French health care." In the latter they claim the French tuberculosis campaign to be "mere incidents, more an endurance than a pleasure," and seem not to understand the early goals of the Health Board when they claim that the French experience caused them to switch from TB to a general public health program. The picture they attempt to portray, I think, is of France acting as an experimental laboratory the outcome of which would "force Washington to adopt similar standards for itself," and where they had learned to extend rural health movements to other areas of the world. Thus, they argue that rural health movements were not an extension of their Southern experience but of the French TB program.
45. E. Rust, *La Tuberculose* (Paris: A. Colin, 1954).
46. Wyatt to Williams, April 20, 1920. RAC. RG.1.1 S.500 B.26 f.259. There is a large literature dealing with the decline of TB. Those favouring social interpretations include Barnes, *The Making of a Social Disease;* T. McKeown, *The Modern Rise of Population* (London: Arnold, 1976); T. McKeown, *The Role of Medicine* (Princeton: Princeton University Press, 1979). Those acknowledging the impact of medical campaigns include A. Mitchell "An Inexact Science," and "Obsessive questions and faint answers"; L. Wilson, "The Historical Decline of Tuberculosis in Europe and America." *J. Hist. Med* 45 (1990): 366–396.
47. Commission for the Prevention of Tuberculosis in France. Summary of the present activities of the Commission in France, 1921. RAC. RG.1.1 S.500 B.31 f.273.
48. Gunn to Russell, January 24, 1924. RAC. RG.1.1 S.500 B.27 f.264. Gunn informed Russell of Risler's letter.
49. W. Rose to A.B. Cook. July 14, 1922. RAC. RG.5.1.2. S.427 B.141 f.1865.
50. Anne-Emanuelle Birn & A. Solórzano, "The hook of hookworm: Public health and the politics of eradication in Mexico," in A. Cunningham and B. Andrews (ed.), *Western Medicine as Contested Knowledge* (Manchester: Manchester University Press, 1997).

# II

# DISEASE ERADICATION

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# 4

# The First Hookworm Campaigns (1913–1920)

Rose's vision of the Health Board as a wedge that would prompt government involvement in developing public health service was not to prevail (although, as explained earlier, vestiges of it remained throughout the life of the organization). Instead, the Health Board focused on disease control and eradication. This was certainly the case when they began work on malaria and yellow fever in 1915. The move away from Rose's vision took longer in hookworm work; not until the early 1920s had the emphasis shifted to hookworm eradication.

Rose never seemed to realize that his long-term goal of developing a public health service was not possible within the constraints imposed by the British Colonial Office. Few colonial governments could afford to develop a public health campaign on their own. Each colony had to be financially self-sufficient, requiring minimal government expenditure, and, above all, minimal costs to the British taxpayer.[1] It was a empire on the cheap; hookworm had caused "grave anxiety" to the Colonial Office, but the political system limited officials to collecting and circulating information, and acting in an advisory capacity.

Once the Health Commission had been formed, Rose lost little time in organizing its first hookworm campaigns outside the South. In August 1913 Rose sailed for London in order, presumably, to set up the British colonial branch of the commission. A dinner at the Marlborough Club, a watering place for the British political establishment, enabled him to present an illustrated lecture on hookworm

to an audience some of whom later beguiled him with stories of the worm from far-flung regions of His Majesty's vast empire. The dinner allowed him to meet the British Colonial Secretary, Lewis Harcourt, who, no doubt fortified by a few glasses of vintage claret, is reported to have enthused, "I should not wonder if in the future we come to look back upon this evening and the gathering around this table as the beginning of a new day in the administration of our colonies and of a better civilization for all tropical countries."[2] It wasn't! The new day scenario of Harcourt had to wait for passage of the Colonial Development and Welfare Acts of the 1940s. Until then, the International Health Board encouraged the status quo by offering hard cash to colonial governments who were unable or unwilling to finance health campaigns from their own meager resources.

In preparation for Rose's visit, the Colonial Office formed an informal committee to consider the hookworm problem in the empire. If Rose—flattered by the attention he received in London and blinded somewhat by his own well-known Anglophilia—saw himself as a carrier of badly-needed ideas and expertise, he would have been disillusioned to read the correspondence of the hookworm committee. The empire was fully capable of mounting an effort to cure hookworm, the committee reported to the Colonial Secretary, "The problem was largely one of money, as to the West Indies, or of treating large numbers of intractable savages as in Tropical Africa, where conditions prevailed of which the American Commission could have no special experience."[3] To the British, the Rockefeller Foundation represented only an unlimited source of hard American cash.

The Colonial Office and Rose agreed that initial efforts would be directed against hookworm in British Guiana and other areas of the British West Indies, to be followed by Egypt, Ceylon and the Malay States. In October, after his return to New York where he presented his encouraging report to the officers of the International Health Commission, Rose set out again, this time for the West Indies including British Guiana.

## British Guiana

Following a meeting with Rose, Dr J. Ferguson, the colony's Chief Medical Officer, submitted a proposal to the Colonial Office for the relief and control of hookworm in Peter's Hall Medical District, south of Georgetown, with funds to be provided by the Health Commission. For the first time in the colony's history, action was to be taken outside the sugar estates. The plan was hastily approved and in March 1914, Hector Howard, former field director of the Sanitary Commission campaign in Mississippi, arrived as the Health Commission's representative. Unlike Rose, Howard was not bedazzled by the respect and courtesy shown by the British colonial officials. "I did not attribute [this] to any personal charm that I possess," he reported, "but rather to the seductive tinkle of the good hard

American coin." Neither did he imagine the task to be an easy one. It would be, he wrote, "a hard nut to crack."[4]

In June, 1914, Howard carried his case to a group of British physicians who made up the British Guiana Medical Association. He stressed that the problems in the Peter's Hall Medical District were similar to those faced by the Sanitary Commission in the U.S. South five years before. The Health Commission was an agency of public education, he told his audience, in that "the most effective and permanent sanitary campaign is that which by education creates in the minds of the people a desire for better sanitation, and thus secures their willing and hearty cooperation for all time."[5] Clearly Howard saw British Guiana as a Caribbean version of Mississippi, and hookworm a disease which "can be used to hold the interest and attention of the masses, which may teach them many useful lessons in sanitation." Howard was very much Rose's man.

The chosen area for the campaign lay on the east bank of the Demerara River. It was divided into three areas, each with a population of approximately 3000. In each several lectures were given before the first census was taken. Black "sanitary inspectors" took a census of each household, painted a number on each house, and gave each member a numbered can. The next day these cans, now containing feces, were collected and the contents examined for hookworm eggs by East Indian "coolies" trained in microscopy. Those whose feces were found to contain hookworm eggs were placed on a "thymol list" and treatments given at home, under medical supervision (Fig. 4.1).

Howard saw some problems. The "Negroes" and "coolies" did not come up to his Mississippi expectations. The former were "constitutionally obstructionists," he complained, "whose evident place is the field." But "patience, perseverance, persuasion and a knowledge of them acquired in the South, has enabled me to get by with them so far." He thought better of the East Indian coolies—"a docile people, easily led." What Howard missed were "competent white men," and the "efficient help of white microscopists." Worse, the population was "shifting" and "irresponsible;" a "motley crew" of mixed races.[6]

Among numerous difficulties with the British authorities, none was more vexatious than the privy problem. Believing the Rockefeller organization had access to an almost unlimited supply of money, the British assumed the Health Commission would be financially responsible not only for the surveys and treatment campaigns against hookworm but for the erection of privies.[7] The Health Commission believed that responsibility rested with the public health department, the government authority responsible for inducing the villagers to build their own primitive privies out of bits of wood and kerosene tins. In the commission's eyes, any hookworm campaign was built around two staffs. One of them, financed by themselves, was responsible for surveying, treating and educating the public. The subsequent introduction and maintenance of any sanitary measures to prevent reinfection was, in contrast, to be left to the second staff, funded by the British government. This

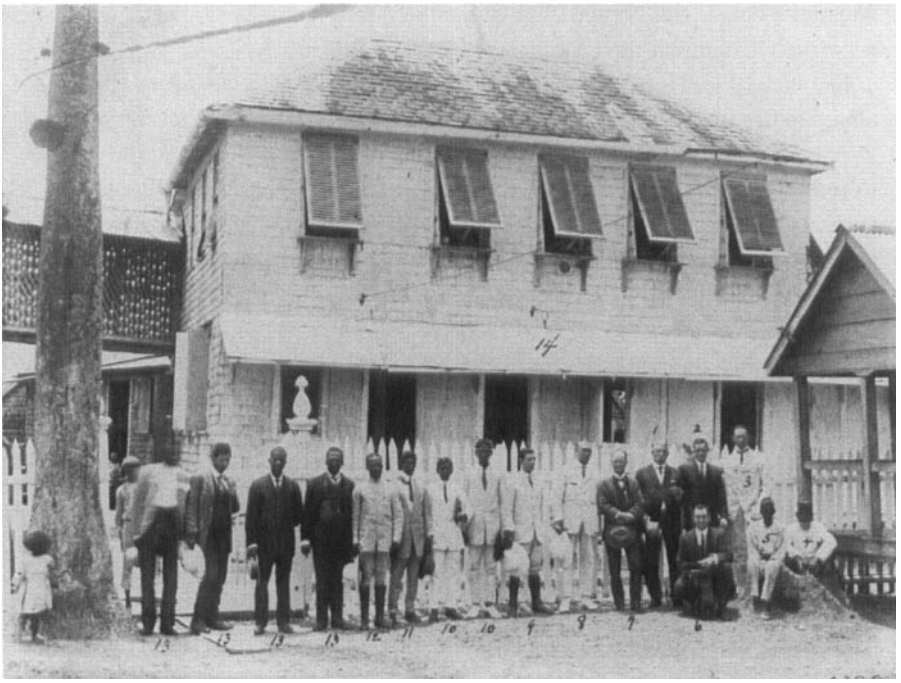

**FIGURE 4.1.** First foreign hookworm campaign, British Guiana. Bene, seated second from right, was the first patient (courtesy of the Rockefeller Archive Center).

fit into the traditional Rockefeller view of philanthropy in that the first staff, funded by the Health Commission, was necessarily temporary, while the second, funded by the recipient government, was permanent.

To add to the difficulties some of the British medical officers and officials believed the treatment campaigns a waste of time and money; privies provided the only long-term solution to the hookworm problem and obviously the Americans should build them. The disagreement came to a head when, in 1917, Dr. A. T. Ozzard of the British Guianan Medical Service, made his views known to a meeting of the Society of Tropical Medicine and Hygiene in London.[8] Lately, he told his audience, they had been joined by members of the "Rockefeller Health Commission, fortunate in the circumstance that they are endowed with a vast sum of money," too much of which had been used to print "numerous statistical tables and reports." None of their money, he complained, had been spent on latrines. "I maintain," he told his British audience, "that if these vast sums of money had been spent entirely on sanitary measures, such as the building of suitable latrines, etc., the results of this Commission would have been far more reaching, and they would have been permanent." Mr. Rockefeller should spend his money on latrines, he urged, "rather than on an uncertain but undoubtedly nauseous drug such as thymol."

A member of the audience, Robert Leiper, lecturer in helminthology at the London School of Tropical Medicine, understood what the Health Commission was about. At the end of the lecture, he jumped up to tell Ozzard that the Health Commission did not enter any colony with the intention of relieving the government from responsibility for the elimination of any disease. What they attempted to do, he told Ozzard, was to demonstrate to the government that by spending money in certain ways hookworm could be controlled. It was not their job to clean up the colonies and leave them free from infection, Leiper said, that was the job of the local authorities.

Work in British Guiana nevertheless went on, gradually expanding into other areas of the colony. By February 1919, the now-standardized report looked encouraging: 144 lectures had been given to 28,299 people; 2,136 pit-privies had been built by the villagers (presumably at their own expense using supplies at hand) representing an 11% increase in homes with them. The cure rate looked even more encouraging to New York eyes: 62% infected, 81% cured.[9]

But it was never quite as it seemed, although officers of the International Health Board—it was renamed thus in 1916—always seemed to take such "objective" data at face value and placed extraordinary faith in hard numbers, even though they were neither as objective nor as truthful as they liked to believe. Was the absence of eggs in a fecal sample a cure, or had the microscopist simply missed seeing any? Did someone actually count the reported 28,299 people said to have attended the 144 lectures? Did anyone point out in New York that the 81.2% cure rate represented the percentage of those treated who were apparently cured, not the percentage of those found infected.

## To the Orient

At the end of 1913, immediately after his return from the West Indies, Rose received permission from the British government to visit Egypt, Ceylon (Sri Lanka) and the Malay States in order to initiate a second round of hookworm campaigns, again with the emphasis on surveys, treatment and education.

Hookworm was clearly a problem in Ceylon, but Rose found little evidence of anemia among rubber estate workers in the Malay States. Thus, no action had been taken by the British on these estates in contrast to the situation in British Guiana. Indeed, most physicians saw malaria, not hookworm, as the major problem, and some estates had hired physicians to deal specifically with malaria. Malcolm Watson himself was working on malaria control in the estates surrounding the village of Klang.

Rose and Simon Flexner agreed to strike up a committee to study the situation in the Malay States and determine whether a hookworm campaign was warranted there.[10] It would be embarrassing to launch a campaign against hookworm when the local authorities focused on another disease and believed hookworm only a

minor affliction. Sam Darling was named head of what came to be called the Uncinariasis Commission. A native of New Jersey and a 1903 graduate of the Baltimore College of Physicians and Surgeons, Darling had served as head of the laboratory service of the Panama Canal commission before joining the Health Commission staff in 1915. Like Rose, he was an Anglophile. I have met a fine lot of Englishmen, Darling wrote to Rose, and most planters were "public school men and gentlemen . . . the hereditary nobility of mankind." He approved of the way in which the British kept their distance from indigenous populations. "As far as I can see," he wrote, "it is only the English and the people of the Southern States that have maintained a proper barrier against certain results of miscegenation."[11] In March 1915, Darling arrived in Kuala Lumpur with his team: Marshall Barber, from the Philippines and Henry Hacker of the Malay States. They quickly ascertained based on work in the Malay States and in Java, that malaria indeed had many times the impact of hookworm.[12] Rose dropped the idea of a hookworm campaign in the Malay States.

## India and Ceylon

The Tamil "coolies" of Southern India and Ceylon were another matter. They had been brought from their homes in the south of India to work the tea, sugar, and rubber estates of British colonies, and they were thought to be major carriers of hookworm, even into the United States.[13] Rose and Darling had nothing but scorn for the Tamils. Rose criticized their lack of ambition and initiative while Darling regarded them as an ignorant, superstitious, servile, unambitious, docile and unstable people. They were known to carry large hookworm loads, and were, according to Darling, disinclined to use toilets, with the result that they had become "an indirect menace to the world."[14] It was clearly time to look at the heartland from whence sprang this menace.

For over a century the Tamils, destined to work the colonial plantations, had been collected at emigration camps in southern India before being shipped abroad. Victor Heiser, after his appointment as Director of the East, travelled in India for a little over three weeks, visiting some of these camps.[15] Much to his displeasure, he found that hookworm examinations were not required at the Ceylon immigration camp in Mandapam—whose medical staff he dismissed as a lazy lot—and that the Malay States government camp in Negapalam, 200 miles south of Madras, seemed even worse. Negapalam was filthy, hookworm was ignored, and there was abundant fecal pollution of the soil. Heiser concluded that many would become infected with hookworm in the camp itself. After meeting with a Captain A. Russell of the Madras Medical College, a Scot whom Heiser immediately recognized as a "real type of American hustler," Heiser concluded that a campaign like those in the West Indies would be successful only if placed under the energetic leadership of an Indian Medical Service officer, and with government sanction, military sup-

port and legal power to build privies. Heiser brought up the question of working in India with the director of the Indian Medical Service, Major General Lukis, and his assistant director during a visit to the famous Himalayan foothill station of Simla. Both were keen about future hookworm campaigns; but they worried about possible nationalist uprisings, and uncertain of what impact the Americans would have. In addition, there was such a demand for medical officers on the Western Front that detaching any to examine Indian recruits for hookworm had become impossible.[16]

Nevertheless, Lt. Col. Clayton-Lane of the Indian Medical Service took charge of a two-year hookworm campaign in the Darjeeling tea estates—funded by the Indian Research Fund Association—where the population could be placed under tight authority.[17] The campaign was not as simple as anticipated. The apathy and fatalism of the population made cooperation difficult; many feared that, once cured, they would be enlisted in the Indian army.[18] In addition, the collection of feces must have met difficulties not mentioned by Clayton-Lane. Everyone was given a numbered piece of paper which, in the absence of latrines, was left in the bush with the feces. The "sweeper" then collected the samples from the bush and placed each in a phial marked with the appropriate number. But not only would the sweepers, the lowest level of the untouchable Hindu cast, have been illiterate, unable to read the numbers, but to touch human feces with ones hand was an act of defilement for any Hindu.

In order, presumably, to gain support from members of the Darjeeling Planters' Association, Clayton-Lane had promised that treatment of even light infections would improve worker efficiency. Sure enough, this self-fulfilling promise materialized as seen in a series of letters from these planters. Workers were healthier and did far more work, according to one, while another claimed a 50% increased productivity. Another enthused that "the change in health, spirits, and appearance of the Labour Force has been really remarkable." These letters were the only evidence advanced by Clayton-Lane to support his conclusion that hookworm "carriers," those with mild or no symptoms, did indeed benefit from treatment, and that the absence of symptoms did not indicate that hookworm was a harmless disease among Indian "coolies," as had been assumed by the British in Malaysia.[19]

News of this campaign reached Heiser, who must have been gratified to learn that hookworm did indeed cause an appreciable diminution in health and energy and a great economic loss to the country. In April 1920, George Paul arrived in Madras to help expand these campaigns.[20] But by the end of the year, Paul's patience was gone. Without latrines people were constantly being reinfected he told Heiser. Between 83% and 100% of Tamils, he said, were "in a pitiful physical condition," and were infected with hookworm showing moderate to severe clinical conditions. Even if the estates were provided with privies it would serve little purpose; the workers regularly returned home to their villages where they were

likely to become reinfected. Clearly, the task was monumental.[21] The Health Board withdrew from India; its one-year campaign had been a disaster. But perhaps campaigns in Ceylon would be more successful?

They were not. Early hookworm campaigns in Ceylon were no more successful than those in India.[22] Rose had first arrived there at the end of April, 1914, and learned that there were about 500,000 Tamils working in over 1900 tea and rubber estates on the island, 100,000 of whom entered each year mostly through the government camps of Mandapam and Tattaparri on the Indian mainland. Each year, however, 60,000 returned to India more heavily infected with hookworm than when they had first arrived. Rose saw much sickness among the Tamils, with an estimated 25% of estate workers ill at any one time, and soil pollution was universal in both the estates and the Sinhalese villages.[23]

The Ceylon Department of Health and the powerful Planters Association had for years been blaming each other for lack of hookworm work. The government felt that any hookworm campaign would be useless unless the estate managers built, maintained, and enforced the use of latrines, and the planters presented social and economic arguments to explain their failure to do so.

According to these managers, a keen and unwholesome rivalry for coolies had developed between the estates. With the Tamils' services at a premium, they told Rose, it would be impossible to enforce sanitary measures to which they objected–and, according to them, coolies clearly objected to using latrines. But that, I suspect, was not the real reason for their reluctance to build latrines. The planters knew, as did the equally ruthless owners of South African gold mines, that there was little competition for coolies. On the contrary, with a high turnover and an inexhaustible supply of cheap labor available in India, funds spent on health and sanitation could not be economically justified.[24] But this was not something they admitted to Rose. Neither were the planters convinced by the argument that healthy workers would be more productive. Across the island a day's pay had been standardized and fixed, based on work a normal anemic worker would be expected to complete in a day. Thus, even if they could do more work, neither the estate nor the worker would gain any financial rewards. Nevertheless, American money once again spoke volumes, and planters fell over themselves promising Rose their hearty cooperation.[25]

Rose recommended that a joint government-planters committee be set up to carry out a hookworm campaign; initial demonstrations would begin on a small number of estates with high infection rates and where the planters had agreed to cooperate. Conditions were not favorable (Fig. 4.2). The "lines" where the Tamils lived consisted of a double row of airless back-to-back rooms, eight-by-ten feet, where the entire family lived. And, except for a small swept area adjacent to these rooms, the ground was covered with trash and feces, virtually none of the estates had provided usable latrines for workers.[26]

When John Snodgrass arrived in Ceylon in December 1915 to take charge of the campaign, after service in the Philippines and the West Indies, he was happy

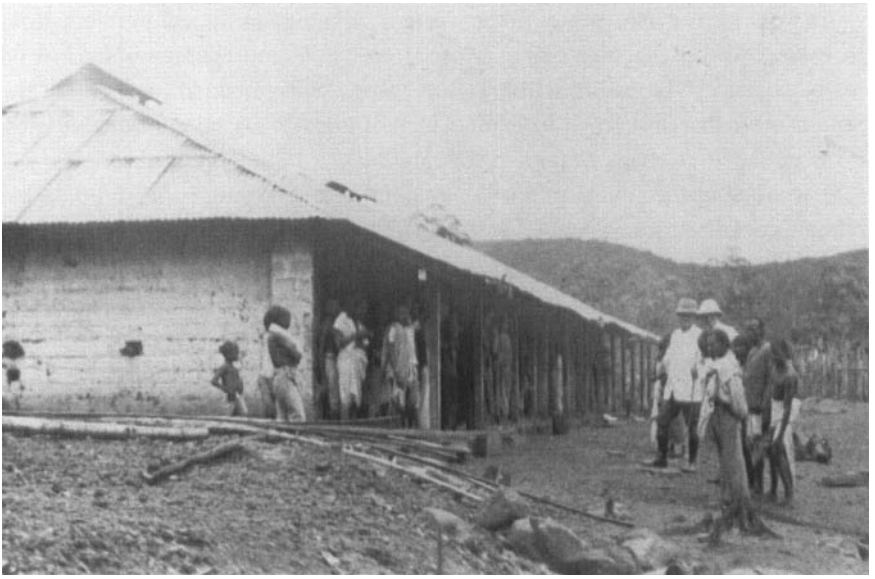

**FIGURE 4.2.** "Coolie" lines, Ceylon (courtesy of the Rockefeller Archive Center).

to find the hookworm committee had already targeted a number of estates in the Matale District of the Central Province for a campaign that was to begin the following month. "It is a new experience for me to enter a community where everyone is alive to the value of a public health measure and ready to support it in a financial way," he enthused, while privately expressing reservations about the planters' willingness to erect latrines.[27] Nor could it have lightened his burden when Heiser warned him of his heavy responsibility. "The success of the Ceylon campaign," he wrote, "is the very keystone of the arch for the development of our entire campaign in the East." If it failed in Ceylon, it would be doomed elsewhere.[28]

Persuading the estate managers to erect latrines was seen as the key. "It is certain," Snodgrass wrote in the *Planting Gazette*, "that a health measure that cures more than 90% of a labor force of a disease that presents the debilitating influence of ankylostomiasis, will have a tremendous influence in improving the working efficiency of this force."[29] During 1916 the government build pit latrines in two of the villages in the Matale area, while the managers of 10 of the estates, who had been pressured to appreciate the economic advantages of a disease-free workforce, erected 51 latrines with a total of 302 compartments. By the end of 1917, when the Matale work was completed, 97% of the Tamils and of the surrounding villagers had been found to be infected and 92% of the 35,785 treated Tamils were pronounced cured.[30] Amazingly, such a ludicrous cure rate was never questioned in New York.

As work moved onto estates in the western, Sabaragamuwa and southern provinces, reports told of a high rate of reinfection and the improper use of latrines on many estates.[31] The habits of the Tamils were usually blamed for this, but the reports noted that the latrines were often built of flimsy material at unsuitable sites, far from the lines, many of them disintegrating and falling into disuse.

Exacerbating the situation, post-campaigns (the re-examination and possible retreatment of previously treated workers) in the Matale region met with a hostile response from the managers; they also revealed a very high reinfection rate.[32] Moreover, planters learned that, contrary to what they had been told, the treatment campaigns had not increased their workers' efficiency. Indeed, in some estates efficiency seemed to have declined. Similar news came from the southern estates. Some managers reported that the main result of the treatment campaigns had been not an increase in worker efficiency but a doubling of births. As one remarked, "The extra vitality in the coolie is more apt to be manifest in an increased population than in tea production."[33]

The year 1919 saw the arrival of the postwar flu pandemic together with a 50% cut in workers' rations brought on by monsoon failure in India and a drastic drop in rice shipments to Ceylon. A year later the price of tea and rubber collapsed; faced with the need to cut back on expenditure, the Hookworm Committee petitioned the governor to abandon the campaigns on the estates. Henceforth campaigns would be limited to government-run campaigns in Sinhalese villages.[34]

The four-year hookworm campaign in Ceylon had not been a success; hookworm-related deaths on the estates had not declined between 1916 and 1920.[35] This was due, officers of the Health Board concluded, to failure to prevent reinfection through the proper use of latrines and the refusal by concurrent estate managers to accept that a healthy work force would increase worker efficiency. Furthermore, William Jacocks, who had replaced the much maligned Snodgrass, was not happy that their campaigns had focused on the estates. He reminded Rose and the governor that the Health Board had done nothing for the permanent population of Ceylon and had not worked to develop a public health system on the island. As far as Jacocks was concerned the Health Board should withdraw and take fresh stock of the situation.[36]

## Pre-Sanitation

By 1920 Health Board officials had come face to face with the pecuniary indifference of the British Colonial Office, the apathy of the Indian and Ceylonese authorities and most estate managers, and the impossibility of arousing a public that obviously, in their eyes, represented various degrees of inferiority. In other words the Sanitary Commission model could not be applied in this situation. They had also come to realize that high rates of reinfection rendered attacks on hookworm futile. The idea grew that the Health Board henceforth would refuse to carry

out hookworm campaigns unless latrines were already in place and in use. This change in approach was reflected in a letter sent by Heiser in March 1920, to Connors, then in charge of the yellow fever campaign in Guayaquil. Because of reinfection the hookworm campaigns in Puerto Rico had been useless, said Heiser. Campaigns would stop "unless the program includes the building of latrines and systematic inspection to ensure their use."[37]

This new plan of operations had first been broached in the West Indies. By 1919 disturbing results from Trinidad showed that despite a reported cure rate of 77.1%, many individuals counted as cured were found to be infected again 9 to 29 months later—a reinfection rate of between 18% and 87% in the 16 areas tested. More significantly, these rates of reinfection varied inversely with the number of sanitary privies in the area ( Fig. 4.3), although the correlation appears far too good to be true.[38]

Thus, in 1919, when the Planters Association of British Guiana asked the Health Board to continue and extend its work, Howard laid down preconditions: Each area to be treated must first be sanitized at British expense to guarantee against reinfection. The Planters' Association agreed, and they and the Health Board proposed a long-term scheme incorporating these ideas, without the implementation of which there would be no further Rockefeller campaign.[39]

But the campaign never took place. Governor William Collette poured cold water on the elaborate sanitation scheme. "I wish to express my emphatic disbelief in the idea that it is possible by any means whatsoever to stamp it out once for

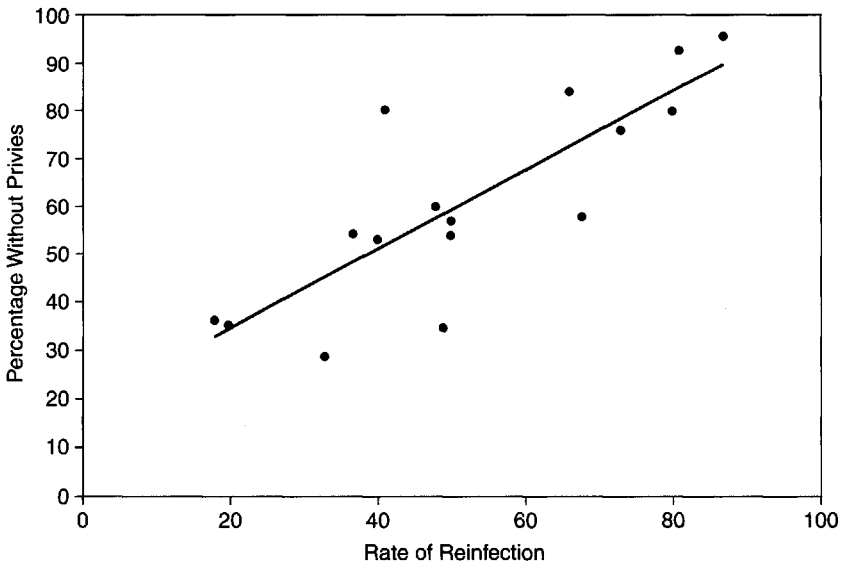


FIGURE 4.3. The relationship between the rate of reinfection with hookworm and the percentage of homes without sanitary privies in sixteen districts of Trinidad, 1919.

all." Treatment and sanitation would reduce it, he agreed, but like the "locust plague of Cyprus" the scourge would return as soon as any campaign ended.[40] Within the British Empire as then financed, he probably had little choice, even though the planters promised to share the costs. But in 1920 sugar prices declined drastically, and economic tightening throughout the Caribbean effectively destroyed any hope for a renewed campaign. And so the first foreign hookworm campaign came to an end. And, Howard pointed out, the campaign in British Guiana was the first where conditions of presanitation were required.[41]

By 1920 there had been a marked shift away from Rose's vision for the International Health Board; an organization which demanded prior sanitation was no longer one which carried out hookworm campaigns in order to pressure governments to take action in behalf of public health. Campaigns in the Caribbean, India and Ceylon which, presumably, had these goals initially, had failed; governments and estate managers failed to act. By demanding prior sanitation, the goals of the Health Board had changed, although there is no evidence that anyone took a step back to realize what had taken place. Given that the field staff were all physicians, this is not surprising, although one might have expected Rose to notice the change. The shift in policy did not happen overnight; but slowly Rose and the Health Board looked increasingly towards the eradication of hookworm as the major goal; the parasitic worm was no longer a means to an end.

## Notes

1. This issue is discussed further in D. K. Fieldhouse, *The Colonial Empires* (London: Weidenfeld and Nicolson, 1966); Fieldhouse, *Colonialism 1870–1945* (New York: St. Martin's Press, 1981); P. Hetherington, *British Paternalism and Africa, 1920–1940* (London: Frank Cass, 1978); S. Constantine, *The Making of British Colonial Development Policy, 1914–1940* (London: Frank Cass, 1984).
2. Reported by Rose, IHC Minutes, October 4, 1913. RAC.
3. Correspondence of Hookworm Committee. Miscellaneous Papers, Colonial Office. CO/ 885/23/297.
4. H. Howard to J. Ferrell, March 13, 1914. RAC. RG.5.1.2 S.423 B.11 f.159.
5. H. H. Howard. Address to the British Guiana Medical Association. June 19, 1914. Ibid.
6. Howard throws these racial slurs, characteristic of that time and place, in letters to Ferrell, April 10, May 19, 1914. Ibid.
7. The literature on latrines is enormous. The issue was discussed in the *International Health Board, Annual Report*, 1918.
8. A. T. Ozzard, "Some sanitation problems of the sugar estates and villages of British Guiana," *Trans. Soc. Trop. Med. Hyg.* 11 (1917): 71–92. Leiper's comments are included in this publication.
9. *Report on work for the Relief and Control of Uncinariasis in British Guiana from March 15, 1914 to February 15, 1919.* RAC. RG.5.2 S.423 B.42 f.252.
10. The tentative outline of work to be done in the Malay States is outlined in Rose, "Memo for the Uncinariasis Commission," April 20, 1915. RAC. RG.5.1.2 S.600 B.15 f.230.

11. Darling to Rose, November 15, 1916. RAC. RG.5.1.2 S.600 B.36 f.559.
12. They discovered that the people of a Javanese fishing village had an average hemoglobin count of 38.9% below average for uninfected people. From malaria-free areas of Java they had calculated that an average of 11.7 worms was required to reduce the hemoglobin level by one percent. Thus, the argument went, since the villagers carried an average of 80.4 hookworms each, their hemoglobin levels should have declined by [80.4/11.7] or 6.9%. But their actual count had declined by 38.9%; so, while hookworm had been responsible for a decline of only 6.9%, the decline caused by malaria was 32%. Among men of this Java village, therefore, malaria caused almost five times as much anemia as hookworm, whereas among the women the cost was an amazing fourteen times as high. Darling et al., *Hookworm and Malaria. Research in Malaya, Java and the Fiji Islands. Report of the Uncinariasis Commission to the Orient (1915–1917)*. RAC RG.5.1.2 S.600 B.15 f.230.
13. H. Tinker, *A New System of Slavery: The Export of Indian Labour Overseas, 1830–1920* (London: Oxford University Press, 1974); P. Emmer (ed), *Colonialism and Migration: Indentured Labour before and after Slavery* (Dordrecht: Martinus Nijhoff, 1986).
14. Rose's comments are recorded in *Rockefeller Foundation History*, Vol. 2; Darling to Rose, November 15, 1916. RAC. RG.5.1.2 S.600 B.36 f.559.
15. Heiser's considerable duties are outlined in Exhibit A. RAC. RG.5.1.2 S.600 B.15 f.233. Basically he was required to carry out medical surveys, direct work on hookworm and other diseases in the East—an area that included Egypt, Ceylon, the Malay States, India, Indo-China and Java. He was also expected to establish training schools for public health physicians and nurses, develop hospitals and dispensaries, and organize public health agencies—all to be done "without large expenditures on the part of the Commission."
16. Heiser describes his Indian trip in his "Notes on 1915 trip." RAC.
17. Clayton-Lane, "An investigation into Ankylostome infection in 11,000 inhabitants of the Darjeeling District of India." RAC. RG.5.3 S.464 B.204. *Indian J. Med. Research*. 4 (1916).
18. One can understand this concern. During World War I Indian troops fought and suffered horrendous casualties on the Western Front and at Gallipoli.
19. Clayton-Lane, "Final report of the Ankylostome inquiry in the Darjeeling District of India," RAC. RG.5.3 S.464 B.204. *Indian J. Med. Research*. 5 (1917).
20. Heiser to Paul, February 13; Paul to Heiser, February 29, 1920. RAC. RG.5.1.2 S.464 B.102 f.1390.
21. Paul to Heiser, November 27, 1920. RAC. RG.5.1.2 S.464 B.102 f.1390. G. Paul, "Incidence of Hookworm Disease, Madras Presidency, India. April-December, 1920." RAC. RG.5.2 S.464 B.49 f.305.
22. The Ceylon campaigns are discussed in greater detail by Soma Hewa, *Colonialism, Tropical Disease and Imperial Medicine. Rockefeller Philanthropy in Sri Lanka* (Lanham: University Press of America, 1995); and "The hookworm epidemic on the plantations in colonial Sri Lanka." *Medical History* 38 (1994): 73–90.
23. Rose's Diary: April 25, 26, 27, 1914. E, Meyer, "Considerations of Importance in Connection with the Control of Hookworm Disease in Ceylon." October 14, 1914. RAC. RG.5.2 B.47 f.288.
24. See "The Economics of Disease," in J. Farley *Bilharzia: The History of Imperial Tropical Medicine* (New York: Cambridge University Press, 1991).
25. Rose's Diary. May 8, 1914. RAC.

26. S. Hewa, *Colonialism, Tropical Disease*, p. 39.
27. J. Snodgrass to Rose, December 24, 1915. RAC. RG.5.1.2. S.462 B.14 f.203.
28. Heiser to Snodgrass, February 7, 1916. RAC. RG.5.1.2 S.462 B.35 f.541.
29. J. Snodgrass, "Uncinariasis in Ceylon," *Planting Gazette*, April, 1916. RAC. RG.5.3 S.462 B.192.
30. *Report on Work for the relief and Control of Hookworm Disease in Ceylon.* January 1916 to December 1917. Ibid.
31. Summary prepared for Principal Civil M.O.—1919 Report; Ancylostomiasis Operations—Ceylon. Summary for 1929. RAC. RG.5.3 S.462 B.193, 194.
32. Post Campaign in Matale, September, 1918. RAC. RG.5.2 S.462 B.48 f.298.
33. "Report on Efficiency of Coolies on Certain Estates in the Matale Area six months preceding and six months following cure for Ancylostomiasis," May 1917; "Report on Time and Percentage of Reinfection after Cure for Ancylostomiasis on Certain Estates in the Matale Campaign Area," May 1917; Post-Campaign at Matale. September, 1918. RAC RG.5.2 S.462 B.48. f.298, 301. *Report on Work for the Relief and Control of Hookworm Disease in Ceylon. Annual Report 1919.* RAC RG.5.3 S.462 B.193.
34. Summary prepared for the Principal Medical Officer, 1919; Ancylostomiasis Operations, Ceylon. Summary for 1920. RAC. RG.5.3 S.462 B.193, 194.
35. S. Hewa, *Colonialism, Tropical Disease.* Table 3.5.
36. Jacocks to Heiser, October 19, 1920. RAC. RG.5.1.2 B.101. f.1386. Jacocks, with an M.D. from the University of Pennsylvania, was appointed director of India and Ceylon in 1927, after obtaining his Diploma in Public Health from Johns Hopkins.
37. Heiser to Connor, March 22, 1920. RAC. RG.5.1.2. S.317 B.96. f.1323.
38. Data from G. C. Payne, *Report on Work and Relief and Control of Hookworm Disease in Trinidad,* August 11, 1914 to December 31, 1919. RAC. RG.5.2 S.451 B.45 f.274.
39. Dershimer, "Plan for Increasing the Efficiency of the Working People of British Guiana by the Eradication of Preventable Diseases from the Colony, for Increasing the Population both by Increasing the Birthrate and Decreasing the Death Rate, and by Making Conditions such that they will be Attractive to Prospective Immigrants." RAC. RG.5.2 S.423 B.42 f.253.
40. Speech by Gov. Collette. December 1919. Ibid.
41. H. Howard. Confidential Report of the 1923 Itinerary. RAC. RG. 5.2 S.420 B.41 f.248.

# 5

## Retreat from Hookworm (1920–1930)

By 1920 the Health Board's hookworm campaigns were in disarray. The authorities in British Guiana, India, and Ceylon had failed to live up to their responsibilities for improving sanitary conditions. The Health Board now believed that without prior sanitation any hookworm campaign would be doomed because of repeated reinfections from polluted soil. Rather than using hookworm as a means to encourage authorities to develop public health agencies by which sanitary conditions could be improved, it now required that these conditions be in place *before* any campaign began. Hookworm campaigns would now focus on the parasite and not on a public health response. What mattered was how well they succeeded in eradicating the parasite; the Health Board now had to come up with an accurate measure of hookworm prevalence in order to assess the outcome of their campaigns.

That had never been easy. Detecting hookworm eggs in fecal smears had always been a hit-and miss affair; failure to locate eggs in light infections must have contributed to the extraordinary cure rates reported at that time. In the mid-1920s, field staff began to use a flotation method which revealed eggs in the feces even when worm loads were low, and a dilution method was used to estimate the total number of eggs produced. These methods had been devised by Norman Stoll of the Department of Medical Zoology at the Johns Hopkins School of Hygiene. Stoll had also linked the egg count with the number of worms expelled after treatment, enabling worm loads to be calculated from the egg count.[1]

The discovery of this new and more accurate egg-counting method led to a fundamental question: When assessing the outcome of a campaign what actually should be measured: the prevalence of the hookworm parasite or the intensity of hookworm disease? The answer seemed to be both, and it arose out of the Health Board's work in Alabama where, using the Stoll method to calculate the number of worms, they gave hard numbers to the well known fact that a subjective evaluation of disease severity was correlated with the worm load.[2]

| | |
|---|---|
| 0–25 worms | no symptoms |
| 26–100 worms | light disease |
| 101–500 worms | moderate disease |
| 501–1000 worms | severe disease |
| 1001–3000 worms | very severe disease |

Field workers now measured the severity of hookworm illness by this method, one which they extended to every country with a hookworm problem. However malnourished the population, and whatever species of hookworm was involved, it was assumed the same number of worms produced the same intensity of disease. This represented an extraordinary assumption, and illustrates again the indifference of the Health Board to the social and economic conditions in which hookworm victims lived.

As if aware of these naive assumptions, staff member Henry Carr, in his 1924 hookworm survey of Mexico, measured the severity of the disease by hemoglobin count, rather than the number of worms. The hemoglobin index, a measure of anemia, seemed to him a far better gauge. He was able to show a high degree of negative correlation between the number of eggs and the hemoglobin index. Moreover, although nearly 10% of males were found to have a severe infection indicated by a hemoglobin count of less than 36.5%, the number of eggs liberated by these males indicated that they carried only about 450 worms, enough only for a moderate infection according to the Alabama data.[3] Nevertheless, most field officers continued to use the egg-count method.[4]

## Ceylon (post-1920)

In Ceylon pessimism was rampant. No provisions were being made to keep estate latrines clean or in good repair, the chief medical officer of the colony complained in 1919, rendering hookworm campaigns "nugatory and futile."[5] This and an economic turndown in Ceylon had resulted in the closure of the hookworm campaigns at most of the estates at the end of 1920.

William Jacocks, recently put in charge of the Ceylon campaign by the Health Board, was not overly distressed by this outcome. He had long complained about limiting campaigns to the estates, and had even threatened to resign over it.[6] Jacocks

eventually had his way; in 1921 intensive hookworm campaigns got underway in the Sinhalese villages of the Western Province, although only where presanitation already existed.

In 1925, the Health Board undertook a hookworm survey of Sinhalese villages, in three northern provinces, where hookworm campaigns had not been carried out, to compare the results with those of treated places in the south, west, and central areas. Dr. Winfield Sweet, transferred from Australia to temporarily replace Jacocks (then on leave), argued, on flimsy evidence, that both the prevalence of the worm and the morbidity of the disease were lower in the treated areas.[7] Despite what appeared to be low-grade infections, Sweet said it was time to begin an island-wide, mass treatment campaign centered in the village schools, the estates, and the Mandapam Camp. Moreover, he concluded, rural sanitation remaining the key to permanent control, rural health units should be formed.[8]

Jacocks agreed with him, recommending that whenever the average egg count exceeded 250 (i.e only 11 worms!), mass-treatment campaigns be put into effect. But, in the long run, Jacocks said, a permanent solution could come about only through general improvement in sanitation.[9] The mass campaign began in 1926, the same year the government established its first rural health unit in Kalutara in the Western Province; school hygiene, maternity clinics, public health education, malaria, typhoid, tuberculosis, and dysentery were ranged alongside hookworm as areas of concern. By the mid-1930, eight such units had been formed, a superintendent of health was appointed after receiving a Rockefeller fellowship to study in the United States, and Jacocks was appointed advisory health officer.[10] Financing came from the government, not the Health Board. Early in 1928, mass treatment campaigns were transferred to the Department of Medical and Sanitary services of the Ceylon government, and the Health Board withdrew from its hookworm work.

The Health Division should have been satisfied; with the opening of health units by the Ceylon government, the Health Division's campaigns had helped to achieve what Rose had originally set out to do. As historian Soma Hewa has noted, "this program became the framework and impetus for the remarkable progress in the field of public health which Sri Lankans have achieved in recent years."[11] But Rose's vision was fading and elsewhere the eradication of hookworm continued to be the major priority. And they clearly had not eradicated the worm from Ceylon.

## India

The hookworm situation in India was no better. The 1920 Health Board-assisted hookworm campaign in Madras Province had come to a halt with the resignation of the director, George Paul. Paul was replaced immediately by John Kendrick, but the future did not look promising.

Still, in 1922, a typical intensive hookworm campaign began in the North Arcot district (now a district of the State of Tamil Nadu) jointly funded by the Madras government and the Health Board. It was realized that 90% of those carrying the worms showed no ill effects, but it was claimed that even light infections rendered individuals more prone to tuberculosis, flu, pneumonia, and malaria.[12] The Health Board continued to insist that the problem of soil pollution had to be addressed before their campaigns began, but in fact this did not occur.

The reason for failure to apply the rules of presanitation, laid down only a few years earlier, probably rested with British officers of the Indian Medical Service, who remained hostile to any suggestion that they pay for privy building. A proposal that government pay 50% of the costs of constructing village privies was brought to the attention of the Surgeon-General of Madras. His response was negative. There was no money available, he told Kendrick. Furthermore, "it would be a mistake for this undertaking to be financed other than by the people themselves for, if it were, the wily Indian would demand that government bear the expense in every other area."[13] The government was willing to support only a treatment and educational campaign; privy building would be left to the people themselves. The British in the West Indies had opposed treatment campaigns as being a waste of time; privies were the answer. Their compatriots in India took exactly the opposite stand. And then came the "miracle of Madura."

## Madura

In 1926 the authorities in the Madura District Board, situated today in the south of the Indian state, Tamil Nadu, took matters into their own hands; they allotted Rs 20,000 towards latrine construction in and around the village of Usilampatti with a population of nearly 11,000. Initially, there were no latrines in the village, inhabitants went barefoot, and 93% were found to be infected. But the construction of 14-inch wide, 14- to 18-foot-deep open-air bore-hole communal latrines, surrounded by a five-foot high privacy-wall, together with lectures on hookworm, soil pollution, latrines and village sanitation, changed everything. Kendrick made a surprise visit to Usilampatti and was astonished to find that latrines had not only been constructed but were being used by large numbers of people. And in spite of being heavily fouled in places, boy scouts could be seen "early every morning, with three or four hundred people following them, singing songs on sanitation which their Tamil teacher had rendered into verse for them."[14]

To the surprise of the authorities, the latrines were so heavily used that construction resumed and by the end of the year a total of 351 were reportedly in place. As is often the case, such innovations generated a great deal of interest among the government elite who swarmed into the village for what today would be called a photo opportunity. "Our work was appreciated by one and all of them," a report

noted with pride, "and the general consensus of opinion was that our work of rural reconstruction was a model fit to be copied in other districts."[15] But, of course, it was not.

This seemed to be the breakthrough that the Health Board had long desired. Here, the local elite had viewed health matters as their responsibility. In a long brief to the Surgeon General, Kendrick pressed home his case. "That we have left almost untouched that factor which alone can produce permanent benefits strikes one as incongruous. We know definitely that the ultimate success of any anti-hookworm campaign depends upon the effective disposal of human excrement, and unless steps are taken to accomplish this the results of all other activities will be of temporary duration."[16] Sanitation is a government function, Kendrick said, and for the work in Madura to be continued another year Kendrick asked for an appropriation of Rs 76,140, 50,000 of which were to be directed towards latrine construction. The Health Board seemed on the verge of a genuine triumph.

This apparent miracle needs to be questioned. Communal latrines in the arid and stiflingly hot conditions of southern India would inevitably be filthy places, swarming with flies and reeking of human excrement. How could they be kept clean when, in the majority of villages, all water was hand-carried by the village women, often over many miles. Also, a few years earlier, Clayton Lane had reported on a facet of hookworm work that had never been commented on before, indeed it seemed almost heretical. The prevalence of hookworm in the two Darjeeling sugar estates with platform or pit privies, he reported, was higher than the average for all estates. He concluded that these privies were as active in promoting infections as soil in estates without privies. "Platform and pit-privies stand condemned," he said; they were "an active and serious danger." The provision of expensive septic tanks was Clayton Lane's only solution.[17] The Health Board ignored this ludicrous observation; privies were the solution, not the problem. They were quite willing to believe that the villagers in Usilampatti were only too anxious to share the facilities built for them and that in so doing the prevalence of hookworm would necessarily fall.

Perhaps the government was more aware of these problems than the Health Board officials, for money was not allotted; none in April and none in August. In December 1927, Heiser informed New York that it was time for the Madras government to take over the campaign. Putting on the most tinted of spectacles, Heiser and Kendrick claimed that "our main objectives have been achieved," and that officials in India now understood the hookworm problem.[18] In February 1928, the Health Division withdrew from hookworm work in India. From their own perspective, they had chalked up another failure. There was still a high prevalence of hookworm; even when the inhabitants of one village showed apparent willingness to use communal latrines, the British had refused to provide them. And with eyes firmly focused on the worms, they had shown the same ambivalence

towards the disease in India as they had in Ceylon. If the disease was as mild as their own worm counts had suggested, why carry on with the campaigns? There were surely more pressing problems.

## Puerto Rico

Success was equally hard to attain in Puerto Rico, where the prevalence of hookworm seemed as high in 1920 as it had ever been. "I have been to a lot of places in the world where they had hookworm," John Grant was to later reminisce, "but in Puerto Rico . . . the majority of people you met would have that peculiar color that heavy infestation gives in hookworm, that no other disease does. Their legs would be swollen, their abdomens swollen, and yet they were trying to carry on their daily work, and you wondered, how? You just didn't understand how they carried on. Oh that was . . . ! And the poverty."[19] By 1923, after attempts had been made to eliminate the worm, the infection rate had been whittled down a little, but the prevalence still remained alarmingly high.[20] Furthermore, resurveys of previously treated areas revealed rates and egg counts had, in most cases, returned to the pre-campaign levels. Treatment campaigns once again were seen as a waste of time; the Health Board's Caribbean director Hector Howard concluded, they must be replaced by intensive education and sanitary campaigns.[21]

Growing tired of these ineffective projects, Fred Russell decided to try another direction. He asked Dr. William Castle of the Harvard Medical School, an expert on pernicious anemia, to study anemia on the island in an attempt to find a causal dietary deficiency which might be counteracted by the addition of iron or liver extract to the diet.[22] Castle and his assistant, Cornelius Rhoads spent the summer of 1931 on the island, carrying out tests on anemic patients at the Presbyterian Hospital in San Juan. The following year Castle informed Russell that the addition of six grams of iron citrate daily in fact brought about rapid improvement, whether or not the patient received hookworm treatment. Iron therapy, said Castle, seemed the most practical measure for alleviating hookworm anaemia.[23] By then, however, Russell was in no mood to listen to Castle. Not only had he learned that these tests had been carried out by an untrained social worker, but Rhoads had managed to embarrass the Health Division and the American government by an extraordinary piece of behavior.

## The Rhoads Affair

Early in 1932, a letter, written by Rhoads and addressed to "Dear Ferdie," signed with the name "Dusty" fell into the hands of one Luis Baldoni, a laboratory assistant, who passed a copy of it to two Puerto Rican physicians at the hospital. Rhoads, expressed the view that Puerto Rico would be a perfect place to live were it not for Puerto Ricans! He wrote: "They are beyond doubt the dirtiest, laziest, most degen-

erate and thievish race of men ever inhabiting this sphere. It makes you sick to inhabit the same island with them. They are even lower than Italians. What the island needs is not public health work but a tidal wave or something to totally exterminate the population." He added: "I have done my best to further the process of extermination by killing off eight and transplanting cancer into several more. . . . all physicians take delight in the abuse and torture of the unfortunate subjects."[24]

The letter soon found its way to the local press, which accused Rhoads of being a "wholesale assassin." The *Asociacion Medica de Puerto Rico* demanded retribution. An editorial in a local paper spoke of the "perverse work of an American," who had "put his studies into practice in order villainously to kill Porto Ricans and help, as he asserts, in this way to exterminate the race."[25] The episode proved highly embarrassing to the political establishment on the island. On February 4, 1932, the very day that James Beverley was inaugurated as governor of Porto Rico, as it was then called, *The Porto Rico Progress* carried a vitriolic letter from Jose Lomeiro, secretary of the Nationalist Party, who used the Rhoads affair to claim that the American government was repeating in Puerto Rico the methods of extermination it had practised earlier against North American Indians. While the government portrayed Puerto Ricans as "a mass of hungry beggars unable to survive without North American charity. . . . the facts confirm a system of extermination."[26] *The New York Times*, published a statement by Castle defending the anemia studies and praising Rhoads for his work.[27]

Rhoads explained the letter was a "parody" of what a violently anti-American nationalist might think of an American physician whose Puerto Rican patient had died, and cabled his apologies to the governor. "Regret very much," he stated, "that fantastic and playful composition written entirely for my own diversion and intended as parody on supposed attitude of some American minds in Porto Rico should have become public document and taken literally by anyone." He had written it, he explained to the medical director of the Hospital, while somewhat inebriated and angry after his car had been broken into.[28] An official enquiry found that no deaths had occurred as a consequence of Rhoads' actions and that no cancer had ever been transmitted. "We are forced to conclude," wrote Jose Quiñones, special attorney general to the government of Porto Rico, "that Rhoads is a mental case or an unscrupulous person"—but he was not a criminal.[29]

And that was that, as far as *Time* magazine was concerned. In an appallingly insensitive article, it spoke of Rhoads as a "jovial, rollicking, young man," who, having gained a Harvard MD cum laude and having served as president of his class, could clearly do no wrong.[30] This article generated another outburst in the pages of the *Porto Rico Progress*, which. however, made a clear distinction between the hated Rhoads and the "noble institution" of the Rockefeller Foundation.[31] One can only speculate what would have happened had the press picked up on a letter sent by Beverley to Sawyer, stating that a second and worse letter from Rhoads had turned up, but had been immediately destroyed by the government.[32]

The Health Division was furious over the episode. "The strongest impression I get from reading about that letter," George Payne, the Health Division's new director in Puerto Rico wrote Sawyer, "is that there were too damned many irresponsible people in that laboratory."[33] Thus they had reacted perhaps more strongly than expected to what they regarded as a sloppy piece of experimentation by Castle's social worker.

A few years later, however, after Payne's wife had obtained similar results with diets supplemented by iron, Sawyer encouraged Payne to continue. Perhaps a serious nutritional deficiency might explain why hookworm seemed so much more prevalent and intense in Puerto Rico than elsewhere. But his hopes for finding another way out of the hookworm problem were quickly dashed. Measuring the effect of antihelminthic drugs and iron carbonate on the hemoglobin levels of 6- to 14-year old children, she found that two doses of iron per day for four weeks did lead to substantial and rapid increase in hemoglobin levels; but levels quickly slipped back when treatment stopped.[34] They were back to where they started, with privies.

## Egypt

Would, as Howard suggested, soil sanitation and privy building campaigns be the answer to the hookworm problem? In 1927 the Health Division provided funds for a sanitary engineer to oversee the sanitization of a number of Egyptian villages, and sent two parasitologists, J. Allen Scott and Claude Barlow, to measure the effects of these privies on the transmission of both hookworm and schistosomiasis, a serious disease of the Nile Delta which Rose had learned about during his first visit to Egypt.[35] But in his final report Scott came to "the inescapable conclusion that sanitation produced no measurable effect on infection with the species of worm parasites under study."[36]

The Health Division's hookworm work seemed a failure; treatment did not work and neither did latrine construction. In addition the Health Board appeared confused over their goals. Were they to be judged by the elimination of the worms or of the disease?

## The American South

In the American South work on hookworm continued unabated, backed by the general belief that the parasite was slowly being eradicated. But hookworm was not one of Frederick Russell's favored diseases; indeed it was unlikely to excite the interest of a laboratory man like Russell. The biology of hookworm was completely understood and its elimination seemed to require only latrines and shoes (although the latter were hardly mentioned). Under his leadership the budget for hookworm declined dramatically (Table 1.2). On the other hand, his astonishing statement in the 1926 Annual Report that "hookworm disease has almost disap-

peared from the United States and is rapidly coming under control in many parts of the world," must have come as a surprise to those who had been working in the Caribbean, India, and Ceylon.[37] But the statement could be taken two ways. Did he mean the parasite had almost disappeared, or was he referring to the disease, a distinction that had become possible with Stoll's egg-counting methods.

True, by 1926 Russell had been inundated by reports from the South showing a reduced prevalence of hookworm. Surveys between 1919 and 1922 showed that its prevalence had decreased quite dramatically from the period prior to the Sanitary Commission's work (1911–1914) and during the early years of intensive hookworm campaigns (1914–1918). Indeed, based on a resurvey of 32,917 schoolchildren in 60 counties of 10 southern states, the average prevalence of hookworm had decreased from 54.6% in the initial surveys to 28.1% by 1922.[38] Russell must have realized that the early data, derived from the unreliable smear technique, were highly suspect. And because the figures were not drawn from the similar age groups, with the same urban-rural mix and the same black-white proportions, they were not comparable.

Was Russell referring rather disingenuously to the disease, rather than the parasite? The hookworm parasite was still present, but in such small numbers in infected persons that the disease had almost disappeared. Evidence in support of this had come from the Health Board's field research unit in Alabama. They had concluded that most people had so few worms in their bodies that they had become merely symptomless carriers, and that the hookworm should be controlled only by sanitation, not treatment. Only those carrying a large worm load and showing symptoms of the disease should be treated and their intensity of infection thereby reduced to a "carrier state." What was now needed in the South, the Alabama workers argued, were intensity surveys using Stoll's egg-count method, not prevalence surveys as in the past. Only by such surveys could the correct preventive regime be initiated.[39]

Intensity surveys on schoolchildren in Covington County, Alabama, on the Florida border, showed that symptoms of hookworm disease were limited to the rural white children of school age. While over 50% of these children still suffered from hookworm disease (having a worm load in excess of 100), only 7% of their urban cousins and less than 10% of rural blacks did so. Only rural white children therefore needed to be treated for hookworm, other children relying on the use of sanitary toilets. The Alabama group argued that these methods would keep hookworm disease under control, but only by economic improvement and better education would the disease and the worms be eradicated. This data from Alabama hardly justified Russell's positive claims that the disease had almost disappeared from the South.

Charles Stiles, who had first drawn the Rockefeller philanthropies into hookworm, remained skeptical of Russell's optimistic pronouncement. In 1932 he told John Rockefeller Jr. that he had found many people on the sand lands of the South with what was called malnutrition but which "is hookworm disease pure and

simple." But when he mentioned this, he wrote, "I am met with the reply that, according to the Rockefeller office, hookworm disease has been practically eliminated from our southern states." He characterized that view as "exceedingly unfortunate, ill-advised, and unfounded."[40] The Health Division did not respond directly to Stiles. But, between 1932 and 1937, field officers in the South published a stream of papers claiming that hookworm in the late 1920s was a less serious problem than it had been in 1914. Since no early data existed on worm loads, they had only anecdotal evidence to substantiate that claim. The evidence was certainly suggestive, but there was no hard statistical evidence to back it up. Pockets of high prevalence persisted in regions with sandy soils.[41]

By 1930 the Health Division had withdrawn from hookworm campaigns in India and Ceylon. In the south, hookworm budgets had been gradually incorporated into state funding for health units, a process that was virtually completed by 1920. Hookworm campaigns continued in Central America, Mexico, and the West Indies during the 1920s, but by the end of the decade all the campaigns had virtually closed down; there were more important diseases that needed to be dealt with.

Failure was never admitted as a reason for closing down, although the Health Division came close to it in Puerto Rico. In the South they argued that the disease had been virtually eliminated as a public health problem. In Ceylon and India, where the disease also seemed to be a minor problem, they remained focused on the parasite and closed on the grounds that they had shown how, by the use of thymol and latrines, hookworm could be eliminated.

Moreover, the Health Division could always fall back on Rose's vision and claim their campaigns had led to an increased interest in public health. In Ceylon and in the short-lived Mexican campaign (1924–28) hookworm did became a wedge for building a permanent public health system. In writing about the Sanitary Commission, author John Ettling has argued that perhaps its most important legacy "was the network of state and local public health agencies it left in its wake," an argument that applied equally to the Health Board's activities in the South.[42]

Clearly the Health Board's hookworm campaigns were caught between the medical goal of eradication and Rose's vision of an opening wedge towards a more important end. But during the 1920s the medical approach moved irresistibly toward center stage as they turned their attention towards two new diseases, yellow fever and malaria.

## Notes

1. N. Stoll, "Investigations on the Control of Hookworm Disease XV. An effective method of counting hookworm eggs in feces." *Amer. J. Hyg.* 3 (1923): 59–70. Basically, a known weight of feces in a flask was thoroughly mixed with 0.1N sodium hydroxide. The eggs, released from the feces, floated to the surface; samples were

examined under a microscope slide, and the eggs counted. Stoll, "Investigations on the Control of Hookworm Disease XVIII. On the relation between the number of eggs found in human faeces and the number of hookworms in the host." Ibid., 156–79. Stoll claimed that each female worm produced on average 44 eggs per gram of feces; thus, dividing the number of eggs per gram by 44 gave the approximate number of female worms in the body. Doubling this number, assuming a 1:1 ratio of male to female, would give the total worm load.

2. W. Smillie, and Augustine, D. L. "Intensity of hookworm infection in Alabama." *J. American Med. Assoc.*, 85 (1925): 1958–62.
3. H. Carr, "Observations upon hookworm disease in Mexico." *Amer. J. Hygiene* 6 (July Supplemental, 1926): 42–61. At that time the hemoglobin index was ascertained by using a hemoglobinometer. According to Maxwell Wintrobe, *Hematology, the Blossoming of a Science* (Philadelphia: Lea and Febiger, 1985), the usual method was to pipette blood between two glass plates and compare the color with those of a graduated colored disc ranging from bright red to pale pink, each grade given a percentage value. Because different machines had different colored discs, the figures were never standardized and the degree of error must have been very high. What is the amount of hemoglobin equivalent to a 100% reading in a hemoglobinometer, a reader asked in 1926? In six machines tested, the amount varied from 13.8 to 17.2 gms/l00 mils, with the new Bausch-Lomb glass-disc hemoglobinometer, equilibrated in percentages, giving 16.92 gms. as the equivalence. "Queries and Minor Notes. " J.A.M.A. 87(1926): 1323. I would like to thank Margaret Humphreys for bringing these works to my attention.
4. Anne-Emanuelle Birn and A. Solóranzo seem confused by this, and state consistently that Carr found only a low level of infection. A-E Birn & A. Solórzano, "The hook of hookworm: Public health and the politics of eradication in Mexico," in A. Cunningham & B. Andrews (eds), *Western Medicine as Contested Knowledge* (Manchester: Manchester University Press, 1997); "Public health paradoxes: Science and politics in the Rockefeller Foundation's hookworm campaign in Mexico in the late 1920s." *Soc. Sci. and Med.* 49 (1999): 1197–1213.
5. Chief Medical Officer to Estate Agent's Association. April 16, 1919. RAC. RG.5.1.2 S.462 B.84 f.1181.
6. Jacocks to Heiser, October 19, 1920. RAC. RG.5.1.2 S.462 B.101 f.1386.
7. In the north, 96% of the inhabitants were found infected, with an average egg count of 1480, whereas, in the other areas, 89% were infected with an egg count of 995. Thus, according to the Alabama figures, those in the northern provinces carried an average of 67 worms, those to the south 45.
8. W. C. Sweet. "Summary report on the Results of a Hookworm Survey of the Island of Ceylon," May to December, 1925. RAC. RG.5.3 S.462 B.195.
9. W. Jacocks, "A Suggested Programme for Permanent Control Operations against Hookworm Disease in Ceylon by Treatment." RAC. RG.5.3 S.462 B.196.
10. S. Hewa, *Colonialism, Tropical Disease and Imperial Medicine* (Lantham: University Press of America, 1995) provides a detailed account of these health units.
11. Ibid., p. 127.
12. Surgeon General, Madras Presidency, "Anti-hookworm Campaign," March 25, 1924. RAC. RG.5.2 S.464 B.49 f.306.
13. J. Kendrick to Rose, August 9, 1922. RAC. RG.5.1.2 S.464 B.144 f.1899.
14. J. Kendrick to Heiser, September 14, 1926; Kendrick to Major General Hutchinson, Surgeon-General, Madras. September 7, 1926. RAC. RG.5.1.2 S.464 B.266 f.3363.

15. L. S. Narayanaswami, "Report on the Work for the Relief and Control of Hookworm Disease in the Madura District, South India," January 1–December 31, 1927. RAC. RG.5.3 S.464 B.205.
16. Kendrick to Major General F. Hutchinson, September 7, 1926. "Proposals for Expansion of Ankylostomiasis Campaign, 1927–1928." RAC. RG.5.1.2 S.464 B.266 f.3363.
17. Clayton-Lane, "Final report of the Ankylostoma Inquiry in the Darjeeling District of India," RAC. RG.5.3 S.464 B.204. *Indian J. Med. Research* 5 (1917).
18. Heiser to New York, December 15, 1927. RAC. RG.5.1.2 B.304 f.3858.
19. Reminiscences of Dr. John B. Grant. Transcript of interviews conducted by Saul Benison for the Oral History Research Office, Columbia University. Copies also in RAC. S.900 Hist. Grant, Vol. 1. p. 77. In 1919 the young Grant had taken his first posting in Puerto Rico where he carried out stool and hemoglobin surveys in nine representative communities. He found the prevalence of hookworm to vary from 77% to 100% while the hemoglobin index averaged 65%. "Hookworm Infection and Malaria Survey of Porto Rico," December 26–January 28, 1920. RAC. RG.5.3 S.243 B.73. In 1921 the Chinese-speaking Grant was withdrawn from the island and appointed representative of the Health Board on the faculty of the Peking Union Medical College, where he became a major player in the Health Division's Chinese, Japanese and Indian programs.
20. R. B. Hill, *Porto Rico Hookworm Control. Annual Report, 1923*. RAC. RG.5.3 S.243 B.73.
21. Howard to G. L. Payne, April 17, 1928; P. Putnam, "Porto Rico. Hookworm Resurveys. 1928–29 reports." RAC. RG.1.1 S.243 B.3 f.36.
22. Russell to Castle, December, 1930. RAC. RG.1.1 S.243 B.1 f.2.
23. Castle to Russell, June 14, 1932. RAC. RG.1.1 S.243 B.1 f.7.
24. RAC. RG.1.1 S.243 B.1 f.4.
25. Editorial, February 3, 1932 and note from Asociacion Medica de Puerto Rico, January 26, 1932. RAC. RG.1.1 S.243 B.1 f.4 and 5.
26. José Lameiro, *Porto Rico Progress*, February 4, 1932.
27. *New York Times*, January 30 and February 2, 1932.
28. Rhoads cable to Gov. Beverley; Press Interview with Galbreath, January 26, 1932. RAC. RG.1.1 S.243 B.1 f.4.
29. José Quiñones to Governor Beverley, February 11, 1932. RAC. RG.1.1 S.243 B.1 f.5. Mental case or not, the affair certainly did not hamper Rhoads's career. In 1940 he became Professor of Pathology at Cornell and a little later head of the Medical Division of the U.S. Army Chemical Warfare Service.
30. *Time*, February 15, 1932.
31. "Writes Time of Dr. Rhoads," *Porto Rico Progress*. Undated. RAC. RG.1.1 S.243 B.1 f.7.
32. Beverley to Sawyer, February 17, 1932. RAC. RG.1.1 S.243 B.1 f.6.
33. G. Payne to Sawyer, February 22, 1932. Ibid.
34. *Puerto Rico Anaemia Reports. First Semi-Annual Report for 1938*. RAC. RG.5.3 S.243 B.2.
35. Details of the campaign with emphasis on the schistosomiasis work is given in J. Farley, *Bilharzia: A History of Imperial Tropical Medicine* (New York: Cambridge University Press, 1991).
36. Scott and Barlow, "Limitations to the control of helminth parasites in Egypt by means of treatment and sanitation," *Amer. J. Hygiene* 27 (1938): 619–648.

37. *International Health Board, Annual Report, 1926*, p. 6.
38. "Hookworm Surveys and Resurveys, 1919 to 1922." RAC. RG.5.3 S.200 B.5.
39. W.Smilllie and D. Augustine, Intensity of hookworm infection in Alabama. *J.A.M.A.* 85 (1925): 1958–62.
40. Stiles to Rockefeller Jr, April 15, 1932. Stiles left his letters to the parasitologist Ernest Faust in whose papers they are now to be found. Faust Papers, National Library Medicine, Bethesda. This episode is discussed in the Epilogue to J. Ettling, *The Germ of Laziness* (Cambridge, Mass: Harvard University Press, 1981).
41. Most of these surveys were published in the *American Journal of Hygiene*. Mississippi: Vol. 19 (1934): 629–56; South Carolina: Vol. 23 (1936): 600–14; North Carolina: Vol. 26 (1937): 437–54; Kentucky: Vol. 23 (1936): 33–45. The Tennessee survey in *J. Prev. Med.* 6 (1932): 161–184.
42. J. Ettling, *The Germ of Laziness*, p. 220.

# 6

# Yellow Fever: From Coast to Jungle

That the goal of the Health Board would be the eradication of disease rather than development of public health agencies became clear long before a shift along these lines had taken place in the hookworm campaigns. This policy change had been initiated by Rose himself, when in 1915 he decided to take on malaria and yellow fever. Neither of these diseases resembled hookworm. There was no cure for either disease and the cause of yellow fever remained a mystery. The malarial parasite could be revealed by staining, but the minute immobile intracellular parasites were hardly likely to excite as much comment as those wriggling hookworms. Both malaria and yellow fever were known to be transmitted by mosquitoes, which appeared to be their Achilles heal. By the 1920s the Health Board was actively engaged in campaigns against yellow fever and malaria, with minimal reference to building agencies of public health.

Yellow fever became the priority. As Figure 6.1 and Table 1.3 show, from 1925 to the late 1930s over 50% of the disease budget went to yellow fever; only after World War II did malaria move up to first place.[1]

In the end, officers of the Health Division regarded their work on yellow fever as their greatest triumph and no other disease had as much impact on the organization. Not only had they been able to remove its vector, *Aedes aegypti*, from many coastal cities, but they had discovered the existence of jungle yellow fever in the interior of Brazil. New York became the site of a laboratory

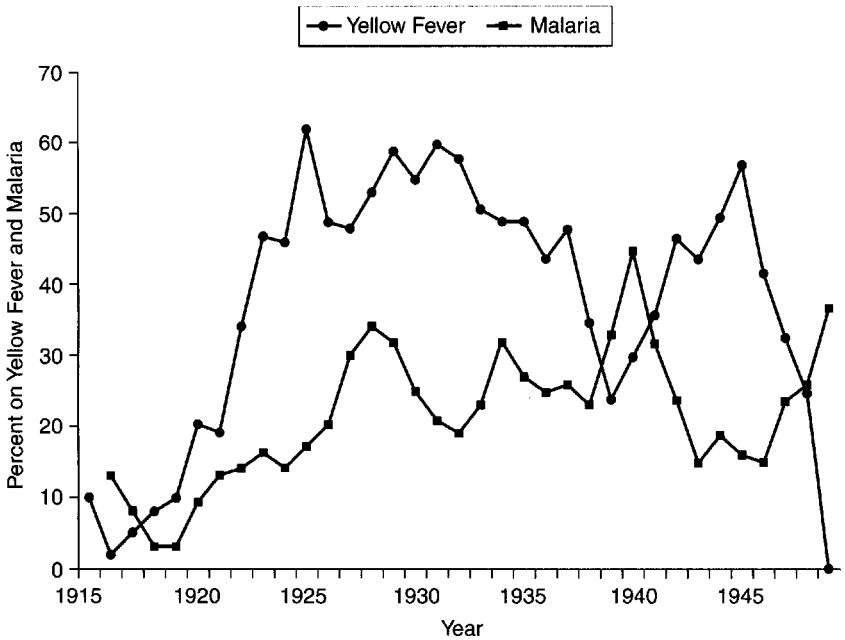


**FIGURE 6.1.** Percentage of disease budget spent on malaria and yellow fever.

dedicated to research that led to the development of yellow fever vaccines. And yellow fever work led the Health Division to pay increasing attention to fundamental medical research.

Yellow fever had long struck Americans with fear, and Rose needed little persuasion to move against it after being urged to do so by General William Gorgas, Surgeon General of the U.S. Army, whom Rose much admired. The opening of the Panama Canal in August 1914 persuaded Gorgas that the disease agent, whatever it might be, could now be easily transmitted to the United States—and even to the Far East, where the mosquito vector existed but the disease did not. Endemic only to South America, the Caribbean and West Africa, its spread seemed a distinct possibility. Rose now joined with Gorgas, Henry Carter of the U.S. Public Health Service, and J. H. White in writing a report saying that yellow fever eradication was feasible.[2]

"We have only to destroy the breeding places of the *Stegomyia* [=*Aedes*] in the five or six endemic foci," the report noted. Destruction of the breeding sites seemed relatively easy because *Aedes aegypti* was known to be a domestic creature, breeding in standing water, commonly found near human habitation. The Panama operation had shown that elimination of breeding sites called for a well-organized, military-style operation. Furthermore these endemic foci, or "seedbeds," were believed to be limited in number.

According to Henry Carter's widely accepted seedbed or "key center" theory—to which Rose subscribed—a yellow fever epidemic required the presence of disease carriers, *Aedes aegypti*, and nonimmune individuals within the population. Because those who survive yellow fever acquire lifelong immunity, this particular combination could occur only in large coastal ports, the seedbeds, into which nonimmune immigrants were constantly entering. Epidemics would recur periodically unless the number of mosquitoes was reduced below a "critical number." By the 1920s this number had come to be defined by the "5% house-index," i.e., the mosquito could be found in fewer than 5% of the households examined.[3] As Rose's map showed (see Fig. 6.2), there appeared to exist only five known key centers: the city of Guayaquil in Ecuador; the Brazilian city of Manaos; and coastal communities in the Brazilian states of Para, Pernambuco and Bahia. There were suspected centers in coastal towns of West Africa, Mexico, Colombia and Venezuela. From these sites, as the lines on Rose's map show, yellow fever could be carried directly to South Africa and India and, through the Panama Canal, to Hawaii, the Philippines, Australia, and, (although not shown on the map) China.

## Guayaquil

In 1916, convinced that eradication of yellow fever was a feasible proposition, the Health Commission constituted Gorgas, Carter and several others as a Yellow Fever Commission, and sent it to Guayaquil. Ships heading for the Panama Canal from Guayaquil, only three-and-a half days away, could not dock there for another two-and-a half days because of quarantine checks. This delay cost steamship companies, most of which were American-owned, up to $1500 per day. The commission reached the city in June 1916 to find epidemics raging of both the plague and yellow fever. Yellow fever mosquitoes were to be found in every room of their hotel, Gorgas told Rose, and during their first night one guest was carted out dead from the plague. They moved!

Insufficient water supply was the real problem, the commission reported. Gravity-fed water supplies from the mountains ran only a few hours each day, and water had to be stored in open containers, potential breeding sites for the mosquito vector. Any mosquito work, they suggested, should be delayed until a proper water supply was put in place, but admitted that a mosquito campaign could be mounted right away if the Rockefeller Foundation itself would provide funding for a water system.[4] Rose quickly squashed the idea, which obviously appealed to city authorities. "It is not possible for us to have any part in financing the water supply project," he told the president of the Bank of Ecuador.[5] But the issue would not go away.

Delayed by World War I, the Guayaquil campaign remained in limbo until the summer of 1918 when a second and larger Yellow Fever Commission arrived in the city. Headed this time by Arthur Kendall, of the Northwestern Medical School

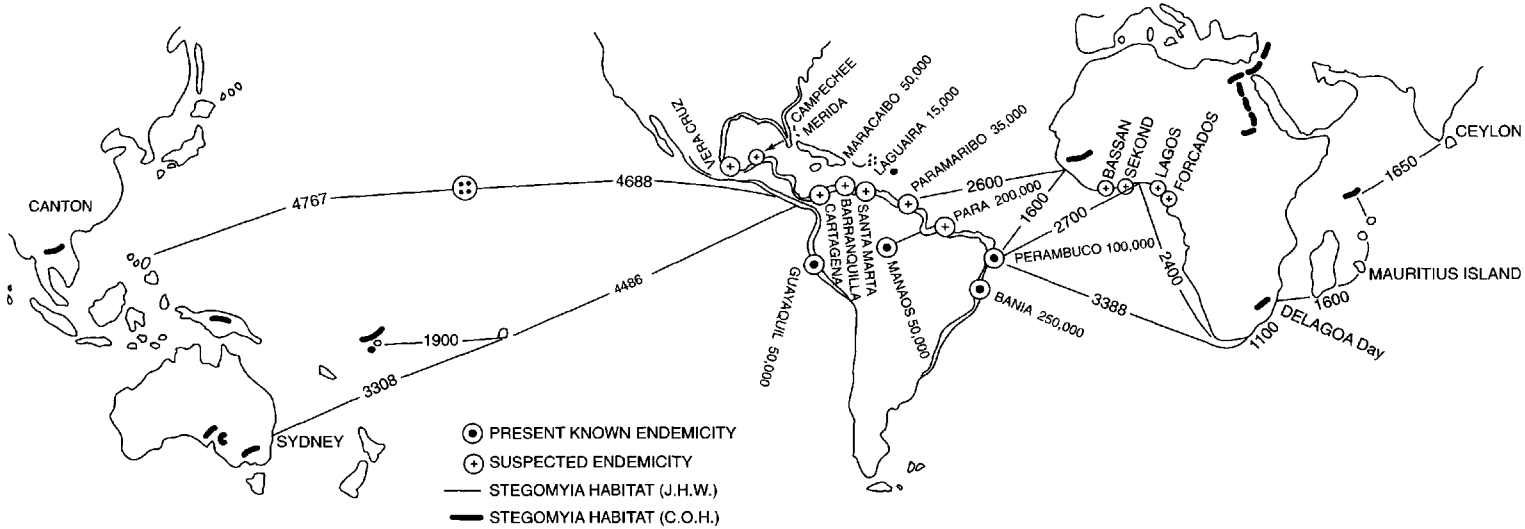


**FIGURE 6.2.** Endemic sites of yellow fever (courtesy of the Rockefeller Archive Center).

in Chicago, it carried a surprisingly broad mandate to examine not only the sanitary situation, but the etiology, clinical aspects and metabolism of yellow fever. Once again, however, the commission's report brought up the contentious issue of the city's water supply.

Between 1913 and 1915 improvements had been made in the water supply and the mortality from both yellow fever and the plague had declined sharply, a coincidence, the commission noted, which was "more than purely accidental." But in 1915 a sharp drop in the exports of cocoa led to a 50% reduction in the sanitation budget. As a result, the mortality from both diseases increased sharply, returning to the level of 1913. The commission once again argued that the city needed better water and sewage systems, street paving, swamp draining, and improved buildings,[6] while Rose believed these projects were the responsibility of the city authorities. (He even turned down a request from the City Council to furnish an engineer to oversee the water supply project.[7]) But Rose agreed to support an anti-mosquito campaign which got underway in November 1918 in order to "undertake the extermination of that seedbed of infection."

The episode says much about the Health Board at that time. Obviously any scheme to improve sewage and water would have paid off in an improved health picture. But the eyes of the Health Board were fixed on disease, as seen through a narrow window. The Health Board's goal, as Rose made clear in his Working Plan for Ecuador, was to eradicate yellow fever from Guayaquil in order to eliminate it from the west coast of South America. They could not wait on the long-term betterment of water and sewage systems.[8] The episode illustrates, too, the Health Board's opposition to any form of emergency relief; if the city authorities required a water and sewage system it was up to them to provide it.

The Guayaquil operation was now taken over by Michael Connor, a native of Massachusetts and a graduate of Dartmouth Medical School. He had served with the U.S. Army Medical Corps in the Philippines and, up until joining the Health Board, had acted as assistant chief sanitary inspector in the Panama Canal project. The statistics were impressive: By the end of 1919, 372,278 houses had been visited, 1,104,862 water containers inspected, and 14,300.5 gallons of oil used. Guayaquil had remained free from the disease for six months, and Connor felt certain that yellow fever had been permanently eliminated from the city.[9] The following August, quarantine restrictions were lifted from Guayaquil shipping, and, in December 1920, the project was handed over to the Ecuador government. But even better news seemed to be at hand.

## Hideyo Noguchi's Discovery

After experiments in Guayaquil and New York, Hideyo Noguchi, a member of Kendall's second Yellow Fever Commission to Ecuador and a staff member of the Rockefeller Institute, asserted that yellow fever was caused by a minute filter-

able spirochete bacterium, *Leptospira icteroides*. He claimed the bacterium was similar to, but immunologically distinct from, *Leptospira icterohaemorrhagiae*, the causative agent of infectious jaundice or Weil's disease, on which Noguchi had been working during the First World War. This claim had to be taken seriously. It was backed by the considerable weight of the Rockefeller Institute, and by Noguchi's own growing stature, gained through his work on syphilis, snake venom, and, more recently, the spirochaete bacteria.[10]

Noguchi claimed that *Leptospira icteroides* could be cultivated from the blood of a yellow fever patient; and when inoculated into guinea pigs could reproduce the disease. He claimed, too, that he had found the bacterium in three of 27 yellow fever victims. On very tenuous grounds, he claimed to have transmitted both the disease and *Leptospira* by the bite of *Aedes* mosquitoes.[11] Following experiments which indicated that inoculation of guinea pigs with killed cultures of *L. icterioides* induced immunity to a subsequent virulent dose of the bacteria, he set about developing and testing a yellow fever vaccine for humans similarly made up of killed cultures of *L. icteroides*.[12]

In October, 1918, Noguchi vaccinated 325 members and families of a non-immune highland battalion and an additional 102 individuals. Of the 427 non-immune individuals who were vaccinated, only five developed yellow fever, while in the population at large 386 cases were reported, of whom 217 died.[13] Impressed by these results, the Rockefeller Institute began preparing a yellow fever vaccine and by the end of 1925 over 20,000 individuals in Guayaquil, Brazil, Mexico, Peru and elsewhere had been vaccinated. In addition, Noguchi claimed that an anti-leptospiral serum given to yellow fever victims within the first three days of the fever, drastically reduced the mortality rate.[14]

In 1921 the Health Board began short-lived campaigns against yellow fever in Mexico and Peru, whose coastal cities were regarded as potential or actual seed-beds of infection. The board's reports out of Mexico glowed with optimism. Not only were their mosquito campaigns successful, but only 17 of over 3000 Mexicans in Tuxpam, Vera Cruz and Manzanillo inoculated with Noguchi's vaccine came down with the disease.[15] With no cases appearing in 1923, the campaign ended and the Health Board withdrew early in 1924. A similar military-like campaign, augmented by injections of Noguchi's vaccine, took place in the northern coastal plain of Peru and once again the disease, which had surfaced in 1919, was brought under control.[16]

These findings convinced many—none more so than Frederick Russell—that the long search for the causal agent of yellow fever was over, and that a vaccine was at hand. In a few short years, the Health Board seemed to have eliminated yellow fever from Mexico and from the west coast of South America and now, with a vaccine available, the likelihood of its return seemed remote. It had been a triumph for the Rockefeller Foundation, for the Health Board and for Western scientific medicine. But that confidence would soon be shattered.

## West Africa

In 1926, Dr. Henry Beeuwkes established the headquarters of the West Africa Yellow Fever Commission on the grounds of the Medical Research Institute in Lagos, Nigeria. Like an earlier commission, it worked on the assumption that *Leptospira icteroides* was the causal agent of South American yellow fever, and that West African yellow fever had an identical cause.[17] But that was not what they found.

Beeuwkes and his co-workers made a series of discoveries that seriously challenged Noguchi's work. Guinea pigs, they discovered, when inoculated with blood from a yellow fever patient, were resistant to infection and no *Leptospira* were found in cultures taken from patients. Immune sera from yellow fever patients mixed with *Leptospira* did not initiate agglutination, nor did immune serum, when inoculated into guinea pigs who were later challenged with a liver-kidney emulsion containing *Leptospira*, generate any resistance to the *Leptospira*. No *Leptospira* were found in mosquitoes fed on the blood of yellow fever patients and no transmission of the fever to guinea pigs took place. All results were negative, totally at odds with those of Noguchi, and, in their own words, "most surprising." Had there been an error, or were there fundamental differences between South American and West African yellow fever?[18]

Given that Muller, one of the authors of the report, was a Noguchi-trained pathologist, they claimed to have shown "conclusively" that there were indeed two diseases, one in South America and the other in West Africa. What was to be done? The answer, according to Kligler, lay in the laboratory. In what must have been music to Russell's ears—two years later he was to press for increased emphasis on laboratory research—Kliger complained about the dominance of surveys and field work. Their discoveries in West Africa dictated a reversal in policy, a change in organization, and a change in personnel. The survey commission should be replaced by a scientific commission.[19] Yellow fever was proving to be a disease where large puzzles needed to be answered and well-equipped and well-organized laboratories were essential to that process.

In April 1927 Russell received a disturbing letter from Beeuwkes, who at the time was on leave in Amsterdam, visiting the laboratory of W. Schüffner, an authority on Weil's disease. Schüffner, who had no great respect for Noguchi, told Beeuwkes that Noguchi's agglutination tests were incapable of differentiating *L. icteroides* from *L. icterohaemorrhagiae*; he hinted that Noguchi may well have been working with Weil's disease, not yellow fever.[20] Realizing that the issue would never be resolved without a laboratory animal susceptible to West African yellow fever (according to Noguchi, guinea pigs were susceptible to the disease in South America), Beeuwkes arranged for chimpanzees, marmosets and Indian rhesus, and crown monkeys to be shipped to Nigeria from the Hamburg Zoo.

On June 30, 1927, blood from a 28-year-old Togoland man, Asibi, suffering high fever for a second day, was injected into a rhesus monkey. Four days later

the monkey developed a fever. It died on the sixth day but not before its blood was injected into another monkey and before a hungry lot of female *Aedes* was allowed to feed on the monkey, strapped to a board and placed in a cage with the mosquitoes. By December, the Asibi strain of "virus" had been passed by mosquitoes through 26 monkeys, all but two of which developed a high fever and died. Postmortem examination of the monkeys by Oskar Klotz, professor of pathology at the University of Toronto, revealed jaundice, hemorrhaging and pathological changes in the liver, spleen and kidneys typical of human yellow fever. "This is the first experimental animal that I have seen inoculated with blood from a yellow fever case, in which the lesions bore resemblances to human yellow fever," Klotz told Russell. It "leads me to believe that the virus of yellow fever has been successfully implanted into these animals."[21]

To those in Lagos the implications of these discoveries were all too clear: "*Leptospira icteroides* had no etiological relationship at least to West African yellow fever."[22] Russell was not ready to concede that Noguchi had made a grievous error, and arranged for him to travel to West Africa. Noguchi was "a unique personality," he told the somewhat miffed Beeuwkes, "who has his own methods, his own technique and the imagination of the real research man."[23] According to Isabel Plesset's account, by the time Noguchi sailed, on October 22, 1927, both he and his colleagues at the Rockefeller Institute knew he had made a mistake. A humiliated Noguchi asked that nobody see him off.

Neither was the Lagos group keen for his arrival. By January 1928, both Beeuwkes and Sawyer in New York had become irritated; Noguchi's vociferous appetite for monkeys and technicians was crippling the research efforts of the others, Sawyer complained. Beeuwkes complained that Noguchi "considers himself in charge of all experimental work at Lagos and Accra."[24] By the spring of 1928, Noguchi had gone through 400 monkeys. But he remained deafeningly quiet. What was he doing, and how could one do careful work with so many monkeys, Beeuwkes wondered. As Plesset has shown, conditions in Noguchi's Accra laboratory were crowded and chaotic. According to Klotz, Noguchi had hired 30 untrained assistants to look after the monkeys. Unfortunately, the animals kept losing their identity collars which were then put back on the nearest available monkey. Chaotic was how Klotz also described the laboratory, and Klotz would know, he was there.[25]

Sawyer, who had taken over from Russell as head of the Health Division's laboratory service, realized that a relatively simple experiment would end the argument. The Lagos group would inject immune sera from South America into their monkeys. If the serum protected the monkeys from subsequent inoculations of yellow fever blood from West Africa, the immunological identity of the two diseases would have been proven and Noguchi's case totally undermined.[26]

Six lots of fresh immune sera from recovered individuals of the 1928 Rio epidemic were shipped to Lagos. Five of them were found to protect monkeys from inoculations of African yellow fever. This virtually resolved the contentious

issue; South American and African yellow fever were one and the same disease, both transmitted by *Aedes* and neither caused by *Leptospira*.[27] The final act soon followed. On May 21, 1928 Noguchi himself died from yellow fever, either by suicide or as a result of sloppy laboratory work. With his death the *Leptospira* controversy came to an end; it had been a terrible embarrassment to Russell and the Health Division.

## Brazil (1923-38)

With the death of Noguchi, attention swung back to South America where the ports of Colombia and northeast Brazil seemed to be the only remaining yellow fever foci. In 1919, concerned that the disease would spread south from these ports, the Brazilian government overcame opposition from state governments—who controlled public health matters—and commenced work in the northeast with *Comissão Federal de Febre Amarela*. Four years later, worsening conditions led the government to invite the Health Board in as consultants and, in 1923, to take control of the *Comissão* campaigns in nine ports of northeast Brazil. The Brazilian government was always more concerned about the populous and more wealthy south than the isolated and impoverished north.[28] Thus the government was willing to allow the Americans to intrude into their northern regions, inhabited, according to Rose, by "shiftless blacks, parasitic whites." The south was not much better, according to Rose. It was inhabited by lazy, undisciplined, leisure-loving Latins, "ardent imitators of the French." Their governments ran "on low gear, and you cannot speed it up for the simple reason that in its making the high gear was omitted."[29]

In fact, the Brazilians had had extensive experience fighting the disease. In 1903, for example, Oswaldo Cruz, director of the Federal Department of Public Health, initiated a yellow fever campaign in Rio based partly on the Gorgas drive in Cuba. It involved isolation of yellow fever patients and destruction of adult *Aedes aegypti* mosquitoes by door-to-door sulfur fumigation. Despite vigorous objections by many politicians and physicians, who disagreed both with the mosquito theory of transmission and with the brutal implementation of fumigation measures, the campaign came to a successful conclusion in 1909 when no new cases of yellow fever were reported.[30] As writer S. Williams notes, "The near deification of Oswaldo Cruz and his methods of fumigation became an entrenched feature of anti-yellow fever orthodoxy." (Fig. 6.3)[31]

In contrast to Brazilian methodology, the Health Board campaign was directed against mosquito larvae; and responsibility for reducing the mosquito population fell on the householders themselves. They were expected to free the area around their houses from potential breeding sites of the *Aedes* mosquito. To this end, they were required to drain, cover, oil, or even stock potential sites with larvae-consuming fish. Unfortunately, such sites were mostly containers holding drink-

**FIGURE 6.3.** Fumigation work against Aedes aegypti (courtesy of the Rockefeller Archive Center).

ing water. One can understand why householders would oppose covering their drinking water with oil, or polluting their water with fish. Each household was inspected weekly by a *Guarda*, who reported any householder with potential mosquito- breeding sites left untreated, and issued the guilty homeowner with an official warning or "intimaçao."[32] The campaign ran, according to the Health Board members, like a well-oiled machine. But in practice local politicians and physicians remained uncooperative, and the public had little sympathy with foreign experts tampering with their water supplies.

An unexpected outbreak of yellow fever in Rio in 1928 led to vociferous attacks in the anti-government press.[33] Their anger was aimed at Dr. Clementino Fraga, the Brazilian Minister of Health, who ran the anti-yellow fever campaign in the city, and at the Health Division itself, whose area of control in the north

was viewed as the source of the epidemic. Faced with this threatening situation, in January 1929 the Brazilian health department agreed to cooperate with the Health Division in setting up a nationwide yellow fever service. The Brazilian department would be in charge of the southern operation, between São Paulo and Bahia, and the Health Division would operate the northern service between Bahia and the Amazon River (Fig. 6.4). Both would use anti-larval methods. At the same time, Brazilians would fumigate houses against adult mosquitoes (see Fig. 6.3). But the situation still failed to improve, and Fraga suggested to Soper, now promoted to South American regional director based in Rio, that the division extend its control southward to include all areas but that of the highly sensitive federal district of Rio.

Soper, the man on the spot, knew that Fraga had made a huge political concession in suggesting that the division should campaign in the more politically important south. He realized also that an attempt to move into Rio, much favored by


**FIGURE 6.4.** Map of Brazil.

Connor and Russell, would mean "the permanent political annihilation of Fraga." He cautioned Russell to be patient, believing that the move to the south, even without the inclusion of Rio, would act as a wedge to facilitate a truly unified service at a later date.[34]

Unlike Connor or Russell, Soper realized that the Brazilians had made considerable progress in Rio. A committee of prominent citizens headed by the archbishop had been formed to assist in the organization of an effective propaganda campaign. In contrast to previous Brazilian emphasis on fumigation, the new information effort stressed control of mosquito-breeding, as practiced by the Rockefeller campaign in the north. Schoolchildren and company employees were told how to find *Aedes* breeding sites; church sermons were laced with advice on overcoming the ubiquitous mosquito. In addition the 36,000-strong *Associação dos Empregados no Commercio do Rio de Janeiro* published brochures and booklets about *Aedes*—its life-cycle and breeding sites.[35] In the end nothing came of Fraga's suggestion; responsibilities remained divided and the Health Division retained its interest in the north only.

Then, suddenly, everything changed. In October 1930 the federal government, weakened by the collapse of the coffee economy and the repercusions of the stock market crash, was overthrown in a military coup. Fraga and other health officials were thrown out of office and Getúlio Vargas, former governor of Rio Grande do Sul, became head of government.[36] The new government, Soper told Russell, promised to be more favorable to American interests. Sure enough, at the end of the month a prominent member of the Brazilian health department, asked Soper whether the division would be willing to extend its yellow fever operations into the south as previously planned. Vargas, whose political power derived from the northern states, Minas Gerais, and the southern state of Rio Grande do Sul, had no qualms about stepping on the toes of the central states which had previously dominated Brazilian politics.[37]

The Unified Yellow Fever Service began work in 1931. It was basically a replica of the Rockefeller-run northern campaign, aimed primarily at finding and destroying the breeding sites of the *Aedes* mosquito. But campaign workers were now backed by the federal government. Article 4 of Decree 21,434, dated May 23, 1932, made this plain. "Anyone who opposes, hinders or objects, in any way to the sanitary work . . . shall be subject to a fine of 100$000 to 1000$000, this to be doubled in subsequent offenses, or an imprisonment of from three to thirty days."[38]

## Rural Yellow Fever

Many Brazilian physicians remained skeptical of the seedbed theory which underlay the Rockefeller campaigns. They were aware of yellow fever cases in large rural estates of inland Bahia, for example, and did not see how they could have originated from coastal ports. Nor did they believe the disease could be eradicated

by limiting campaigns to seedbeds.[39] The Health Board's early confidence in the theory was also beginning to erode. Connor speculated that there might be flaws in the seedbed theory when, in 1926, outbreaks spread to interior towns along the São Francisco River Valley and to Juazeiro in Ceara State (see Fig. 6.4). Nevertheless, he held to the theory, arguing that nonimmune revolutionaries and federal troops from the south had passed through these areas in 1925 and 1926 and might have been carriers.[40]

Harder questions began to be asked after the 1928 Rio outbreak, the first in the capital since 1908. Because yellow fever continued to be absent from the controlled communities where mosquito campaigns were continuing, Conner reluctantly concluded that it might be widely distributed in Brazil, "smoldering in the interior of several states." Guarding the seedbeds had not been sufficient, he concluded.[41]

That such was the case seemed obvious after unexpected cases of yellow fever were discovered by use of a "liver punch." A liver punch, or "viscerotome," was a device used to puncture a corpse and in about 30 seconds snip off a small piece of liver without mutilation of the body. These sections, taken from the bodies of those who had died of an undiagnosed fever, were examined microscopically in the São Salvador laboratory (which the Health Division had opened in 1928). Many showed the characteristic pathological signs of yellow fever and revealed the existence of the disease in areas of Brazil where Soper and his staff had assumed it to be absent (Fig. 6.5). Indeed, by the end of the first year of operation of the Unified Service, nearly 14,000 liver sections had been examined: yellow fever was shown to exist in seven states and 25 rural municipalities where no outbreaks of the disease had ever been reported, or even suspected, by the Health Division staff.[42]

The service had the power to prevent burial of anyone dying of a fever until a "viscerotomia" had been performed; this aroused intense feeling among some villagers, although no mention of it appeared in official reports. A diary kept by staff member Bruce Wilson reveals that some service employees were killed or injured by relatives and friends of the dead victims whose livers had been punctured. Mr Fandino, one of the workers, was stabbed to death by a victim's friends who told him the body would be buried locally, unpunctured. "We are going to remove a liver specimen from you," they told him. Another employee, Arthur Gomas, was stabbed by the father of a child who had been viscerotomized against the father's wishes. Wilson and other members of the yellow fever service, unable to appreciate the villagers' anger, regarded such individuals as ignorant and criminal, standing in the way of progress, science and public health.[43]

Soper also received news of yellow fever cases in the Valle de Chanaan, 75 kilometres inland from Vitória (Fig. 6.4), the state capital of Espirito Santo, where no *Aedes aegypti* were found.[44] Soper raced to the area, confirmed the existence of the disease, found no *Aedes aegypti* but assumed that the disease had been picked

**FIGURE 6.5.** Viscerotomy (courtesy of the Rockefeller Archive Center).

up in the fields through the bite of an inefficient vector, *Aedes scapularis*.[45] By the end of 1932, he concluded that rural yellow fever might well be of fundamental importance; yellow fever was not necessarily a coastal and urban disease transmitted only by *Aedes aegypti*.[46] They no longer had the "holy formula," he told Russell in the summer of 1932, and, while remaining optimistic, he was reluctant "to set the date of victory."[47] Even that shaky optimism was soon to be replaced by a more somber mood.

## Jungle Yellow Fever

In June 1934, Soper learned that yellow fever fatalities had occurred in a sparsely-populated area of Coronel Ponce, 180 km. east of Cuiabá in the state of Matto Grosso (Fig. 6.4). Sawyer, who was visiting the Bahia laboratory at the time, joined Soper on the long inland trip—a two-day flight from São Paulo to Cuiabá and a

12-hour overland drive in a rented Ford with a driver. They found that 14 cases of yellow fever had been reported, of whom 13 had died. For three days Soper and Sawyer toured the dry upland country. "It is so empty," Sawyer exclaimed, "that yellow fever would seem impossible if the proof of the diagnosis in some of the cases were not already at hand." People believed the disease had been contracted while working in the clearings (*roças*) where villagers grew rice, maize, beans, and sweet potatoes. These clearings were situated 2 to 10 kilometers from houses in the high jungle that lined the streams and covered the slopes. All the victims were vigorous young men who had been working at those sites. None of the men, women, and children living and working in the serrada has been infected. Repeated searches failed to locate a single larva or adult of *Aedes aegypti*.[48]

The seedbed theory had collapsed. Soper came to the conclusion that "yellow fever is primarily a rural disease and that only incidentally are cities infected." He suggested that the disease in Matto Grosso did not depend on human hosts but was transmitted through monkeys.[49] Soper's suspicions were confirmed when captured monkeys were found to be positive in laboratory protection tests—in other words these monkeys, unlike the normal laboratory monkeys, did not succumb to an injection of blood from a yellow fever victim. They were immune.[50]

While urban and some rural yellow fever could be controlled by lowering the *Aedes*-Index, Soper told a medical meeting in Rio, there seemed little chance of eliminating the problem if, as seemed likely, jungle yellow fever existed as a disease of monkeys that could be passed to young men working in jungle clearings.[51] The existence of jungle yellow fever throughout Brazil seemed likely to constitute a continuing threat to urban centers unless the *Aedes*-index could be consistently held down. Soper believed the threat to be minimal, but a 1936 outbreak of urban yellow fever in the state of Parana, which seemed to have occurred after an invasion by the jungle yellow fever virus, had a sobering effect. Jungle yellow fever, Soper reported a few months later, "must be considered a possible permanent source of virus for the re-infection of cities and towns where high densities of *Aedes aegypti* are tolerated," and its presence "paradoxically forces the organization of permanent anti-*aegypti* measures throughout threatened regions." Contrary to earlier beliefs, yellow fever now seemed to be a disease that would never disappear.[52]

Some in the Health Division felt the time had finally come to leave Brazil. Faced with the seemingly hopeless task of defying jungle yellow fever, Sawyer, for one, who had taken over from Russell, was losing interest. There seemed to be only two options: maintaining a permanent yellow fever service in urban areas, or undertaking a mass vaccination campaign against jungle yellow fever. The former seemed less problematical than in earlier years. The campaign against *Aedes* had proved so successful that the insect had virtually disappeared from many towns and cities.[53] Nevertheless mass vaccinations seemed to be the wave of the future. The preparation of a yellow fever vaccine had always been one of the major re-

search goals of the Yellow Fever Laboratory in New York, opened in 1928; the discovery of jungle yellow fever had only increased pressure to find and test a suitable vaccine (see Chapter 11).

Yellow fever vaccines stood for a laboratory-centered future; anti-mosquito brigades for a field-orientated past. Soper was not a great admirer of vaccines. He worried lest what he called "a decided psychological change" would come over health officers in Brazil when an efficient vaccine became available. Vaccines, he said, were particularly beneficial to "important people" and to foreign travelers; once these people gained protection Soper feared that budgets for mosquito control would rapidly decline.[54]

As a result Soper decided to switch to malaria work. He believed that eradication of mosquito vectors was possible; it had virtually taken place with *Aedes* in many Brazilian towns. But the challenge of dealing with the enormous diversity of *Anopheles*, the malarial vector, was a quantum leap beyond that of confronting a single species of house-loving *Aedes*.[55]

## Notes

1. Marcos Cueto, "The cycles of eradication: The Rockefeller Foundation and Latin American public health, 1918–1940," in P. Weindling (ed), *International Health Organisations and Movements 1918–1939* (Cambridge: Cambridge University Press, 1995), presents a table which shows how much was spent in each South American country on yellow fever, hookworm, and malaria. The figures show that seven times more was spent on yellow fever than on malaria. The figures themselves have not been converted to standard dollars.
2. W. Rose, "Yellow Fever: Feasibility of its Eradication." October 27, 1914. RAC. RG.2 S.300 B.22 f.134a. M. Gorgas & B. Hendrick, *William Crawford Gorgas: His Life and Work* (Philadelphia: Lea & Febiger, 1924). The early history of yellow fever has been told many times. The American story in H. A. Kelly, *Walter Reed and Yellow Fever* (New York: McClure, Phillips, 1906); W. Bean, *Walter Reed: A Biography* (Charlottesville: University of Virginia Press, 1982) and has been critically analyzed by F. Delaporte, *The History of Yellow Fever* (Cambridge: M.I.T. Press, 1991). M. Humphreys, *Yellow Fever and the South* (New Brunswick: Rutgers University Press, 1992) is one of many books and articles dealing with the disease in the American South.
3. H. R. Carter, "The mechanism of spontaneous elimination of yellow fever from endemic centres," *Annals Trop. Med. and Parasitol.* 13 (1919): 299–311. For greater detail see H. R. Carter, *Yellow Fever: An Epidemiological and Historical Study of Its Place of Origin* (Baltimore: Williams & Williams, 1931). Henry Carter of the U.S. Public Health Service was asked to write his book on yellow fever by Wickliffe Rose.
4. "Report of the Yellow Fever Situation in Guayaquil." RAC. *Yellow Fever Commission, Bound Reports*, RG.5.2 Vol. 18.
5. Rose to L. A. Carbo, July 5, 1917. RAC. RG.5.1.2 S.317 B.46 f.705.
6. A. Kendall and M Lebrado, "Sanitation in Ecuador, with special reference to Quayaquil." RAC. RG.5.2 S.300 B.30 f.181.
7. Gorgas to Arzube Cordero, July 12, 1919. RAC. RG.5.1.2 B.79 f.1120.

8. Rose, "Working plan for Ecuador," December 1916. RAC. RG.5.2 S.300 B.30 f.181.
9. M. Connor, "Yellow Fever Control in Ecuador, Final Report," RAC. R.G.5.2 Bound Reports, Vol. 18.
10. For details of the life and work of Noguchi see G. Eckstein, *Noguchi* (New York: Harpers, 1931); Isabel Plesset, *Noguchi and His Patrons* (Cranbury, NJ: Associated University Presses, 1980).
11. This was not the first time that some bacterium or other had been accused. See, M. Warner, "Hunting the yellow fever germ: The principles and practice of etiological proof in late 19th century America." *Bull. Hist. Med.* 59 (1985): 361–82.
12. H. Noguchi. "Comparative Immunological studies on *L. icteroides* and L. *icterohaemorrhagiae*." *J. Expt. Med.* 31 (1920): 135–158; "Etiology of Yellow Fever IV. The acquired Immunity of Guinea pigs against *Leptospira icteroides* after the inoculation of blood of yellow fever patients." *J. Expt Med.* 30 (1919): 1–8; "Etiology of Yellow Fever XI. Serum treatment of animals infected with *L. icteroides*." *J. Expt. Med* 31 (1920): 159–168.
13. H. Noguchi, *No. 7319*. "Report of Expedition to Ecuador as Bacteriologist of Yellow Fever Commission." February 25, 1919. RAC. RG.5.2 Bound Reports, Vol. 18; Noguchi and W. Pareja. "Prophylactic inoculation against yellow fever. *J.A.M.A.* 76 (January 8, 1921): 96–99.
14. H. Noguchi, "Etiology of Yellow fever XI. Serum treatment of animals infected with *L. icteroides*." *J. Expt. Med* 31 (1920): 159–68.
15. Yellow fever Reports. RAC. RG.5.3 S.323 B.147. Vaccine numbers from Noguchi, "Yellow fever research 1918–1924: A summary." *J. Trop. Med. Hyg.* 28 (1925): 185–193.
16. Marcus Cueto, "Sanitation from Above: Yellow fever and foreign intervention in Peru," *Hispanic American Historical Review* 72 (1992): 1–22; M. Cueto, *The Return of Epidemics. Health and Society in Peru during the Twentieth Century* (Burlington, Vt: Ashgate, 2001). Peru was never an important site of the Health Division's activities.
17. The first commission, consisting of Gorgas—who had by then retired from the U.S. Army—and Robert Noble arrived in Liverpool in 1920, where they were informed by British skeptics that Noguchi had probably mistaken yellow fever for Weil's disease. Gorgas died in England and Noble, from his base at Lagos searched, without success, for yellow fever and *Leptospira icteroides*.
18. I. J. Kligler and H. R. Muller, "Summary of the Studies on West African Yellow Fever" (May to September, 1926). RAC. RG.5 S.2 B.52 f.332.
19. I. Kligler, "Discussion of the studies on West African yellow fever." Ibid.
20. H. Beeuwkes to Russell, 10 April, 1927. RAC. RG.1.1 S.495 B.1 f.6.
21. O. Klotz to Russell, October 3, 1927. RAC. RG.5 S.1.2 B.305 f.3881.
22. *West Africa Yellow Fever Commission, Third Annual Report*, 1927. RAC. RG.5. S.3 B.214. Beeuwkes to Russell, September 17, 1927. RAC RG.5. S1.2 B.306 f.3887. A. Stokes, Bauer.J, and Hudson, P. "Transmission of yellow fever to *Macacus rhesus*, preliminary note." *J.A.M.A.* 90 (1928): 253; "Experimental transmission of yellow fever to laboratory animals." *Amer. J. Trop Med.*, 8 (1928): 103–164.
23. Russell to Beeuwkes, 23 September, 1927. RAC. RG.5 S.1.2 B.306 f.3387.
24. Beeuwkes to Russell, 26 December, 1927. RAC. RG.5 S.1.2 B.306 f.3890; Sawyer to Beeuwkes, 24 January, 1928. RAC. RG.1.1 S.495 B.1 f.6.
25. Klotz kept a diary of his West Africa trip which was kindly made available to me through the good offices of Dr H. J. Barrie of the University of Toronto.

26. Sawyer to Beeuwkes, 25 January, 1928. RAC. RG.1.1 S.495 B.1 f.6.
27. Paul Hudson, J. Bauer & C. Philip, "Protection tests from serum of persons recovered from yellow fever in the Western Hemisphere and West Africa." *Amer. J. Trop. Med.* 9 (1929): 1–16; 223–232.
28. An excellent account of some of these problems is given by S. Williams, "Nationalism and Public Health. The convergence of Rockefeller Foundation technique and Brazilian federal authority during the time of yellow fever (1925–1930)," in M. Cueto (ed.), *Missionaries of Science* (Bloomington: Indiana University Press, 1994) 23–51.
29. W. Rose, "Observations on the Public Health Situation and Work of the International Health Board in Brazil." October, 1920. RAC. RG.5.3 S.305 B.107.
30. This campaign is mentioned briefly in Nancy Stepan, *Beginnings of Brazilian Science. Oswaldo Cruz, Medical Research and Policy, 1890–1920* (New York: Science History Publ., 1976) and more fully in Ilana Löwy, "What/who should be controlled? Opposition to yellow fever campaigns in Brazil, 1900–1939." In A. Cunningham and B. Andrews (ed.), *Western Medicine as Contested Knowledge* (Manchester: Manchester Univ. Press, 1997): 124–146.
31. S. Williams, "Nationalism and Public Health," p. 29.
32. A detailed account of the yellow fever organization is given in *Annual Report, Brazil Yellow Fever Service 1930.* RAC. RG.5.3 S.305 B.115. A more formal 60-article decree, *The Regulations of the Service for the Prevention of Yellow Fever in Brazil* was accepted in May 1932. RAC. RG.1.1 S.305 B.21 f.170.
33. According to Williams, "Nationalism and Public Health," p. 37, beginning in 1926, the authorities in Rio had drastically reduced their anti-mosquito work under the assumption that the Health Board and the federal government had everything under control.
34. These issues are discussed in a series of letters from Soper, March to May, 1929. RAC. RG.1.1 S.305 B.20 f.160–61.
35. For details see Soper to Russell, May 2, 1929. RAC. RG.1.1 S.305 B.20 f.161.
36. J. Bello, *A History of Modern Brazil (1889–1964)* (Stanford University Press, 1966); J. Young, *The Brazilian Revolution of 1930 and the Aftermath* (New Brunswick: Rutgers University Press, 1967); J. Dulles, *Vargas of Brazil* (Austin: University of Texas Press, 1967).
37. Soper to New York, October 24, 28, and November 12, 1930. RAC. RG.1.1 S.305 B.21 f.165.
38. *Regulations of the Service for the Prevention of Yellow Fever in Brazil.* L. Lowy, "What/who should be controlled?" discusses the brutality of these campaigns similar to those in Peru as described by M. Cueto, "Sanitation from Above."
39. Joseph White, "Statistics of Yellow Fever Work, 1924." RAC. RG.5.3 S.305 B.114.
40. "Report of Yellow Fever Outbreak in Brazil, 1926." RAC. RG.5.3 S.305 B.114. The presidency of Artur de Silva Bernades (1922–26) was marked by permanent military and civilian crises.
41. Connor to Russell, May 21, 1928. RAC. RG.1.1 S.305 B.20 f.158.
42. Soper to Russell, August 24, 1930. RAC. RG.1.1 S.305 B.21 f.165. The viscerotomy problems are discussed in Löwy, "What/who should be controlled."
43. Bruce Wilson Diaries. Volume 1937, 6–12. RAC.
44. Soper to Russell, March 12, 1932. RAC. RG.1.1 S.305 B.21 f.169.
45. Soper to Russell, March 30, 1932. Ibid.
46. Soper et al. "Yellow fever without *Aedes aegypti.* Study of a rural epidemic in the Valle de Chanaan, Espirito Santo, Brazil 1932." *Amer. J. Hyg.* 18 (1933): 555–87. Also *Yellow Fever Service. Annual Report, 1932.* RAC. RG.5.3 S.305 B.116.

47. Soper to Russell, May 14 and July 14, 1932. RAC. RG.1.1 S.305 B.21 f.169.
48. Sawyer to Russell, June 22, 1934. RAC. RG.1.1 S.305 B.22 f.173.
49. Soper to Sawyer, September 13, 1934; Soper to Russell October 6 and 11, 1934. RAC. RG.1.1 S.305 B.22 f.174.
50. *Annual Report. Yellow Fever Service, 1937.* RAC. RG.5.3 S.308 B.121.
51. Soper, "Jungle yellow fever. A new epidemiological entity in South America," paper to National Academy of Medicine, Rio. October, 1935. *Revista de Hygiene e Saude Publica* 10 (1936): 107–44.
52. Soper, "The newer epidemiology of yellow fever." *Amer. J. Public Health* 27 (1937): 1–14; "Present-day methods for the study and control of yellow fever." *Amer. J. Trop. Med.* 17 (1937): 655–676.
53. Soper to Sawyer, August 15, 1938. English copy of report to Xth Pan American Sanitary Conference, Bogatá RAC. RG.1.1 S.305. B.23. f.185
54. Soper to Sawyer, April 18th 1938. RAC RG.1.1, S.305 B.23 f.184.
55. Some idea of this diversity can be gleaned from A. Spielman and M. D'Antonio, *Mosquito. A Natural History of Our Most Persistent and Deadly Foe* (New York: Hyperion, 2001).

# 7

# Malaria: Killing Mosquitoes and Anophelines (1915–1935)

From the start, the possibility of taking on malaria had been discussed by members of the Rockefeller Foundation.[1] At the time, experts were divided on how best to attack the disease. Some favored campaigns against mosquito larvae; others supported the use of medications, particularly quinine, against the malarial parasite. Depending on the dosage, quinine was assumed to act either as a prophylactic to prevent further infection or as a sterilizer to remove the parasite from the blood. Additionally, houses could be screened to keep out mosquitoes. The Health Board began testing for the most effective method of attack.

By 1920, Rose had concluded that targeting mosquito larvae held the greatest promise of success in urban situations. But a few years later, the scene changed dramatically with the development of a new chemical compound, Paris green, which specifically attacked the larvae of the vector anopheline species, and seemed applicable to both rural and urban areas.

Rose had been introduced to mosquitoes early on. In London he had met Ronald Ross, discoverer of the malaria-mosquito life cycle; and in the Malay states, he had spoken to Malcolm Watson. Both were passionate believers in reducing the mosquito populations as a means to control, if not prevent, malaria.[2] Ross had fought a long and frustrating battle to convince others to engage in mosquito battles. Many thought him naive and optimistic. They pointed to the failure of the infamous mosquito campaign at Mian Mir, a British army cantonment near Lahore,

used as an experimental site to test the feasibility of mosquito control.[3] To Ross's horror, the two eminent British malariologists who directed the campaign, Rickard Christophers and Price James, insisted that Mian Mir had shown clearly the practical difficulty of mosquito control. Indeed, by the time Ross met Rose, he was still engaged in a bitter and vindictive row over the Mian Mir affair.

Three months after meeting Ross, Rose's travels brought him to the Malay states and to Malcolm Watson, who had been engaged in mosquito and malaria control in the highly malarious Klang district rubber estates, and in the nearby town of Port Swettenham, near Kuala Lumpur. Although he had gained more success than at Mian Mir, it did not come cheaply. Watson had been forced to clear back the jungle, drain and fill marshes, and dike the land.[4] He came to appreciate that more than one vector species was often involved, each with its own breeding sites and flight behavior. Removing the breeding sites of one vector could sometimes create breeding sites for a second. But if Watson warned Rose of the problems inherent in mosquito control, Rose does not appear to have heard.

## Mississippi and Arkansas

In 1915 the Health Commission decided to begin two sets of malaria experiments, one in Mississippi and the other across the mighty Mississippi River in Arkansas.[5] The goal was to determine if—and by which methods—malaria could best be controlled in the so-called "temperate" climate of the American South, and, equally important, at what cost. Bolivar County, Mississippi, a most intemperate place, was chosen as one of the sites. Situated on the east bank of the Mississippi in the heart of the so-called Mississippi Delta, with a population of about 50,000, 75% of whom were black, it was said to have about as high a prevalence of malaria as any place in the United States. With the banks of its many streams slightly elevated to form natural levees behind which the land slopped away to form poorly-drained back swamps and uncleared land, it was an ideal site for mosquito breeding. Most of the black population were impoverished sharecroppers living in squalid housing bordering on swamp land, totally unfit for screening. It seemed an ideal place to try quinine treatment as a means of control.[6]

This campaign was headed by one of the country's foremost malariologists, Charles Bass of Tulane University. The method followed was roughly similar to that used in intensive hookworm work, although more complex and time consuming. A census was taken of the population; blood samples and a malarial history were collected from each member of every household. This led to the development of a "parasite index" and a "history index" of malarial infection.[7] Quinine was then distributed to those found to be harboring the malarial parasite, or to have had a history of malarial fevers. Recipients were asked to take an unpleasant "sterilization dose" of 10 grains each day for eight weeks, in the

hope of destroying asexual stages of the parasite in the blood. A week later, blood specimens were again collected, and those found to be still infected again treated with quinine.

Similarly situated locations on the Arkansas side of the river were chosen for a series of poorly thought out experiments. Screening, quinine treatment and a combination of the two were planned for plantations in Lake Village, Chicot County. In Crossett, a lumber town in Ashley County, in which over 50% of the population was white, an anti-mosquito campaign without major draining works was planned; it was to involve minor draining, ditch-digging and larvicidal spraying with a crude oil and phenol compound mix.

By the end of the first year of operation, Health Board officers were fed a series of numbers in what was becoming standard practice. "It is certain," one is astonished to read in Bass's report, "that malaria in the entire 225 square miles, including a population of more than 20,000, has been reduced from 50 to 90% and in some small groups even as much as one hundred percent." At the same time, the authors warned that it was impossible to be precise.[8] It seemed malaria could be reduced by the use of quinine at reasonable cost.

Similar positive results streamed out of the Arkansas experiments. News from the anti-mosquito campaigns in Crossett was especially encouraging. Freedom from malaria was claimed on the basis of a physician "call index," that is, the number of office, hospital or house calls related to malaria. The dramatic reduction in such calls between 1915 and 1917 in Crossett was taken to indicate an equally marked decline in the prevalence of malaria. This community, the Arkansas malarial report noted, "has been paying annually almost four times as much in doctor bills alone for the privilege of having malaria as it has expended during the current year to be practically free from malaria and from the mosquito as a pest."[9] Similar drops in the call and parasite indexes in Hamburg, a few miles northeast of Crossett, led the physician in charge to note "the effectiveness and practicability of measures directed exclusively against the propagation of mosquitoes."[10] Costs, also, were remarkably low; a third the cost of screening and only a few cents more than quinine. "These demonstrations have left no room for doubt," George Vincent, the foundation's president proclaimed, that "malaria elimination is feasible, scientifically and economically. It represents a striking contribution to community progress and human happiness."[11]

But Rose, perhaps aware of the controversies over malarial control then being waged in Europe, seems to have been less sure. In 1917 he asked Frederick Hoffman, statistician to the Prudential Insurance Company of America, president of the American Statistical Association, and a well-known critic of any form of universal compulsory health insurance, to visit Bolivar County and Crossett-Hamburg in order to assess the success of the work. His report to Rose was highly critical of the Bolivar quinine campaign, and full of praise for the anti-mosquito work in Crossett and Hamburg.[12]

The brunt of his attack was directed toward the Bolivar team's record-keeping and statistical competence. For example, were records of one period strictly comparable to records of later periods. "To be comparable," he pointed out, "they should have been at least for identical units, and, as far as practicable, for identical persons," noting that newcomers entering the area would undermine the validity of the results. But that was exactly what was taking place in the Delta at the time. Between 1915 and 1920, 100,000 blacks left Mississippi and migrated north to Chicago. The Delta became the staging area of the great migration as waves of blacks moved briefly into the area before moving on.[13] One suspects there might have been deeper reasons for questioning the Bolivar results. Hoffman knew the quinine campaign entailed active cooperation on the part of the population. In his eyes, ignorant black sharecroppers were innately unreliable.

On the other hand, he failed to question the results from Crossett and Hamburg where call indexes again constituted the major evidence. In Hamburg, for example, over 2000 calls were made in 1916 from a population numbering 1285. The statistical utility of these figures was put in doubt because many patients made more than one call. And factors other than the anti-mosquito campaign could have reduced the number of calls. What pressures must have been felt by black sharecroppers to work rather than call on physicians during cotton harvest, as they lived out their lives of "toil, ignorance, hopelessness, animal stupidity and bestiality?"[14] The landowners and overseers knew there were anti-mosquito campaigns going on and thus expected the number of malaria victims to decline. Most curious of all, Hoffman never questioned the lack of controls of even the most basic kind. He objected also to the high permanent cost of quinine campaigns. That was not the case with anti-mosquito work, he naively argued, which would gradually become less difficult and cheaper as time went on and the number of mosquitoes declined.

Perhaps because of Hoffman's report, Rose became convinced that, in small towns, malaria could be controlled at reasonable cost by anti-mosquito measures only. "That work," he concluded, "I regard as no longer experimental."[15] But, he said, anti-mosquito campaigns were not universally applicable and might not work in rural situations. There, quinine was required.

The Health Board and the state of Mississippi decided to split the cost of a malaria campaign in Sunflower County, situated in the Delta immediately to the east of Bolivar County.[16] As a result of these experiments, using anti-mosquito measures and screening in the towns and quinine sterilization in the rural areas, Rose threw caution to the winds and announced that "there are few diseases that present so many vulnerable points of attack and none perhaps the control of which may be made more definite and certain."[17] The Health Board thereupon agreed to cooperate with the USPHS in campaigns against malaria in the South; by the time the Health Division discontinued its funding of them in 1934, slightly over $7 million (1990) had been spent.[18]

Strictly speaking, these early urban campaigns were not directed towards malaria. The authorities quickly learned that what appealed most to the community was not a reduction in malaria but freedom from mosquitoes in general. The Health Board had to conclude that "as long as you have mosquitoes puncturing the hide of the average farmer you are going to do very little work in malaria control unless you go after the whole tribe."[19] Because these campaigns were designed more to eradicate mosquitoes than malaria, they accidentally dovetailed into the task of county health units, which had been created to educate the public in matters of public health and sanitation. What better way to bring the board's work to the attention of the public than by reducing the clouds of mosquitoes which blighted every summer night? The mayor of Yazoo City, a wonderfully–named town in the Mississippi Delta, wrote an appreciative note in 1923. "I can't recollect," he told the Health Board, "when we have been able to remain on our front porches without fighting the blood sucker until this summer. Thank you again."[20]

Despite optimistic statements about a decline in malaria, it was not possible to accurately measure the prevalence of the disease. The only accurate guide available was that of measures taken to combat the disease, most of which were related to killing mosquitoes. So large was the scope of this activity, it seemed the prevalence of malaria could do nothing but decline, even if that decline had not been measured. Alabama provides a good example of this. By the end of 1922, their campaigns had resulted in:

| | |
|---|---|
| Miles of Ditching | 309.5 |
| Miles of Clearing | 70.3 |
| Miles of Brush cleared | 460.1 |
| Miles of Oiling | 5793.8 |
| Places stocked with Gambia | 1158 |
| Cost of above | $72,162 |
| Population protected | 354,660 |
| Square Miles protected | 737 |
| News Articles | 278 |
| Schools taught | 316 |
| Pupils taught | 22,841 |
| Number of Lectures | 290 |
| Attendance | 23,990 |
| Literature distributed | 13,974 |
| Conferences held | 5123 |
| Attendance | 11,413 |
| Field Demonstrations | 1493 |
| Attendance | 21,432 |
| Exhibits | 23 |
| Attendance | 55,040[21] |

But had malaria declined? The Health Board always assumed so. But, in these early years, the question itself and their inability to answer it were of little consequence.

In the 1920's, the Health Board expanded its malaria program beyond the nation's borders. Nicaragua, Puerto Rico and Brazil were the first countries in which it carried out malaria campaigns. It soon came face to face with some of the massive problems that such countries present to those believing the disease could be controlled with relative ease, as seemed to have been done in the South. These first campaigns were modelled on those in Mississippi and Arkansas: mosquito eradication in towns; screening and quinine distribution in rural areas where eradication seemed unfeasible. These campaigns seemed always to satisfy the officers in New York who, by now, expected sharp declines whenever one of them got underway. But as Daniel Molloy, in charge of the malaria campaign in Nicaragua, noted, "If we succeed in *demonstrating* malaria control . . . I shall be very glad, but I am not going to manufacture evidence to prove our case. On the whole, I believe that we will be able to present a satisfactory piece of work, as a demonstration. That the control measures have been at least fairly successful I haven't the slightest doubt. It's up to us to prove it." Measuring malaria, Molloy wrote, "seems to be the bete noire of practically all of the men working on malaria control."[22] None of these doubts seemed to be a concern for officers of the Health Board in New York, who were inclined to confuse insect control with malaria control. "Simple, inexpensive measures already employed in the Southern States can be successfully followed in tropical areas," the Annual Report of 1922 confidently noted.[23]

Frederick Russell, who became director of the board in 1923, probably realized that there were problems. In 1926 he appointed statistician Persis Putnam to analyze field staff reports in an attempt to replace soft opinions with statistically significant figures. A Wisconsin-born statistician she had been assistant director of educational work with the USPHS since 1918, and had recently graduated from Johns Hopkins with a D.Sc. in statistics. From her appointment to the office until her retirement 22 years later, field officers were expected to pass their reports through what Lewis Hackett ruefully called her "clairvoyant gaze" for vetting. All of them, at least to my knowledge, learned to their chagrin that what they had confidently asserted before she came were often found not "statistically significant." Her critical responses to field reports were soon to reverberate around the Health Board, particularly after a new antimalaria method came into operation.

## Paris Green

In the mid-1920s the Health Board began to use a new weapon against mosquito larvae. Paris green, a mixture of copper arsenite and copper acetate, mixed with road dust and spread by a hand blower, was discovered in 1921 by M. A. Barber

of the USPHS to be an effective larvicide. Moreover, it was selective. Until then, oil larvicides had indiscriminately killed all mosquito larvae by blocking their respiratory systems; Paris green killed only those larvae that consumed small particles floating in the water, particles which included those of Paris green. As a result, it killed only filter-feeding anopheline larvae, whereas the predatory larvae of *Culex* and *Aedes* remained unaffected.

This new pesticide changed the malaria situation appreciably. Rather than indiscriminate spraying of mosquito-breeding sites, only anophelines, and even specific anopheline vectors, would be targeted. As a result, before any malaria control program started it was necessary to collect, identify and locate the breeding sites of local *Anopheles* mosquitoes. To further pinpoint the target, the actual anopheline vector of the area and its breeding site needed to be found. This selective removal of the vector mosquitoes, termed "species sanitation," had first been articulated by the Dutch zoologist, Nicolas Swellengrebel.[24] It ended the wholesale removal of mosquitoes, a method favored by Southern farmers. And species sanitation seemed as feasible in rural areas as in urban situations. "The strategy is sound," the Annual Report of 1926 noted, "and, if continued systematically and persistently, is sure to result in a definite reduction in malaria in the rural districts."[25]

An accurate measure of malaria prevalence in any control area now became essential. There was no other way to measure the success of a single-disease focused campaign, than by showing a decline in the illness; measuring miles of ditches was no longer adequate. Persis Putnam and her statistical acumen had become crucial to the Health Division.

There was another reason that anti-anopheline campaigns grew in importance. Mounting evidence in the 1920s showed that quinine was of little use, partly because success rested on cooperation between patients and physicians. And, as W. E. Deeks of the United Fruit Company noted in 1930, "Mosquitoes can be infected when fed on malaria carriers who are taking full doses of quinine. . . . Apparently no amount of quinine will prevent mosquito infection."[26] With quinine apparently unable to prevent infection or transmission, the pendulum had swung firmly toward antimosquito work just as a new and better larvicide appeared on the market.

There were those who did not share the Health Board's faith in the mosquito strategy, let alone species sanitation; in 1924 the two sides came face to face. The place was Italy, where the Health Board was to spend almost one-sixth of its malaria budget.[27]

## Italy and Sardinia

Economic crises of the early 1920s—strikes, riots, rising unemployment—and a split in the old Italian Socialist Party, provided a perfect environment for the growth of the Italian fascist movement. On October 28, 1922, the fascists marched on

Rome, the king lost his nerve, and Benito Mussolini was invited to form a new government. He was granted dictatorial powers with the assent and authorization of the Italian establishment. The old liberal state ceased to exist.

The need to keep peasants on the land played a major role in Mussolini's policies. To that end he launched drainage and land reclamation schemes, or *Grande Bonification*, in low-lying river deltas and in the Pontine marshes south of Rome. Over the course of centuries, the marshes, an area about 50 kilometers in length and from 8 to 30 kilometers wide, had become a huge swamp dotted with tall woods, low scrub, bushes, reeds and stretches of pastureland. By the 1920s the only settled population lived on the slopes of Monti Lepini. In September they went down with their families to cultivate the nearest tracts of land, living until the following June in windowless, thatched, conical huts, or *capanas*. They became targets of anopheline mosquitoes before they returned to the uplands; and, because they were forced to return in September to prevent their ripening crops from being stolen, they became targets again of anopheline mosquitoes, still abundant at that time of year. Prior to World War I, 80% of the population of Sermoneta, a town on the lower slopes of Monti Lepini, showed enlarged spleens, and almost half the town's young men were rejected for army service on account of chronic malaria and other ills (Fig. 7.1). Devoid of human habitation in the summer months, Prof. Ilvento, Assistant Director-General of Public Health in the 1930s, called the Pontine marshes, a "landscape of desolation and death."[28]

Ignoring the economic activities of the landowning classes, the government claimed malaria was to blame for the south's underpopulation, poverty, and lack of economic progress. Thus, its control became an important plank in the fascist program. To that end, the government approached the Rockefeller Foundation for help, a fairly astute move given Mussolini's popularity in the United States. According to the U.S. Ambassador, Mussolini was no less than "the Spartan Genius," and correspondents of the *New York Times* fell over themselves in their uncritical support of Mussolini.[29] In 1924, the year when bonification work started in the Pontine marshes, Lewis Hackett was pulled out of Brazil, where he had been in charge for eight years, and sent to carry out a six-month malarial survey of Italy, Europe's most malarious country. Hackett found that malaria was endemic in 52 of Italy's 69 provinces, mostly in the Po Valley, the west coastal region between Rome and Grosseto, and in the south including Sardinia and Sicily.[30] (Fig. 7.2)

Italy was to be a major test, for there Hackett confronted those who did not accept the Health Board's emphasis on mosquito control. Many Italian malariologists and government officials as well as members of the Malaria Commission of the League of Nations who were at the time investigating the malarial situation in Europe, believed that malaria was primarily a social disease. That the Malaria Commission, led by Alberto Lutrario, the director general of public health in Rome, included Col. Price James who had been involved in the Mian Mir episode, did not bode well for Hackett.[31]

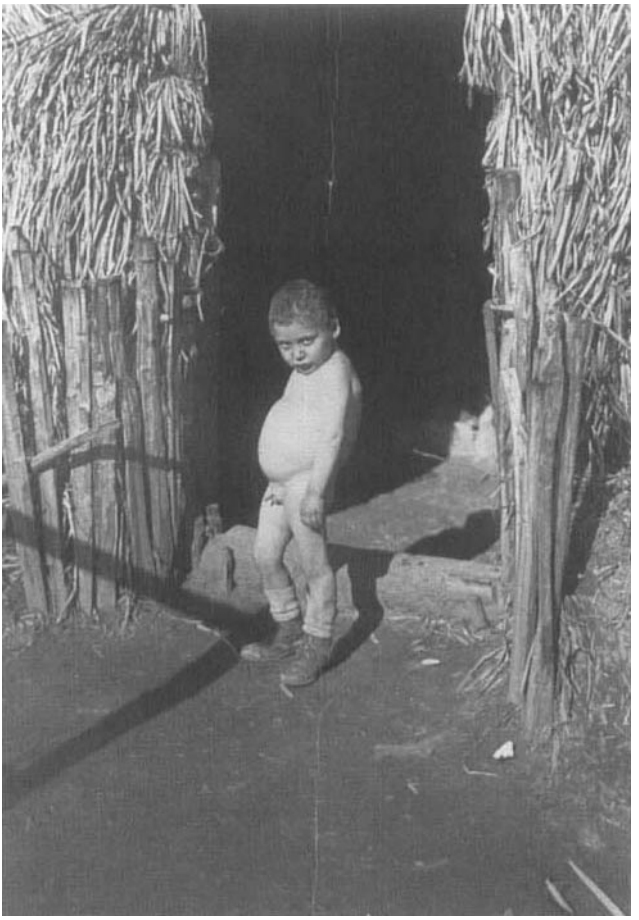

**FIGURE 7.1.** "Almost all spleen." Hackett's photo of a young malaria victim from the Pontine marshes taken in front of a capana (courtesy of the Rockefeller Archive Center).

Italian malariologists since 1902 had placed their faith in the therapeutic and prophylactic distribution of quinine, which was prepared in Turin, and distributed without cost to needy individuals. No other measures had been consistently applied. The Italians were suspicious of antimosquito campaigns for two reasons: the presence of large numbers of anopheline mosquitoes in areas where malaria was absent (so-called anophelism without malaria) and the existence of major drainage operations (*Grande Bonifica*) along the Po River valley and delta where, in spite of an increase in mosquito-breeding sites, malaria had declined. Indeed, in some areas of the north, increased prosperity had resulted in the "spontaneous" disappearance of malaria without the operation of any drainage or treatment schemes whatsoever. But no such improvement had taken place around Rome, in the south, or in Sardinia and Sicily.

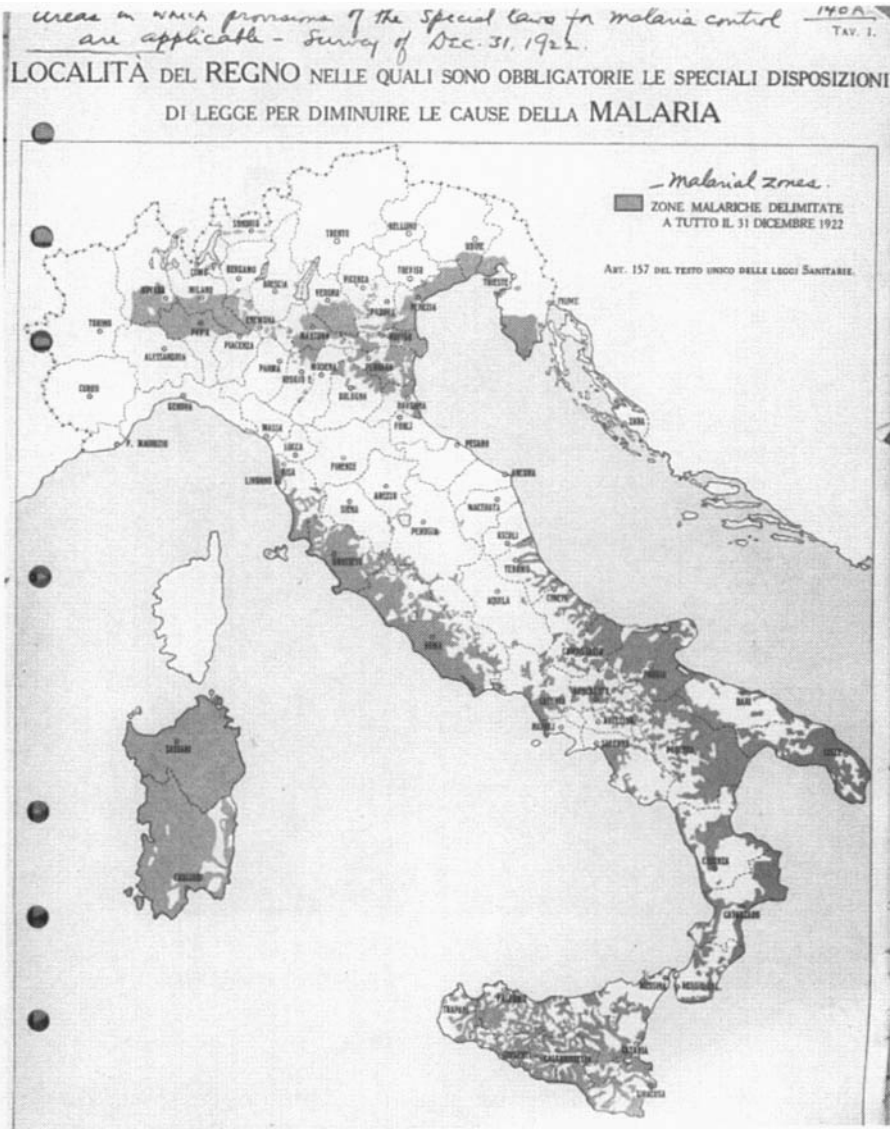


**Figure 7.2.** Malaria zones, Italy (courtesy of the Rockefeller Archive Center).

There, according to Hackett, malaria needed to be "strenuously combated" in the face of a quinine distribution system that had become bureaucratized. "There is no doubt in my mind as to the diagnosis of the Italian malaria situation in general," he told Russell. "It may be summed up as muscular incoordination due to deficiency in the cerebellum. Certain movements are well executed (the distribution of quinine . . .) but others are spasmodic, weak or lacking."[32]

The Malaria Commission, in its 1927 report, characterized mosquito eradication as a "tyranny. . . . over men's minds," that needed to be "thrown off."[33] The problem could be solved only by improving the living and working conditions of the malarious population, as was occurring in the north of Italy and was just beginning in the Pontine marshes. The solution, Hackett was told on many occasions, was to "treat the malaria of Italy with roast beef."[34] In the short term, the only viable procedure was to "endeavour to reduce the incidence and severity of the disease." To achieve this, the commissioners said, "the gratuitous distribution of quinine is the only antimalarial measure which the countries concerned can afford to carry out." Where poverty existed no antimalarial measures could be effective except bonification, said the report. Bonification should not be seen as a method of mosquito control; drainage operations did not result in a reduction of mosquito breeding sites, nor in a reduction of mosquito numbers; rather, they resulted in improved living conditions which, in turn, led to a decrease in the prevalence and severity of malaria. The Italians knew, that, "in an area where bonification has been completed, and where in consequence the inhabitants settle permanently in better houses and in all the other circumstances of a moderately good standard of life, malaria tends more or less quickly to lose its importance as a cause of sickness and death.[35] But the people themselves needed to share in this improved prosperity; there would be no improvement in malaria if the land was worked by minimally-paid laborers living in temporary huts and hovels—precisely the situation then existing in the sharecropping hell of the Mississippi Delta as well as, earlier, in the Pontine marshes south of Rome.

By the early 1930s all natural waterways through the Pontine Marshes had been regulated; drainage canals, including the huge Benito Mussolini Canal dug; and 12 pumping stations erected. In the hands of members of the National Ex-Servicemen's Association, cultivated land replaced stagnant pools and marshes. These veterans and their families were finally settled in permanent houses in new communes scattered throughout the area, and the impact of malaria subsided.

James and Swellengrebel, authors of the Malaria Commission's report, visited the United States in 1927 and said there had been a "natural" reduction in the disease, without any reduction in anopheles mosquitoes. Moreover, the downward trend had not been more marked in those places where antimosquito measures had taken place.[36] They correctly ascribed the popularity of antimosquito work to success in removing the "pestiferous mosquitoes," and not because the chills and fevers of malaria had lessened. In other words, the problem in North America was mosquitoes, not malaria. In Europe, where the inhabitants were more disturbed by malaria than by mosquitoes, antimosquito campaigns used in the United States were inappropriate.[37]

The report helped to bring into the open fundamental disagreements between the Health Board and the Malaria Commission. The Health Board would not accept that nothing could be done before people became better housed and fed ex-

cept alleviate distress through the use of quinine. To members of the Health Board disease was the basic cause of poverty and priority must be given attacking it directly. In 1937, Hackett concluded his *Malaria in Europe* with words that reflected the Health Board's view:

> Ignorance, poverty, and disease constitute the vicious triangle of human social inadequacy. An attack on any one of them helps to dissipate the other two. But the causes of malaria, at least, are in the main independent of the ignorance and poverty of its victims and can be separately handled. It is easier to believe that release from the burden of malaria will help to bring prosperity and knowledge than that a higher standard of living and education must precede the eradication of malaria.[38]

Easier to believe, perhaps, but was it true? Hackett also believed, with justification, that much time would elapse before attempts would be made to improve living standards in the Italian south. "Let us not allow ourselves to be discouraged by theorists, who . . . adopt an attitude of detachment if not of scorn towards the work of those whose inclination and whose duty is to fight the disease *now* with weapons already proved useful, albeit imperfect, rather than to fold the hands while awaiting a problematical *therapia magna* of the future." But whether antimosquito campaigns were useful remained a moot point in 1924.

There was another reason for Hackett's disdain of the Malaria Commission. Their approach seemed to ignore the existence of the new public health professionals, whose training had recently been seriously undertaken by the International Health Board. "It looks as though the medical field malariologist is doomed to extinction in Europe," he wrote. Instead, malaria was to be left in the hands of clinicians and engineers.[39] The Health Board's model of human progress that ascribed disease as the primary cause of human underdevelopment, their faith in the necessity of public health experts, and their antimosquito methodology of attacking malaria were all opposed by the European malariologists. Hackett had a fight on his hands.

Hackett realized that the success of the Health Board's malaria campaigns in Italy lay in accurately measuring the prevalence of malaria. Unless that was accomplished critics would pounce on the Health Board's usual optimistic and unsubstantiated statements. Although he continued to measure the parasite index and attendance records at local malaria dispensaries, he began to place emphasis on measuring the spleen size of pre-teenage children. Sam Darling, at the malaria field station in Leesburg, Georgia, had already shown that the "spleen index" (a measure of the size of a palpable spleen) in pre-teenage children to be the most useful indicator of malaria prevalence.[40]

## The Italian Campaign

In January 1925, Hackett and Prof. Alberto Missiroli of the Italian health department, one of the few Italians who shared Hackett's views, selected Portotorres

and Bianconovo for larvicide spraying with Paris green. Children in Portotorres on the northwest coast of Sardinia (see Fig. 9.2) and in Bianconovo on the coast of Calabria, at the toe of Italy, had been found to have spleen rates (percentage with enlarged spleens) of 46.8 and 56.2 respectively. Nevertheless, although Paris green would be the major weapon in these two areas, children with swollen spleens would still receive quinine therapy—that was the law.

Spraying Paris green began in March, and by July Hackett claimed that *Anopheles maculipennis*, as the vector was known at that time, had disappeared from Portotorres and that malaria prevalence had declined. "None of this evidence is scientific," Hackett noted, "but it is encouraging."[41] Hackett received further encouragement when the Royal Commissary of Rome offered the Health Board permanent quarters in the city (Fig. 7.3) if it would begin a campaign in the Rome area. The organization, directed by Missiroli, became stationed in Rome and took the title of *Stazione Sperimentale per la Lotta Antimalarica*. It expanded its anti-mosquito work into the Pontine marshes and villages near Rome, and into the Sicilian town of Catania, as well as other villages in Sardinia and Calabria. By the

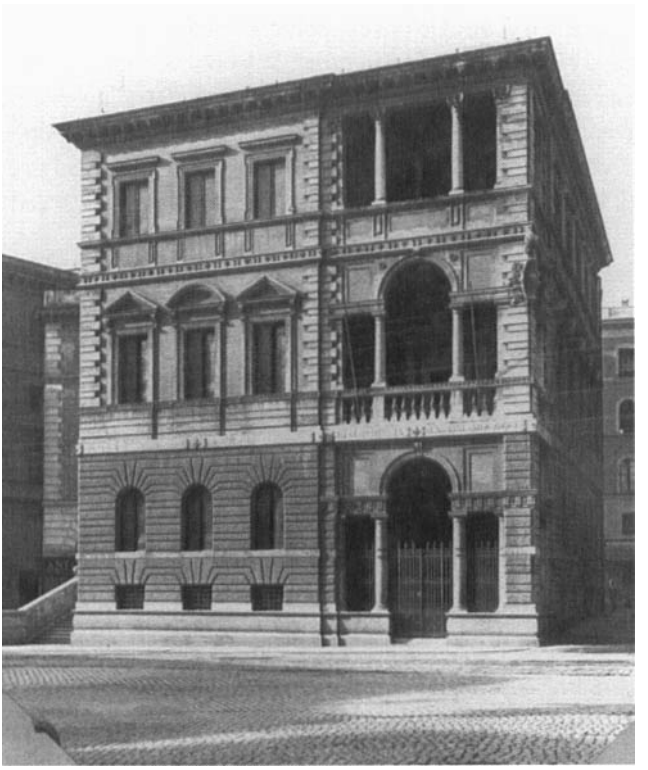

**FIGURE 7.3.** Headquarters of *Stazione Sperimentale*, Rome (courtesy of the Rockefeller Archive Center).

end of 1926, after only two-years' work, Hackett reported that malaria had decreased in Portotorres and had been practically eliminated from Bianconovo.[42] The assessment was based on a decrease in the number of individuals seeking treatment at the local dispensary, a decrease in the parasite index and decreases in what Hackett called "the spleen mass."[43]

Suddenly, however, he was faced with Persis Putnam. In May, 1927, Hackett received a copy of Putnam's critique of his optimistic 1926 report. It must have made for a sobering read! She poured scorn on the use of dispensary attendance to measure the prevalence of malaria, the chief value of which, she asserted, was "in furnishing an illustration of the danger of drawing conclusions from insufficient data." Graphing these figures on semi-log paper showed that the slope of the line, that is the rate of decline, had not changed one iota. "We certainly are not justified," she wrote, "in drawing conclusions from such confusing evidence as this." She also subjected some of Hackett's data to the chi-square test and showed that the differences between the spleen masses of 1925 and 1926 in both Portotorres and Bianconovo were not significant. She was concerned also over the lack of controls. "We do not know," she wrote, "the trend of these indices for the years prior to the beginning of control activities. In other words, we have no statistical control to our experiments, and we do not know what would have happened if nothing had been done. We believe that there is an association between reduction in incidence and the anti-malarial measures, but we do not have the proof of this relationship." We have to know, she reported, "whether malaria is decreasing at a greater rate in the areas being worked than it is in the rest of Italy. We must know, then, the trend in malaria in unworked sections of the country."[44]

In July 1927, Hackett had attended the annual meeting of the British Medical Association, where Price James of the Malaria Commission had spoken about the commission's report. Hackett, throwing caution to the winds, responded by speaking on his malarial work—only to be brought up sharp by James. Few conclusions could be drawn from the data, James warned, because malaria was a disease which fluctuated drastically from year to year through natural causes. Before coming to any conclusions, he continued, the Health Board's experiments had to be continued long enough to distinguish their results from these natural variations.[45] Putnam could not have agreed more; Hackett had been far too quick on the trigger.

As a result of these criticisms, the *Stazione Sperimentale* began to make adjustments in 1927, designating three Sardinian villages, Siniscola, Lodé and Posada, as controls (see Fig. 9.2), where no antilarval measures would be taken although, confusingly, quinine continued to be distributed.[46] Again Hackett's positive reports were sent on to Putnam, whose reaction, this time, was somewhat more encouraging. She revealed, for example, that in all cases where antilarval work

had been carried out for more than one year, the percentage with very enlarged spleens had decreased slightly more rapidly (the line had a greater slope) than the percentage with less-enlarged spleens. Without controls, she warned, it would be impossible to come to any hard conclusion, but comparison with a single year's data from the control village of Siniscola suggested to her that control work may have been responsible for some of the decline. The evidence, however, was still very weak.[47] Unfortunately, by the end of 1927, authorities in the control villages had demanded that they, too, must be allowed to treat or control malaria; they were no longer prepared to be guinea pigs. From then on, no true control data could be collected.

In 1929 a much less critical Putnam presented her analysis of the data from work in Portotorres and elsewhere. She reported that both the parasite index and the spleen masses in Bianconovo and Portotorres had declined, and calculated, from the slopes of her semi-log graphs, that, in general, the rate of the most enlarged spleens had decreased more than that of enlarged spleens. On the other hand, in the villages where only quinine had been used, the spleen index had generally remained the same.[48]

Hackett saw this as a vindication of his work. In 1929 he presented his results to a meeting of the Royal Society of Tropical Medicine and Hygiene in London, and predicted that in Portotorres "the trend is toward complete disappearance of malaria rather than to a new endemic level."[49] But his old nemesis Price James was in the audience and was not impressed. He was sorry, he said, that the Portotorres test had not been as clearly defined as he had hoped since it was not a test of what might have been accomplished by antilarval methods alone; it was a test of what had been done by these measures plus intensive quinine treatment. But, of course, Hackett had no other choice. Italians had the right to demand quinine treatment in whatever village they inhabited.

By 1930 the Health Division began to hand over its malarial campaigns to the Italian government, confident that it had convinced the Italian authorities antilarval work was necessary for malaria to decline.[50] But Hackett remained dubious. In 1934 Mussolini opened the Rockefeller-endowed Institute of Public Health in Rome, and the *Stazione* was transferred to its malaria division. Hackett noted that the League of Nations course in malariology, to be given at the Institute, would stress a clinical approach over field studies and that their students would visit only the quinine-treatment sites in Sardinia, not Portotorres. He worried too lest the Rome laboratory become a center for studies on bird–and monkey-malaria, studies that could equally well be done in non-malarious countries. 'The absence of field stations," he warned, "seems somehow to have changed the entire character of the institution."[51]

In their survey of the 10 years work in Sardinia, which, for some reason, was not published until 1946, Hackett and Putnam reported a basic decline in malaria

in the control areas although they were forced also to acknowledge that failure to supply control data "makes it difficult to appraise the results of a project satisfactorily."[52] It was not a totally convincing report.

## Anophelism without Malaria

By 1934, any doubts that might have existed in the Health Division over the long-term viability of antimosquito work had been finally erased by Hackett and by Missiroli's work on "Anophelism without Malaria." Critics of the original mosquito transmission theory of malaria, and more recently of the anti-larval method of malaria control, had drawn support from widely-known instances of anopheline abundance in areas where there was no malaria even though conditions seemed ripe for it. In Italy, for example, the distribution of *A. maculipennis* did not coincide with the distribution of malaria even though it was known to be the most efficient vector. There were areas of the country where malaria did not occur despite being surrounded by malarious regions and having abundant *A. maculipennis* mosquitoes.

Many hypotheses had been mounted to account for this paradox, among them the view that in some areas *A. maculipennis* fed on domestic animals rather than man and thus failed to act as a malaria vector.[53] But why? Some, as Fantini has explained, favored a Lamarckian explanation, arguing that with the availability of animal blood the physiology of the mosquitoes slowly changed to produce zoophilic races. As a result the view gained support that there were different races of *maculipennis* some of which fed on animal blood, or inhabited areas not shared by man. Some reports even recognized the existence of such races by anatomical characteristics. Thus, for example, N.H. Swellingrebel, another member of the Malaria Commission, recognized two anatomical races of *maculipennis* in Holland, one found in malarious areas, the other not.

As the Health Board slowly withdrew from Italy, Hackett's attention turned to this problem, and for three years the Health Division supported a comparative study of *maculipennis*. Three areas were chosen: Emden in northern Germany where malaria was still endemic, Baden, where there was no malaria, and Tuscany. As a result of this and other work, five varieties of *maculipennis* were eventually recognized on the basis of egg shapes and coloring. Endemic malaria occurred only in the presence of *A. maculipennis var elutus* and *var labranchiae*, both of which were always found to associate with humans. "The existence and distribution of the various races of *A. maculipennis* is one of the main factors," Hackett and Missiroli concluded, "in the explanation of the irregular distribution of malaria in Europe," and thus there is "a definite relation between the geographical distribution of the varieties and that of malaria."[54] This provided evidence, Hackett wrote, that malaria had disappeared in parts of Europe, including northern Italy not because of a rise in the standard of living or improved housing but because "a dangerous race of anophelines has disappeared."[55]

## India

Were the techniques of Europe and North America applicable to places like India where, according to Lt. Col. Sinton, director of the Indian Medical Service, malaria victimized 2880 Indians every day?[56] In 1933, Heiser, confident of the Health Division's malaria techniques, dispatched Paul Russell from the Philippines to look at the situation in India with a view to setting up a limited campaign to demonstrate the possibilities of antimosquito malaria control.[57]

Russell knew that mosquito work in India was not popular because of the legacy of Mian Mir, now called the Lahore Cantonment. On December 15th, 1934, he paid a short visit to the cantonment and wrote a confidential report on the Mian Mir project. He belittled the old experiments and concluded that this "'Mian Mir bogey,' as Ross aptly called it, should be laid at rest. . . . As a 'lesson' it had no 'soundness' at all and it should not block the way as it has undoubtedly done in the past."[58]

Russell then completed a lengthy tour of the country which took him to Bombay and Madras, into Bengal and the mountains of Assam, up to Delhi and down into the desert of the Sind. It was a trip of a lifetime even to a seasoned traveler like Russell, and his fascination is reflected in his report of his tour. There were, he said, 53 species of anophelines in India, of which 13 were suspected to be vectors of the malarial parasite. He believed that it would be possible to control malaria in certain areas by antilarval work and there could be no excuse for not doing so. "Oil and Paris green . . . will control malaria in India just as they will anywhere else."[59]

Between 1936 and 1940 the Health Division carried out antimosquito campaigns in irrigated areas of the Madras Presidency.[60] When they were over Russell enthused that no competent malariologist could doubt that this was the only way to control malaria. In direct contrast to the opinions of the Malaria Commission, Russell wrote:

> There is not the slightest evidence, for example, that better food will help either to prevent infection or to produce a milder attack. Well-fed soldiers in perfect physical condition go down with malaria as rapidly and as seriously, with as high a death rate as average villagers. Better housing has no anti-malaria value at all, unless one is talking about screening. . . . It is very difficult to raise anaemic, malaria-beaten individuals to a plane of greater agricultural or industrial activity by any amount of political afflatus or social service . . . But it is surprisingly easy to stimulate a community which has been lifted up from the sloughs of chronic malaria by anti-mosquito measures.[61]

Sir Rickard Christophers, one of British India's most eminent malariologists, would not have agreed. He had argued, in 1911, for example, that the high prevalence of malaria in the coolie plantations of northern Bengal was due not only to the number of anophelines, but to the general squalor, the hand-to-mouth existence, conditions of depression, privation, and extreme hardship—what he called "physiological poverty." That, plus the constant introduction of nonimmunes into the plantations, led to what Christophers likened to "heaping fresh fuel upon an

already glowing fire." What worked for the European population (quinine, screening, and antimosquito measures) offered, "no hope of even partial success," when applied to the "coolies."[62] Surprisingly, Paul Russell, who must have known about Christopher's views from his work in India, failed to mention him in this context in his *Mastery of Malaria*; as far as Russell was concerned Christophers was an exemplary malariologist who lacked modern tools and, in the context of the 1950s, his social views could be safely ignored.

Russell and most of the other medical barons subscribed to a simple, scientific, culture-free model of health in which all could be blamed on a few microparasites, indifferent to the host of other factors involved in ill health. Indeed there are some today who argue that poverty, malnutrition and starvation actually reduce the severity of malaria.[63] By the 1930s officers of the Health Division felt they held the key to successful control of malaria in any part of the world.

This faith was reinenforced by their work on typhus and malaria during World War II, and by the discovery of the residual effects of DDT. In addition, Fred Soper became the major advocate of a new approach: the view that a permanent solution to the malaria problem in some countries could be achieved by eliminating the malaria vector once and for all—so-called species eradication.

## Notes

1. J. Greene. Draft memo for Publication. RAC. RG.3 S.908 B.11 f.123.
2. The hundredth anniversary of Ross's discoveries has led to the publication of W. F. Bynum and C. Overy (eds), *The Beast in the Mosquito: The Correspondence of Ronald Ross and Patrick Manson* (Amsterdam: Editions Rodopi B.V., 1998); a much less valuable *Ronald Ross:Malariologist and Polymath* by E. Nye and M. Gibson (London: Macmillan Press, 1997), and D. Haynes, *Imperial Medicine: Patrick Manson and the Conquest of Tropical Disease* (Philadelphia: University Pennsylvania Press, 2001).
3. For a recent account of this campaign see W. Bynum, "An experiment that failed: malaria control at Mian Mir," *Parassitologia* 36 (1994): 107–120.
4. Watson's campaign is discussed briefly in G. Harrison, *Mosquitoes, Malaria and Man: A History of Hostilities since 1880* (New York: Dutton, 1978) who presents a fine, popular account of the malaria wars.
5. "Plan for malaria control in Mississippi, 1918." RAC. RG.5.3 S.227 B.46.
6. Margaret Humphreys paints a vivid picture of the sharecroppers' hell in her wonderful *Malaria. Poverty, Race and Public Health in the United States* (Baltimore: Johns Hopkins, 2001). She presents an account of the Rockefeller campaign, pp. 74–77.
7. For some reason, the existence of swollen spleens was not used to measure the prevalence and intensity of malaria in the United States until the mid-1920s. Margaret Humphreys, who is puzzled by this too, drew my attention to a 1916 paper (R. von Ezdorf "Endemic Index of Malaria in the US," *Public Health Reports* 31:819–828) which states that the widespread use of quinine-containing chill tonics may prevent spleen enlargement.
8. C. Bass, W. Leathers and W. Rose, "A demonstration in the control of malaria." RAC. RG.5.3 S.227 B.46.

9. *Report Malaria–Arkansas 1916–17.* RAC. RG.5.3. S.204. B.12. *International Health Board, Annual Report*, 1917.
10. H. Taylor, "Malaria control demonstration at Hamburg, Arkansas," April–December, 1917; Taylor & R. Derivaux, "Urban malaria control." RAC. RG.5.3 S.204 B.12.
11. *Rockefeller Foundation, Annual Report, 1918.*
12. F. Hoffman to W. Rose, November 26 and December 19, 1917. RAC. RG.5.1.1 B.22 f.386.
13. For a totally absorbing account of life in the Delta see, James Cobb, *The Most Southern Place on Earth: The Mississippi Delta and the Roots of Regional Identity* (New York: Oxford University Press, 1992).
14. B. Ratliff, quoted in Cobb, *The Most Southern Place*, p. 120.
15. Rose to Hoffman, December 26, 1917. RAC. RG.5.1.1 B.22 f.386.
16. "Plan for Malarial Control in Mississippi, 1918." RAC. RG.5.3 S.227 B.46.
17. W. Rose, "Field experiments in malaria control," *J. American Med. Assoc.* 73 (1919): 1414–1420.
18. Information from J. A. Ferrell, *History of County Health Organizations in the United States, 1908–1933* (Washington: U.S. Government Printing Office, 1936); P. A. Mead. *The Rockefeller Foundation Operations and Research in the Control and Eradication of Malaria.* October, 1955. RAC; Humphreys, *Malaria.* pp. 95–96 points out that although malaria went into decline at this time, the mortality rate soared again during the Depression.
19. *International Health Board, Annual Report, 1922.*
20. E. P. Swain to Health Board, January 26, 1923. RAC. RG.5.3 S.227 B.47.
21. *Malaria-Alabama, Annual Report, 1922.* RAC. RG.5.3 S.201 B.9.
22. Molloy to Rose, August 12, 1922. "Progress Report Malaria Control—La Puebla-Rivas: Outlook for the future in malaria control," RAC. RG.5.3 S.325 B.150.
23. *International Health Board, Annual Report, 1922*, p. 23.
24. D. J. Bradley, "Watson, Swellengrebel and species sanitation: environmental and ecological aspects." *Parassitologia* 36 (1994): 137–147.
25. *International Health Board, Annual Report, 1926*, p. 123.
26. Quotation from Humphreys, *Malaria.* She discusses the collapse of quinine therapy, pp. 77–79.
27. D. Stapleton. "Internationalism and nationalism: The Rockefeller Foundation, public health, and malaria in Italy, 1923–1951." *Parassitologia* 42 (2000): 127–134.
28. A. Ilvento, "The reclamation of the Pontine marshes." *Quart. Bull. of Health Organization, League of Nations.* 3 (June, 1934): 157–201.
29. J. Diggins, *Mussolini and Fascism: The View from America* (Princeton: Princeton University Press, 1972).
30. Hackett, L. "Summing up briefly the situation in Italy," Hackett Diaries 1924. RAC. RG.5.2 S.751 B.59 f.382.
31. The clash between the Malaria Commission and those who favoured anti-mosquito campaigns is discussed in H. Evans, "European Malaria Policy in the 1920s and 1930s: The Epidemiology of Minutiae," *Isis* 80 (1989): 40–59; also G. Harrison, *Mosquitoes, Malaria and Man.*
32. Hackett to Russell, August 28, 1924. RAC. RG.5.1.2 S.751 B.206 f.2632.
33. "Principles and Methods of Antimalarial Measures in Europe." *Second General Report of the Malaria Commission, League of Nations Health Organization* (1927) C.H./Malaria/73. Geneva.
34. Hackett to Russell, September 24, 1924. RAC. RG.5.1.2 B.206 f.2633.

35. *Second General Report of the Malaria Commission*, p. 29.
36. James, S. P and Swellengrebel, N. H. *Report on a Tour of Investigation of the Malaria Commission in the United States in 1927*. C.H./Malaria/86. The reasons for the disappearance of malaria from the United States is the subject of M. Humphreys, *Malaria*. She does not mention the European survey on the grounds, I am sure, that it had minimal impact on the situation in the United States and because the use of quinine, so much supported by the Commission, was shortly to come under attack.
37. Evans, "European Malaria Policy," argues that the European failure to adhere to the American method reflected, in part, their hostility towards the United States when it failed to join the League of Nations. They became determined, as he wrote, "to conquer malaria on their own."
38. L. Hackett, *Malaria in Europe* (London: Oxford University Press, 1937), p. 320.
39. Hackett to Russell, October 18 and November 3, 1924. RAC. RG.5.1.2 B.206 f.2633.
40. S. Darling to Russell, January 9, 1924. RAC. RG.5.2 S.212 B.8 f.48. Darling, "Ascertaining the splenic index." *J.A.M.A.* 80 (1923): 740–43. I do not understand why it took the Americans so long to use this measure. See note 7.
41. *Annual Report of the International Health Board in Italy during 1925*. RAC RG.5.3 S.751 B.247.
42. *Annual Report of the International Health Board in Italy during 1926*. Ibid.
43. Spleens were numbered 0 to 8, representing the amount of swelling, 0 being reserved for normal spleens, 1 for spleens swollen to edge of costal margin, 2 and 3 for those swelling between the costal margin and the level of the umbilicus, and so forth. Multiplying such numbers by the number of individuals in each group gave the spleen mass at each level, and averaging those masses gave a measure of the average spleen swelling in the community. If transmission was being controlled, then the average spleen mass would decrease as the size of the spleens shifted gradually upward and the spleen mass at each level would also decrease.
44. P. Putnam, April, 1927. "A statistical review of Dr. Hackett's reports," RAC. RG.1.1 S.751 B.7 f.81.
45. Hackett to Russell, July 25, 1927. RAC. RG.5.1.2 S.751 B.312 f.3961.
46. *Annual Report of the International Health Board in Italy during 1927*. RAC. RG.5.3 S.751 B.247.
47. Putnam, P. Memo to Russell, September 7, 1927. RAC. RG.5.1.2 B.312 f.3961.
48. P.Putnam, Memorandum for Dr Russell, June 14, 1929. RAC RG 1.1 S.751 B.6 f.73; "The course of malaria intensity in the five field stations, 1924–1928." RAC RG.5.3 S.751 B.247.
49. Hackett L, "Malaria control through anti-mosquito measures in Italy." *Trans, Roy. Soc. Trop. Med & Hyg*. 22 (1929): 477–506.
50. According to Professor Ilvento's 1934 report on the Pontine marshes, pyrethrum powder was sprayed in the inside of the settlers' screened houses every evening and Paris green applied to prospective breeding sites. They also surrounded the houses with a protective ring of pig-sties designed to attract the mosquitoes and thus divert them from humans, an idea which Ilvento claimed to have been "entirely successful." Ilvento, "Reclamation of Pontine marshes."
51. "Second Quarterly Report of work of the IHD in Italy, April to June, 1935." RAC. RG.5.3 S.751 B.249.
52. Putnam P & Hackett L (1946), "An appraisal of the malaria endemic in protected and comparison areas of Sardinia in the years 1925–34." *J. Nat. Malarial Soc.* 5: 13–37. They continued to use doubtful numbers such as number of anophelines captured and

patient visits to the malaria dispensary. In addition a single regression line was fitted both to the spleen rate and parasite rate graphs, despite the fact that both rates clearly levelled off after 1929. Some secondary sources on the Italian malaria campaign seem to have accepted the Health Board's statistics at face value and claim the campaign as a success. D. Stapleton, "A Success for Science or Technology? The Rockefeller Foundation's Role in Malaria Eradication in Italy, 1924–1935," *Medicina nei Secoli Arte e Scienza* 6 (1994): 313–228; Stapleton, "Internationalism and nationalism."

53. This brief account is taken from B. Fantini, "Anophelism without malaria: An ecological and epidemiological puzzle." *Parassitologia* 36 (1994): 83–106.
54. *Annual Report of the International Health Board in Italy, 1932*. RAC. RG.5.3. S.751 B.248. Hackett, Martini and Missiroli, "The races of *A. maculipennis*." *Amer. J. Hyg.* 16 (1932): 137–62; Hackett and Missiroli, "The varieties of *Anopheles maculipennis* and their relation to the distribution of malaria in Europe." *Riv. Malariol.* 14 (1935): 45–109.
55. *Annual Report of the International Health Board in Italy, 1931*. RAC. RG.5.3. S.751 B.248. This is discussed at length in Hackett, *Malaria in Europe*.
56. Quoted in Paul Russell, "Some Aspects of malaria in India," July 5, 1941. RAC. RG.1.1 S.464 B.11 f.87.
57. Sinton to Heiser, April 5, 1933; Heiser to F. Russell, February 7, 1934; F. Russell to W. Jacocks, August 16, 1934. RAC. RG.1.1 S.464 B.11 f.8.
58. P. Russell, "The Lesson of Mian Mir," RAC. RG.1.1 S.464 B.11 f.88.
59. P. Russell, "Malaria in India. Brief summary of impressions from study tour," April 11, 1935. RAC. RG.1.1 S.464 B.12 f.90.
60. P. Russell, Malaria Investigations-Madras: A resume describing activities 1936–40." RAC RG.1.1 S.464 B.11. f.88. V. Muraleedharan & Veeraraghavan, D., "Anti-malarial policy in the Madras Presidency: An overview of the early decades of the twentieth century." *Medical History* 36 (1992): 290–305. The malarial campaigns are discussed at length in S.N. Kavadi, *The Rockefeller Foundation and Public Health in Colonial India 1916–1945* (Pune: Foundation for Research in Community Health, 1999).
61. Russell, "Some Aspects of Malaria in India."
62. S. R. Christophers and C. Bently, *Malaria in the Duars* (Simla: Government Press, 1911).
63. I. A. McGregor, "Malaria and Nutrition," in W. Wernsdorfer & I. McGregor, *Malaria: Principles and Practice of Malariology* (London: Churchill Livingstone, 1988). For a critique of this, see S. Zurbrigg, "Did Starvation Protect against Malaria?" *Social Science History* 21 (1997): 27–58.

# 8

# World War II: DDT, Typhus, and Malaria

When war broke out in September 1939, the Rockefeller Foundation had over 100 European projects in operation. They had no desire to withdraw from Europe and resolved to salvage as much as possible from their ongoing commitments. But this time, in contrast to their experiences during World War I, they decided to avoid any involvement in emergency relief. If the money were used "to ameliorate human distress caused, for example, by famine or flood or earthquakes or any other calamity, our funds would soon be exhausted with no permanent result," the trustees concluded during their meeting of December 1939. When the Health Division was faced with a $20 million request from the American Red Cross to engage in just such emergency work, Sawyer and Strode decided that a general war program to aid government health organizations was in order, but it needed to be administered by a separate agency from the Health Division. Thus, in July 1940, the foundation set up the "Health Commission in Europe" under Wilber Sawyer, with George Strode acting as assistant director.[1] (A year later Europe was dropped from the title to allow war work outside that continent). Its main headquarters were located in the Paris Rockefeller office, with another office in neutral Lisbon acting as a link between France and New York. Setting up a parallel organization of this kind to the main body is normal procedure for many organizations. It freed the Health Commission to carry out its own wartime activities without changing the goals or mandate of the parent International Health Division. As Fred Soper

noted, the commission "was free of many of the bureaucratic controls of its International Health Division."[2]

The commission basically became the European branch of the Health Division during the war, and was able to become involved in activities prohibited by the mandate of the parent body. It was thus free to cooperate with the Red Cross, the U.S. and the Allied military—and eventually the United Nations—and to engage in war-related emergency activities.[3]

The best-known of these activities occurred when the commission, with Soper in command, became involved in early field testing of a new chemical, DDT. The powerful insecticide was first used against typhus-transmitting lice in Egypt and Algeria, and, later, against malaria mosquitoes in Egypt and in war-torn Italy, all part of U.S. army operations in the Mediterranean theatre of war.[4]

## Typhus

Louse-borne epidemic typhus was probably the most feared disease of World War II. As Stanhope Bayne-Jones, a major player in the typhus story, wrote, "It has been notorious as one of the great pestilential scourges accompanying war, famine, and the dismal calamities of nations. It has determined the outcome of military campaigns and has had a fateful influence upon the course of history."[5]

In 1939, with the threat of U.S. involvement in the war on the horizon, Surgeon General James Magee had taken the first steps to enhance preventive medicine in the U.S. army. In 1940, he appointed Col. James Simmons to direct the preventive medicine subdivision. And, in January 1942, the Cox typhus vaccine, consisting of killed rickettsia that had been grown in egg yolk, was adopted for use in the Army. In December of that year the United States Typhus Commission was established by President Franklin D. Roosevelt to study and control typhus as it threatened the military population. A forward echelon of the commission arrived in Cairo in January 1943, two months after Allied landings in Algeria and Morocco—Operation Torch—began to drive the Germans out of French North Africa. The efficacy of the Cox vaccine and of new anti-louse powders were to be tested on the civilian population by the commission. Over 77,000 cases of typhus were reported in French North Africa and another 23,000 cases in Egypt; the disease was clearly a major threat to the military population. Admiral Charles Stephenson, director of the Typhus Commission, invited Fred Soper and the entomologist Charles Wheeler to attach themselves to the unit—on loan from the Rockefeller Foundation—and take charge of the anti-lousing experiments.[6] The pyrethrum-based MYL louse-powder had already been successfully tested at the Rockefeller Institute's "louse lab," and at a camp for conscientious objectors in New Hampshire, whose inmates presumably volunteered to be artificially infested with lice.[7] Now the Typhus Commission was anxious to test the powders on naturally-infested human populations.

Working together in Cairo led to bad blood between Soper and the Health Commission on one side and the Typhus Commission and the U.S. army on the other. Years later, Bayne-Jones, who was eventually to take charge of the Typhus Commission, said he had "rarely been involved in a more troublesome and acrimonious controversy."[8] The inclusion of two civilians in the commission had drawn criticism from some of its military members, but, with the backing of Stephenson, their status seemed secure. That security was not to last. Stephenson suffered a heart attack and was replaced in February 1943 by Col. Leon Fox, another irascible individual, who disliked Soper. "The trouble with Soper," Fox told Simmons a few months later, "is that he is not only personally a stinker but he is just plain dumb."[9]

Fox was angered by Soper's inconclusive results in the field–testing of MYL louse-powder, an outcome which Fox ascribed to poor design and execution of the experiments. Initial tests at the village of Esbe Rameses showed impressive reductions of lice after two applications of the powder, 15 days apart. The village of Bidsah, where a typhus outbreak had recently occurred, was chosen to test whether an epidemic could be blocked by the powder. This time the reduction of lice was far less than expected, and subsequent testing of the powder back in Esbe Rameses also proved disappointing. A great deal of acrimonious debate was generated when Soper claimed the army had supplied defective louse powder. The process was also slow and cumbersome, requiring complete disrobing so clothes could be turned inside out and dusted. Fox and other commission members decided that vaccines would be a better weapon.[10] Soper was not an admirer of vaccines after his experiences in Brazil.

Meanwhile, Strode, seeking an opportunity for the Health Commission itself to test the new powders, had visited Algeria with a representative of the American Red Cross. He agreed, with the support of the Surgeon General of the North African theatre of operations, to set up a Rockefeller typhus team under the auspices of the Red Cross and the Institute Pasteur in Algiers. With bad feelings all around, Soper was released from the army-sponsored commission to take charge of the new independent Rockefeller unit and to once again test louse powders on the Arab population. Simmons clearly would have liked this Algerian work to be carried out by the Typhus Commission but, as he told Fox, this had become "completely undesirable because of personalities and other disturbing factors." He acquiesced in the virtual firing of Soper and his transfer to the Rockefeller team in Algeria. As Fox caustically told Sawyer, "Soper's loss was our gain."[11]

In Algeria, Soper and his team encountered DDT for the first time. After testing 10% DDT and the usual MYL powder against lice in the Maison-Carée prison, the commune at L'Arba, and four prisoner-of-war camps, they discovered that MYL worked faster, but DDT had a longer residual effect; a single dose provided protection for at least one month. They also developed a dust gun, which made delousing possible without undressing. This allowed large-scale and faster opera-

tions among the Arab population. Soper's report noted that "common felons, civilian populations and prisoners of war do not like being lousy and welcome the relief noted following the application of powder."[12] Soper was triumphant; he had been vindicated. But he once more aroused indignation by claiming credit for the design of the dust gun; Bayne-Jones claimed it had been pioneered and used by Wheeler as a member of the Typhus Commission.[13]

By the end of the Mediterranean campaigns in North Africa, Sicily and Italy, only 104 cases of typhus had been reported among U.S. troops, with no deaths, despite being surround by a civilian population in the grip of typhus epidemics; 67,000 civilian cases were reported in 1943 alone in Egypt and French North Africa. The vaccines, delousing powders and other preventive measures had successfully protected U.S. troops.[14] The British were less lucky. Only medical personnel of the British First Army, making up the Eastern Task Force of the Torch landings, had been vaccinated, with the result that there were 50 typhus cases, of whom 14 died. Action was quickly taken to vaccinate men of the Commonwealth 8th Army, who were to take part in the Sicilian and Italian campaigns.[15]

## The Italian Campaign

When British and U.S. troops entered Naples on October 1, 1943, they found a full-scale typhus epidemic on their hands, with over 400 fatalities. The disease had appeared in March after the arrival of refugees from the Balkans, Italian prisoners of war from Tunisia, and Italian troops from the Russian front who had been sent home sick with typhus. To make matters worse, the Germans opened up the Naples prisons before retreating and allowed several prisoners with typhus back on the streets. Damage caused by German sabotage and by Allied bombing of Naples had demolished parts of the main aqueduct bringing water into the city. With a major rail bottleneck adding to the disruption, many residents crowded for shelter into limestone caves with no sanitary facilities.[16] Many of the sick, dead and dying were still crowded into what Wheeler called, "Caves of Death."[17]

In December, the Allied Military Government of Occupied Territory (AMGOT), made up of both British and American officers, arranged for the Rockefeller typhus team to be transferred from Algeria to Naples. On arrival, the team organized a mass delousing program using both DDT and MYL. But AMGOT was primarily concerned with its military responsibilities. In its work among civilians, the typhus team remained a tiny four-man unit, frustrated by shortages and forced, according to Soper, to beg, borrow and steal. At this stage, Soper's old nemesis, Brig. Gen. Fox of the U.S. Typhus Commission, arrived to look over the situation in his new capacity as director of field operations, after having resigned as head of the commission two months earlier. He was not impressed by what he saw. "The balloon has gone up," wrote Col. Crichton, the British head of

AMGOT's public health section to which the typhus team was attached. "General Blitz—sorry Fox—has blown up completely. He paced around my office like an angry tiger." But with the power of the U.S. President behind him, Fox prevailed upon the military government to allow the Typhus Commission to take over. "I understand," Crichton noted sarcastically, "that the whole Typhus Commission is coming over here, that 300 persons are to be employed at once, that DDT is being made available in clouds, and transportation in profusion."[18]

On January 3, 1944, the Typhus Commission took over, with the Rockefeller typhus team attached to it. Wheeler, who had been given a commission in the Sanitary Corps, took charge of contact delousing in homes, prisons, hospitals and shelters, while Soper took command of mass delousing in 50 planned delousing stations. But the epidemic had actually peaked in the first week of January, when approximately 240 cases were reported: by the end of February the crisis had passed. The Typhus Commission withdrew on February 19; Soper remained to continue his delousing work. The Typhus Commission began its work when the epidemic had already peaked and remained in Naples only six weeks.

That did not stop Brig. Gen. Bayne-Jones from claiming responsibility for the decline in disease prevalence thereby opening yet another area of controversy with Soper. "The control thus exercised over this epidemic of typhus at Naples," he boasted, "is regarded as one of the most outstanding achievements of modern preventive medicine."[19] Bayne-Jones, former dean of medicine at Yale, had taken over command of the Typhus Commission from Fox, having been called back into service after serving as a much-decorated frontline surgeon in the British and U.S. trenches of World War I.[20]

Soper made his objections known. He wrote Strode, "the decrease in cases in fact began so early in January that, making due allowance for the incubation period and the delay in notification of cases, *it could be attributed only to events previous to January 3*." [author's italics] What the Typhus Commission did, Soper concluded, was to get the cooperation of the U.S. army in ensuring delivery of supplies, and that was all. In contrast, "the [Rockefeller] Foundation made the plans, developed the techniques, began the task, laid the pattern for the delousing effort, and has remained on the job throughout."[21] The Rockefeller team, not the Typhus Commission, had shown that with the use of inexpensive and easily-applied DDT powder, the louse infestation in a community could be reduced below the threshold of typhus transmission.[22]

Officially, the blowup never happened. Col. Chalke of the Allied Control Division, blandly remarked in 1946 that the Typhus Commission, the Rockefeller team, and medical personnel of the British and U.S. armies worked "in full cooperation and harmony."[23] Bayne-Jones, exasperated by the whole affair, admitted that the dispute had become so acrimonious that the British had suggested they and the Americans should put their heads together and write a joint report on the typhus campaign.[24]

The ad hoc committee set up to write this report felt obliged to invite Soper to become a member. Soper declined the honor. He pointed out that a full, objective report had already been drawn up by himself and two members of the Allied Control Division. It had been approved by the Allied Division, the Surgeon General's Office of the Mediterranean theatre of operations, and the Rockefeller Foundation; but was blocked in the Surgeon General's Office in Washington, which was probably trying to put a lid on the whole matter. Should this report be published, Soper pointed out, there would be no need of the planned joint report. He complained that control of the joint report seemed to have been vested entirely in U.S. hands, and he made the very strong point that "American policy is for those doing the work and making the observations to publish the results and take the responsibility for the accuracy of the conclusions drawn. Success should be credited to those who must take the blame for failure." Finally, in a grand flourish, he concluded that, "a war for the four freedoms is still being fought and we cannot afford to sacrifice one of them thus early by refusing the individual worker the right to publish his own interpretation of his work.[25] With bad blood on all sides, the idea of a joint report was aborted.

As if that were not enough, Soper returned his Typhus Commission medal to the Surgeon General, on the grounds that other members of the Rockefeller team and U.S. officers who had worked with the Allied Control Division had not received the medal, while others, with less involvement, had. The medal had been awarded to further the fiction that the Typhus Commission assumed responsibility for the Naples typhus program as early as December, 1943—and indeed is so reported in the British History of World War II.[26] Soper was incensed to learn that Major General Hilldring, Chief of the U.S. army's civil affairs division, had been awarded the U.S. Legion of Merit for his work on typhus control. Incredibly, the citation said Hilldring had been responsible for planning the Naples campaign and "prevented what might have been a catastrophe"[27]

## Malaria

Typhus was not the only disease threatening the health of allied troops in the Mediterranean region; there was also malaria to worry about. From the start, successful preventive measures had been taken against typhus, but the Allies seemed indifferent to the threat of malaria. The results were only too predictable.

On July 10, 1943, U.S. and Commonwealth troops invaded Sicily en route to Italy and the European mainland. This was the beginning of a fierce campaign that lasted nearly two years as the Allies struggled northward against the grain in Italy against a German army which, even then, was superior in weapons, tanks, training, organization and leadership to any of the Allied armies.[28] Montgomery wrote in his *Memoirs*, that the campaign was "a first class administrative muddle."[29]

There was a malarial muddle too, and British, Canadian and American troops who passed through the malarious zones of Sicily suffered accordingly. The British estimated that the Allies suffered over 200,000 cases of malaria during the Sicilian campaign, a loss in fighting effectiveness equivalent to two infantry divisions. The British history says, "they had been trained for battle but not instructed how to remain healthy in a malarious environment." The Commonwealth 8th Army as a whole, had a case rate of 40 per thousand per month, while Canadian troops suffered an "epidemic of serious proportions" with a case rate reaching as high as 128.[30] In August 1943, the rate among American troops rose to 192 per thousand.[31]

It is clear the causes were mainly lack of environmental protection and inadequate antimalarial organization. The 8th Army, for example, had a notoriously informal dress code, or even no clothes at all—an open invitation to mosquitoes. The brass hats back in Headquarters, in the middle of a brutally hot Sicilian summer, ordered the men to wear slacks, putties and gaiters, instead of shorts, and long-sleeved shirts buttoned at the neck and wrists, at least from dusk. The order was nearly impossible to enforce, particularly when Montgomery himself seemed to have had little sympathy with it. As the future Canadian writer and then subaltern Farley Mowat wrote, "The heat was brutal—and there was no water. The sun became an implacable enemy and our steel helmets became brain furnaces."[32]

But it was possible to require the troops to take two tablets of Mepacrine (the U.S. army used Atabrine) twice a week; to educate the troops about the disease; to provide mosquito-nets or mosquito-proof bivouacs and repellent creams—none of which had been done before that Sicilian summer. These changes, plus the establishment of malaria-control units and malaria-training courses were assumed to have been mainly responsible for drastically cutting the malaria rate during the Italian campaign. They were put in place by Col. Paul Russell, formerly of the Health Division and now chief malariologist for the entire theatre of war.[33]

Malaria continued to be a threat, however. Thousands of peasants had been driven from their homes after the retreating Germans flooded an estimated 98,000 hectares of land. They had blocked and mined the canals and destroyed many of the pumping stations vital to the bonification schemes, especially those in the Pontine marshes south of Rome. By September 1944, over 36,000 cases of malaria had been reported in the Italian population south of Rome, 38 times its prewar level. Alberto Missiroli, the Italian malariologist who had earlier worked with Hackett, warned of the potential for "the greatest epidemic recorded in modern history."[34] Allied Headquarters were equally fearful of malaria among their troops.

By the spring of 1944 the Allies possessed the new weapon of DDT in their fight against mosquitoes and malaria. At the request of the U.S. army, Sawyer's Health Commission developed practical methods of using the new insecticide against anopheline larvae and adults. Soper assumed control of a new malaria unit

which began to spray houses with DDT in kerosene, and apply Paris green and DDT in diesel oil to vector breeding sites in Castel Volturno, north of Naples, and later in the Campagna Romana. "We have," the Health Commission announced the following year, "apparently checked entirely the transmission of malaria . . . where *Anopheles labranchiae* is the only vector it appears malaria can be controlled through a single application of DDT once a year."[35] Even after the 8th Army entered the notoriously malarial Po river mouths and Lake Commachio area at the end of 1944, mass spraying of Paris green and DDT seemed to work magnificently. During the last summer of the war there were 0.36 cases per thousand per week. "It is an outstanding fact," Paul Russell reported "that the Allied malaria control program in Italy in 1944 has effectively kept this disease in check . . . in spite of a very high malaria epidemic potential, a disorganized public health service, and a serious lack of transport and supplies, in a country defeated and disrupted in total war."[36]

Care should be taken in assessing these glowing reports. A captured German medical officer who had served in the Po valley area reported that there had been little malaria in the German army despite their lack of DDT. This was due, he said, to efficient personal precautions. "It is possible," the British history notes, "that enemy discipline in this matter was better than that of the Allies."[37] Few who have ever fought against the German army would be surprised by this claim.

As far as malaria was concerned, the use of DDT seemed to have revolutionized preventive measures. The world looked forward—naively as it turned out—to a world free from the disease. Despite the discovery of DDT, however, Soper continued to believe that the only long term solution lay in the total eradication of vector species. As the war drew to a close, he urged the Health Commission to make one final push in that direction.

## Notes

1. *Report of the Rockefeller Foundation Health Commission*. Vol. 1, June 1940–June 1941. RAC. RG.1.1 S.700 B.5 .f.31.
2. J. Duffy (ed.), *Ventures in World Health: The Memoirs of Fred Lowe Soper* (Washington: Pan American Health Organization, 1977), p. 259.
3. An edict from the State Department initially prevented the commission from working in Vichy France, and restricted its activities to the occupied zone (Sawyer to Strode, October 16, 1940). But the uncomfortable relations between the commission and the Germans came to a head after President Roosevelt's fireside chat of December 29, 1940. The New York office now became concerned over Strode's situation in Paris. In January 1941 Strode managed to get out of Paris, and after a journey of four nights and three days arrived in Lisbon. Later that month, Strode, ignoring the State Department memo, travelled to Marseilles and remained there for two months, attempting to establish an institute of health. The commission carried out typhus and nutrition studies in Spain, it assisted in a flu vaccination project; and it began nutrition studies in England. When the United States eventually entered the war, the commission lost

contact with France, ended its typhus work in Spain, and expanded its nutrition work in England. For details RG.1.1 S.700 B.3. f.21–25; B.5 f.31.

4. For a discussion of wartime DDT work, plus the role of the Rockefeller Foundation in the early testing of the chemical, see Darwin Stapleton. "The dawn of DDT and its experimental use by the Rockefeller Foundation in Mexico, 1943–1952." *Parassitologia* 40 (1998): 149–58; "The short-lived miracle of DDT." *Invention and Technology* (Winter 2000).
5. *Medical Department United States Army: Preventive Medicine in World War II. Vol. 7. Communicable Diseases. Arthropod borne Diseases other than Malaria.* Chap. 10. Stanhope Bayne-Jones, "Epidemic Louse borne Typhus" (Washington: Office of the Surgeon General, 1964).
6. S. Bayne-Jones, "The U.S. Typhus Commission," *Army Med. Bull* 68 (1943): 4–15; "The control of typhus fever," *Rhode Island Med. J.* 30 (1947): 423–30; United States of America Typhus Commission. *Army Med. Bull.* 68 (1943): 4–15. Details of Operation Torch from *History of the Second World War: The Mediterranean and Middle East.* Vol 4. I.S.O. Playfair, *The Destruction of the Axis Forces in Africa* (London: H.M.S.O., 1966). George Howe, *Northwest Africa: Seizing the Initiative in the West* (Washington, Army Center of Military History, 1957).
7. Darwin Stapleton. "The dawn of DDT."
8. S. Bayne-Jones, "Epidemic Louse borne Typhus."
9. Fox to Simmons, May 26, 1943. Stanhope Bayne-Jones Papers, MS C. 371. Typhus Commission, 1942–47. National Library of Medicine, Bethesda.
10. Soper to Strode. Memo: "Summary of Louse Powder Studies January 1943–July 1944." F. Soper Papers 1919–1975. MS C. 359 B.65. National Library of Medicine, Bethesda.
11. Simmons to Fox, May 4, 1943; Fox to Sawyer, June 27, 1943. Stanhope Bayne-Jones Papers.
12. Soper et al., "Louse powder studies in North Africa," *Arch. Inst. Pasteur d'Algérie* 23 (1945): 183–223; Soper, "Summary Louse Powder Studies;" Soper's side of the story is also told in J. Duffy (ed.), *Ventures in World Health.*
13. S. Bayne-Jones, "Epidemic Louse borne Typhus," p. 212.
14. S. Bayne-Jones, "The control of typhus fever."
15. F. A. E. Crew. *The Army Medical Services. Campaigns. Vol. 2. Northwest Africa* (London: HMSO., 1957).
16. *History of the Second World War.* C. R. S. Harris, *Allied Military Administration of Italy 1943–1945* (London: H.M.S.O. 1957).
17. C. Wheeler, "Control of typhus in Italy, 1943–44 by use of DDT." *Amer. J. Public Health.* 36 (1946): 119–129.
18. Col Crichton to Col. Cheyne, December 23, 1943. Fred Soper Papers.
19. S. Bayne-Jones, "The Control of Typhus." Draft Memo 10 November, 1944. U.S. Typhus Commission. 1942–47 Stanhope Bayne-Jones Papers.
20. A. Cowdrey, *Stanhope Bayne-Jones and the Maturing of American Medicine.* (Baton Rouge: Louisiana State University Press, 1992).
21. F. Soper Papers.
22. Soper et al., "Notes on experience with powders in the control of typhus in Italy, 1943–45," *Reimpresso de las Mem. Primera Reunion InterAmericano del Tifo.* 7–13 October 1945; "Typhus fever in Italy, 1943–45, its control with louse powder." *Amer. J. Hyg.* 45 (1947): 305–334.
23. H. D. Chalke, "DDT: Experiences of its use during the Italian campaign." *Proc. Royal. Soc. Medicine.* 39 (1946): 165–68.

24. S. Bayne-Jones, "Epidemic Louse borne Typhus," p. 230.
25. Soper to The Secretary of War, Washington. Undated. 1945. RAC. RG.1.2 S.700 B.12 f.103.
26. C. R. S. Harris, *Military Administration of Italy*, p. 420.
27. Soper to Surgeon General Norman Kirk. July 17, 1945. RAC. RG.1.2 S.700 B.12 f.103.
28. Details of the campaign are given in G. Nicholson, *The Canadians in Italy, 1943–45*, Vol. II. *Official History of the Canadian Army in the Second World War* (Ottawa: Queen's Printer, 1966); D. Dancocks, *The D-Day Dodgers. The Canadians in Italy, 1943–45* (Toronto: McClelland & Stewart, 1991); *History of the Second World War. The Mediterranean and Middle East.* Vol. 5, C. J. C. Molony, *The Campaign in Sicily 1943 and Campaign in Italy, September 1943 to March 1944* (London: HMSO., 1973); Vol. 6. C. J. C. Molony, *Victory in the Mediterranean.* 3 parts (London: HMSO, 1984,87,88). *United States Army in World War II.* A. Garland & H. Smyth, *Sicily and the Surrender of Italy* (Washington: Office of Chief of Military History, 1965); Carlo d'Este. *Bitter Victory: The Battle for Sicily, 1943* (New York, 1988); Carlo d'Este, *Fatal Decision: Anzio and the Battle for Rome* (New York. 1991); John Ellis, *Cassino: The Hollow Victory* (London 1984).
29. B. Montgomery, *The Memoirs of Field Marshall The Viscount Montgomery of Alamein* (London: Collins, 1958), p. 190; 203; Carlo d'Este, *Bitter Victory*, in my opinion the best American historian of World War II, presents a detailed account of this mess.
30. F. A. E. Crew, *The Army Medical Services. Campaigns. Vol. 3, Sicily, Italy and Greece* (London: HMSO, 1959); W. Feasby, *Official History of the Canadian Medical Services 1939–45* (Ottawa: Queen's Printer, 1956).
31. *Medical Department United States Army in World War II. Vol. 6. Communicable Diseases: Malaria* (Washington: Office of Surgeon General, 1963).
32. Quoted in Dancocks, *D-Day Dodgers*, p. 47.
33. For organizational details see Charles Wiltse, *United States Army in World War II. The Technical Services. The Medical Department: Medical Service in the Mediterranean and Minor Theaters* (Washington: Office of the Chief of Military History, 1965).
34. A. Missiroli, "Malaria in Italy during the war and proposed control measures for the year 1945," RAC. RG.1.2 S.700 B.12 f.103.
35. *Health Commission Report, July 1945–June 1946.* RAC. RG.1.1 S.700 B.6 f.32. Soper et al., "Reduction of Anopheles density by the pre-season spraying of building interiors with DDT in Kerosene, at Castel Volturno, Italy in 1944–45 and in the Tiber Delta in 1945." *Amer. J. Trop. Med.* 27 (1947): 177–200.
36. P. Russell, "Memorandum on malaria and its control in liberated Italy, 1 January–30 September, 1944"; Henry Kumm, "Malaria control west of Rome during the summer of 1944." RAC. RG.1.2 S.700 B.12 f.101, and 102.
37. Crew, Campaigns Vol. 3. p. 528. A more negative assessment is given in Wiltse, *The Medical Department*, Appendix D, The German Medical Establishment.

# 9

# Malaria: The Ultimate Kill

Fred Soper's long-held theory about the possibility of species eradication—the total eradication of a vector species so as to eliminate forever the need for antimalaria campaigns—had its roots years before in work in Brazil. There he had discovered that the house-loving *Aedes aegypti* had been virtually eliminated from some of the old seedbeds of yellow fever. And if that were possible for *Aedes*, why not for anophelines?

After the discovery of jungle yellow fever Soper wanted to move elsewhere. And he knew exactly where that was. In 1930, Raymond Shannon, an entomologist with the Yellow Fever Service, discovered *Anopheles gambiae* at Natal in northeast Brazil, carried there, he assumed, by visits of French destroyers based in Dakar, West Africa. By 1931 this species, described by Patrick Manson in *Tropical Diseases*, as "the best known, most dangerous and widely spread carrier of malaria in the whole of Africa," had been blamed for a malaria outbreak in Natal; the threat subsided after a severe drought, but it reappeared in 1934 (see Fig. 6.4). By 1938 the mosquito had moved north and west along the coast and had penetrated inland via various river valleys, with severe outbreaks of malaria reported. A survey in August by Evandro Chagas reported that 81% of the population of Russas, on the Jaguaribe River, was infected, many with *Plasmodium falciparum*, the most pathogenic of all malarial species. The outbreak could be directly re-

lated to the presence of *A. gambiae* in the area, the Brazilian authorities concluded, and "immediate and energetic" measures would be needed to halt its spread over the whole country threatening that "in half a century only the ruins of our race will remain."[1]

Soper knew what these measures should be. "It has been found to be much more economical to obtain and maintain a zero index," Soper noted in his 1938 report to the Pan American Sanitary Conference.[2] Indeed the Yellow Fever Service's campaigns had been so successful that the *Aedes aegypti* mosquito had been virtually eradicated in many places. On this evidence species eradication seemed possible, particularly if the species could be attacked while still limited in area. Soper speculated that a newly-arrived invader would be less well adapted, and more susceptible to attack than an indigenous species. The insect could also be eradicated more easily before it began to surge inland.[3] He pressed his case by warning the mosquito could spread into North America. If it reached the Maranhao River, he argued, "all thought of interrupting its further invasion of much of South, Central, and even North America, must be abandoned (Fig. 9.1)."[4]

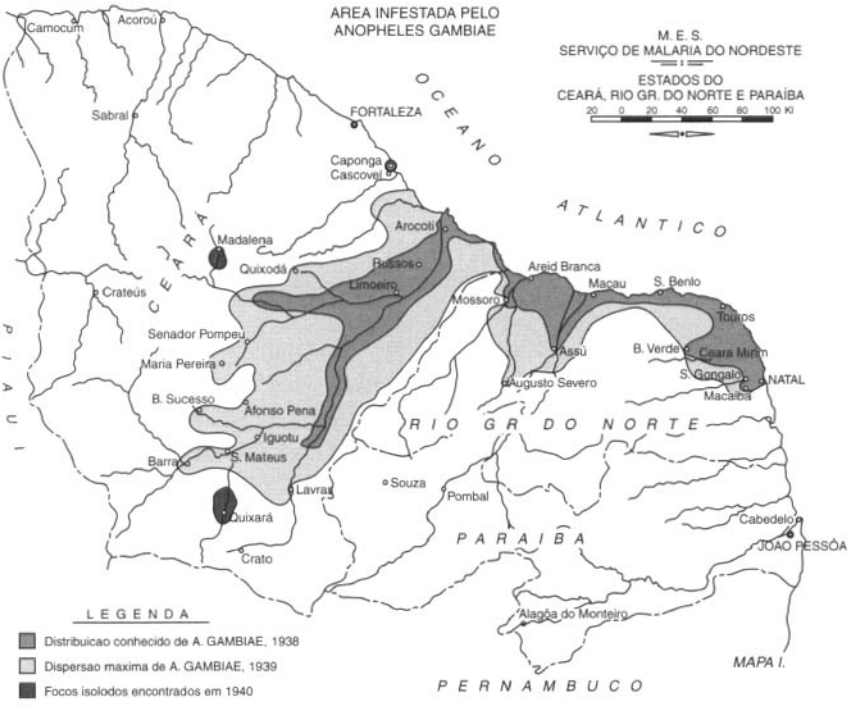


**FIGURE 9.1.** *Anopheles gambiae* in Brazil (courtesy of the Rockefeller Archive Center).

In November 1938, Soper and staff member Bruce Wilson traveled to New York to discuss setting up an anti-gambiae service on the yellow fever model. Shannon felt that *gambiae* would be vulnerable to attack during the long dry season when the insect would be confined to precarious breeding sites in river pools.[5] Surprisingly Soper hesitated. He worried that "it may take as much time and money to get rid of the last one percent of *A. gambiae* as of the first 99%."[6]

The Health Division finally agreed to assist the Malarial Service of the Northeast, established by President Vargas in January 1939. The Brazilian unit took responsibility for fumigation, treatment and education and the Health Division directed the main attack against the breeding sites of the enemy. Organized along the military-style lines of the anti-Aedes campaigns, the goal was not the control or elimination of malaria, but, rather the eradication of the vector species.[7]

In the spring of 1939 Soper began bombarding New York with demands for additional manpower and more financial support. The amount and severity of malaria in the northeast was "beyond comprehension." If *gambiae* spread "I see no possibility of ever again even dreaming of eliminating it, or preventing its widespread dissemination." At issue he announced, was the future of the whole American continent. The attitude of the Board of Trustees reflected a lack of vision, he exploded, after being informed that budget cuts were likely. "It is as true today as in the time of Isaiah that where there is no vision the people perish," Soper thundered.[8]

By November 1939, in the midst of the dry season, a report of new *gambiae* findings near the western boundary of Ceara and the state of Piaui, uncomfortably close to the Amazon watershed, heightened an air of crisis, although the campaign seemed to be going well elsewhere. The key, everyone realized, lay in the forthcoming wet season. As one newspaper reporter wrote dramatically: "The Marginot Line must be held . . . This is war to the death; for until the last surviving pair of gambiae is dead, millions of people in the Western Hemisphere cannot be safe.[9]

And the line was held. By the end of the 1940 rainy season only the Russas area remained problematical. *Anopheles gambiae* was limited to the lower valley of the Jaguaribe (Fig. 9.1). By the second dry season, most of the attack was concentrated in that area. As a result, no *gambiae* adults or larvae were found during the last 47 days of 1940 (the last larvae having been picked up on November 14) "Exit Gambiae (for whom the bell tolls)." Soper triumphantly exclaimed. "Those directing the gambiae campaign no longer consider it rash to speak of the "eradication" of gambiae from Brazil."[10]

This experience made Soper a fervent advocate of species eradication as a solution to vector-transmitted diseases: malaria and yellow fever.[11] In their first published paper on the subject, Soper and Wilson argued that only the actual elimination of *Aedes* would make urban dwellers safe from an invasion of jungle yellow fever. With the success of the *gambiae* campaign, this now seemed a universally

applicable method. "Species reduction," they wrote, "has been pushed to its logical conclusion, namely 'species eradication.'"[12]

## Egypt

When, in January 1943, Soper arrived in Cairo with the forward echelon of the U.S. Typhus Commission, he learned that during the previous spring a serious outbreak of *falciparum* malaria had broken out in Aswan province in the upper Nile Valley, near the Sudan border. A medical entomologist discovered that *Anopheles gambiae* was breeding there. Subsequent research located the species as far north as Asiut and Manfalut, only 200 kilometers up river from Cairo. With a very limited budget, the Egyptian Malarial Service began distributing large amounts of antimalarial drugs.[13]

Soper gained an audience with the Egyptian under secretary of state at the Ministry of Health. He went on to a hurried four-day visit to Girga, without, one assumes, the permission of the Typhus Commission. In a letter to the Egyptian under secretary, copied to the U.S. ambassador, he expressed alarm over the potential risk to Cairo and to the heavily populated Nile Delta. He said the *gambiae* should be eradicated from the Nile by use of Paris green and pyrethrum, along the lines of the methods already established by his team in Brazil. Soper told the U.S. Ambassador that he had recommended, verbally, that the Egyptian government contact the Health Division for assistance.[14]

By advising the Egyptian health authorities that the epidemic in the upper valley was "an entirely new experience for this region," Soper stepped into a political firestorm where antagonisms already existed among the Egyptian king, the Egyptian government and the British authorities. The government had accused the British of carrying the mosquito into the country from the south, an accusation made more plausible by Soper's claim that an epidemic of that kind had never before occurred. The British denied the accusation and claimed that *gambiae* had often moved into the upper Nile valley. But it had never penetrated north and was unlikely to do so. Soper's letters also heightened British suspicion of the Americans who were seen as interfering in a British sphere of influence. Soper noted in his diary that he left a meeting with the British malarial authorities under the impression they thought him "a dangerous individual."[15]

At a meeting of the Middle East Medical Advisory Committee in January 1943, which Soper attended, the Egyptian delegates asked the British for 500 tons of Paris green, and for pyrethrum. There was not that much available in their stores in Egypt and the British were hesitant about allocating shipping space to Paris green when there were serious problems getting supplies to the Commonwealth 8th Army as it pushed Rommel back west following the previous October's victories at El Alamein. Soper argued that the possibility of an epidemic in the Delta left the British no choice but to supply the Paris green. The British argued that

*gambiae* had appeared in the upper valley before, only to spontaneously disappear.[16] Over the next few months the Ministries of Supply and War Transport in London haggled with the British military authorities in Cairo, demanding documentation attesting to the need for such high priority. The military in Cairo tended to agree, they wrote back, but Soper and the Americans had raised the alarm. "There might be serious repercussions should any repetition of exceptional spread occur in 1943 and adequate supplies to meet it are not available," they warned.[17] For his part, Brigadier Sinton, consultant malariologist to the British army, accused Soper of "making a great deal of unnecessary fuss."[18]

In March, and again in May 1943, Lord Killearn, the British ambassador, covered himself by advising the Egyptian Prime Minister to seek the advice of experts in the Rockefeller Foundation who had experience in attacking *A. gambiae.* He did so after learning that the U.S. ambassador—influenced by Soper—had written an "unduly alarmist" note to the Egyptian prime minister. Reporting to foreign secretary Anthony Eden, the ambassador said an epidemic outbreak would trigger severe criticism from the Egyptians and the Americans. After the letter, neither should be able to claim the British had not offered their help. Killearn did not recommend that Soper be included among the experts to be consulted. The British would be happy to see the arrival of American experts, the ambassador told Eden, although cooperation would be "stultified" by the "uncompromising personality of Dr. Soper." The Egyptians, with a long history of exploitation and interference by foreigners, wanted none of it. The Egyptians, the prime minister told Killearn, had had long experience with malaria, and "I am persuaded that [our] specialists can alone assume the mission . . . against Anopheles with success as they have done up to the present." He reiterated his request for Paris green, pyrethrum, and 3.5 million malarial tablets.[19]

Reports persisted among the British and Americans that the malaria campaign was not being properly handled by the Egyptians. Nobody was very surprised when serious outbreaks of malaria occurred in Idfu, Qus, Kom Ombo and elsewhere, with a huge increase in malaria mortalities and catastrophic social and economic effects.[20] Historian Nancy Gallagher notes in her book that 100,000 people are estimated to have died in the epidemic! By then, however, the U.S. Typhus Commission had managed to push Soper out of Cairo; he had antagonized them as well.

In February 1944 the Egyptian prime minister, finally conceded the seriousness of the situation in a four-and-one-half hour speech to the Chamber of Deputies. His hand had been forced. When news of the malaria epidemic reached Cairo in January 1944, high-ranking officers of two women's volunteer organizations, the Mabarra and of the Red Crescent, with close links to the crown, began relief operations. They distributed medicines, clothes and food; and they suggested the government had been lax in its response to the situation. Their actions presented King Faruq with a golden opportunity. Early in February he left Cairo by special

train to spend his birthday among his malaria-stricken subjects in the upper Nile valley. Amid carefully staged photo-opportunities he donated money, and helped generate much anti-government publicity.

In the tumultuous months that followed, all sides played political football with the suffering *Fellaheen,* with the prime minister taking two lavish trips himself to the south. The Wafd government found itself under mounting criticism from political opponents, the British, and influential landowners who were losing money with their laborers sick and their land unworked. The government capitulated. In April 1944, they agreed to call in the Rockefeller Foundation, and it invited Soper back from Italy to make a preliminary investigation.

Soper's report to the minister of public health was predictable. A special anti-gambiae service was needed whose goal would be to eradicate *A. gambiae* from Egypt.[21] The government quickly agreed, and in May 1944 the gears began to turn. J. Austin Kerr was appointed director of the service two months later.

The first few months were not successful and Soper quickly lost faith in Kerr. But a reorganized Paris green campaign began in March 1945, and success followed nine months later.[22] November 29, 1945, became a "red-letter day," in that three months after the Paris green treatment had ended, no *gambiae* were found. "These facts," Kerr boasted, "justify the conclusion that the *Anopheles gambiae* mosquito has been completely eradicated from all parts of upper Egypt,"[23] Soper, of course, had no doubts. "The end of the gambiae story in Egypt is a happy one," Soper enthused, and "this invader was annihilated—completely eradicated."[24] In 1950, John Weir told Strode that *A. gambiae* were back in Egypt; the British authorities may had been correct all along![25]

Soper, with the backing of Sawyer, now wanted his pet theory of species eradication to be put to a definitive test. Critics had claimed his success in eradicating *gambiae* from Brazil and Egypt was achieved because the insect was a temporary invader, not well adapted to its new surroundings and thus susceptible to attack. To prove his theory Soper had to take on a well-ingrained indigenous species and eradicate it.

Earlier attempts to achieve this in Peru had been unsuccessful. There, in 1941, Hackett had told Sawyer that Peru seemed a perfect spot to attempt species eradication of *Anopheles pseudopunctipennis,* the only malaria vector on the Pacific coast.[26] It seemed a perfect spot because the mosquito bred in pools left during the dry season and because the river valleys were separated from each other by desert. Thus, in theory, the vector could be eliminated one valley at a time without danger of reinfestation from an adjacent valley. But by the end of the first year the campaign was suspended because outbreaks of Verruga or Carrión's disease, had disrupted the program.[27] In 1944, Hackett managed to obtain DDT from the US military. He saw it as the perfect chemical for eradication. "It seems to me that 'one good heave' with the all-out use of DDT might turn the trick," he told

Strode, before handing over the program to the Peruvian authorities. Hackett was not happy that DDT was regarded as a residual spray which merely broke malarial transmission.[28] Soper agreed; DDT was also a wonderful weapon to achieve species eradication.

The problem of cost (everyone realized that species eradication was a hugely expensive proposition) was solved when, in 1944, Wilbur Sawyer resigned from the Health Division to become director of the United Nations Relief and Rehabilitation Administration (UNRRA). The organization had been formed the previous year to provide food, clothing and shelter to the liberated populations of Europe and to "aid in the prevention of pestilence and in the recovery in the health of the population." This giant emergency relief organization, flush with $168 million for medical work, seemed the perfect agency to fund an eradication program.

Soper, following Alberto Missiroli's advice, had suggested to Lt. Col. Dudley Reekie, chief of UNRRA's medical section, that the island of Sardinia would be a logical test site. Sardinia provided a perfect spot for anti-larval methods, according to Missiroli who had worked with Hackett in the 1920s. There was little marsh land, little rain, and *A. labranchiae* breeding was restricted to brackish water near the coast.[29] And eradication seemed a more viable option on an island site.

In October 1945 the Health Commission agreed to cooperate with UNRRA and the Italian government in the Sardinia project, the largest-scale attempt ever made to eradicate an indigenous anopheles vector. It was a curious decision. DDT had after all, seemed to alter everything: it was capable of interrputing the transmission of malaria without requiring the huge expense of eradicating the vector. As Hackett wrote years later, "DDT provided a master key to all the different problems and seemed to make quinine, drainage, screens and larvicides obsolete at once."[30] Did it not therefore also render species eradication obsolete? Soper conceded that it did when he wrote to Strode, "The use of DDT as a house spray is the answer to the health workers' prayer." Malaria control could now be achieved by a single spray without bothering with other methods.[31] Nevertheless, they did bother.

Kerr, now completing his report on *A. gambiae* eradication in Egypt, was skeptical too. The fact that the mosquito had been eradicated in Brazil and Egypt, he told Strode, did not mean it was feasible to do it elsewhere. Apart from the cost, there was a danger of reinfection across the narrow passage separating Sardinia and Corsica "Only if it was conclusively shown that long-distance dissemination of *Anopheles* did not occur over a period of years," he wrote, "would eradication be attempted." He was equally aware that the discovery of DDT had altered the picture entirely. DDT residue might make malaria control so inexpensive, he told Strode, "that Anopheles eradication is not the method of choice for malaria control."[32] The foundation went ahead anyway and, amazingly, Kerr was chosen to direct the Sardinia campaign. Soper had his way. Spring would be spent applying Paris green, and houses would be sprayed with DDT against "hibernating" mosquitoes during autumn and winter.

## The Sardinia Campaign

In April 1946, the Kingdom of Italy established the *Ente regionale per la lotta antianofelica in Sardegna* (ERLAAS), supervised by the Italian high commissioner for hygiene and public health, financed to the tune of 300 million lire by UNRRA and directed by experts of the Health Commission, with J. Austin Kerr as its first field director.[33] The object of ERLAAS, according to Article 2 of the Official Gazette, was to eradicate the *Anopheles* vector of malaria.[34]

Sardinia, a rugged, roughly rectangular-shaped island, covering an area of 250 km by 150 km, and lying some 180 km west of the Italian mainland, was long known as one of the most malarious areas in the Mediterranean region (see Fig. 9.2). With alluvial soils limited to the Campidano rift valley and the area around Sassari, most of the island consists of the barren deforested uplands of Regione Vulcanica and the palaeozoic rocks of Nurra, Inglesiente and Barbagia. These boulder strewn eroded lands, fit only for goats and sheep, marked by gnarred trees, steep gullies, and marshy hollows, provided perfect breeding sites for various anopheline species, including, as Kerr was soon horrified to learn, the main malarial vector *Anopheles labranchiae*. A Sardinia campaign against mosquitoes not only had to do battle with these physical obstacles, but also with the "most static traditional society to be found in Western Europe." Centuries of impoverishment and neglect had left an illiterate society, ruled by a code of silence which allowed violent crime, livestock rustling, and kidnapping to flourish and set family against family, village against village, uplander against lowlander, and Sard against the mainland "Continentali."[35] This was not going to be as easy as Soper imagined.

## The Kerr Years

Previous campaigns against *Anopheles gambiae* in Brazil and Egypt provided the organizational model for Sardinia. But, from the start, a series of irritants and minor catastrophes dogged the campaign. In fact, they commenced even before the campaign got underway. Earlier that year, Soper learned that UNRRA—for which Soper had no great liking—had not yet ordered Paris green and was "simply not yet geared to handle its responsibilities."[36] In May, the Italian government loaned most of the vehicles earmarked for Sardinia to an anti-grasshopper campaign on the mainland. Of these vehicles, one-third had been incapacitated by accidents during the first week; those that were returned were missing spare tires, jacks and air-pumps. Later, DDT became a headache. More valuable than gold to any enterprising Sard, some was stolen from the docks; and the DDT sprays were found at the end of the year to contain only 3% not 5% of the chemical—a loss of 40%! The Italian government continually appropriated DDT for use on the mainland. To add insult to injury, the U.S.-manufactured chemical proved to be of inferior

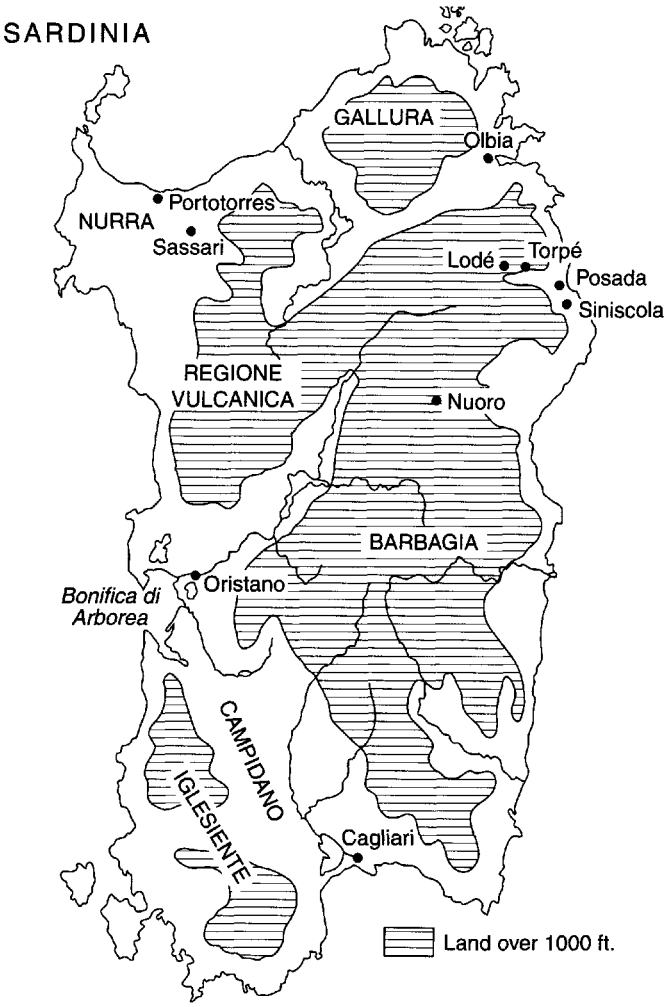


**FIGURE 9.2.** Sardinia.

quality. There were financial problems brought on by the declining value of the Italian lira, and massive inflation, which was not brought under control until 1949. These problems were exacerbated by bank hold-ups and in one case by a bank issuing forged notes to the Sardinian workforce. Local staff were hard to handle and continuous complaints were directed towards their "unreliability " and "untrustworthiness" by the American staff.

But what most plagued the campaign was self-inflicted: a fundamental disagreement among the staff over the goal of the organization. Sometimes it was assumed to be the eradication of *Anopheles labranchiae* from Sardinia; at other times, it was seen as an experiment to determine the feasibility of eradicating an endemic

species (*A. labranchiae*) from the island. In the first case the campaign would fail unless every last mosquito was eradicated; in the second, failure to eradicate could be seen as a successful experiment showing that species disappearance was not possible. To confuse matters even further, the campaign was sometimes seen as a public health program to control malaria on Sardinia. The campaign seemed doomed from the start.

Kerr's earlier doubts about the operation came to a head after entomologist Thomas Aitken conducted a survey of the island's anopheline mosquitoes. He found, to his surprise, that *A. labranchiae* was not simply a lowland-breeding mosquito which migrated to the hills only during the summer months, but seemed to be a permanent resident of the upland areas. That being the case, Aitken warned Soper, one would have to remain skeptical about the chances of eradication.[37] Organizing an anti-larval campaign in these rugged uplands would be an impossible task, Kerr told Soper: "I am of the opinion that the aim of the project there should be changed from the eradication of anopheles to the *eradication* of malaria," by the use of DDT as a residual spray. "Call me a pessimist if you will," Kerr said, "but the word impossible is in my vocabulary, and I intend to keep it there."[38]

Strode, to whom Kerr's letter was passed, found it "somewhat disturbing." So too did Soper. It was known that killing adult mosquitoes by the residual effects of DDT spraying would control malaria, he told Strode—"the repetition of this experiment in Sardinia will not add to our knowledge." Soper's eyes were fixed on the mosquito, not the disease. Strode agreed. "We should stick to our guns," he told Soper, and aim at species eradication.[39]

Strode and Soper became suspicious of Kerr, and concerned lest the objectives of the campaign became lost. But there was continued confusion over what these objectives actually were. Strode may have made plain that the objective was to eradicate the anopheline vector, but he also suggested, in a letter to Sawyer, that it was also to ascertain whether an indigenous species of anophilines could be eradicated.[40] This may seem like splitting hairs, but in evaluating a project such as this there was a great difference between eradicating a species, and determining whether eradication was possible. If the first, then Kerr was within his rights to suggest the abandonment of the project as soon as he felt eradication was impossible; if the second, it had to go forward so that the experiment could be evaluated.

But Kerr continued to complain. After a hurried visit to the island, Strode opened the gate to failure by stating that the reputation of ERLAAS rested on its success in eradicating the mosquito vector from the island.[41] Kerr remained irritatingly pessimistic and refused to assure Strode that the mosquito could be eradicated. By the summer of 1947 Kerr had reached the inevitable conclusion that the eradication of *A. labranchiae* from Sardinia was not feasible. Aitken, whose initial survey of the island had disturbed Kerr so much, had provided yet another blow.

In his summer report, he had not only discovered the mosquito breeding in mountain pools, but reported that it fed off sheep and goats and did not seek out man or his dwellings. House-spraying would therefore be ineffective in reducing the number of mosquitoes—unless they were willing to spray every animal shelter as well.[42] Kerr asked to be relieved of his post. "I do not have either the mental or physical stamina required for this task," he told Strode, "which I am convinced is certain to fail."[43] In September 1947, William Logan, who had come onto the staff the previous May, enthusiastically took over the reins.

## The Logan Years

Unlike the cautious Kerr, Logan had no doubts about the goal and the outcome of the campaign. A month after taking office, throwing caution to the winds, he told the Italian high commissioner for public health and hygiene what was at stake in what he believed a brilliantly-conceived and carefully-planned project. Success entailed species eradication of *A. labranchiae*. "Nothing less than this goal can be sought for," he told the commissioner, "and nothing less than this would be worthy of the effort, planning and money." ERLAAS, he concluded, " can stand for a hitherto undreamed-of epoch in medical history."[44] There was no hedging of bets for Logan.

Having now committed themselves, the possibility of public failure stared the Health Commission in the face. "This is a project for complete eradication," Paul Russell told Strode the following spring, "and there can be no leeway."[45] It implied, that the campaign would be judged on its ability to destroy every last individual of the species on the island.

In October, 1947, DDT was sprayed on every manmade structure whether inhabited by man or sheep. It was believed that a 95%–99% kill would be achieved that way. The following spring an island-wide anti-larval campaign would kill the larvae of those insects that had somehow escaped the winter onslaught. Logan optimistically concluded that in the autumn of 1948, *A. labranchiae* would no longer exist on the island. In August 1948, Logan was less certain eradication could be achieved so soon.[46] Following Paul Russell's advice, ERLAAS invited the Health Commission to remain another year with the goal targeted for the end of 1950 when *A. labranchiae* would have disappeared from the island.[47] Meanwhile, the ever-optimistic scientific directors in New York announced that the project "was about to show for the first time that it is possible to eradicate completely a single insect species from a single area"—although they had to admit that the campaign had become "fabulously expensive."[48] At the end of the year, partly in response to continued attacks from the Communist press, Logan assured the skeptical Italians that ERLAAS had already attained "larval negativity" and that in 1950 "it is our intention to turn over to the people of Sardinia an island completely free from malaria *for all time*."[49] Was Logan being devious in this

**FIGURE 9.3.** Hunting for *Anopheles* larvae, Sardinia (courtesy of the Rockefeller Archive Center).

claim, aware that monthly reports continued to show active breeding sites? Freeing the island from malaria was not what they were supposed to be about.

Enthusiasm became muted, and even Paul Russell complained of a "vacillating and shifting policy."[50] Logan remained optimistic, but Paul Russell, whose eyes were fixed on species eradication, found it disturbing that 84 sections still reported mosquito breeding. "Unless there is at the end of the project one complete year of efficient scouting without finding any labranchiae, adults or larvae," he warned, "the project cannot properly be called successful" (Fig. 9.3). The fact that breeding sites were being found in 1949 indicated to Russell that sites would also be found in 1950, and thus, it would be necessary to continue scouting into 1951.[51]

By the end of 1949, with approximately 8% of the sectors still positive for mosquito larvae, officials began to shift objectives again in an effort to avoid the scent of failure.[52] There were two options: the project could be said to have succeeded because it was an experiment whose success did not rest on the outcome, but on the validity, of its findings. Equally, the project could claim to be successful because malaria had been brought under control on the island, as it certainly had.

Paul Russell was the first to validate the project by reference to its experimental success. In November 1949, in contrast to claims made only a few months before,

he now claimed the campaign had always been an experiment to determine whether *labranchiae* could be eradicated. He reiterated this claim the following June, by arguing that the experiment had not been a failure because it had shown the inadvisability of attempting to control malaria by eradicating the vector.[53]

Marston Bates made the same argument in his preface to the official account of the campaign. The scheme may have failed as a mosquito eradication project, he said, but that was not so say it had failed as an experiment. "The experiment does not fail, because the results differ from expectation."[54]

Russell and Logan weighed in on the other side, suggesting that if things had been done differently eradication might well have been successful. This suggested that the experiment had been a failure because it was poorly designed and executed. But there was little hint of this when Russell wrote in *Man's Mastery of Malaria* that "the experiment appears to have confirmed the majority opinion that it is more practical in most places to eliminate malaria by regularly spraying habitations with residual insecticides. . . . than by attempting to eradicate the transmitting mosquito."[55]

Others were happier to point to malaria; the vector may not have been eradicated, but malaria had been. Russell, while claiming the project was an experiment aiming to test the possibility of vector eradication, said it had been worthwhile because malarial transmission had been broken. Strode too noted in his diary that the disease had been essentially eradicated and the island could at last be developed.[56] Logan himself pointed to malaria control, claiming in his final report that it began as a means to eradicate malaria. "The year 1950 may well go down in the annals of Sardinia as the most significant in its long and varied history;" he said. It had seen the virtual elimination of malaria and its insect vector.[57]

The campaign, ended in September, 1950. It had been extraordinarily expensive; by the late 1940's, it was generally believed the burden of malaria could have been lifted far more cheaply. In his final report, published as a book a few years later, Logan conceded that while it would have been cheaper and easier to carry out a DDT control program, ERLAAS had, nevertheless, made it possible for people to live and work in safety on all parts of the island. But he then went on to claim that the project had moved far beyond malaria. "It has been, in essence, a rehabilitation project."[58]

The campaign which set out to eradicate a vector ended in failure. But when presented as an experiment, as malaria control, or even as a rehabilitation project, Logan was able to claim success. Brown was correct when he called the campaign "failure as success."[59] Fourteen years after the end of the campaign, Thomas Aitken revisited the island. "Great social and economic changes have occurred," he reported, "since the burden of malaria has been withdrawn."[60]

## Notes

1. R. Shannon, "An African Anopheline migrant in Brazil"; G. de Souza Pinto, "Draft of a plan for malaria control in the lower Jaguaribe Region." RAC. RG.1.1 S.305 B.16 f.138.

2. F. Soper. "Report to the 1938 Pan American Sanitary Conference, Bogota." RAC. RG.1.1 S.305 B.23 f.185. The goal of the *Aedes* campaigns had been to so reduce the number of mosquitoes that they could be found in only 5% or less of the houses.
3. It might be intuitively obvious but it is not biologically true. There is no way to predict an invader's ability to become established in a new area. Some rapidly establish new niches; others do not.
4. Soper to Sawyer, May 4 and August 1, 1938; Soper to Russell, July 9, 1938. He was presumably referring to the river which drains into the sea at Saõ Luis (Fig. 6.4). RAC. RG.1.1 S.305 B.16 f.138.
5. R. Shannon, "Anopheles gambiae in Ceara," Memo of November 17, 1938. Ibid.
6. F. Soper. Diary entries, November 20 and December 27, 1938. Soper Papers. MS C 359. Box 9. National Library of Medicine. Bethesda.
7. R. Packard and P. Gadelha, "A land filled with mosquitoes: Fred Soper, the Rockefeller Foundation, and the *Anopheles gambiae* invasion of Brazil." *Parassitologia* 36 (1994): 197–213. (See also Chap. 17.)
8. F. Soper to Sawyer, April 18, 1939; F. Soper to Sawyer, July 7, 1939. RAC. RG.1.1 S.305 B.16 f.139.
9. "A Maginot Line against Malaria." *Hinton News*, West Virginia. April 8, 1940.
10. F. Soper. "Report on Yellow Fever 1940." RAC. RG.1.1 S.305 B.16 f.141.
11. F. Soper, "*Anopheles gambiae* 1941," Ibid.
12. F. Soper and D. Bruce Wilson, "Species eradication. A practical goal of species reduction in the control of mosquito-borne disease." *J. Nat. Malaria Soc.* 1 (1942): 5–24.
13. A detailed and fascinating account of the malaria outbreak in Egypt is given by Nancy Gallagher, *Egypt's Other Wars. Epidemics and the Politics of Public Health* (Syracuse: Syracuse University Press, 1990). (See also Chap. 17.)
14. Soper to Dr. Shousha Bey, January 25, 1943; Soper to U.S. Ambassador, January 27, 1943; Soper to Sawyer, January 28 and 31, 1943. RAC. RG.1.1 S.485 B.2 f.15.
15. Soper's Diary, January 15, 1943. Soper Papers, NLM.
16. Meeting of MEMAC, January 30, 1943. Public Record Office, London. FO 370/799 Health File #70.
17. Ministry of War Transport to Cairo, April 13, 1943; Cairo to Ministry of War Transport, May 19, 1943. Ibid.
18. Sinton memo. February 2, 1943 Public Record Office, London. WO 177/30.
19. Killearn to Prime Minister, March 22 and May 21, 1943; Killearn to Anthony Eden, 31 May, 1943; Prime Minister to Killearn, 15 June 1943. Public Record Office, London. FO 370/799 Health File #70.
20. The seriousness of the problem was noted in letters from the British Embassy to the Prime Minister of Egypt. October 10, November 20, 1943. Ibid.
21. Soper. Report to the Minister of Public Health. May 19, 1944. RAC. RG.1.1 S.485 B.2 f.16.
22. Wilson to Strode, January 7, 1945; Soper to Strode, March 4, 1945. RAC. RG.1.1 S.485 B.2 f.17.
23. Final Technical Report of the Director for Months October, November and December 1945. RAC. RG.5.3 S.485 B.214.
24. J. Duffy (ed), *Ventures in World Health: The Memoirs of Fred Lowe Soper* (Washington, Pan American Health Organization, 1977), p. 253.
25. John Weir to Strode, September 27, 1950. RAC. RG.1.1 S.485 B.2 f.18.
26. M. Cueto, "The meanings of control and eradication of malaria in the Andes," *Parassitologia* 40 (1998): 177–182. He points out that malaria was endemic in two

areas of the country: the coastal zone between the sea and the Andes, and the Peruvian Amazon. In these areas children died or developed some degree of immunity while those born in the Andes highlands and later migrated to the coast for work, living in intolerable conditions, suffered severe attacks of the disease. Details of the campaign in RAC. RG.1.1 S.331 B.5 f.42.

27. Verruga, endemic to the narrow valleys of Peru, is marked by skin eruptions and caused by bacteria which inhabit blood cells and are transmitted by sandflies.
28. Hackett to Strode, July 13, and September 11, 1945. RAC. RG.1.1 S.331 B.5 f.42.
29. A. Missiroli, "Malaria in Italy during the war and proposed control measures for the year 1945." F. Soper. "Memorandum for Lt. Col. Dudley Reekie." Undated, 1945. RAC. RG.1.2 S.700 B.12 f.103.
30. Hackett, MS. *History of the International Health Division*, Chap. 9.
31. Soper to Strode, January 22, 1946. RAC. RG.1.2 S.700 B.12 f.104.
32. Kerr to Strode, October 1945. RAC. RG.1.1 S.485 B.2 f.17.
33. This campaign has been discussed in B. Fantini, "La lotta antimalarica in Italia fra controllo ed eradicazione: L'esperimento Sardegna," *Parassitologia* 33 (1991): 11–23; J. Farley, "Mosquitoes or malaria? Rockefeller campaigns in the American South and Sardinia." *Parassitologia* 56 (1994): 165–173; P.J. Brown, "Failure-as-success: multiple meanings of eradication in the Rockefeller Foundation Sardinia project, 1946–1951." *Parassitologia* 40 (1998): 117–130.
34. *Gazzetta Ufficiale del Regno d'Italia*. Art. 2. Roma: Sabata, 20 Aprile 1946. Copy in RAC. RG.1.2 S.700 B.12 f.104.
35. D. S. Walker, *A Geography of Italy* (London: Menthuen, 1958); Russell King. *Sardinia* (Newton Abbot: David and Charles, 1975); E. Tannenbaum & E. Noether, *Modern Italy. A Topical History since 1861* (New York: New York University Press, 1974).
36. Soper to Strode, January 18, 1946; Soper to New York, April 16, 1946. RAC. RG.1.2 S.700 B.12 f.104.
37. Aitken to Soper, June 1, 1946. RAC . RG.1.2 S.700 B.12 f.104; T. Aitken, "Report on an anopheline survey of the island of Sardinia during the summer of 1946." RAC. RG.1.2 S.700 B.15 f.124.
38. Kerr to Soper, June 6, 1946. RAC. RG.1.2 S.700 B.12 f.104.
39. Strode to Soper, June 18, 1946; Soper to Strode, June 22, 1946; Strode to Soper, June 26, 1946. Ibid.
40. Strode to Sawyer, October 10, 1946. RAC. RG.1.2 S.700 B.12 f.105.
41. Strode's Diaries, April 3 to 7, 1947.
42. Aitken's Report, July 5, 1947. Kerr to Bauer, Aug 26, 1947. RAC. RG.1.2 S.700 B.13 f.110.
43. Kerr to Strode, September 8, 1947. RAC. RG.1.2 S.700 B.13 f.111.
44. Logan to Perrotti, October 13, 1947. Ibid.
45. Russell to Strode, June 19, 1948. RAC. RG.1.2 S.700 B.13 f.112.
46. Logan to Grant, August 4,1948. RAC. RG.1.2 S.700 B.13 f.112, 113.
47. Russell to Strode, June 19, 1948. RAC. RG.1.2 S.700 B.13 f.112.
48. *Report Sardinia Anopheles Eradication Project*, August 7, 1948. RAC. RG.1.2 S.700 B.13 f.113. Minutes, Scientific Directors Meeting, I.H.D., September 24, 1948.
49. Logan to Spallicci, December 3, 1948. RAC. RG.1.2 S.700 B.13 f.113. Throughout the campaign the Communist press kept up a barrage of hostile comments against what they described as a "neo-fascist organization," accusing ERLAAS of being ready to take over the island and convert it into a giant U.S. air base. The intensity of these attacks grew to such an extent that, at the request of Chester Barnard, president of the

Rockefeller Foundation, the public relations department of ERLAAS prepared an internal report on communism in Sardinia. The Italian Communist party had been founded in 1921 by a Sard, Antonio Gramsci, as a splinter group of the Social Democratic Party. The Social Democrats had had strong support in areas of Sardinia, but the collapse of the party during the fascist era allowed the Communists to take advantage of the postwar power vacuum and to gain support in the mining areas of Iglesiente. Although supported by about one-fifth the population of Sardinia, the work of ERLAAS would be unlikely to suffer from their hands, said the report, "as long as it is confined to anti-malarial measures." K. Chesney, "Report on Communism to Rockefeller Foundation," Jan 7, 1949. RAC. RG.1.2 S.700 B.13 f.114.

50. Russell to Strode, April 19, 1949. Ibid.
51. Russell, "Confidential memo to JBG (Grant) from PFR regarding ERLAAS," August, 1949. RAC. RG.1.2 S.700 B.13 f.115.
52. Logan Report, October 17, 1949. Ibid.
53. Russell to Missiroli, November 3, 1949; Russell to Strode, June 27, 1950. RAC. RG.1.2 S.700 B.14 f.117.
54. Marston Bates, Preface to J. Logan, *The Sardinian Project* (Baltimore: The Johns Hopkins University Press, 1953).
55. Paul Russell, *Man's Mastery of Malaria* (London: Oxford University Press, 1955), p. 240.
56. Russell to Missiroli, November 3, 1949; RAC. RG.1.2 S.700 B.14 f.117. Strode's diary. June 23, 1950.
57. *Annual Report ERLAAS*, 1950. RAC. RG.1.1 S.700 B.15 f.131.
58. J. Logan, *The Sardinian Project*. Chapter X, "Critical Review," pp. 279, 302.
59. P. J. Brown, "Failure-as-success."
60. T. Aitken, "Report on a Trip to Sardinia to Review the Island's Malaria Program, 27 Sept–11 Oct, 1964." RAC. RG.1.2 S.700 B.15 f.125.

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# III

# A RESEARCH PROGRAM

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# 10

## Reorganization and Research Laboratories (1928-1940)

Although the International Health Division of the Rockefeller Foundation is best known for its field campaigns against hookworm, malaria, and yellow fever, it also supported laboratory-based research programs. These dealt not only with yellow fever and malaria but also with other diseases. In the process the IHD attracted a great deal of criticism from members of its own field staff.

The move towards research began in October 1928, five months after Noguchi's death, and at a time when the Rio yellow fever epidemic was casting doubt on the seedbed theory. Frederick Russell acquired two small rooms in the Rockefeller Institution's animal house on the east side of Manhattan to act as a yellow fever laboratory. In a memorandum to the trustees of the Rockefeller Foundation one month later, he argued that the Health Division had become overly concerned with public health administration and disease control, and had "lost contact with the scientific aspects" of its work. He urged that staff members be imbued with a "scientific attitude of mind, that is, with the spirit of inquiry and desire to increase knowledge."[1]

He pointed to the importance of research in the business world. The American Telephone and Telegraph Company, for example, did "not devote all its energies to building lines and renting telephones" but also undertook research, some of which had no obvious practical application. The Health Board had no similar research department; any discoveries had occurred under poor conditions and at great

expense. A centrally-located laboratory was needed, to place emphasis on fundamental research.

Russell's concern can be partly attributed to the yellow fever problems being experienced in Brazil. But it also represented a smart political move; his plea coincided with a demand from many in the Rockefeller Foundation for a more consolidated framework in which the overriding influence of medicine and public health would be diminished.[2] Russell was ultimately able to turn this friction to the Health Board's advantage, for in the process it became the International Health Division, gained considerable strength and independence, and took on a greater research role.

## Reorganization

The Rockefeller philanthropies in the 1920s comprised five bodies: the Rockefeller Institute for Medical Research; the Laura Spelman Rockefeller Memorial; the General Education Board; the International Education Board; and the Rockefeller Foundation, each with its own budget and its own board of trustees.[3] The Rockefeller Foundation, by far the richest of the five, was made up of two boards and two divisions, with an emphasis on medical education and public health (Table 10.1). Considerable overlap had developed over the years between the General Education Board and the Rockefeller Foundation's Division of Medical Education as well as among the four bodies making up the foundation.

To rationalize the system, foundation President George Vincent early in 1926 set up an interboard committee on reorganization chaired by Raymond Fosdick. Its task was to devise a sensible framework into which these often-competing programs would fit.

The committee found an imbalance between the two boards on one side and the two divisions on the other. The China Medical Board and the International Health Board each had its own board of trustees, enabling them to operate with a degree of autonomy not shared by the Divisions of Medical Education and of Studies. It recommended that all four should become divisions with a single board

**Table 10.1.** The Rockefeller philanthropies in the early 1920s

1. Rockefeller Institute for Medical Research
2. Laura Spelman Rockefeller Memorial
3. General Education Board
4. International Education Board
5. Rockefeller Foundation
   a. China Medical Board
   b. International Health Board
   c. Division of Medical Education
   d. Division of Studies

of trustees for the entire Foundation.[4] As a result, effective April 1, 1927, the International Health Board was renamed the International Health Division, reporting directly to the foundation's board of trustees. Its wings had been clipped, or so it appeared.

Raymond Fosdick was asked to head a second committee on reorganization, this time to rationalize the often-competing functions among the divisions. By the end of 1927, Fosdick concluded that if the Rockefeller Foundation became a "board for the advancement of knowledge," many of its internal conflicts would disappear. In this scenario, the foundation would divest itself of its operating agencies. The China Medical Board (now Division) and the International Health Division would become separate operating agencies outside the foundation. The foundation itself would no longer focus on medical education and public health; it was to be reinvented around the advancement of knowledge.[5]

The foundation now appeared headed towards a research-orientated five-division structure (Divisions of Natural Sciences, Social Sciences, Humanities and Arts, Medical Science, and Agriculture and Forestry), while the Health Division was to be cut loose and perhaps allowed to die. "Its program might wisely be gradually reduced and ultimately terminated," Rockefeller Jr. told his father. Vincent eagerly anticipated the retirement of this troubling and too-powerful division.[6]

The power and influence of the "medical barons, " to use Robert Kohler's apt term, were clearly being threatened. But Fred Russell and his fellow barons were too powerful to be cast aside that easily. Aware of these dynamics, the Fosdick committee had asked Simon Flexner, director of the Rockefeller Institute, to look into the future of the Health Division with a view to giving it a "status of considerable autonomy with dependence upon the Foundation," and to report back in November 1928. Russell saw his opportunity and, in that memo, claimed that the Health Division was not simply a field agency, but one with a mandate for research, and a need for a centrally-located laboratory. It had, in other words, a natural and prominent place within the framework of the reorganized foundation as recommended by Fosdick. Russell had made an astute move.

Flexner's report recommended not only that the Health Division be retained within the foundation, but that it be accorded special status. Much of its work, Flexner argued, was of such a technical and scientific nature that the lay board of trustees, to whom all divisions were now required to report, would find it impossible to understand. Instead, the Health Division should report to a Board of Scientific Directors, that would recommend to the trustees the size of the division's annual budget and how to allocate it.[7]

That was exactly how the Rockefeller Institute had been organized. In 1910 its original Board of Directors had been replaced by a Board of Scientific Directors—in charge of scientific programs—and a board of trustees who basically handled the budget.[8] In 1928, following a resolution by the board of trustees, the Health Division was given a special place within the Rockefeller Foundation by virtue of

having its own Board of Scientific Directors.[9] The scientific directors were called upon to be independent and strong, argued Vincent, who, surprisingly accepted the idea. In an apparent reference to Fred Russell, he said "a board of good-natured acquiescent men would simply perpetuate the one-man system."[10]

## The Health Division's Laboratory Service

Neither Rose nor Russell before 1928 had envisaged a laboratory service as a vehicle for scientific research. Eleven years earlier, Rose had sought the advice of Hermann Biggs, New York State Commissioner of Health on developing public health laboratories, which were by then beginning to play an important role in the public health movement.[11] In 1919, following Biggs's positive report,[12] Rose invited Fred Russell to develop a laboratory service: a year later, Russell became director of the Health Board's Public Health Laboratory Service. These laboratories were to act as an educational and diagnostic tool, with the added responsibility for testing water and milk. Only in one of Russell's early memos is research even mentioned.[13] And that was to caution that research "should not monopolize the attention of the director and prevent adequate routine service being rendered."[14] The first such laboratories were set up in Mobile, Birmingham, and Decatur, Alabama. They serviced practicing physicians, about 55% of whom were taking advantage of the service by the end of 1923.[15]

Russell believed public health laboratories should also act as training bases for potential recruits to the Health Board. In October 1922 the first such training base was opened in Andalusia, Alabama, and Wilson Smillie, who had worked in Brazil, was appointed director. Smillie found the people "illiterate, stubborn, lawless, devoted to moonshine, and prone to violence and bloodshed." Nevertheless, he was full of enthusiasm for his new job. He wanted to enhance public health in the area and teach the craft to the green recruits posted there. "I have affiliated myself with the Masons, the Presbyterian Church, joined a Sunday School class, made an address before the County Teachers Association, the Kiwanis Club, the Rotarian Club, the joint meeting of the Women's clubs," he told Russell, and "have bought a cob pipe."[16] Models of unsanitary and sanitary farms and privies were prepared for the county fair, and two brothers with severe hookworm, aged six and nine, were put on display. "We showed these boys all week," Smillie said, "and will treat them the last day of the fair, and show them again next year. You may be sure that we will nurse these two boys through the year with the same care that a stockman feeds his prize Duroc Jerseys."[17] Rose must have enjoyed that!

## The Research Controversy

Russell had his enemies, and the issue of research broached so forcibly in his 1928 memorandum, was a magnet to those opposed to his appointment as director. Victor

Heiser and C-E.A Winslow opposed the research ideas, saying that there was no need to change an existing program that worked and was still needed."[18] Wade Frost, a scientific director, was less critical than others, but he feared that Russell's kind of laboratory would attract researchers anxious to build their own careers through the publication of scientific papers and would thereby divert attention from public health work.[19]

Another opponent was Simon Flexner, who felt threatened by the idea of a competing central research laboratory. The Rockefeller Institute, he told Rufus Cole, had to retain its research emphasis "unimpaired." Fundamental research was the job of the Institute, not that of the Health Division.[20] Flexner was correct. If Russell's ideas were accepted, the Health Division would cross into Rockefeller Institute' territory. As Institute historian, G. Corner notes, "The Institute's definition of the key words in its title, 'Medical Research,' was . . . to include any kind of investigation that might contribute to the understanding of health and disease no matter how widely it ranged, from the body of a suffering man to subatomic particles. . . . Within these laboratories, capacity for discovery was to be the only test of acceptance." Indeed as the influence of Johns Hopkins spread, and more medical schools opted to carry out research, the pressure to work in applied areas waned and researchers were devoting more time to fundamental research in a wide range of disciplines. The staff at the Institute was given almost complete freedom to choose a research topic, from leucocyte function, chemical ions, hormones and bile salts to snake venom, polio, meningitis and spirochaetes.[21] Russell seemed to be suggesting that the Health Division should follow the same guidelines.

But Russell would now have to broach his research idea to the scientific directors, aware that many of them were not overly enthusiastic about his call for basic research. He began to soft-pedal. A month before a crucial meeting with them, set for October 1929, when the research memorandum would be discussed, he stressed that while the organization "can make a greater contribution to public health by pursuing scientific studies," these studies would be of an applied nature, directed towards concrete situations, "that is, the epidemiology and, if there be one, the method of control of any given disease in a definite locality."[22]

Applied research had in fact been flourishing for six years at the Health Division's station for malaria field studies, in Leesburg, southwest Georgia. From the start this laboratory was seen as an important center for "an intelligent and economic attack" on malaria.[23] Samuel Darling, recovering from brain tumor surgery after being sent home from Brazil, was chosen to direct the Georgia station. In its first year of operation, it showed that the spleen index for children up to 12 years of age was a better indicator of disease prevalence, then the parasite or call indexes.[24] Nor had Smillie been idle in the Andalusia laboratory. He had suggested a series of grim experiments to test the effects of carbon tetrachloride on patients with a history of alcoholism. He saw in condemned criminals in Ala-

bama a perfect tool for an experiment of this nature. Varying doses could be given at various times before execution, with and without alcohol, and organs subsequently examined for pathological changes after the state had carried out its gruesome task.[25] But research of this nature was obviously applied and as such would have garnered little criticism from anyone.

The crucial meeting of the scientific directors took place on October 14, 1929. The directors rejected Russell's request for a central research laboratory, but allowed him to retain a central yellow fever laboratory where, presumably, only research of an applied nature would go on. It ended, said Vincent, in Russell's defeat, particularly when Smillie, a lone ally, decided to remain silent.[26] The policy of the division was to remain unchanged, Vincent told Flexner; the central laboratory idea had been abandoned but work would continue on yellow fever.[27] The patient and canny Russell may have lost a battle, but he certainly had not yet lost the war.

In the long run, he still planned a research-dominated work program. The timing of his first move to push the organization in this new direction was triggered by Fosdick's concept of focusing on the advancement of knowledge. But the idea must have come from the Health Board's yellow-fever work. As a laboratory man himself, Russell was only too aware that work on this extremely dangerous disease, caused by an invisible virus, required a well-trained cadre of virologists working in a well-maintained and organized central laboratory. Continuing to rely on small, isolated, and minimally-maintained laboratories in West Africa and Brazil was inefficient and dangerous.

In 1930 and 1931 he again argued for a home-station. During successful negotiations with Simon Flexner for an extension of the lease on the Institute's animal-room space, Russell told him of hopes to carry out laboratory investigation of diseases other than yellow fever, naming malaria, undulant fever and influenza.[28] He told Fosdick that the Health Division now needed "a university type of person" with "the same facilities, tenure, and working conditions" as those offered in universities.[29]

## The Tallahassee Malaria Laboratory

In 1930 Russell made his move. He and Mark Boyd, a distinguished malariologist in the Health Division, decided that a laboratory for scientific studies of malaria had become necessary. A native of Saint Paul, Minnesota, Boyd earned his M.D. at Iowa University in 1911. Before joining the Health Board staff in 1921, he taught bacteriology and preventive medicine at the Universities of Nevada, Iowa and Texas. After working on malaria in Brazil between 1922 and 1925, Boyd won a name as a top man in his field.

Boyd believed the laboratory should be located in an area with a high prevalence of malaria among blacks rather than "intractable poor whites," and where little control work had been initiated. (Blacks were useful in laboratory testing.)

Access would also be needed to experimental subjects in mental hospitals. There, inoculation with malarial parasites had recently become a method of treatment for general paralysis of the insane brought on by tertiary syphilis.[30] Boyd had another idea. Why not inject the malarial parasite in order to investigate the progress of the disease rather than cure of the patient. Why not indeed?

Boyd decided to set up the laboratory at the State College for Women in Tallahassee, Florida, and to use the Chattahoochee state hospital for the induced malaria experiments. The patients selected would be isolated in screened wards and would be fully available to laboratory personnel. The staff could carry out autopsies when necessary, visit the patients at any time to collect blood, and allow laboratory mosquitoes to become infected by controlled feeding on the patients.[31] This was, in effect, the first laboratory of the Health Division that fulfilled Russell's demand for a "spirit of inquiry and desire to increase knowledge." It may not have been in New York, but, tucked away in distant Florida, it remained out of sight and out of mind to Russell's critics in the north.

Experiments began in June, 1931. A white youth in relapse became the first "gametocyte carrier," who willingly, we are told, allowed wild anophelines to feed on him for four days. These infected mosquitoes were then assigned to paretic cases and the transmission cycles began. Induced benign tertiary malaria, brought on by strains of *Plasmodium vivax*, was successfully passed through white human-anopheline transfers during the first year, and 26 patients showed symptoms of the disease. Different stages of the disease (initial, developed and terminal) began to be described. By the second year, an insectary had been built in which their own mosquitoes, *Anopheles quadramaculatus*, were produced, "as large, vigorous and blood thirsty as the best wild specimens."[32] These mosquitoes successfully passed five strains of *P. vivax* with very little risk to the patients, with 41% of them reporting improvement.

By the end of 1933, nine publications entitled "Studies on *Plasmodium vivax*" and "Studies on benign tertian malaria" had been published by the Florida laboratory in *Science* and the *American Journal of Hygiene*. Three years later the number had grown to 50, some in the *American Journal of Tropical Medicine*. By 1939 ninety papers had been published, only one of which dealt with therapeutic aspects.[33] As Boyd noted in his 1935 annual report, "the investigational activities of the station continued to cultivate the apparently inexhaustible opportunities for research afforded by the malaria therapy service."[34]

He and his staff found that blacks could not share the benefits of malarial therapy because they were resistant to *P. vivax*.[35] This tolerance among blacks, Boyd argued, was racial, not acquired. He came to this conclusion after discovering that a five-year-old black child sick with hereditary syphilis failed to show clinical malaria after two inoculations with *vivax*-infected mosquitoes (what on earth was he doing inoculating a child?).[36] Thus, since blacks appeared to be refractory to *P. vivax*, several were treated with the far more pathogenic *P. falciparum*, the cause

of malignant tertian malaria. Boyd was clearly uneasy about this and admitted that some blacks became seriously ill and at least one died.[37]

The Florida laboratory had become equivalent to an academic research department, with Boyd, in particular, building up a lengthy list of refereed research papers. Although some of the information collected at the Florida laboratory was used to change the treatment regimes, basically the laboratory had moved in the direction feared by Russell's critics.

## Research Victory

Controversy over the place of research within the Health Division essentially ended in April 1933 when, at a special meeting of the Rockefeller Foundation Trustees, Russell again argued that failure to solve certain disease problems resulted from insufficient knowledge.[38] He had already won over the scientific directors. Charles Winslow, his most fervent critic, was no longer a member; new directors included Alphonse Dochez who, as will be described later, was not only a former member of the Rockefeller Institute but a recipient of the Division's research funding.[39] By this time, Russell's old nemesis, George Vincent, had retired as president of the Rockefeller Foundation. In the absence of these powerful opponents, Russell won, and won handsomely. The yellow-fever laboratory was renamed the Laboratory of the International Health Division, and given its own budget as well as responsibilities for diseases other than yellow fever. A past director of the yellow-fever laboratory, Dr Wilbur Sawyer, was chosen to succeed Russell on his retirement as director of the Health Division in 1935. A major change in the organization had taken place and Russell had won the war.

## The International Health Division Laboratory

The laboratory's budget grew considerably as staff and space were added. Malaria soon became its main focus. Sawyer planned to study induced malaria with another group of paretic patients at the Manhattan State Mental Hospital. He planned to study also monkey malaria, which, if successfully transmitted by insectary-bred *A. quadrimaculatus* in the laboratory, would, "offer a set of practically limitless opportunities."[40] *Plasmodium knowlesi*, the agent of monkey malaria, was also used to test whether "complement fixation" could be used as a diagnostic tool, exactly the type of research more appropriate to the Rockefeller Institute than to a public health laboratory. A test on this order of complexity would hardly serve the interests of public health workers in the field.[41]

Characterized by extraordinarily large numbers of parasites in the blood, *P. knowlesi* was inoculated into paretic black patients. In other words, being refractory to the benign *P. vivax*, they were infected with a potentially virulent malaria. This barbaric practice seemed acceptable, if indeed they were much

TABLE 10.2. Percentage of Foundation funds allotted to the five divisions, 1925–1940

| DIVISION | 1925 | 1930 | 1935 | 1940 |
|---|---|---|---|---|
| International Health Division | 40% | 24% | 20% | 31% |
| Medical Sciences | 38% | 17% | 19% | 15% |
| Natural Sciences | | 41% | 19% | 25% |
| Social Sciences | | 17% | 30% | 17% |
| Humanities | | 2% | 12% | 12% |
| China Medical Board | 15% | | | |
| Division of Studies | 7% | | | |

worried about such matters, because researcher L. Coggeshall found that some of the recently-discovered sulfanilamide drugs were effective against *P. knowlesi* in rhesus monkeys where it is usually fatal, while they had no impact on *P. vivax* in humans. Thus, it seemed safe to inoculate human black "volunteers" with agents of monkey malaria; if things went wrong they could perhaps be rescued with the new drugs.[42]

Both Hackett and Soper were put out by this type of research, although not for moral reasons. Soper complained that it was no longer possible to distinguish between the Health Division and the Rockefeller Institute; and, it may have been a major reason for his resignation to take up the post of director of the Pan American Sanitary Bureau. Research, he told George Strode, "should be limited to those problems its staff faces in the field. The laboratory should be the handmaiden of the control service."[43] That seemed to be no longer true.

The IHD had not only survived but, arguably, had grown stronger. In his 1928 report Flexner's proposed that the Health Division's annual budget be gradually reduced to ensure a more equitable distribution of funds among the other divisions, but little came of it; the IHD lost some ground to Social Sciences, one of the weaker divisions, but the Health Division still retained the lion's share of the budget (see Table 10.2).[44]

Russell's call for more emphasis on research helped keep the Health Division at the center of Rockefeller philanthropies. "The policy of seeking new knowledge in the field of public health," Russell said, "would harmonize with the accepted activities of the Foundation."[45] The medical barons were still firmly in the saddle.

## Notes

1. F. Russell, "Memo concerning future developments of the IHD," November, 1928. RAC. RG.1 S.100 B.11 f.91.
2. My account of this reorganization is drawn from Robert Kohler, *Partners in Science: Foundations and Natural Scientists 1900–1945* (Chicago: University of Chicago Press,

1991). Also J. Farley, "The International Health Division of the Rockefeller Foundation: The Russell Years, 1920–1934. In P. Weidling (ed.), *International Health Organisations and Movements 1918–1939* (Cambridge: Cambridge University Press, 1995), Chap. 10.

3. Laura Spelman was the wife of John D. Rockefeller Sr. Following her death in 1915, Rockefeller endowed the Laura Spelman Rockefeller Memorial to support research in the social sciences.
4. "Report of Interboard Committee," November 1, 1926. RAC. RG.3 S.900 B.17 f.122.
5. Fosdick to S. Flexner, November 1, 1927; Fosdick, "Confidential Memo re Reorganization of the Rockefeller Foundation." December 21, 1927. RAC. RG.3 S.900 B.17 f.123.
6. Rockefeller Jr. to J. D. Rockefeller. March 15, 1928. RAC. RG.3 S.900 B.17 f.124.
7. S. Flexner. Divisional Committee for the IHD. Report and recommendations as to the future status of the International Health Division. November, 1928. RAC. RG.3 S.908 B.12 f.129.
8. Details in G. Corner, *A History of the Rockefeller Institute* 1901–1953 (New York: Rockefeller Institute Press, 1964).
9. Meeting of the Board of Trustees of the Rockefeller Foundation, November 9, 1928. RAC. RG 3 S.908 B.11 f.123.
10. Vincent to S. Flexner, July 24, 1928. RAC. RG.3 S.908 B.11 f.124. In addition to Fred Russell, "the one-man system" who acted as secretary, the Scientific Directors Board had six members: chairman Rufus Cole, director of the hospital of the Rockefeller Institute; Wade Frost, professor of epidemiology at Johns Hopkins; C-E. A. Winslow, professor of public health at Yale Medical School; Eugene Bishop, commissioner of public health in Tennessee; Louis Dublin, statistician and vice President of the Metropolitan Life Insurance Company, and Wilson Smillie of the International Health Division whose name was put forward by Flexner on the arguable grounds that having experience of the Health Board he would be able to act as a powerful check on Russell.
11. W. Rose to H. Biggs, 18 December 1917. RAC. RG.3 S.908 B.15 f.174.
12. H. Biggs to W.Rose, February 21, 1918. Ibid.
13. F. Russell. Memorandum to Mr. Rose. "Suggestions for a modification of the plan of the I.H.B. in new countries." October 25, 1921. RAC. RG.3 S.908 B.15 f.174; *Tenth Annual Report, International Health Board*, 1923; Russell, "Statement of Policy." May, 1925. RAC. RG.3 S.908 B.12 f.128.
14. "Principles and Policies of the International Health Board with regard to Public Health Laboratories in the United States." 1926. RAC. RG.3 S.908 B.15 f.174.
15. S. Welch to F. Russell, February 4, 1922. RAC. RG 5.1.2 S.201 B. 126 f. 1694; L. Havens, "Annual Reports of the Laboratories of the Alabama State Board of Health for 1923, 1924 and 1925." RAC. RG 5.3 S.201 B.11.
16. Smillie to Russell, October 7, 1922. RAC. RG.5.1.2 S.201 B.126 f.1699.
17. Smillie to Ferrell, October 28, 1922. Ibid.
18. Heiser's reply to Russell's 1928 memo. September 16, 1929. RAC. RG.3 S.908 B.12 f.129.
19. W. Frost to Russell, June 19, 1929. RAC. RG.1 S.100 B.11 f.91.
20. S. Flexner to Cole, July 3 and 12, 1929. Ibid.
21. G. Corner, *A History of the Rockefeller Institute*, p. 80 and 151.
22. F. Russell, "The program for future work of the International Health Division." September 9, 1929. RAC. RG.3 S.908 B.11 f. 124.

23. S. Darling to Russell, January 9, 1924. RAC. RG.5.2 S.212. B.8 f.48.
24. "Report of Year's Work, 1923, in malarial control in Lee County, Georgia." Ibid. His findings were published in the *J. Amer. Med. Assoc.* 80 (1923): 740–43. See Chap. 7, note 7.
25. Smillie to Heiser, November 24, 1922. RAC. RG.5.1.2 B.126 f.1700. Smillie, testing carbon tetrachloride in Brazil after claims that it could be used to expel hookworms, found that it might harm alcoholics. I have no idea whether these experiments were ever carried out.
26. Two days after the meeting Smillie told Russell he agreed the Health Division should be concerned with "activities and investigations which have no direct bearing upon some concrete control problem," and that progress demanded research become an "independent function." Smillie to Russell, October 31, 1929. RAC. RG.1 S.100 B.11 f.91. Other than allocating $40,000 for studies at the yellow fever laboratory, the minutes of the October 14 meeting make no mention of the laboratory issue.
27. Vincent to S. Flexner, October 22, 1929. RAC. RG.1 S.100 B.11 f.91.
28. Russell to S. Flexner, October 28, 1930; Flexner to Russell, November 5, 1930. Ibid.
29. Russell, Memorandum to Fosdick, July 9, 1931. Ibid.
30. Boyd to Russell, September 2, 1930. RAC. RG.1.1 S.211 B.1 f.2. Julius Wagner von Jauregg of the University of Vienna had first used induced malarial therapy for the treatment of syphilitic paresis in 1917, for which he received the 1927 Nobel Prize. The technique became the standard procedure worldwide before the penicillin era. S. James, "Some general results of a study of induced malaria in England." *Trans. Roy. Soc. Trop. Med. Hyg.* 24 (1931): 477–588. J. Braslow, "The influence of a biology therapy on physicians' narratives and interrogations: The case of general paralysis of the insane and malaria fever therapy, 1910–1950." *Bull. Hist. Medicine.* 70 (1996): 577–608.
31. Boyd to J. Folmar, March 21, 1931. RAC. RG.1.1 S.211 B.1 f.2.
32. Boyd, M. *Florida. Malarial Research Station. Annual Report*, 1932. RAC RG.5.3 S.211 B.19.
33. Boyd, M. et al. "A review of the results from the employment of malaria therapy into the treatment of neurosyphilis in the Florida State Hospital." *Amer. J. Psychiatry* 94 (1938): 1099–1114.
34. Boyd, M. *Florida. Malaria Research Station. Annual Report*, 1935. RAC. RG.5.3 S.211 B.19.
35. Many blacks are protected from *P. vivax* because their red blood cells lack what is called the Duffy antigen. For a detailed account of the relationship between malaria and the black population, see Margaret Humphreys, *Malaria. Poverty, Race, and Public Health in the United States* (Baltimore: Johns Hopkins Press, 2001).
36. Boyd, M & W. Stratman-Thomas. "Studies on Benign Tertian Malaria 4. On the refractoriness of negroes to inoculation with *P. vivax*." *Amer. J. Hyg.* 18 (1933): 485–89; "Studies on Benign Tertian Malaria 5. On the susceptibility of Caucasians." *Amer. J. Hyg.* 19 (1934): 541–545.
37. "Induced Autumnal Malaria" in *Florida Research Station Annual Report,* 1931 and 1932. RAC. RG.5.3 S.211 B.19.
38. Rockefeller Foundation Agenda for Special Meeting, April 11. 1933. RAC. RG.1 S.100 B.11 f.91.
39. By this time the scientific directors included, in addition to Russell, Cole, Frost and Smillie, the new names of John Fitzgerald, of the Toronto University School of Hygiene; Edwin Jordan, professor of bacteriology, University of Chicago; Waller Leathers, dean of medicine, Vanderbilt University, and Alphonse Dochez.

40. Sawyer to Russell, July 12, 1934. RAC. RG.5 S.4 B.25 f.284.
41. "Complements" are plasma proteins which play a role in immunity and inflammation, acting in association with, and thus complementing, specific immune responses associated with antigen-antibody reactions. Thus, if an antibody to an invading antigen is already present in the blood, and the organism is said to be immune to it, the inactive plasma proteins react with the antigen-antibody complex (become fixed) to initiate the production of a complement pathway—a series of other complement proteins (named C1,C2, C3 etc. based on the order of their discovery) which augment the body's defence mechanisms by enhancing phagocyte activity of white blood cells and the destruction of the invading microbe. But if the body is not immune to the invading microbe and no antigen-antibody reaction takes place, the plasma proteins remain inactive, no complement pathway is set in motion and the invading microbes face little opposition. The complement-fixation test is based on these phenomena. It is set up to detect whether a specific antigen, say *P. falciparum*, and its homologous antibody are present in the body; if they are detected there has been a positive diagnostic test for *P. falciparum*.
42. L. Coggeshall & M. Eaton, "The complement fixation reaction in monkey malaria." *J. Expt. Med.* 67 (1938): 871–882; "Complement fixation in human malaria with an antigen prepared from the monkey parasite, *P. knowlesi*." *J. Expt. Med.* 69 (1939): 379–398.
43. Soper to Strode, February 1, 1946. Fred Soper Papers, MS C 359 B.17. National Library of Medicine, Bethesda. Hackett, MS. *History of the International Health Organization,* Chap. 7, "Accent on research," RAC. RG.3 S.908 B.5 f.34.
44. Table X. The Rockefeller Foundation. Appropriations and Expenditures for the years 1925, 1930, 1935, 1939 and 1940. RAC. RG.3 S.900 B.19 f.141.
45. F. Russell, "The program for future work."

# 11

## Yellow Fever Vaccines: A Slap in the Face

In 1928, with the acquisition of two small rooms in the Rockefeller Institute's animal house in New York to serve as a yellow fever laboratory, the production of a vaccine to protect the laboratory workers became the number one priority.[1] By 1937 the Health Division had developed such a successful vaccine that the U.S. army turned to them for assistance on the outbreak of World War II. In 1942 disquieting news began to be heard which led to what became the most embarrassing episode in Health Division history.

At first, the yellow fever laboratory was not a safe place to work. The monkeys, the only laboratory animals then known to be susceptible to the virus, were housed in the largest of the two rooms. According to the technician Thomas Norton, they were "more important than the men who merely worked on them."

> It was called the "dirty" room. It was here that monkeys died of the virulent Asibi virus, and here that the danger lay of infectious blood unknowingly being sprayed or splattered about from some sick monkey with bleeding gums or bloody stools. No one touched anything unless he were dressed for it—white trousers, long white coat, rubber gloves, and rubber apron. Just one object in the monkey room was regarded as "clean"—the doorknob on the screen door leading to the inner room. No person working in the "dirty" room ever touched it. If it became necessary to go out while still in working clothes, some "clean" person had to open the door from the other side, or else rubber gloves had to be taken off, disposed of, and fresh ones put on. This procedure was time-consuming and often irritating, but it was vital inasmuch a way had to be left for a caller to enter and pass through

into the inner room without the danger of touching a contaminated door. Workers on finishing tasks in the monkey room would immediately strip off their gloves and coats and drop them into a container of Lysol. Later the garments were boiled.[2]

Precautions, however extreme, could never hope to stop accidents; during the first two years, seven of the laboratory personnel—including Sawyer, S. Kitchen and Wray Lloyd—came down with yellow fever, although luckily none died. What was desperately needed was a cheaper and safer laboratory animal with which to work, yet none of the usual laboratory animals seemed to be susceptible to the virus when injected into the body cavity in the usual way.

The first breakthrough came in 1928 when Max Theiler at Harvard's Department of Tropical Medicine (who would later join the staff of the Yellow Fever Laboratory) discovered that when mice were injected *intracerebrally* with serum from dying monkeys, they succumbed to what was assumed to be yellow fever. The disease-causing virus could then be passed serially mouse to mouse by the intracerebral injection of brain suspension. By January 1930, 75 mouse-brain passages had been completed and the time of death after injection had declined somewhat during these passages, indicating an increase in virulence. "After repeated passages," Theiler reported, "the virus became so virulent that if the hypodermic needle was merely dipped into the infective brain suspension [brain-tissue plus saline] and then thrust into the brain of the mouse, infection occurred."[3]

The implications of this discovery went far beyond the ability to substitute mice for blood-spraying and highly dangerous sick monkeys. They pointed to the possibilities of a vaccine; mice injected *intraperitoneally* with massive doses of virus rarely became ill while, at the same time, two-thirds of them developed an immunity to a subsequent *intracerebral* injection of the virus—the virus that usually killed. Even more significant was Theiler's discovery that a couple of *monkeys*, injected intraperitoneally with mouse brains of the 29th and 42nd passage, did not die from yellow fever as expected and one of them proved to be immune to an injection of virulent monkey virus 22 days later. As Theiler noted, "assuming that the virus in mice is yellow fever," the experiment "warrants the conclusion that continuous passage of the virus through mice leads to an attenuation for monkeys."[4] An attenuated virus, if it also worked in man, could become the basis of a human vaccine.

But even after over 100 passages of mouse-brain virus, the virus still produced fever when injected into monkeys. With the addition of immune serum, however, the monkeys remained well and developed immunity. Thus, in preparing human vaccine, fresh, sterile, human immune serum had to be added to the attenuated mouse-brain virus. Between May 13 and June 29, 1931, ten volunteers from the yellow fever laboratory, after signing a consent form, were given the new vaccine (see Fig. 11.1). None developed yellow fever although some showed elevated temperatures.[5] But as Norton reports, large amounts of immune serum were needed, all to be taken from those laboratory personnel who had recovered from yellow

**YELLOW FEVER LABORATORY OF THE INTERNATIONAL HEALTH DIVISION OF THE ROCKEFELLER FOUNDATION AT THE ROCKEFELLER INSTITUTE**

Date _____

Dear Dr. Sawyer:

I have been working in the Yellow Fever Laboratory since , and the dangers of the work have been fully explained to me. I desire to be vaccinated against yellow fever in order to be protected as much as possible against the danger of accidental infection.

I am quite aware of the experimental nature of the vaccination at the present time, but desire to have it carried out. I assume all responsibility in making this decision.

Sincerely yours.

**FIGURE 11.1.** Copy of the vaccination form signed by all volunteers.

fever and thus had lifetime immunity. Such a vaccine was not feasible for mass vaccinations.

But how did human volunteers know that they had developed an immunity to yellow fever? They could hardly be exposed directly to the Asibi virus and hope for the best! But they could be tested indirectly through the so-called mouse protection test. If mice survived a brain injection of volunteer serum together with mouse brain virus, then the volunteer had become immune. If, on the other hand, the mice died, then the patient was not immune.[6]

To find a vaccine suitable for mass use, the virulence of the virus needed to be further reduced so that immune serum would no longer be necessary. In the early 1930s, Max Theiler and his co-workers were able to cultivate the Asibi virus by passage through whole mouse embryonic tissue (minced mice foeti removed from the uteri of pregnant mice and cultivated in a fluid medium). As hoped, the virulence diminished. But immune serum still needed to be added to act as a passive immunizing agent, before active immunization kicked in.[7] Nevertheless this was the basis of the second yellow fever vaccine, the so-called 17E vaccine prepared in whole-mouse embryonic tissue. The vaccine appeared to be safe and effective but the amount of immune serum required still rendered the vaccine impractical.[8]

Luckily, by this time a third vaccine, 17D, which required no immune serum, was at hand. The origins of this vaccine went back to 1933. Knowing that passage of the virus through mouse-brains enhanced its virulence, possibly the virulence

would decrease if the virus were cultivated in embryonic tissue from which the central nervous system had been removed. This indeed transpired. The virus, when passed through brain-less tissue cultures, became less and less virulent until, after 114 passages, none of the monkeys inoculated with the virus showed any signs of the disease.[9] Clearly such a virus could provide a vaccine in which immune serum would no longer be required. However, *in order to retain the viability of the virus itself, normal human serum had to be mixed with the virus when producing the vaccine.*[10] This was to prove an Achilles heel.

In September 1937, after successful laboratory and field trials, mass vaccinations began with 17D in the coffee faziendas of Minas Gerais, Brazil. That first year 59,532 workers were vaccinated; Soper, no longer so hostile to vaccines, confidently pronounced that "the efficient protection of populations exposed to jungle yellow fever is in sight."[11] Both Brazilian and American officials could announce that "there is now available a practicable, safe method of large-scale immunization."[12]

But in the rush to vaccinate, Kerr reported a "very wide occurrence of post-vaccination jaundice."[13] Similar reports had come from Africa in 1937 and 1938, also ascribing the problem to a jaundice-causing agent, either accidentally introduced into the tissue culture or into the vaccine by way of the serum.[14] John Fox, asked by Sawyer to prepare a report on the matter, felt that proof linking the jaundice cases to specific lots of the vaccine were "indisputable."[15] Sawyer decided to introduce a new strain of virus into the New York laboratory, and to heat the human serum to 56 degrees C, believing any jaundice-producing agent would be killed. "We should feel very much safer if we knew the nature of the agent or agents," he told Soper.[16]

## Vaccine Problems: World War II

In 1940, the Advisory Committee of Tropical Diseases of the National Research Council recommended that U.S. military personnel should be vaccinated against yellow fever. They asked the Health Division to supply the necessary doses. The impetus for this action came from Lt. Col. James Simmons who, in 1939, had been posted to the office of the Surgeon General to reorganize the preventive medicine service, which had little standing at that time. He was particularly worried about germ warfare and the possibility that the Germans and Japanese could release clouds of mosquitoes infected with the yellow fever virus. And Simmons's fears were justified; the Japanese had attempted, without success, to obtain the yellow fever virus from the Health Division for that very purpose. It was he who used the potential threat of biological warfare to persuade a reluctant general staff to sanction the vaccination program.[17] In response to this and to a similar request from the Public Health Service, Sawyer set up a temporary vaccine laboratory at the

Rockefeller Institute.[18] The War Department began its yellow fever vaccination program in January 1941. By April 1942, the Health Division had supplied a total of 4,643,320 doses of the 17D vaccine, in 141 numbered lots. Another 1,819,000 doses were supplied to the British government following a severe 1940 outbreak of yellow fever in the Anglo-Egyptian Sudan which left 1500 dead.[19]

On March 20, 1942, there arrived a shocking telegram from Monroe Eaton, who was in California on behalf of the Health Division to work on infectious or catarrhal hepatitis. The message read: "121 cases of mild jaundice in army personnel 8–10 weeks after vaccination with 17D [in the military camps at] Stockton, Salinas and Riverside. Lot 331 used in Stockton. Am testing blood by intracerebral inoculation of mice."[20] He was testing blood to make sure he was not dealing with a yellow fever outbreak. Apart from jaundice and bile in the urine, the patients complained of loss of appetite, lassitude, depression, irritability, inability to concentrate, drowsiness or insomnia, general weakness and sometimes a low-grade fever.[21]

On hearing the news, Sawyer and Johannes Bauer took the first plane out to San Francisco after recommending to Colonel Simmons that members of the recently-established Commission of Tropical Diseases should study the problem.[22] According to Karl Meyer, a member of the commission and director of the California-based George Williams Hooper Foundation for Medical Research, the British had made him aware of post-vaccination jaundice while in London. Bauer denied having any trouble with their own vaccine and suggested instead that the British vaccine had become contaminated. When called on to examine early jaundice cases in the Stockton camp, Meyer became suspicious and relayed his impressions to Simmons. Initially, Meyer relates, Simmons didn't believe him. The vaccine is "perfectly safe," Simmons is reported to have replied. "The Rockefellers made it."[23] Simmons had much to lose, of course, if indeed the jaundice was a result of the vaccination program which he had pressed on a reluctant general staff.

Sawyer had much to lose too. He went to California to protect the Rockefeller name and his own. Initially he took the position that the jaundice cases were a result of infectious hepatitis passed into the military camps by infected civilians. Two days after his arrival, Sawyer discovered a jaundiced secretary who had not been vaccinated. Clutching at straws, he told Strode that the case of the secretary made him increasingly confident that they were dealing with an epidemic of infectious hepatitis unrelated to the vaccine. "Gradually the evidence accumulates," he enthused.[24] But the cause of this hepatitis was unknown at the time, and there was no specific diagnostic test for it. Now Sawyer, Bauer, Meyer and others were left to compare the clinical and other characteristics of the jaundice outbreak with those of infectious hepatitis. Differences appeared, but not enough to rule out the possibility that both diseases had a common agent. However, they could find no evidence of unusual amounts of hepatitis in the civilian population in the vicinity

of the camps. Stanhope-Jones complained that Sawyer had not been very helpful. Sawyer, he wrote, "might tend, just naturally, as he did, to emphasize the importance of the element of naturally-occurring hepatitis in this situation."[25]

Meyer disliked Sawyer intensely, believing him to be too smooth and over-ambitious. Meyer found out about the post-vaccine jaundice in Brazil only after he had completed the California tour, and was infuriated that Sawyer had failed to mention it. To Meyer this represented another "dark page" in vaccine work. "I never will forget," he told an interviewer, "the way Sawyer and Bauer tried to suppress the thing."[26]

Initially, Simmons and Sawyer tried to convince themselves that the evidence did not incriminate the vaccine. But by April, Sawyer had to admit that the vaccines "had something to do with the jaundice." Bauer discontinued the vaccine's manufacture, suggesting that they produce a vaccine from which all human serum had been omitted.[27]

A few days later, the study group phoned in its preliminary reports to Simmons. 817 soldiers in California camps had developed jaundice, and most had been vaccinated with lots 331, 335 and 338, whereas lower-number lots, produced earlier, appeared safe. Cases of infectious jaundice had been reported among civilians living near some of the camps, but that alone could not explain the outbreaks. They recommended that vaccinations with lot numbers over 330 be suspended and that emergency cases be vaccinated with vaccines manufactured at the USPHS laboratory in Hamilton, Montana, where the serum used was not from donors in the New York area. Preliminary analysis, the study group concluded, strongly suggested that the jaundice-causing agent was a virus present in the serum of certain healthy blood donors.[28]

Participants at a conference at the Surgeon General's Office, held the day after the preliminary report had been phoned in, heard further strong views from Ken Maxcy of Johns Hopkins, who had been called upon to investigate jaundice outbreaks in the central regions of the country. "It was ascertained," Maxcy reported, "that on Sunday, December 28, about 6000 men [ in Jefferson barracks] had been vaccinated with lot No. 335 and about 2000 with lot No. 332. These two lots of vaccine were used more or less indiscriminately. In many instances part of the men in the same organization had received one, part the other. They ate, slept, and worked together and were exposed to the same environmental conditions, yet it was evident that the cases had occurred almost exclusively among those who had received Lot No. 335." To make matters worse, one Missouri-born soldier died and a postmortem revealed a case of "acute yellow atrophy" of the liver.[29] It was time to stop. Simmons had no choice but to concur, and order an end to vaccinations.

Production of the Rockefeller vaccine was halted until such time that the laboratory could produce a vaccine that no longer required the addition of normal human serum. There was work to be done if the Health Division was to recover

its scarred reputation. Meyer claimed that Sawyer was taken aback by Simmons's decision. He told an interviewer that when Sawyer received a telegram telling him the Health Division's vaccine had been cancelled, "the cat was out of the bag and [Sawyer's] facial expression I will never forget."[30] That claim is short on credibility: Sawyer was well aware by then that he had no choice.

Two days before Simmons's decision, Bauer had a lengthy interview with Simmons and Bayne-Jones; it was clear that it was Simmons, who was dragging his feet, not the Health Division. Bauer told them the Health Division had discontinued making the vaccine, although they had over one million doses on hand, and were trying to discover whether human serum could be omitted from the vaccine. Bayne-Jones was inclined to attribute the jaundice to the human serum in the vaccine, but Simmons was still not sure. It had taken him a year to convince the general staff to initiate vaccinations, and once cancelled, he felt, there would be no going back.[31]

Sawyer had retreated from his earlier position placing the blame on infectious hepatitis and now faced a more serious threat to his vaccine. At a conference in April with Simmons, Bayne-Jones and Surgeon General James Magee, the three military officers informed him that a modified, mutated, yellow-fever virus could not be ruled out as the cause of the jaundice. This was a worst-case scenario for Sawyer, for it implicated the vaccine itself, not simply an accidental contaminating agent. He protested that mouse-protection tests with the jaundiced blood both in the U.S. and Brazil had shown this to be untrue. Now, faced with this situation, he became a believer in a contaminated serum. In this he was assisted by the publication of John Fox's paper on post-vaccination jaundice in Brazil; it argued that the vaccine was contaminated with a virus which gained entrance through the human serum and which then persisted through tissue culture passages. In Brazil, 164,000 individuals had been successfully vaccinated with a new vaccine in which the supernatant of a chick embryo had been used in lieu of the human serum. This paper would not have been good news to Sawyer in March, but it certainly was in April.[32]

News of jaundice outbreaks continued to come in from military camps in the United States, all of which placed the blame on one vaccine lot or another. On April 18, the peripatetic Maxcy found that while 14% of the 305 cases at Fort Knox, Kentucky, home of the 3rd and 7th Armored Divisions, had been vaccinated with lot no. 335, 64% had received lot no. 338.[33] In May, a similar report from the Mojava antiaircraft range at Barlow, California, said all 464 cases had been vaccinated with lots 335 and 338. At West Point, all of the 209 reported cases had been vaccinated with lot 368, at a rate of 132 per thousand. These facts left no doubt, Maxcy concluded, "as to the association of the administration of yellow fever vaccine of lot 368 with the occurrence of these cases of jaundice. No other common factor could be discovered." At Camp Custer in Michigan, lot 368 was also implicated in most of the cases, while 97% of the ill patients in Camp Lola,

Louisiana, had been vaccinated with Lot 369. In October, a report from Fort Huachuca, New Mexico, a station for black troops of the 93rd Division, revealed that 360 of the 5860 vaccinated with lots 367 and 368 had become sick, a rate of 61 per 1000 vaccinated.[34] But, Capt. Joe Still, medical officer at Fort Belvoir, Virginia, faced considerable opposition when he supported the link between vaccination and jaundice. Lt Col. Harry Plotz, the commanding officer, disagreed and managed to suppress Still's report for three years.[35]

Eventually the news reached the press, forcing Secretary of War Henry Stimson to report at a July press conference that 28,585 cases of yellow jaundice had developed following vaccination, of whom 62 had died. *Time Magazine* called it a "Jaundice Rampage," while the *Minneapolis Morning Tribune* ran a longish article hinting perhaps the vaccine was not responsible. The *Chicago Daily Tribune* was more critical, saying the Surgeon General himself had originally opposed the vaccination program (a statement which the army strongly denied) and charged the army with "a grievous error of judgment." This, in turn, triggered the ire of the American Medical Association, which accused the *Tribune* of injuring morale among the troops and of doing "a disservice to American medicine."[36]

By then, the Health Division's statistician, Persis Putnam, had joined the investigation team. She was detailed to help organize a punch card analysis of a fact-finding questionnaire sent out from the HQ 9th Corps in Fort Douglas, Utah. Once again, answers to the questionnaire revealed that jaundice sufferers were found to have been disproportionately vaccinated with certain lots. By the end of February 1943, Putnam and her team had analyzed data from 26,770 cases of jaundice. Of the 98 lots used, 6 had been responsible for 86% of the jaundice cases, and three of them, lots 367,368 and 369, were responsible for a remarkable 57% of the cases (see Table 11.1).[37]

Support for the vaccine hypothesis came also from data which revealed the time of onset of the disease after vaccination. In the Californian outbreaks, the timing of the disease showed two peaks, one during the third week of March and the other nearly three months later. But when the time after vaccination was considered, all the cases fell into a sharp unimodal curve with the mode at about 13 weeks after vaccination (see Table 11.2).[38] The statisticians concluded that infectious hepatitis could not be ruled out completely, but the evidence "strongly suggests that certain vaccine lots contained an icterogenic factor which was responsible for the jaundice epidemics studied."[39]

With the link between jaundice and vaccination established, Col. Bayne-Jones and Maxcy began a complicated investigation of all blood donors in an attempt to ascertain the source of the contaminant. The serum had been taken from two sources: so-called professional blood-donors in New York City, and volunteer students, faculty and staff at Johns Hopkins. They managed to trace 93% of the approximately 1200 Johns Hopkins donors and about one-third (367) of the donors from New York used to produce the Californian vaccines, and found that

**TABLE 11.1.** Jaundice cases in the United States camps as analyzed by Persis Putnam and Capt. Douglas Walker, February 1943.

| LOT NUMBER | NUMBER OF CASES | PERCENTAGE OF CASES |
|---|---|---|
| 331 | 1750 | 6.5 |
| 335 | 3861 | 14.4 |
| 338 | 2235 | 8.3 |
| 367 | 4643 | 17.3 |
| 368 | 5360 | 20 |
| 369 | 5288 | 19.8 |

only a few of them had a history of jaundice. Furthermore, while all highly icterogenic lots of the vaccine contained the serum of at least one donor with a history of jaundice, 18 of the moderately icterogenic vaccines were prepared with serum where there was no such history. But if the human serum from these donors was the source of the agent then there must have been an extremely high concentration of it in the donor's blood. Evidence suggested, for example, that only one donor passed the icterogenic agent to three highly icterogenic lots, 367, 368 and 369. This implied that 250 mils of this donor's blood contained sufficient amount of agent to be infective to about 240,000 doses! For that reason alone, Sawyer and Bauer remained skeptical of the role played by this human serum in the vaccine scare, although even they had to admit that the finger of guilt pointed to no other possibility.[40] But a study by Arthur Kornberg at the University of Rochester Medical School showed that the contaminants were probably far more common than anyone had believed. He told Bauer that 6% of the students there had sub-clinical chronic jaundice.[41]

The evidence indicated that there was nothing fundamentally wrong with the Rockefeller vaccine; it was an accident for which they could hardly be blamed. At the same time, Bauer reported that the two-year search for a vaccine without human serum was bearing fruit. A method had been found to store the chicken

**TABLE 11.2.** Number of days between vaccination and onset of jaundice, through July 1942.

| LOT NUMBER | PEAK OF JAUNDICE OUTBREAKS-DAYS AFTER VACCINATION |
|---|---|
| 331 | 81 |
| 335 | 87 |
| 338 | 97 |
| 367 | 83 |
| 368 | 92 |
| All lots | 87 |

embryo pulp which had replaced the human serum.[42] In June 1942 vaccine without human serum was accepted and produced by the International Health Division's laboratory and the USPHS laboratory in Montana. In addition the number of doses per vaccine lot was reduced to limit the damage should a jaundice outbreak recur. But it never again recurred.

Approximately 50,000 U.S. troops were estimated to have come down with vaccination-induced jaundice from the nearly 8 million doses given, of whom 81 died.[43] Given that no case of yellow fever was reported among the U.S. military during World War II, this appears to have been an acceptable price, although few U.S. troops served in areas endemic for yellow fever. But the outbreak was frightening at the time. As Bayne-Jones has noted, "it was more shocking to the commander of the Army, I think, than any bombardment. At the moment that was going on so many people were sick, and there were so many critical things in the offing and going forward. For example the Battle of Midway was won by air pilots some of whom were suffering from jaundice, and great secrecy was imposed."[44]

There were aftershocks; the problem did not simply disappear at the end of 1942. The story might have been a major factor in the deliberations of the Nobel Prize Committee, which in 1951 decided to award the prize to Max Theiler for his work on yellow fever. Greer Williams has written about this episode, saying Sawyer felt the prize should have been his, and that its award to another was a denial of his whole career. His wife noted, that "the award of the Nobel Prize to Max Theiler killed my husband."[45]

A far greater aftershock was waiting to happen. At the time of his death in 1951 Sawyer did not know that serum hepatitis, or hepatitis B (HBV), is caused by an extremely stable virus that has the potential to be far more serious than the 81 deaths and 50,000 jaundice cases would indicate. A study of 22,000 Taiwanese, beginning in 1974, showed that liver cancer appeared in 1,952 of 3,454 hepatitis B carriers, compared with only 9 from 19,253 non-carriers.[46] Since 330,000 troops had been inoculated with the contaminated vaccine lots there seemed a strong possibility that a large number of troops would have died of liver cancer 20 to 30 years after the war ended. That this was a possibility became clear after a follow-up study in the 1980s which showed that vaccinated troops indeed carried the hepatitis B antibodies—the proof that they had been infected with the hepatitis B virus.[47]

How many had died of liver cancer? Fortunately nowhere near the number that would have been suspected from the Taiwanese study. A 1993 study showed that those who had been injected with the contaminated lots but remained well had a "standardized mortality ratio" of 1.17 (in other words 17% in excess of expected numbers) and 35% in excess of those who had been hospitalized or had received the serum-free vaccine.[48] (This was to be expected as it was known that chronic hepatitis, which leads to liver cancer, has a greater frequency after symptom-free virus infection than after actual jaundice). But the number who

died of liver cancer was amazingly low. Twenty-six of the 1,056 total cancer deaths in those vaccinated but with no illness, against 22 of 1,498 and 17 of 1,333 in those hospitalized and those who had received the serum-free vaccine respectively. The study showed too that the number of those carrying the virus antigen was well below the expected carrier rate; 0.3% compared with the anticpated rate of between 5% and 10%. Thus, the authors concluded, "healthy adults seldom become carriers after acute HBV infection," and "in contrast with young children and immunocompromised adults, healthy adults who succumb to and recover from acute HBV infection have at the most only very slightly increased risk for subsequent chronic liver disease and [`cancer]."

The 1942 vaccine scare was certainly a slap in the face for the Health Division, but it could have been far, far, worse.

## Notes

1. G. K. Strode (ed), *Yellow Fever* (New York: McGraw Hill, 1951) is still the best general account of the disease from that period. It includes A. Warren, "Landmarks in the conquest of yellow fever"; M. Theiler "The virus"; and K. Smithburn, "Immunology."
2. T. Norton. "Recollections of the Yellow Fever Laboratory," October, 1946. RAC. RG.1. S.100 B.11 f.92. Thomas Norton was engaged as a technician shortly after the lab opened and remained there for the rest of his working life.
3. M. Theiler, "Studies on the action of yellow fever virus in mice." *Annals Trop. Med. Hyg.* 24 (1930): 249–272.
4. Ibid., p. 263.
5. W. Sawyer, "Program for the active immunization of members of the staff of the yellow fever laboratory." 25 March, 1931. RAC. RG.210.3 (Business Manager/Subject Files) IHD 1927–34. Sawyer, Kitchen and Lloyd, "Vaccination of humans against yellow fever with immune serum and virus fixed for man." *Proc. Soc. Expt. Biol. Med.* 29 (1931): 62–64; *J. Expt Med* 55 (1932): 945–969.
6. M. Theiler, "Neutralization tests with immune yellow fever sera and a strain of yellow fever virus adapted to mice." *Ann Trop. Med. Parasitol.* 25 (1931): 69–77; "A yellow fever protection test in mice by intracerebral injection." Ibid., 27 (1933): 57–77.
7. W. Lloyd, M. Theiler and N. Ricci, "Modification of the virulence of yellow fever virus by cultivation in tissues in vitro." *Trans. Roy. Soc. Trop. Med & Hyg.* 29 (1936): 481–529.
8. Soper & H. Smith, "Yellow fever vaccination with cultured virus and immune and hyperimmune serum." *Amer. J. Trop. Med.* 18 (1938): 111–134.
9. Theiler & H. Smith, "The effect of prolonged cultivation in vitro on the pathogenicity of yellow fever virus." *J. Expt. Med* 65 (1937): 767–86; "The use of yellow fever virus modified by the in vitro cultivation for human immunization." *J. Expt. Med* 65 (1937): 787–800.
10. For details see M. Theiler, "The virus," in G. Strode (ed.), *Yellow Fever.*
11. Soper to Fosdick, January 17, 1938. RAC. RG.1.1 S.305 B.23 f.184.
12. H. Smith, H. Penna & A. Paoliello, "Yellow fever vaccination with cultured virus (17D) without immune serum." *Amer. J. Trop. Med* 18 (1938): 437–68.

13. Kerr to Sawyer, May 17, 1940. RAC. RG.1 S. 100 B.17 f.146; depending on the site, up to 32% of those vaccinated became jaundiced, with 32 deaths.
14. G. M. Findlay & MacCallum, F. "Note on acute hepatitis and yellow fever immunization." *Trans. Roy. Soc. Trop. Med. Hyg.* 31 (1937): 297–308; "Hepatitis and jaundice associated with immunization against certain virus diseases." *Proc. Roy. Soc, Med.* 31 (1938): 799–806.
15. J. Fox. "About laboratory investigations into the etiology of icterus," November 5, 1941; RAC. RG.1 S.100 B.16 f.129. Fox et al., "Observations on the occurrence of icterus in Brazil following vaccination against yellow fever." *Amer. J. Hyg.* 36 (1942): 68–116.
16. Sawyer to Soper, December 6, 1941. Ibid.
17. Oral History Interview of Stanhope Bayne-Jones by Harlan Phillips, 1966. National Library of Medicine, Bethesda. The role of germ warfare is discussed in A. Cowdrey, *War and Healing. Stanhope Bayne-Jones and the Maturing of American Medicine* (Baton Rouge: Louisiana State University Press, 1992). P. Williams & D, Wallace, *Unit 731. Japan's Secret Biological Warfare in World War II* (New York: Macmillan, 1989), Chap. 8.
18. Sawyer to H. Gasser [Director of the R.I.] July 19, 1940. RAC. RG.210.3 IHD files, 1935–42.
19. R. Kirk, "An epidemic of yellow fever in the Nuba mountains, Anglo-Egyptian Sudan." *Ann. Trop. Med. Parasitol.* 35 (1941): 67–112. This epidemic is discussed in Health Bell. *Frontiers of medicine in the Anglo-Egyptian Sudan, 1899–1940* (New York: Oxford University Press, 1999).
20. Sawyer's Diary, March 20, 1942.
21. The post-vaccination jaundice problem has been described in detail by W. Sawyer, K. Meyer, M. Eaton et al, "Jaundice in the army personnel in the western region of the United States and its relation to vaccination against yellow fever," Part I. *Amer. J. Hyg.* 39 (1944): 337–432; Parts II, III and IV. *Amer J. Hyg.* 40 (1944): 35–107. It is also discussed in A. Long, "The Army Immunization Program," in *Preventive Medicine in World War II.* Vol. 3, *Personal Health Measures and Immunization;* J. Paul & H. Gardner, Chap. 17 "Viral Hepatitis," in Ibid Vol. V *Communicable Diseases.;* S. Bayne-Jones "Yellow Fever," in Ibid., Vol. 7. *Communicable Diseases*; W. Havens, "Viral Hepatitis," in *Internal Medicine in World War II.* Vol. III. *Infectious Diseases and General Medicine*; J.A. Rogers, "The outbreak of jaundice in the army." *Military Surgeon,* October, 1942; D. Walker, "Some epidemiological aspects of infectious hepatitis in the U.S. Army." *Amer. J. Trop. Med.* 25 (1945): 75–82.
22. In January 1941, Simmons had established a "Board for the Investigation and Control of Influenza and Other Epidemic Diseases" (later the Army Epidemiological Board), which could call upon investigative teams responsible for "fire-fighting" epidemics. The board included a Commission of Tropical Diseases under the direction of Sawyer, and a Commission of Epidemiological Survey under Stanhope Bayne-Jones. Members of these commissions were called on to investigate the jaundice outbreaks: Sawyer and Karl Meyer in California, Kenneth Maxcy in the central regions and Francis Blake of Yale in the east. Stanhope Bayne-Jones Papers; Oral History Interview of Stanhope Bayne-Jones. For a history of the Epidemiological Board see T. Woodward, *The Armed Forces Epidemiological Board: Its First Fifty Years* (Washington: Office of the Surgeon General, 1990).
23. K. F. Meyer. Medical research and public health; transcript of interviews by E. T. Daniel in 1961 and 1962. Bethesda, National Library of Medicine. OH 68 2935154

R. J. Steele, "Karl Friedrich Meyer, Biographical Notes." *J. Infectious Dis.* 129 Supplement. 1974.

24. Sawyer to Strode, March 23 and March 25, 1942. RAC. RG.1 S.100 B.16 f.130.
25. Oral History Interview of Stanhope Bayne-Jones, p. 690.
26. K. F. Meyer. Medical research and public health.
27. Sawyer to Strode April 9 and 11, 1942; Bauer to Sawyer, April 11, 1942. RAC. RG.1 S.100 B.16 f.131.
28. Study Group investigating Jaundice in the Army. Summary of Preliminary Findings and Recommendations. April 13, 1942; Sawyer, "The Activities of the Commission on Tropical Diseases," April 29, 1942. Ibid.
29. Hepatitis 1942. Jefferson Barracks, SGO Conference, April 14, 1942; Maxcy, Final Report, Jefferson Barracks, Scott Field, Chanute Field, Surgeon General's Office, November 19, 1942. *Hepatitis 1942, Yellow Fever Vaccine Outbreaks at Post and Camps Continental US.* National Archives, Washington. 390/16/27/3 B. 1123. I would like to thank the archivists at the National Archives for digging out these extraordinary files and making them available to me.
30. K. F. Meyer. Medical research and public health.
31. J. Bauer to Sawyer, April 11, 1942. RAC. RG.1 S.100 B.16 f.131.
32. Conference Magee, Simmons, Bayne-Jones & Sawyer, April 23, 1942. Ibid. J. Fox, et al, " icterus in Brazil."
33. Hepatitis 1942. Fort Knox. *Hepatitis 1942, Yellow Fever Vaccine Outbreaks.*
34. K. Maxcy, "Preliminary Report of an Investigation of Jaundice among Troops of the United States Army Stationed at West Point, New York, June 22nd to 25th, 1942." Capt J. Sadierk, Camp Polu, Lo. June 26, 1942; Lt M. Romansky, Fort Huachuca, October 10, 1942. *Hepatitis 1942, Yellow Fever Vaccine Outbreaks.*
35. Hepatitis Outbreak, Fort Belvoir. *Hepatitis 1942, Yellow Fever Vaccine Outbreaks.*
36. *Minneapolis Morning Tribune*, July 21, 1942; *Chicago Daily Tribune*, July 25, 1942; Editorial, *J.A.M.A.* August 1, 1942; *Time Magazine*, August 3, 1942.
37. HD 710 (Virus Diseases) Jaundice Z1, *Summary Reports. Yellow Fever Vaccinations, Ninth Corps Area.* National Archives, Washington. 390/16/27/3. B.1123.
38. "Comparison of Mean Days of Jaundice Occurrence and of Mean Intervals since Vaccination by Vaccine Lot." HD 710. Jaundice Z1, *Summary Reports, Ninth Corps Area.*
39. Eaton & Putnam, "Report on epidemics of jaundice among military personnel in the 9th Corps area," June 1942. RAC. RG.1 S.100 B.16 f.133.
40. Maxcy to Sawyer, May 4, 1942. RAC. RG.1 S.100 B.16 f.132.
41. Discussed in Bauer to Hargett, May 21, 1942. Ibid.
42. Ibid.
43. Stanhope Bayne Jones was correct when he noted that the number of cases of hepatitis will never be known. Oral History Interview of Stanhope Bayne-Jones. Sawyer reports 26,771 cases of army personnel in the western region of the United States, plus 709 cases in the navy, but admits these figures are very conservative. They certainly were! W. Sawyer et al, "Jaundice in the army personnel." Parts II, III and IV. The official medical history of World War II notes that 49,233 cases of hepatitis were admitted to army hospitals in 1942, most of which were caused by the yellow fever vaccine; 33,569 cases in the United States, 15,664 from overseas theatres. W. Havens, "Viral Hepatitis," in *Internal Medicine in World War II.* Vol. III. *Infectious Diseases and General Medicine.* There must also have been thousands of near symptomless cases which may or may not have been reported.

44. Oral History Interview of Stanhope Bayne-Jones, p. 692.
45. Greer Williams, *The Plague Killers* (New York: Scribner, 1969).
46. Beasley et. al., Hepatocellular Carcinoma and Hepatitis B Virus. *Lancet* 2 (1981): 1129–1133; W. Szmuness, Hepatocellular Carcinoma and the Hepatitis B Virus: Evidence for a Causal Association. *Progress in Medical Virology* 24 (1978): 40–63. Vyas. G., Dienstag, J. L. et al., *Viral Hepatitis and Liver Disease* (Orlando: Grune & Stratton, 1984).
47. L. Seef et al, "A serological follow-up of the 1942 epidemic of post-vaccination hepatitis in the United States Army," *New England J. Medicine* 316 (1987): 965–70. Ninety-seven percent of troops who had been hospitalised with jaundice carried the antibodies, compared with 76% of those who had been vaccinated with the contaminated lots but remained well and thirteen percent who had been vaccinated with the serum-free vaccine.
48. J. E. Norman et al. "Mortality follow-up of the 1942 epidemic of hepatitis B in the U.S. Army." *Hepatology* 18 (1993): 790–97.

# 12

# Diseases for Research

Fred Russell's decision to expand the research effort of the Health Division, was reflected not only in the activities of the New York and Tallahassee laboratories but in the decision to investigate diseases other than hookworm, yellow fever, and malaria. From 1927 on, the amount of money spent on these other diseases increased steadily, so that by the early 1940s they received between 30% and 35% of the disease budget (see Table 1.3).

The diseases included the flu and common cold, rabies, syphilis and yaws, as well as tuberculosis. All shared a need for fundamental research, often targeted towards vaccine production, and in every case the laboratory proved to be pivotal. The choice of disease was often generated by a desire to support the research activities of a well-known expert whose qualities were admired by Russell. Indeed, the Health Division at times resembled an academic-granting agency.

## Respiratory Diseases

Influenza and the common cold received the most funding and best fit Russell's model of diseases to be investigated. Because the causal agents were unknown at the time, both demanded research at the most basic level. In addition, they offered scope for vaccine research. In anticipation of this, the Health Division funded the research activities of Dr. Alphonse Dochez of Columbia University, formerly

a staff member of the Rockefeller Institute who, with Biggs, had written the report which led the Health Board into France.

In 1927 Russell and Simon Flexner set up a joint Health Division-Rockefeller Institute project (indicating how close they had grown) to study acute respiratory diseases in isolated communities. From a New York perspective, that setting seemed "quiet, simple and placid," one where environmental factors could be understood better than in complex urban settings.[1] The first research team began work in Alabama, studying the children of a one-room rural school in the small community of Happy Hollow. A year later, another team was dispatched to the trapper and First Nations community of Northwest River, Labrador, built around a trading post of the Hudson Bay Company, situated a few kilometers from what is today the NATO air base of Goose Bay. In 1929, a third team spent a year on the tropical Virgin Islands, and finally, in 1930, when the study had been taken over completely by the Health Division, yet another group spent the winter in the isolated coalmining community of Longyear City, Spitsbergen in the Barent Sea north of Norway.[2]

Believing at first that both influenza and the common cold were caused by bacteria, each research team cultured bacteria from nasopharyngeal swabs before, during, and after cold and flu epidemics. But none of the bacterial inhabitants of a normal healthy nasopharynx seemed related to the cold or flu, including the long-suspected *Bacterium influenza*. "It would be quite consistent with our observations," the Spitsbergen team noted in the last report of the group, "to assume that the epidemic of "colds" . . . was due to a filterable virus of the type described by Dochez and Long."[3]

Throughout the 1920s Dochez had been working on the common cold, and he too had concluded that nasopharyngeal bacteria did not change with the arrival of colds; he decided to focus on filterable viruses. After experiments on chimpanzees and human volunteers, Dochez concluded that "the infecting agent belonged to a type that commonly goes under the name submicroscopic virus."[4] Dochez was able to culture the virus in chick embryo tissue and, in the 1930s, began to investigate the possibility of producing vaccines.[5]

In 1931 Dochez received a three-year grant from the Medical Science Division, and by the end of that period, felt his work was about finished. He had shown the causative agents to be filterable viruses, easily cultured *in vitro*, and he had protected chimpanzees from the common cold by injections of the cultured living virus. All that remained to be done, he believed, was to perfect these immunizing techniques and apply them to humans.[6]

Dochez's optimism faded when he discovered that both diseases were associated with a multiplicity of viral strains, which would severely complicate the production of vaccines. Far from being near the conclusion of his work, he realized it had barely begun.[7] The Health Division was only too willing to take on the influenza-common cold problem. It not only took on the financial support for

Dochez's work but transferred Thomas Magill and Thomas Francis from the Rockefeller Institute staff to their own, in order to work on the problem in the New York laboratory.

Over the next few years their vaccine research was frustrated by the isolation of new strains of flu and cold viruses, and by the discovery that immunity against one strain did not confer immunity against others. Work was hampered also by the inability to discover a suitable small mammal susceptible to the cold and flu viruses; Dochez was forced to admit, in his 1940 report, that there was "no immediate prospect of making further advances towards the solution."[8] Nevertheless the Health Division continued its support for Dochez's work until 1945.

## Tuberculosis, Again

Perhaps the most astonishing example of Russell's new enthusiasm for research arose in 1927 when he agreed to finance a tuberculosis survey in Jamaica, the beginning of a 17-year commitment to the disease on the island. Although it was never financed to anything like the degree of the French tuberculosis campaign, it was nevertheless a strong commitment and a very surprising one. After France, neither Rose nor Russell seemed at all keen on engaging the Health Board in another campaign against this chronic social disease. Why then did Russell reverse that decision and agree to take on tuberculosis for the second time?

One answer comes from the change that had taken place in the nature of tuberculosis itself. By the late 1920s, tuberculosis had come to be seen as a disease like most others, in line with those usually targeted by the Health Division, rather than primarily a social disease. Tuberculous patients with mild cases of the disease could now be admitted into sanatoria where, at least according to the medical experts, they could be cured by medical intervention.[9] In the words of writer Katherine McCuaig, "with the growing use of surgery and x-rays, sanatoria were slowly evolving into hospital-like institutions rather than summer camps and primitive rest homes."[10] At the time talk of a tuberculosis vaccine was in vogue; the medical world had become embroiled in debates over the safety and efficacy of prophylactic vaccines against the disease. In 1924, for example, Albert Calmette and his assistant Camille Guérin, produced a new strain of an avirulent bovine tubercle bacillus by repeated subculturing, and used it to prepare a vaccine. While the BCG vaccine was readily accepted and used in France, Québec and other French-speaking areas, physicians in English-speaking Canada, Germany and the United States, remained skeptical.[11] But tuberculosis had now moved within the compass of modern scientific medicine.

As with the other new diseases, however, the research angle appealed to Russell and, once again, the figure of a respected researcher lay behind the decision to take on tuberculosis. Eugene Opie, Director of the Henry Phipps Institute at the University of Pennsylvania, was much admired by Russell, who seemed to have

developed a blind faith in Opie's abilities. Indeed, some of Russell's critics in the organization felt he had lost his critical judgment in respect to Opie; who, Hackett once remarked, was a flatterer with a persuasive tongue. Opie would have had all the right credentials in Russell's eyes. A medical graduate of Johns Hopkins and a member of its pathology department from 1898 to 1904, he had accepted an appointment at the Rockefeller Institute, where he began his studies on tuberculosis. He remained there until 1910 when he accepted a position as professor of pathology in St Louis. Finally, in 1923 he became director of the Henry Phipps Institute for the study, treatment and prevention of tuberculosis. The Institute opened in 1903 with an endowment from Henry Phipps, a Pittsburgh industrialist and a partner of Andrew Carnegie.[12] A few years later, the Institute ran into problems and in 1910 was transferred to the University of Pennsylvania.

The relationship between Opie and the Health Division began in Jamaica. The Health Board first become aware of the seriousness of tuberculosis on the island when Ben Washburn, in charge of the Jamaica program, said in his 1926 report that tuberculosis killed more people on the island than any other single disease.[13] Urged by Howard, director of the Caribbean program, to support a tuberculosis survey, Russell turned to Opie who in February 1928 arrived in Jamaica to investigate the prevalence of tuberculosis there and to begin his long association with the Health Division.

He remained on the island for only two weeks. In that time he determined that the existing tuberculosis statistics for both prevalence and mortality were of questionable accuracy, and recommended as a first priority that an information-gathering and record-keeping dispensary be set up in Kingston, as had been done in Philadelphia through the Phipps Institute. It would be a center for research into the epidemiology of tuberculosis.

> It would be the purpose of this clinic to act as a diagnostic center; to follow the progress of cases; to examine persons in contact with tuberculosis and to determine how the disease spreads in families and in other groups where healthy persons come into contact with those who have tuberculosis. It would stimulate the interest of physicians in tuberculosis and would serve as a center for instruction.[14]

By 1928 the Phipps Institute was running an efficient dispensary of this kind in Philadelphia, with elaborate record-keeping devoted to discovering as much as possible about the cause of the disease. After a patient had been diagnosed as tuberculous, by means of a physical examination, tuberculin test, sputum examination and X-ray, similar tests were made on all members of his family. A "family graph" was then compiled to show, visually, the effect of contact with the disease on each member of the family.

Figure 12.1 shows a family graph for a Syrian family in Philadelphia, which Opie sent to Russell to illustrate the work of the dispensary. The father had died

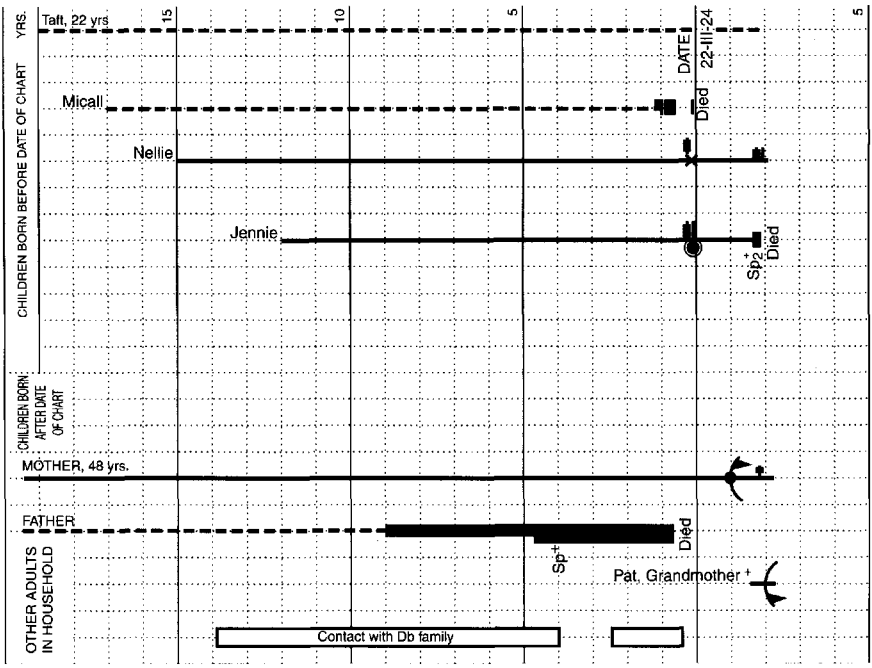


**FIGURE 12.1.** A family TB graph (courtesy of the Rockefeller Archive Center).

of tuberculosis about six months before the rest of the family was placed under prophylactic care by the dispensary on March 22, 1924. On that date, his 12 year-old daughter Jennie was diagnosed with a focal tuberculous lesion of the lung (heavy dot to the left of her "life-line"), and a positive tuberculin test (horizontal bars to the right of the line). Approximately 20 months later (shown below the March 22 base-line) Jennie died, showing manifest tuberculosis (a thick column to the right of her lifeline similar to the column of her father who had had the disease for over eight years), and tubercle bacilli in her sputum (a thick column to the left of the lifeline marked with Sp+, again similar to her father who had tested sputum-positive for over three-and-one-half years). An older sister, Nellie, had been lucky. She received an X-ray on March 22, (a cross on her lifeline), tested tuberculin positive on two occasions, but remained free of the disease. Neither of her two brothers had been living in the household for many years (a broken life-line), although the youngest of them appears to have died of tuberculosis just before the baseline date. Neither her 48-year-old mother nor her grandmother, who had come to live with the fatherless family, showed signs of active tuberculosis, although both tested tuberculin positive and showed lung lesions of various kinds (shown by arcs and triangles). In addition, the graph showed that the family had had contact with another family (Db) for most of thirteen and a half years.[15] Graphs

of this nature containing similar and even more information were prepared for tuberculous families at the dispensary and were, Opie warned, of little value unless kept up-to-date.

Russell had become aware of how much information Opie had garnered about the epidemiology of the disease, and how much more needed to be done after the 1928 publication of "The Contagion of Tuberculosis."[16] Different from the usual tuberculosis dispensaries, Opie's units appealed to Russell. The Health Division agreed to fund what they called a "study-clinic" in Kingston, modeled after the dispensary in Philadelphia. It opened its doors in July 1928. As Howard told Washburn, Russell did not wish the Health Division to become involved in another campaign to control tuberculosis, but rather that "we emphasize, and be consistent in, supporting the study and investigational activities of the clinic."[17]

Opie was particularly interested in tuberculosis in individuals who had migrated to the cities and had not been exposed to the disease as children, attributes common to black Southerners and rural Jamaicans. Both groups acquired an acute and fatal form of tuberculosis normally seen only in infants. After one year of study Opie found that 78% of adult Jamaicans who had migrated to Kingston died within a year of acquiring the disease! Most were tenement drifters, living with two or three others in small dark rooms surrounding a tenement yard with a communal open-fire kitchen, tailor-made for tuberculosis transmission.[18] By the time this type of tuberculosis had been diagnosed, he wrote, "the fatal end is usually in sight."

Storm clouds of inevitable tension between Opie's study clinic and the more control-oriented approach began to appear early in 1929. The Jamaican government, Washburn, and other field workers wanted to include the study clinic within the county health unit program, directed towards the treatment and control of diseases and the spread of knowledge about hygiene.[19] Opie denied his research would interfere with control procedures, and hoped that the data-collecting and research effort of his clinic would continue.[20] He wished the study clinic to be allowed to provide exactly the same information as that collected by the Phipps Institute, and generate the same type of family graph. The two sides compromised. The study clinic continued to expand family-graph data, while, the health units carried out intensive surveys with a view to future control measures. But research into the disease's epidemiology remained the priority of the Health Division. The important thing, Opie told Russell, was "maintaining proper records."[21]

The Health Division now took another giant step in its involvement in tuberculosis. Late in 1929, Rufus Cole, one of the Health Division's scientific directors, suggested to Russell that Opie be appointed to a staff position so that the Health Division could finance his research at the Phipps Institute. The division, Cole told Russell, should stress its interest in research, and promise Opie that he would not be pressured to mount a preventive campaign in Jamaica—which should remain incidental to the research activity.[22] Opie was gratified and forwarded a budget to

Russell for the enlargement of his department at the Phipps Institute.[23] In the spring of 1930, Opie was appointed associate director of the Health Division which agreed to finance his "scientific work" on tuberculosis, and to expand facilities at the Phipps Institute for a period of three years. In many ways the Phipps Institute became the "home station" for tuberculosis, similar to the yellow-fever laboratory at the Rockefeller Institute. There was opposition in the Health Division to this arrangement; Winslow, for example, argued that the organization should not fund research, but rather aid health departments in their campaigns against tuberculosis.[24]

In 1932, the ever-ambitious Opie moved to the pathology department of Cornell University Medical School, where he began to replicate his earlier work in Philadelphia—a study of the epidemiology of tuberculosis in a portion of Manhattan, rural Tennessee and Alabama. The Health Division continued to fund Opie's work—by the time it withdrew its support, 57% of its tuberculosis budget had been allocated to Opie's Cornell work, almost twice as much as had been allocated to Jamaica.

The most exciting news to have come out of Opie's early work at the Phipps Institute was the report that he had begun to develop on a new heat-killed tubercule bacillus vaccine. This had the potential to be safer than the attenuated BCG vaccine, which, being manufactured from living bacilli, remained suspect to American eyes. This discovery probably played a major role in the decision to continue funding Opie even after he left the Phipps Institute. The Health Division was only too conscious of the controversy surrounding the BCG vaccine and the rewards that would come from the production of a genuinely safe vaccine.[25]

In the early 1930s, Opie began testing the new vaccine on laboratory animals. The injection of bovine tubercle bacilli, the most cosmopolitan of the tuberculosis bacilli, into rabbits produced a progressive and fatal disease, but after injection with the heat-killed vaccine survival time increased. Opie concluded that immunizing people with the new vaccine might "influence favorably the course of infection."[26] Human testing began in 1932 at the mental hospital in Kingston, whose patients, being black, once again provided convenient guinea pigs for testing. From some very suspect data, Opie claimed, in his report to the Health Division, that the vaccine conferred some protection for an 18-month period.[27] But in the published report, he came to a more sober conclusion and admitted that the experimental method had been flawed.[28] Despite these problems, Opie provided a vaccine to all Health Division workers in Jamaica, and recommended that a large-scale test vaccination of children and young adults begin in Kingston. By the end of 1940, 7739 tuberculin negative children had been vaccinated, but no attempt was ever made to ascertain its impact. When Sawyer succeeded Russell as director in 1935, Opie's close relationship with the division ended. Opie retired from Cornell in 1941 and Sawyer closed down the Cornell and Jamaica programs. Opie, apparently, could not sweet-talk Sawyer.

## Syphilis

A similar series of events unfolded when the Health Division supported the research of Dr. Thomas B. Turner, a leading syphilologist from Johns Hopkins University, for 17 years. As with tuberculosis, the association began in Jamaica where the government requested help to control yaws, a highly contagious disease of hot humid climates, which, similar to syphilis, is caused by a treponema spirochete.[29] For advice Russell turned to Turner, who convinced Russell that research on yaws was badly needed.[30] As a result, Turner arrived in Jamaica in January 1932 as part of the Yaws Commission. A comprehensive study of yaws, Russell subsequently wrote, would advance public health more than the forthcoming malaria campaign in Panama.[31] It was a short-lived campaign, marked by tension between Turner and the field workers. In 1937 the Health Division closed down the Yaws Commission believing they had shown how a practical method of yaws control could be carried out.[32]

But the withdrawal seems to have been a result of Turner's desire to return to the United States. He wished to return to syphilis research, hinting that the Health Division might be interested in supporting this research.[33] He must have been aware that Thomas Parran, recently appointed Surgeon General of the U.S. Public Health Service, had begun a campaign to bring venereal diseases to national attention. For too long, Parran argued, venereal diseases had been swept under the carpet. Serious efforts needed to be directed against them; campaigns that were to be, above all, scientific.[34] Syphilis, Parran argued, could best be controlled by testing and treating rather than by calls for moral and behavioral reform. But treatment had become an extremely difficult and time-consuming procedure, what researcher Elizabeth Fee calls " a punishment for sin."[35] Only 25% of those admitted to clinics for treatment actually remained to complete the minimum 20 injections of arsenical drugs over a 70-week period. With little real hope of keeping tabs of patients under treatment, a shorter, more intensive—and more hazardous—treatment was adopted.

After prolonged negotiations, the Health Division found itself supporting two syphilis programs in Baltimore: A public health project in the Eastern Health District, a mixed racial neighborhood close to the Johns Hopkins School of Hygiene where approximately 30% of the 35–39 age group were reported to be syphilitic,[36] and Turner's own "straight research" into immunity to syphilis. The Health Division not only funded Turner's research but agreed to expand his laboratory into a "real center for research on spirochetal diseases." It would also be transferred from the medical school to the School of Hygiene.[37] The division allocated to Turner's laboratory three times the amount assigned the Eastern Health District, and accounts of this laboratory work thereafter took priority in Turner's annual reports to the division. Sawyer supported this emphasis. Turner ought to concentrate on research, his strongest asset, Sawyer remarked to a Dr. Means at

the Massachusetts General Hospital.[38] One must wonder again why a project more appropriate to the Division of Medical Science or the Rockefeller Institute should have been taken up by a division concerned with public health.

Turner was not entirely divorced from the problem of syphilis control, and he became concerned with the problems of evaluation—the old nemesis of the Health Board. How was the success or failure of a control program to be measured? He told Ferrell, how to measure the amount of syphilis in a community remained a basic problem which, until solved, prevented the evaluation of any control program.[39] As a result Turner made clear what the priority of the Baltimore program would be. "Undoubtedly," he wrote, "syphilis control programs will be greatly expanded in the immediate future and it is exceedingly important that the results achieved by these programs be measured in some tangible fashion." Initially the project was designed as a data-gathering project to study the "incidence, prevalence and trend of syphilis in the Eastern Health District," to find how best to determine syphilis trends in an urban community.[40]

The frequency of a disease can be measured in two ways: counting the number of persons in a group who are infected, or counting the persons in a group who had become infected in a given time period. The former measurement, Turner explained, is called *prevalence*, while the latter, the rate at which persons become infected, should be called *incidence*.[41] Turner had first latched on to this problem during his time with the Yaws Commission. His final report on yaws had come after his discovery that what he called the "attack rate," (and later called "incidence") had declined drastically in the second, third and fourth years of the research unit's operation.[42] Until then the Health Division had used the static concept of prevalence to ascertain a campaign's success. But this measurement was not sensitive enough to indicate whether the transmission cycle had been broken—only the measurement of incidence could do that.[43]

Determination of incidence was never easy. For the next four years, until joining the U.S. Army Medical Corps as director of the army's venereal disease unit, Turner bombarded the Health Division with data collected at the clinic. This required the maintenance of a highly organized and up-to-date filing system in which all recorded cases of syphilis in the community would be listed. Using the syphilis register in Baltimore for the years 1932 to 1938, statisticians in the Department of Biostatistics at Johns Hopkins School of Hygiene, set about measuring the incidence of the disease and discovered that in general 18 times as many blacks contacted syphilis in any year than whites.[44]

These figures came as no surprise, syphilis having been redefined by then as a black disease. Of course, as Fee says, such figures had to be treated with suspicion. Whites were much more likely than blacks to seek help from private physicians who were hardly likely to report these cases to the health department.[45] There was another reason for the higher figure among blacks. In 1944 one of the statisticians involved in the study noticed a statistical bias in the samplings. In 1939,

for example, 43.5% of the black population between the ages of 15 and 24 had been tested, as compared to only 6% of whites.[46]

Turner's measurement program was certainly in line with the public health concern of the Health Division. Supporting this work also enabled the division to avoid engagement in the ongoing dispute between those who looked to medicine and the clinic for their answers, and others who supported moral and social crusades against vice and prostitution.[47] In his last report in 1942, Turner said he was still seeking "trustworthy measuring rods," the lack of which "has rendered it difficult to evaluate the effectiveness of various procedures designed to reduce the incidence of these diseases in civil communities."[48] At this time Turner began to prepare a series of publications on the Baltimore project, indicating the overall academic caste to his work.[49]

The figures he produced were sobering. They revealed the failures of the Johns Hopkins outpatient clinic in operation for 25 years, and of the East Baltimore clinic, open for 15 years. In any single year, the report concluded, about 20 times as many blacks became infected as whites, and the black numbers were going up. Only whites, it seemed, where the prevalence and incidence were reported to be fairly low at the beginning, seemed to have benefited from the program.[50] Turner's papers were strictly concerned with measurement problems; the fact that the control program in Baltimore did not seem to have worked did not concern him or the Health Division to any great degree. Indeed, the division agreed to continue funding the project after Turner had published his major papers and left to join the army.[51] It was a long-term project, Turner said. There was a need to follow up on any changes in wartime Baltimore, a city with the second highest syphilis rate among draftees.[52]

There was always an undercurrent of racism in these syphilis studies, most dramatically illustrated by a 1944 sex-education and serological survey in a white Baltimore high school. It was part of a campaign to teach "sex hygiene," funded by the School of Hygiene and the city's health department. It was done, to compare the rates with a black high school carried out a year before. But, unlike the black study, a sex-education program was included in the study of the white school. A series of talks were given to 14- to 19-year olds in which sexual abstinence before marriage was presented as the only way to avoid venereal diseases. The white pupils of Patterson Park High School were informed that while none of their 726 pupils tested was positive for the disease, that was not true of students in the black school. The message was hammered home.

> One of the places we are sure it does *not* exist in great abundance is in high school populations, such as yours. We think we do know where most of it comes from, but to prove the thing scientifically we must show, not only where syphilis comes from, but that it does not come from other places.[53]

With penicillin replacing hazardous arsenical drugs, and fear that returning troops would contribute towards an increase in the prevalence of syphilis, Turner

was able to convince the Health Division to continue its support for his research after the war. He continued to argue that the effectiveness of control measure was still limited by inadequate scientific knowledge.[54] He also reactivated his spirochaete laboratory at Johns Hopkins, where he worked on the treponemes of rabbits. Four years later Turner approached the division for increased support, saying the project should be continued for a further 10 years. The U.S. Public Health Service was currently spending $16 million on venereal disease control, he warned, without having the means to measure the effectiveness of their programs.[55] But by that time Turner had shown how to measure incidence and thus had the means to measure any campaign's effectiveness.[56] Perhaps aware of this, the Health Division turned down Turner's request. In 1949 it withdrew support from Turner's laboratory studies and from the Baltimore syphilis project.

## Rabies

Research, a laboratory, and support for an eminent researcher, the common links in these investigations of the flu, syphilis and tuberculosis, were repeated in 1936 when the Health Division decided to support work on rabies and to open the world's only rabies laboratory, in Birmingham, Alabama. The city was believed to be the rabies capital of North America, with perhaps 1% of its dogs rabid. A rabid stallion—surely a terrifying sight—was reported to have bitten 40 mules, one cow and one pig in a matter of a few minutes![57] With the state spending $22,000 a year on the preparation, distribution and administration of dog and human rabies vaccines, Dr. D. Cannon, of the Alabama public health department, wrote to Russell in 1934 requesting assistance.[58] He wanted to build an "experimental plant" in order to ascertain whether prophylactic dog vaccines then in use were effective and to find a more rapid diagnostic animal inoculation test. (Inoculating rabbits with brain tissue from a suspected rabid animal which had bitten a human was of little use because of the long incubation period. The decision to undertake the complex and dangerous rabies vaccination regime had to be taken as early as possible after the bite).

Russell turned down the request, but two years later Sawyer, after a visit to Alabama, agreed that rabies had become a serious matter which warranted support. But, as he himself admitted, Sawyer would not have given the situation much thought had not Leslie Webster of the Rockefeller Institute recently developed a mouse inoculation test to detect the presence of the rabies virus.[59] With such a powerful laboratory tool on hand, together with the necessary expert, Sawyer understood the research potential and, with eyes focused more on the laboratory than on public health, quickly allocated money to the project.

The rabies laboratory opened in 1937 and was set up in the grounds of Kilby Prison, the State Penitentiary.[60] "Volunteer" prisoners, most of whom were presumably black, handled the rabid dogs, a disagreeable and dangerous job. When

out of their cages, the dogs were held at bay by an iron pipe with an internal steel cable looped around the dog's neck. How they first placed the noose around the dogs' necks was not reported (Fig. 12.2).

By agreeing to become involved, Sawyer stepped into a political hornets nest. In 1934 the state of Alabama had attempted to put in place compulsory rabies vaccination for all dogs.[61] Not surprisingly, dog lovers, second only to American gun-lovers in their passion for misguided causes, took strenuous efforts to delay or abolish any form of compulsory vaccination for their beloved pets. In this they were encouraged by some veterinarians who remained opposed to compulsory vaccination on the perfectly feasible grounds that the vaccine did not seem to work. An article in *House and Garden* poured scorn on the idea, saying, "the rabies racket" had become "one of the cheapest and most dangerous rackets in the history of medicine."[62]

Here again was the classic confrontation. Could rabies be best controlled by the application of prophylactic vaccines to dogs—the scientific approach supported by Sawyer—or could it be controlled by laws that obliged the authorities to destroy stray dogs, quarantine bitten dogs or dogs that had bitten humans, and force dog-owners to keep their animals muzzled when a rabies outbreak took place. Critics of vaccination had much evidence to support their claim that compulsory vaccinations of all dogs did nothing but create a false sense of security.

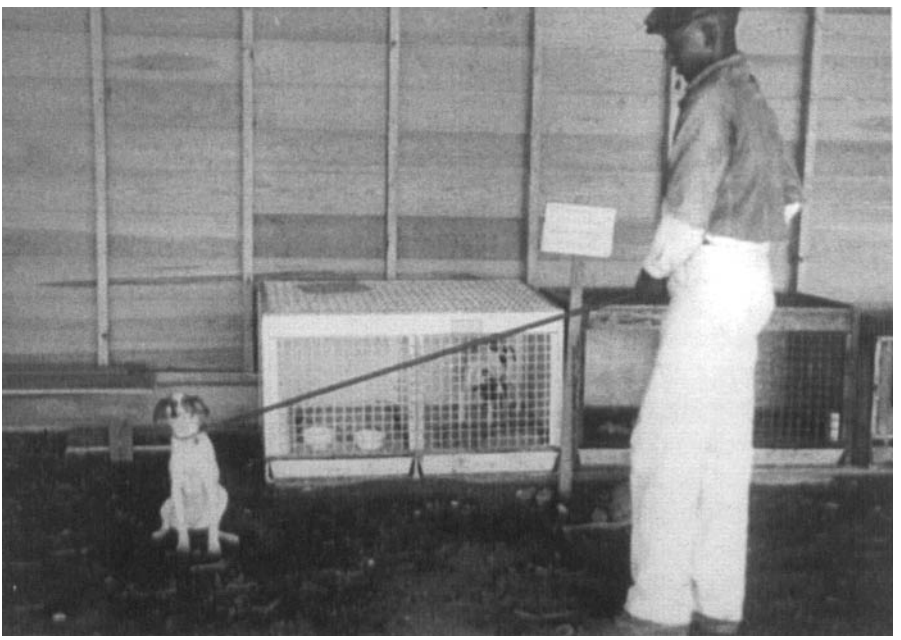

**FIGURE 12.2.** Handling rabid dogs (courtesy of the Rockefeller Archive Center).

Initial reports from the laboratory only confirmed that the dog vaccines were ineffective.[63] News from Webster's laboratory at the Rockefeller Institute, however, promised better things to come. Viruses that had undergone at least 60 serial transfers through a mouse brain-tissue culture, appeared to be a possible source of vaccines. All that remained, Webster claimed, was to further reduce the virulence of these vaccines without destroying their immunizing powers. In a public lecture, Webster threw his weight behind Alabama dog-lovers by urging that the introduction of canine vaccination should be deferred until a more effective tissue-culture vaccine became available.[64]

But public health authorities in Alabama continued to push for a compulsory vaccination program, realizing, that unless they did so, the entire anti-rabies program in the state might collapse. After much delay brought on by three appeals to the Alabama Supreme Court, and despite the well-known problems with the vaccines, the compulsory vaccination law went into effect in 1938. At the owners' expense, all dogs were required to wear a licensed dog tag indicating they had been vaccinated. Owners of non-vaccinated dogs were subject to a 10-dollar fine; and dogs that had not been vaccinated were to be destroyed.

Attempts to culture a non-virulent attenuated virus for use in a dog vaccine failed. By 1940, 128 transfers of the virus through mouse embryro brain tissue had failed to decrease its virulence. Sawyer wanted out, particularly after Webster struck again. In 1940, he announced that none of the vaccines gave any protection and in his major publication, *Rabies*, which appeared in 1942, Webster concluded that all vaccines were useless. "No vaccine has as yet been developed with sufficient qualifications to justify compulsory vaccination of dog and other animal populations," Webster concluded.[65] "Here's hoping," Dr. Sellers of the Georgia public health department said a few months before the book's appearance, "that something can be done to keep this book off the press."[66]

For some reason, the U.S. military kept the Health Division's rabies laboratory in action throughout the war. Eventually, in 1945, despite protests from Georgia politicians, the division handed over the laboratory to the USPHS who converted it to a regional virus diagnostic center.

From the late 1920s on, the Health Division had responded to Russell's call for greater emphasis on research. Their work on yellow fever had clearly benefited from this activity and had not only resulted in a powerful new vaccine but had enabled them to quickly respond to problems brought on by wartime contamination of the vaccine. In other areas, the results were more questionable. In its research emphasis, the Health Division seemed to have taken on the mantle of the Rockefeller Institute. Lewis Hackett, from his perspective as one of the division's most influential field-officers, viewed this change as "a major turning point" in the history of the organization, and one which he did not fully approve.[67]

## Notes

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9. This intervention, collapse therapy, involved procedures such as introducing air between the pleural membranes so as to collapse the infected lung; removal of rib portions to collapse the entire chest wall against the lung; or the paralysis of the diaphragm by cutting one of the phrenic nerves.
10. K. McCuaig, *The Weariness, the Fever, and the Fret: the campaign against tuberculosis in Canada. 1900–1950* (Montreal: McGill-Queens University Press, 1999) p. 40.
11. For a brief but solid account of collapse therapy and the BCG controversy see R. Y. Keets, *Pulmonary Tuberculosis. A Journey down the Centuries* (London: Baillière Tindall, 1978). McCuaig, *The Weariness*, also discusses collapse therapies, and the controversies over BCG in the United States are detailed in G. Feldberg, *Disease and Class: Tuberculosis and the Shaping of Modern North American Society* (New Brunswick: Rutgers University Press, 1995).
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19. Washburn to Howard, November 3, 1930. RAC. RG.1.1 S.437 B.2 f.35.
20. Opie to Russell, May 3, 1929. RAC. RG.1.1 S.437 B.2 f.28.
21. Opie to Russell, June 13, 1931. RAC RG.1.1 S.437 B.3 f.36.
22. R. Cole to F. Russell, November 15, 1929. RAC. RG.1 S.100 B.112 f.1041.
23. Opie to Russell, December 10, 1929. Ibid.
24. Russell to Opie, January 9, 1930. Ibid.
25. For a look at the various vaccines that have been developed in an attempt to prevent tuberculosis see J. A. Myers, "The ever-continuing search for immunity in tuberculosis." *Postgraduate Medicine* 12 (1952): 101–117; 264–282; 469–488. Myers was one of the most vociferous critics of the BCG vaccine in the U.S. and his bias shows in this article.
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30. Turner to Russell, April 30, 1931. RAC. RG.1.1 S.437 B.10 f.122.
31. Russell to D. Molloy, May 14, 1931. Ibid.
32. *The Jamaica Yaws Commission. Annual Report*, 1936. RAC RG.5.3 S.437 B.187.
33. Turner to Washburn, April 25, 1934. RAC. RG.1.1 S.437 B.10 f.125.
34. My short account of these changes is taken from A. Brandt, *No Magic Bullet. A Social History of Venereal Disease in the United States since 1880* (New York: Oxford University Press, 1987). Also, J. Heller, "The venereal-disease control program in the United States." *Canadian J. Public Health*, 35 (1944): 16–25.
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41. T. Turner, "Studies on syphilis in the Eastern Health District of Baltimore City. I Principles concerned in measuring the frequency of the disease." *Amer. J. Hyg.* 37 (1943): 259–272.
42. *The Jamaica Yaws Commission. Annual Report*, 1936.

43. The first attempt to measure incidence is often ascribed to J.A. Doull's work on leprosy in the Philippines [*International J. Leprosy* 10 (1942): 107], but clearly Turner deserves the greatest credit. For further details about the incidence-prevalence problem, see J. Farley, *Bilharzia. A History of Imperial Tropical Medicine* (New York: Cambridge University Press, 1991).
44. Turner, "Syphilis Program for Baltimore," February 7, 1938; Turner, "Factors influencing the interpretation of discovery rates in syphilis." RAC. RG.1.1 S.200 B.28 f.321
45. E. Fee, "Sin vs. Science," p. 145.
46. E. G. Clark. "An analysis of serological tests done for 1937–1939." RAC. RG.1.1 S.200 B.29 f.325
47. This is the issue discussed in Fee's, "Sin vs. Science."
48. "Syphilis Program in Baltimore—Annual Report for 1942." RAC. RG.1.1 S.200 B.29 f.328.
49. T. Turner, Studies on Syphilis in the Eastern Health District of Baltimore City. I. "Principles concerned in measuring the frequencies of the disease." *Amer. J. Hyg* 37 (1943): 259–72; II. "Discovery rates as index of trend." *Amer. J. Hyg.* 37 (1943): 273–88; III "Study of the prevalence of syphilis based on scientific age groups of an enumerated population." *Amer. J. Public Health* 32 (1942): 307–13; IV "Syphilis among parturient women as an index of the trend of syphilis in the community." *Amer. J. Hyg.* 46 (1947): 260–67.
50. *Johns Hopkins University Syphilis Study. Annual Report*, 1939. RAC. RG.5.3 B.5.
51. Turner. "Principles concerned in measuring"; "Discovery rates as index of trend." G. Clark & Turner, III. Study of the prevalence of syphilis based on specific age groups of an enumerated population. *Amer. J. Public Health* 32 (1942): 307–313.
52. Turner to Ferrell, June 10, 1942. RAC. RG.1.1 S.200 B.29 f.324. Fee, "Sin vs Science," reports that Baltimore had a rate of 101.3 per 1000!
53. J. Porterfield, "A talk on sex hygiene for high school students," in *Johns Hopkins University—Syphilis. Annual Report* 1945. RAC. RG.1.1 S. 200 B.29 f.328. The black high school study was published. E. Hesbacher, "A study of syphilis in a negro high school." *J. Ven. Dis. Information* 27 (1946): 200–204.
54. Turner to H. Smith, November 13, 1945. Turner, "Proposal for the continuation of studies on syphilis at the Johns Hopkins School of Hygiene and Public Health." RAC. RG.1.1 S.200 B.29 f.325.
55. Rockefeller Foundation Grants for Syphilis Research. July 8, 1948. RFA. RG.1.1 S.200 B.29 f.326.
56. Turner and his co-workers, for example, provided the tools by which similar studies were conducted in California (1939–42), North Carolina (1939–50) and Finland (1946–49). The RAC houses details of all these projects: California RG.1.1 S.205 B.1 f.6–12; North Carolina RG.1.1 S.236 B.3 f.30–31, B.4 f.35–42; Finland, RG.1.1 S.787 B.1 f.3.
57. C. Leach to Webster, April 27, 1937. RAC. RG.1.1 S.201 B.1 f.13.
58. D. Cannon to F. Russell, August 31, 1934. RAC. RG.1.1 S.201 B.1 f.10
59. Sawyer to Strode, February 21, 1936. RAC. RG.1.1 S.201 B.1 f.11. L. Webster and J. Dawson, "Early diagnosis of rabies by mouse inoculation." *Proc. Soc. Exper. Biol. Med* 32 (1935): 570–73. In this test, brain tissue from a suspected case was emulsified and injected into the brains of mice, which Webster had found to be many times more susceptible to rabies than rabbits. If the test tissue contained the rabies virus the mice would become paralysed and most would be dead by the tenth day. Mice could

be used also to test the value of dog vaccinations. If a vaccination were successful, a dog's blood would come to contain antibodies which would then protect mice from a subsequent inoculation of potentially fatal rabies virus.

60. In some desperation, Charles Leach, a malariologist with little knowledge of viruses had been placed in charge of the rabies project (Sawyer could find nobody else willing to work in Alabama); but was later replaced by an experienced virus-man, Harold Johnson from the University of Nebraska, who was willing to work there.
61. "The Prophylactic Vaccination of Dogs against Rabies with the One-Injection Method," US Department of Agriculture, April 20, 1935.
62. Dr. W. Bruette, "The Rabies Racket." *House and Garden*, May 1937.
63. *Alabama Rabies Study, Annual Report* 1937. RAC. RG.5.3 S.201 B.8.
64. L. Webster, "The increasing incidence of rabies. Its control and treatment." Talk before the Massachusetts Medical Society, June, 1937; "The New Work on Rabies," October 1937. RAC. RG.1.1 S.201 B.1 f.14 and 15.
65. Webster and Casals, "Antirabies vaccines." *J. Expt. Med.* 71 (1940); L. Webster, *Rabies* (New York: Macmillan Press, 1942).
66. T. Sellers to Leach, September 4, 1941. RAC. RG.1.1. S.201 B.2 f.23.
67. L Hackett, MS *History of the International Health Division*. Chap 10. RAC. RG.3. S.908. B.5 f.37.

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# IV

# TRAINING THE EXPERTS

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# 13

## Frustrations in Sao Paulo; The Wrong Step In Rio

That an organization devoted to developing public health services where none existed before should have allocated a large share of its budget (27% in total; see Table 1.2) to the endowment and maintenance of schools of public health and schools of nursing as well as the provision of fellowships to study in them is not surprising. Only six months after it had been formed in 1913, the Health Commission pressed the Rockefeller Foundation to address the issue of public-health training. From the Sanitary Commission's hookworm work in the South, the Health Commission knew much about the incompetence of part-time patronage-appointed health officers, untrained in public health, who often owed their allegiance to their own private practices. As a result, the Rockefeller Foundation invited the General Education Board to examine ways to train a new cadre of full-time public health professionals.

The endowment of high quality, research-oriented public health institutions attached to universities and medical schools was determined to be the best way to raise standards and provide a scientifically-grounded public health education. These schools aimed, in the words of William Welch, dean of Johns Hopkins Medical School, "to cultivate and advance the science of hygiene and not to meet the immediate needs of the public health service." Wickliffe Rose, first director of the Health Commission, wished to support smaller schools geared to teaching the practical needs of public health departments; but the board opted for the Ger-

man research model. They favored schools of hygiene, with biomedical research focused on bacteriology and the germ theory of disease, and chose the medical school at Johns Hopkins University in Baltimore as the site of their model school of hygiene and public health.[1] Surprisingly, given Rose's opposition, the Health Board agreed with the Welch plan, and the most prestigious schools of hygiene which they endowed in Toronto and London were of the Johns Hopkins type.[2] Elsewhere, however, they were less successful and, somewhat to their annoyance, many of their schools came to have very different emphases.

In contrast to their support for schools of hygiene, officers of the Health Board were generally uncomfortable with the idea that nurses were worthy recipients of their benevolence. This antipathy scarcely comes as a surprise given the overwhelming dominance of male physicians on the board. They must have been aware also that in many countries nursing was still a low-status female career, particularly where dominated by religious orders. But any organization concerned with upgrading public health could hardly ignore completely the role played by public-health nurses, so, with far fewer resources than they were prepared to allocate to schools of hygiene, the Health Board nevertheless agreed to begin the endowment of nursing schools.

Their first schools of hygiene and nursing opened in Brazil. Perhaps at that time they intended to open similar schools in the world's poorer tropical countries, believing that the plans for staffing the Brazil schools would have general application elsewhere. In these plans, the new institutions were to be directed initially by Americans, with selected members of the host country sent to Johns Hopkins for training. The returnees would then assume leadership roles in the new institutions as the Americans withdrew. But these plans did not materialize. Instead, after Brazil, the Health Board switched its attention northwards and thereafter, with a couple of exceptions, their schools of hygiene and nursing were opened in Canada and Europe.

## Saõ Paulo

In October 1915 the Health Commission agreed to inaugurate their public health work in Latin America by sending a commission to Brazil to report on a whole range of public-health issues, including medical education. The commission, comprising Richard Pearce, professor of medicine at the University of Pennsylvania, John Ferrell of the Health Commission, and Bailey Ashford of the U.S. army, reported back in mid-1916. They recommended that the Health Commission endow chairs of hygiene and pathology in the recently-founded São Paulo Medical School so that it "might gradually overcome the traditions of the schools of Rio de Janeiro and Bahia [the two old and established medical schools in Brazil] and exert a most important influence on the system of medical education in Brazil."[3]

Historically, the Brazilian educational system had been modeled after that of the French. It was highly centralized, and all schools were required to have a curriculum identical to that at Rio. This conformity was imposed because any medical graduate wishing to hold a government position outside his own state needed to meet federal requirements. This meant that every professional school in Brazil had to be exactly equivalent to those in Rio—the same standards and the same curriculum. As a result, Dr. Geraldo de Paula Souza complained to Rose, Brazilian medical schools were static and "sunk in profound inertia," teaching only classical ideas reputed to be incontrovertible. In such an environment many of the faculty had become what Souza called, "simple phonographs." Furthermore, the faculty were part-time, Rose learned, and "all think first of their private practice." But São Paulo was different, Souza claimed. It was the richest, most active and progressive state in the federation, and its medical school, founded in 1912, was planning to gain recognition throughout Brazil by a special act of congress. Once that occurred, Souza proclaimed, there could be a radical reorganization of its medical curriculum.[4]

As a result, in May 1917, the Health Board contributed $1,300,000 (1990) for a five-year period to organize and maintain the Department of Hygiene within the *Faculdade de Medicine e Cirurgia de São Paulo,* with plans to eventually expand it into a new institute of hygiene. Dr. Samuel Darling, previous head of the Uncinariasis Commission, was appointed first professor of hygiene and director of the laboratories, with Dr. Wilson Smillie of the Health Board's staff acting as his assistant. Darling served only two years. In 1920 he was invalided home with a brain tumor which was successfully operated upon. He was replaced by Smillie, a native of Colorado, who graduated in medicine from Harvard in 1912 and earned his doctorate in public health under Milton Rosenau four years later. At the same time, Souza, and one other Brazilian were granted fellowships to study at Johns Hopkins School of Hygiene, following which they would be expected to replace the two Americans and assume leadership roles. The department would then grow into an independent school of hygiene. At least that appears to have been the plan.

But in 1921, only one year before the Health Board's contract expired, Rose learnt from Souza that events were not unfolding as expected. According to Souza who had by then returned to Brazil as Smillie's assistant, the Department of Hygiene was not in the best of health.[5] Without further funding, Souza warned, the hygiene department would decline and die. The issue had "reached a critical point."[6]

Souza was also exercised over what he called the "public calamity of laboratories." In the Rio and Saõ Paulo areas numerous laboratories had opened to fill various niches, reflecting the French influence. In 1892, the state of São Paulo opened the Bacteriological Institute, a little later, the Vaccination Institute for the preparation of smallpox vaccine. With the arrival of bubonic plague in 1899, a vaccine and serum laboratory, the Butantã Institute, was opened. These three labo-

ratories plus the famous Oswaldo Cruz Institute in Rio, what Souza called, "a nest of intrigue," made up a mosaic of laboratories in the central highly-populated region of the country.[7]

By 1921, Smillie and Souza had developed plans to expand the Department of Hygiene into an independent institute of hygiene and to consolidate the laboratory systems. The laboratories in Sao Paulo needed to be sold, they said, and their various functions gathered under two roofs: the institute itself, where research would be emphasized, and the existing Butantã Institute for routine service work. Would the Health Board be willing to aid the government in this task?[8]

The director of the sanitary service in São Paulo, Dr. Pedroso, felt threatened by Souza and his American backers. After a meeting with Hackett and Smillie, Pedroso agreed that the laboratory service needed reorganization and that a strong central autonomous institute of hygiene, devoted to research and the training of health officers, would be desirable. But he believed—looking into the mirror—that the institute should be placed under the direction of the government's sanitary service, not that of the medical school.[9] To the Health Board, a medical school was always to be the home of an institute of hygiene, as it was at Johns Hopkins. Partly to protect his own position and partly in response to Butantã's mishandling of a cattle plague epidemic, Pedrosa then fired that institute's director and replaced him with a Dr. Krauss from Argentina. Described by Smillie as "a typical Teuton scientist, of dominating, aggressive and somewhat unsympathetic personality," he was expected to prevent any takeover or downsizing by aggressive Americans.[10] Neither Smillie nor Souza felt much regard for Pedrosa, who was an elderly man with no special training in public health. He was also, at the time, at swords' points with the faculty of the medical school—which in itself was witnessing clashes between American and French factions, and between the latter and the Health Board. "There is a strong, though small group in the Medical School who have a clear conception of what a good medical school should be," Smillie noted with the assurance of someone who knows he is in the right, "and we may count on them for support." But those trained in France, he added, will be hard to convince.[11]

According to Smillie, the real power in the medical faculty lay with the French-educated Dr. Alves Lima, brother-in-law of the state president, and not with its director, Dr. G. Xavier. Apparently, Lima disliked Americans, distrusted the motives of the Health Board, and was in turn distrusted and disliked by Souza and Pedrosa. But Smillie respected Lima and felt the climate would improve were the Health Board able to "win his respect and confidence."

Such an opportunity soon presented itself; Dr. Lima was invited to the United States to receive an honorary degree from the American College of Surgeons. "We have spent months in breaking down his prejudice against Americans," Smillie told Rose, and while Lima was in the United States the Health Board should show him just what it had to offer Brazil. Smillie suggested royal treatment for the visitor. He is, after all, a "Latin," Smillie reminded Rose, and as such "enjoys enter-

tainment and the little social niceties." If, by these and other means, Dr. Lima could be won over to "our point of view as to the proper methods of development of a strong Medical School and Institute of Hygiene in São Paulo," Smillie wrote, then "a big step will have been taken towards success in our work here."[12] But, all in all, Smillie had little confidence in the future of the school. He did not believe, he told Rose, "that the petty quarrels, and bickerings, jealousies and tiresome intrigues and inter-mural politics," would change. Was the Health Board, he wondered, "wasting trained men and money that could be used to better advantage in some other place."[13]

In February 1922, after much political maneuvering, Fred Russell and Richard Pearce—now director of the newly-formed Division of Medical Education—arrived to examine the situation at first hand. A month later, Pearce presented his report to the medical faculty, in Sao Paulo, which by then had appointed a more "progressive" director to replace Dr. Xavier. Not surprisingly he called for reforms so that it came to resemble Johns Hopkins. These included a full-time faculty, limited enrolment, a central location with a teaching hospital, and departments with a strong laboratory and research base, including a department of hygiene. By these means, "São Paulo would have a Medical and Public Health center worthy of the important place which the City and State of São Paulo occupy in the affairs of Brazil."[14]

A year later Souza, who by then had replaced Pedrosa as director of the state's sanitary service, and had succeeded Smillie as director of the hygiene department, wanted the department reorganized into a research-oriented institute of hygiene.[15] In November 1924, after another year's delay, Souza's plan was presented to the state congress with the backing of the president. "Seeing harmony come from and use made of all the energy which is engaged in the solution of our sanitary problems," the English translation of the Congress presentation proclaimed, "we conceived the plan for an enlarged Institute of Hygiene, directly subordinate to the Secretary of the Interior and destined to lend its services to the Sanitary Service as well as to medical education of the Faculty of Medicine." To that end the state's Vaccine Laboratory, Bacteriological Institute, and the Batantã Institute would be amalgamated with the Department of Hygiene to form the *Instituto de Hygiene de São Paulo*.[16]

Frederick Russell was not overly impressed with Souza's scheme. He remained committed to the Hopkins model. Institutes of hygiene needed to be integral parts of medical faculties, not subject to the whims of governments.[17] Faced with possibile amalgamation, others felt directly threatened and acted accordingly.

Dr. Caralho Lima, director of the Bacteriological Institute, in a 15-page letter of wounded pride, voiced his concern to the Secretary of the Interior, with an English translation finding its way to the Health Board.[18] In order to support his glowing endorsement of the threatened Bacteriological Institute he listed its important work in yellow fever, the plague and other diseases. But there were flaws in his

argument. Most of the work cited had taken place before 1903, after which the institute declined and research impetus shifted to the Oswaldo Cruz Institute; the Bacteriological Institute spent increasing amounts of time on routine bacteriological work. Neither was he impressed by Souza's ideas which were to him, simply American. " Not all which is magnificent for them [Americans] can be applied among us," he wrote. "To implant suddenly in Brazil all the American organization would be to perturb greatly our services already organized." Consolidating all the laboratories with a Rockefeller-maintained Institute of Hygiene, he concluded, "would wound our dignity."

Dr. Lima's dislike of American interference had been stoked a few months earlier after attending a lecture by Lewis Hackett, (previous director of the Health Board in Rio,) given at the Harvard Medical School and entitled "The Adventures of a Sanitarian in South America." The lecture was, according to Lima, filled with insulting remarks about Brazilians such that "its contents and objectives were purely to ridicule our country." Hackett, according to this onlooker, portrayed the country as a land of *Jécas*—simple, ignorant country-folk who lived in mud houses covered in straw, where the only visible improvements had been brought about by Americans and the Rockefeller Foundation.[19] With such people in charge, Lima complained, any amalgamation with the Institute of Hygiene would enable them to tell the world that to the Rockefeller Foundation alone must go all the credit. Souza, whose career was intimately tied to the status of the Health Board, came to Hackett's defense, claiming that in the past Hackett had related amusing incidents without intending to be derogatory. Strode, in his reply, seemed much more sympathetic and understanding of Lima's position than did Souza, and noted that, with so much opposition, Souza's proposed amalgamation might be secured "at a price quite out of proportion to its worth . . . might it not be wise to postpone or entirely drop the matter until a more favorable occasion?"[20]

Souza had powerful political backing, so that despite attacks from members of congress, the government closed the Bacteriological Institute and took over what was called the *Instituto de Hygiene de São Paulo* as of January 1, 1925. They immediately submitted a formal request to the Rockefeller Foundation for funds to erect a building for it.[21] Strode reminded Russell that the arrangement was satisfactory only so long as Souza remained as director of the Sanitary Service and of the Institute. There was obviously some unease that in future the institute would distant itself from the medical school. Strode, however, convinced himself that building the institute on the university site "will engender the general and continuous impression that the Institute is first and foremost a teaching and research division of the Faculty of Medicine," just as the Health Board planned it to be.[22] Thus in May 1925, the Health Board allotted just over $1 million (1990) to help erect and equip the new institute.

Another three years were to pass before the final contract was signed, and the Health Board agreed to contribute approximately 65% of its total funding. In those

years the dean of the medical faculty, Dr. Pedro Dias, tried but failed to reduce the Institute of Hygiene to a simple department within the medical school, in an attempt to gain control of the Health Board's grant and supplement the funds being used to build the medical school. In December 1928, seven months after the contract had been signed, Fred Soper, who had replaced Strode in the Rio office, was called to blunt a further attack—this time from the opposite quarter. An attempt had been made in congress to reduce the institute's role to that of public health education, and to turn over all the laboratory funds to the Butantã Institute. This plan was supported by the then director of the sanitary service and a group of "nortistas" from the northern states who were not only hostile to Americans and the Rockefeller Foundation, but wished to destroy the new institute. Their threats immediately galvanized Dr Dias to repeat his earlier attempt to "aggregate" the institute within the medical school in order to protect it from its enemies within the sanitary service and Butantã. "Under the guise of imminent outside danger," Soper noted, "he was attempting to put across the same thing that he failed in last year."[23]

Other crises followed. Construction was continually threatened by the state treasury's policy of single-year budgeting and the requirement that all unspent money be returned at the end of each year. In 1930 construction ground to a halt when the uncompleted building was taken over as a barracks during the Vargas revolution. The success of this revolution resulted in a new dean of medicine, and another reorganization of the institute. In 1931, with construction underway again, it became attached to the medical school much as the Health Division wanted. Not that this stopped the infighting, as the sanitary service, the Butantã Institute and the medical school continued to struggle for control of the institute and its funds. Despite calls for help from Souza, the Health Board refused to become involved in what they saw as an internal fight. Finally, in 1933, with the building still incomplete, Russell informed the Brazilian authorities that no further aid would be forthcoming; henceforth all the Health Board's efforts in Brazil were to be directed towards yellow fever.[24]

The Health Division's first attempt to build an institute of hygiene had ended in at least partial failure as the various Brazilian factions continued to fight over the spoils. "A kind of terrorism prevails," Souza complained. This experience was, I suspect, extremely significant in the long run. The Health Division had learned that dealing with a docile population in the U.S. South or in territories where the British exercised control was different from working in Brazil. It was a huge, politically unstable, and independent country; many of its proud and well-educated elite looked toward Europe, and felt hostile towards what they saw as interfering, rich and often insensitive Americans. "Now you see," wrote Soper, a year after taking over in Rio, "why I am getting so many gray hairs in Brazil."[25] The next schools of hygiene were to be built in what appeared to be the more comfortable climes of Europe, Britain and Canada; once bitten twice shy.

## The Anna Nery School of Nursing, Rio

As the Health Board set about building a school of hygiene in Brazil, it was persuaded by three prominent figures—Brazil's Minister of Health, Dr. Carlos Chagas of the Oswaldo Cruz Institute, and Dr. Barbosa, head of the Brazilian tuberculosis program—to develop a training course for visiting public health nurses. As a result, in September 1921, Ethel Parsons, of the Texas Bureau of Child Hygiene, was appointed director of the Bureau of Nursing.[26] Her plan, outlined in a letter to Chagas, was in line with that followed by the Health Board in other fields. Well-trained American public health nurses demonstrating their skills would awaken the interest of the Brazilian public in public health nursing. The work would be carried out initially by Americans, but Brazilian women of "intelligence, education and good breeding" should be encouraged to enter the field and undertake the necessary training. This required the establishment of a modern hospital training-school which would attract a higher social class of Brazilian women. According to Parsons, Brazilian nurses at that time were no better than "laundresses and cooks."[27] Backed by Chagas and others, Parsons began to establish a modern nursing service.[28] With a staff of seven Spanish-speaking public health nurses recruited from the United States (it would have been nearly impossible to find Portuguese-speakers), she set up a six-month emergency course for the existing cadre of health visitors who, although minimally trained and of the "ignorant class," were already running tuberculosis, venereal disease and child hygiene clinics in Rio.

Meanwhile, the São Francisco de Assis hospital was opened in November 1922 with a nearby house serving as the nurses residence. Miss Louise Keininger, formerly superintendent of nursing at Christ's Hospital in Topeka, Kansas, was appointed superintendent of nursing with a staff of two American and two British nurses. In May 1923, the first student nurses, some of whom, Parsons was pleased to announce, came from the very best families and had been educated in Europe, entered the 28-month program. At the same time, a ten-month course for health visitors was offered, replacing the emergency six month course.[29]

Both groups, the health visitors and the student nurses, shared a preliminary four-month program devoted to elementary science, first aid, and basic nursing practices. The hospital nurses went on to a standard two-year hospital course, and health visitors followed a six-month program that included social problems, hygiene, tuberculosis and venereal diseases, with supervised visiting in selected districts in Rio. After completing her hospital program, the student nurse was required to spend four months specializing in a selected branch of nursing, including, Parsons hoped, public health nursing.[30] Slowly, according to the annual reports, the quality of the student nurse intake increased. Many were trained teachers, and an increasing number came from families of a high social standing. Political pull was sometimes attempted. In 1923, for example, some of those who had failed the

entrance examinations used political connections to seek acceptance to the school, but were outmaneuvered by Parsons.[31] In 1925, the first class of 13 students graduated, nine of whom took an additional four-month course in public health nursing. By then, the 10-month course had been discontinued, with the idea that health visitors would be replaced by fully-trained public health nurses. Everything was moving forward according to plan.

Chagas sought foundation funding for a new nursing school that would include classrooms, laboratories, and a residence. The goal was not only to attract women of better social standing, but to increase student intake class size which had been limited by the small size of the house used as a nurses residence.[32] As a result, in 1924, Fred Russell opened negotiations with the Brazilian government for a 50:50 share of the cost.[33]

By the end of the year, both Chagas and Strode in the Health Board's Rio office, had become convinced that it would be impossible to secure enough Brazilian financing and that if a school were to be built at all, most of the funding would have to come from the Health Board. A financial crisis in Brazil brought on by excessive debts, declining exports, a devalued currency and political instability had led to a 41% reduction in the national budget, with all branches of government, except that of the military, feeling the effect. The Interior Minister, while supporting the school, told Strode that his hands were tied financially. Although Russell believed the government should contribute financially, the Health Board made $1 million (1990) available without insisting on any financial undertaking by the Brazilians, a fundamental break with tradition. It was understood, however, that the Brazilian government would be obligated to build the school within a reasonable time in accordance with a plan approved by both parties.[34]

A few weeks later the government found a way out of its financial impasse. Much to the consternation of Parsons, the area adjacent to the hospital, where the school was planned to be built, had become a red-light district. "Whole streets," she told Russell, "where there were family residences have been changed to streets of houses of prostitution." In an astute move, the Minister of Justice, who must have received the full weight of Parson's wrath, offered her the *Hotel Sete de Setembro* which had been built for the 1922 centennial celebrations marking independence from Portugal, but never used (Fig. 13.1). Parsons was thrilled. Although situated over four miles from the hospital at the entrance to the Praia de Botafogo, it contained over 60 large rooms and "the beautiful wood-paneled barroom would do admirably for a library by taking out the bar and a few wall mirrors" (Fig. 13.2). The hotel was so well known, she enthused, that recruitment would surely be stimulated. "It seems almost too good to be true."[35] In one stroke everything had been solved. The Brazilian government's gift of the building had fulfilled their half of the bargain and the Health Board's grant could now be used to remodel the hotel and add a small pavilion at the hospital for rest-areas, classrooms and laboratories.[36]

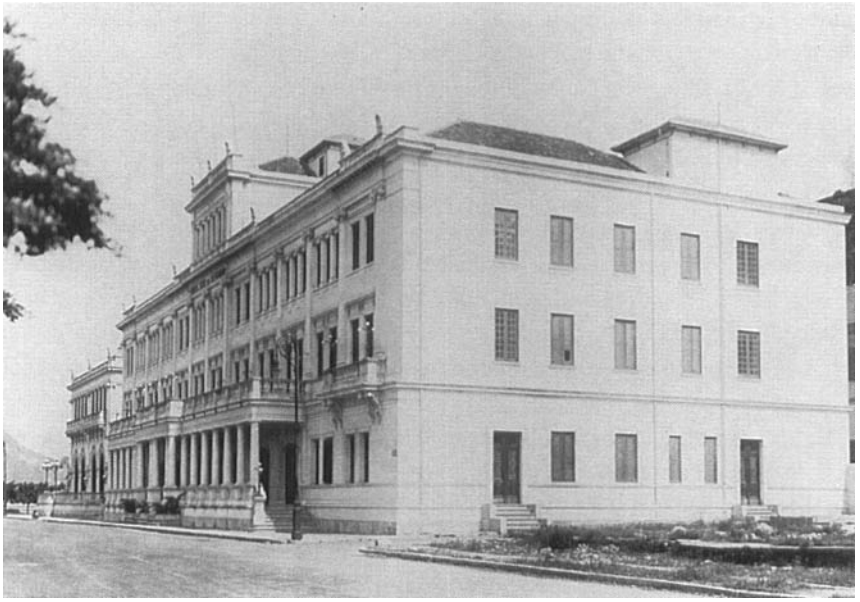

**Figure 13.1.** Nurses residence, Hotel Sete de Setembro, Rio (courtesy of the Rockefeller Archive Center).

**Figure 13.2.** The library, nurses residence (courtesy of the Rockefeller Archive Center).

The grand opening of the *Escola de Enfermeiras Anna Nery* took place in July 1926. The school was named for Anna Nery, called the Florence Nightingale of Brazil, who had volunteered as the only woman battlefield nurse during the war against Paraquay (1865/70). It was also an American occasion. On the wall of the school a plaque was raised to honor Parsons, and she received a gold plate with an inscription reading:

> The D. Anna Nery School of Nursing, on the date of the formal inauguration of the permanent installation, wishes, through her students, to express to Mrs Ethel Parsons, the soul of this undertaking, heartfelt gratitude for this new service, which confirms her title of *Creator of the Brazilian Nurse.*

"The occasion was one of the moments of high light in my own life," she told Russell with obvious and justified pride.[37]

The school was never free from criticism by the Brazilian opposition. The news, for example, that many students had been refused admission to the school because they were black had aroused the fury of some opposition newspapers. The news was in fact true, as the Health Board's report made clear, and fit into the Board's often stated policy of raising the social standing of Brazilian nurses. "It is true," the report noted, "that the policy of the school . . . has been to avoid by tact and strategy the admission of negroes until the public conception of the profession of nursing has changed. This was imperative if the better class of women were to be attracted to the new profession." The report went on to claim that, in reality, no such racial discrimination had taken place. "In instances when colored girls had applied for entrance to the School of Nursing," the report was pleased to note, "it had always happened that there were other good reasons why they were not qualified, so the definite problem had not arisen." Indeed, they concluded, there were three students in the school at that very time who, "although white, showed some traces of negro blood."[38]

Conscious of the hostile press, the health department in 1928 advised the school to admit a black student who had the necessary qualifications, and who was suspected of being a newspaper plant. The poor girl, although helped by her "reserve and willingness to cooperate," was met with a storm of protest from her European-Brazilian peers, who made it clear that "they hoped no other negroes would be admitted for some time."[39]

Despite such problems, this first foray into nursing had been an outstanding success. Contrary to what was taking place in São Paulo, everything had gone as planned: they had built a first-class hospital school of nursing. But the Anna Nery School did not become the Rockefeller model. It may have been a model hospital school of nursing, but that was not the direction the foundation eventually took. As with the schools of hygiene, the next nursing schools were to be university schools opened in Canada and Europe. Neither had nursing as yet found favor with the Health Division. Proceed with care, Russell told Soper in 1928, two years

after the Rio school had opened, "it is not the field of the International Health Division of the Foundation to develop schools of nursing."[40]

## Notes

1. E. Fee, *Disease and Discovery: A History of Johns Hopkins School of Hygiene and Public Health. 1916–1939* (Baltimore: The Johns Hopkins University Press, 1987); E. Fee & R. Acheson, *A History of Education in Public Health* (Oxford: Oxford University Press, 1991), particularly Chapter 5, "Designing schools of public health in the United States."
2. Fee argues in her "Designing schools of public health," p. 193, that had Rose's plan been adopted, "it could have helped build a strong constituency for public health both locally and nationally. It could have solved the problem of preparing sufficient numbers of qualified personnel for public health programmes; it would probably have improved both the quality of those programmes and the health of the population." Also, J. Duffy, "The American medical profession and public health. From supporters to ambivalence." *Bull. Hist. Med.* 53 (1979): 1–22.
3. Medical Education in Brazil. Diaries, Memoranda, Notes, Reports by the Medical Commission to Brazil, 1916. RAC. RG.1.1 S.305 B.2 f.15.
4. Souza to Rose, August 11, 1921. RAC. RG.1.1 S.305 B.18 f.152.
5. Report by de Souza, for September to December, 1920. RAC. RG.1.1 S.305 B.19 f.154. Such criticisms seemed justified; the research strengths of the department centred on bacteriology and parasitology, which were both well represented in other departments of the medical school. Indeed, at that time, none other than the eminent French parasitologist, Emile Brumpt, occupied the chair of parasitology at the São Paulo Medical School.
6. Souza to Rose, August 11, 1921.
7. Nancy Stepan, *Beginnings of Brazilian Science*. Oswaldo Cruz, *Medical Research and Policy, 1890–1920* (New York: Science History Publ., 1976). Surprisingly, the Health Board showed no interest in the Oswald Cruz Institute. It had been opened by the federal government in 1899 at Manguinhos, north of Rio, after cases of plague had spread to Rio. Oswaldo Cruz, who trained at the Pasteur Institute in Paris, was eventually appointed director of the institute as well as director of the Federal Department of Public Health. His success in fighting yellow fever between 1903 and 1909, raised Cruz's profile, and he was able to slowly move the laboratory away from its narrow focus as a supplier of vaccines. In 1906 it became the Institute of Experimental Pathology, an independent center for bacteriological instruction and research into infectious and tropical diseases. Following Cruz's triumphs at the XIIth International Conference of Hygiene in 1908, the institute was renamed the Oswaldo Cruz Institute. As Stepan has pointed out, this was Brazil's first research institute, the first to enhance Brazil's reputation for science and the first to make scientific contributions over a long period of time. After the death of Cruz in 1917, he was succeeded by Carlos Chagas, who won an international reputation for his discovery of *Trypanosoma cruzi*, the agent of what came to be called "Chagas disease." That these discoveries were announced in the institute's own scientific publication, *Memorias do Instituto Oswaldo Cruz*, brought it additional fame.
8. Smillie to Rose, April 6, 1921. RAC. RG.1.1. S.305 B.18 f.152.
9. Smillie to Rose, August 13, 1921. Ibid.
10. Smillie to Rose, June 11, 1921. Ibid.
11. Smillie to Rose, August 13, 1921. Ibid.

12. Smillie to Rose, September 8, 1921. Ibid.
13. Smillie to Rose, August 13, 1921. Ibid.
14. R. Pearce, Report on the *Faculdade de Medicina e Cirurgia de São Paulo*. March, 1922. RAC. RG.1.1 S.305 B.3 f.22.
15. Souza to Russell, October 30, 1923. RAC. RG.1.1 S.305 B.18 f.152.
16. Institute of Hygiene. Enclosure No. 1. Strode to Russell, November 28, 1924. RAC. RG.1.1 S.305 B.18 f.153.
17. Russell to Strode, December 17, 1924. RAC. RG.1.1 S.305 B.18 f.152.
18. J. Carvalho Lima to Secretary of the Interior. September 30, 1924. RAC. RG.1.1. S.305 B.18 f.153.
19. The word Jécas comes from the fictional character Jeca-Tatu, created by the Brazilian author Monteiro Lobato. Jeca was used in the 1920s and 1930s to teach Brazilian children sanitary habits so as to avoid hookworm and other intestinal parasites. This information was given to me by my good friend Angel de Perez of Itajai.
20. Souza to Strode, November 18, 1924; Strode to Souza, November 25, 1924; RAC. RG.1.1 S.305 B.19 f.153. There is, as far as I know, no extant copy of the lecture, but I think it fair to comment that what was meant by Hackett to be amusing and harmless fun, could easily have been taken the wrong way by a Brazilian who was suspicious of the Americans at the best of times. One thing is certain—Hackett could never be described as "an ugly American," even if, perhaps, he should have toned down this particular lecture.
21. Souza to Russell, January 5, 1925. Ibid.
22. Strode to Russell, January 5, 1925. Ibid.
23. Soper to Russell, December 3, 1928. Ibid.
24. Russell to Souza, August 9, 1933. Ibid.
25. Soper to Russell, December 3, 1928. Ibid.
26. Executive Committee Meeting, International Health Bureau. July 6, 1921. RAC. RG.1.1 S.305 B.7 f.46. "The Development of a Service of Nursing in Brazil, 1921–1926." RAC. RG.5.3 S.305 B.110.
27. E. Parsons to C. Noyes (American Red Cross), March 28, 1922. RAC. RG.1.1 S.305 B.7 f.46.
28. Parsons to Rose, September 11 and 21, 1921; Recruiting form letter, December 1921. Ibid.
29. E. Parsons to F. Read, December 8, 1922; Parsons to Chagas, June 17, 1921: Ibid.
30. "The Development of a Service of Nursing in Brazil."
31. Parsons to Read. March 15, 1923. RAC. RG.1.1 S.305 B.7 f.47.
32. Chagas to Vincent. September 1, 1923. Ibid.
33. Strode to Russell, October 15, 1924; Meeting Board of Trustees, IHB. November 6, 1924. Ibid.
34. Strode to Russell, April 20, June 24, 1925. Executive Minutes, IHB. July 30, 1925. RAC. RG.1.1 S.305 B.7 f.48.
35. Parsons to F. Russell, August 5, 1925. Ibid.
36. Executive Minutes, IHB. November 5, 1925. RAC. Ibid.
37. Parsons to Russell, August 4, 1926. RAC. RG.1.1 S.305 B.7 f.50.
38. "The Development of a Service of Nursing in Brazil."
39. Ibid.
40. Russell to Soper, August 14, 1928. RAC. RG.1.1 S.305 B.8 f.52.

# 14

## Northern Lights: London and Toronto

In the 1920s, the Health Board endowed two schools of hygiene, one in London and the other in Toronto, which they regarded as model institutions, sharing with Johns Hopkins the distinction of being centers of research with the very highest standards—and so recipients of by far the most funding. At the same time, it turned its back on Rio and hospital schools of nursing, deciding instead to endow university schools of nursing, none more important than that at the University of Toronto. The Brazilian experiments were not to be repeated.

Initially, both London and Toronto were seen as schools of the British empire, the former the central home and the latter serving Canada and the British Caribbean. Once the idea that schools should be opened in the poorer countries had seemingly been abandoned, London and Toronto would have seemed the logical next step where colonial nationals could be trained in first-class institutions. But such a smooth transition did not occur. European schools that had no link with the tropics predated the Health Board's endowments of London and Toronto, although the European schools were never to attain the same status as Toronto and London. And in the end, the Health Board seemed more concerned with opening schools of hygiene in London and Toronto to serve the health needs of the British and Canadians than in creating centers of colonial training. Clearly, the Health Board's attention was turning northward away from the problems of the tropical and colonial countries where they had started.

## London

In May 1919, the Health Board made it known that it was interested in establishing a school of hygiene in London and would be prepared to consider "a thoroughly matured plan and budget."[1] Training in tropical medicine in preparation for work in the colonial service was well organized at that time. Through the efforts of Patrick Manson, the London School of Tropical Medicine opened at the Albert Docks in Greenwich, to the east of London, in 1899. In 1919 the school and a new hospital for tropical diseases were moved to central London, housed in the remodeled Endsleigh Palace hotel. This not only brought the school into close proximity with University College, but raised its status to that of a school of London University.[2]

If tropical medicine was well organized at the time, the same could not be said for public health. All medical officers of health in the larger urban centers were required to hold a diploma in public health, earned by passing an examination set by various examining boards, none of which provided instruction in the field. Not surprisingly, the General Medical Council in London found the standards required for the diploma to be mediocre. University College provided the only exception; through its Department of Hygiene it taught courses in public health and offered an M.D. degree in state medicine. To add to the confusion, 1913 saw the creation of the Medical Research Committee, which was made responsible for the allocation of government funds in the field of medical research. Six years later Britain's first ministry of health was formed. The new minister convened a Postgraduate Medical Committee, under the chairmanship of the Earl of Athlone, to "investigate the needs of medical practitioners and other graduates for further education in medicine in London." Among those requiring postgraduate instruction in tropical medicine, the committee reported, all was well, but instruction in public health needed to be reorganized. What was required was an institute of state medicine where "all the work—which can only be taken after qualification—should be concentrated."[3]

With these ideas milling about London's medical circles, Vincent, Heiser and Rose were invited to participate in a special conference at the Colonial Office.[4] The Colonial Office was interested in gaining support for tropical medicine and in training men for the tropics, while the Health Board was interested in work, "not only for the sake of the British Colonies . . . but for the sake of public health the world over." Thus Vincent made clear to the Colonial Office that the Health Board was willing to support a Johns Hopkins-like central institute of hygiene with which the School of Tropical Medicine would be closely affiliated, if not organically part of it.

Faced with the foundation's desires as expressed in this conference and the report of the Athlone Committee, the Minister of Health, Alfred Mond, set up a seven-member committee to "draft a provisional scheme for the Institute of State Medi-

cine." In a letter to Vincent, Mond argued that the institute (now called a school of hygiene) should provide services for the inhabitants of Britain as well as the British empire. Somewhat illogically, he stopped short of recommending the incorporation of the School of Tropical Medicine into the new institute, suggesting instead that they should be neighbors in the same university quarter.[5]

The Health Board quickly responded. At the board meeting of October 25, 1921, its officers were authorized to confer with the British Minister of Health on a cooperative effort to establish a new school of hygiene in London. It was to be situated in immediate proximity to the School of Tropical Medicine and was to be "imperial in scope open to qualified students from all countries."

> The Board has had in mind the importance of London as a strategic center for an institution of this kind; its incomparable wealth of experience in tropical medicine and practical public health administration; its commanding position in medicine and general science; the scope and distribution of the empire of which it is the political, economic and intellectual center; the part which the English-speaking people have to play in the development of civilization.[6]

Their vision of the institute sounded very much like that of Mond's but so imperial in scope as to render the School of Tropical Medicine almost superfluous. In March 1922, the Health Board authorized the purchase of the present site immediately south of the main University College campus. It was to be a center of training *and* research for the maintenance of health and the prevention of disease in both temperate *and* tropical climates.

By this time Patrick Manson, the doyen of tropical medicine, had become alarmed over the impact of the proposed new institute on his School of Tropical Medicine, which, he informed Rose, had a large enrolment and was financially sound. Shortly before his death in April 1922, Manson proposed to Rose that his school be housed in a wing of the new institute so that "in the near future you will be our neighbors as well as our friends." Rose, in his answer, assured Manson that the new institution would not duplicate the work of the School of Tropical Medicine (he never explained how) and that "we are interested to see that the relation of the new school to the present school of Tropical Medicine is to be intimately and mutually helpful. This we regard as fundamentally important." Nevertheless Rose tactfully remarked that the Health Board could not determine the policies of the new institute or its relationship to other institutions.[7]

That spring, the minister's site and planning committee took the next step and concluded that the School of Tropical Medicine would indeed become part of the new institute which would henceforth be called the London School of Hygiene and Tropical Medicine. Andrew Balfour, former director of the Wellcome Tropical Research Laboratories in Khartoum, was appointed director of the school, and Royal Charter was granted in August 1924. The place of tropical medicine in the new school seemed assured.

The new building opened on July 18, 1929. It was almost an exact replica of Johns Hopkins School of Hygiene, although with more emphasis placed on tropical medicine (Fig. 14.1). Manson need not have worried. The Division of Public Health, the Division of Tropical Medicine and Hygiene and the Division of Medical Zoology (Helminthology, Protozoology and Entomology) were the three largest of the six divisions, with Balfour in charge of the Division of Tropical Medicine. By 1932, over 1000 medical graduates had received the diploma in tropical medicine and hygiene, but only 127 had gained the diploma in public health. While the Empire lasted it essentially remained a school of tropical medicine. It was also, intoned the Prince of Wales at the opening ceremony, "a signal example of the bond between the two great English-speaking races of the world." In his reply, Sir Gregory Foster, vice-chancellor of the University of London, expressed his hope that "from its doors would issue many students, coming from all parts of the world, who would go out as missionaries to spread the word of health and hygiene, and as soldiers to fight against pestilence and disease."[8]

By the endowment of an institute in London which had amalgamated with the old London School of Tropical Medicine, the Health Board can be seen as furthering, almost by accident, the cause of the tropical countries with which they had been originally involved. But it must be seen as an accident. Throughout the negotiations Rose focused on a new school of hygiene and seemed indifferent to

**FIGURE 14.1.** The London School of Hygiene and Tropical Medicine (courtesy of the Rockefeller Archive Center).

the School of Tropical Medicine and to any obvious overlap of the work they would be doing. Nevertheless, they had certainly found a new way of training the new health professionals in colonial countries, who would be sent to an up-to-date research and teaching institute in London, the heart of the empire. Now two models existed: Johns Hopkins and London. But in 1927, two years before the London School opened, they had added a third, at the University of Toronto.

## The Toronto School of Hygiene[9]

The Health Board's interest in Canada stemmed from the wishes of John D. Rockefeller who, shortly after the end of World War I, reminded the Trustees of the Rockefeller Foundation of his own personal interest in the country. "The Canadian people are our near neighbors," he told them, "closely bound to us by ties of race, language and international friendship, and they have without stint sacrificed themselves—their youth and their resources—to the end that democracy might be saved and extended. For these reasons, if your Board should see fit to use any part of this new gift in promoting medical education in Canada, such action would meet with my very cordial approval."[10]

Rockefeller's attitude toward Canada was not only a result of World War I, and his deep Anglophilia. In September 1913, three months after the formation of the International Health Commission, the United Mineworkers had walked out of the mining camps of the Rockefeller-owned Colorado Fuel and Iron Company. Seven months later the National Guard opened fire on the strikers and killed 40 of them, including two women and 11 children. The reaction of the Rockefeller family to this was so insensitive and coldblooded that the Ludlow massacre "threatened to become the albatross around Junior's neck for the rest of this life."[11] That this did not happen had much to do with a Canadian, William Lyon Mackenzie King, who was later to become Canada's longest-serving prime minister. In 1914 he accepted a job with the Rockefeller Foundation to advise the Rockefellers on improving their relationship with labor. He himself had served as Deputy Minister of Labor and as Minister of Labor in the Canadian government.

Rockefeller's hopes were quickly rewarded. Richard Pearce, director of the Division of Medical Education, and George Vincent visited the Canadian medical schools in 1920 and recommended funding for most of them. Over the next few years the Faculty of Medicine at the University of Toronto received approximately 7 million dollars (1990) from the medical education division.

In contrast to the Health Board's problems in São Paulo, the situation in Toronto indicated fairly smooth sailing. But, this was not entirely the case. Many university presidents in Canada, none more so than Robert Falconer, president of the University of Toronto, were uncomfortable with increasing encroachment of American ideas into Canadian universities. A Nova Scotian Scottish Presbyterian, educated in Trinidad and the University of Edinburgh, Falconer was one of the

greatest university presidents Canada produced, transforming the university from a collecton of warring and jealous denominational colleges to a rejuvenated, modern, and fully national university.[12]

Much of this transformation was driven by his concern over the migration of Canadian graduates to the United States for postgraduate and professional education. The fostering of Canadian identity required that this brain drain be halted by the upgrading of professional education and graduate work at the University of Toronto; until that could be accomplished, Canadian students were best redirected away from the United States to Britain. In 1923 the University of Toronto established its School of Graduate Studies and began attracting graduate students from the rest of the country, its prestige enormously enhanced by the discovery of insulin in 1922 and the Nobel prize for Banting and MacLeod a year later.[13]

But Falconer did not appear to have been uncomfortable with the Rockefeller grant to the medical school, as it encouraged the Johns Hopkins-like reforms necessary to keep pace with American advances. Perhaps too he realized that many members of the foundation, although American, to some extent shared his view of the British empire. George Vincent, for example, was a cousin of Vincent Massey, the aptly named "Imperial Canadian," who in 1952 became Canada's first native-born Governor General. When Vincent addressed the Empire Club of Canada in 1929, Falconer, who may well have been in the audience, could not but have agreed with his sentiments.

> It has been a stimulating and stirring experience to meet all the fine men and women around the world and in the British Empire with whom we have been brought into contact. It has been a most gratifying and delightful experience . . . to cooperate with the Empire at home and in the Dominions and Colonies in helping to build up great causes whose significance and influence spread beyond national borders and are a real contribution to that thing we call the civilization of the world.[14]

But the grant to the medical school had stirred up a hornet's nest in the Faculty of Medicine and led to the crisis of 1922 and 1923.

Many practicing physicians, some of whom had not all that long ago returned home from the trenches of World War I, and had previously taught part time at the medical school, now found their services were no longer required: in line with the Johns Hopkins model, only full-time academicians would be hired. Protesting at the loss of independence brought about by accepting "alien funds" with strings attached, these physicians led a campaign against German and American models of medical education. "It is a very bad mess," Dr A.B. Macallum informed Pearce, "and the possibilities appear to be such that I came away from Toronto thoroughly discouraged."[15] Finding little sympathy from Falconer, the protestors took their case to the press and to the populist Ontario Government of the United Farmers of Ontario who were decidedly suspicious of the university and its urban and "foreign" ways. In October 1922, as a result of backbench pressure and attacks in the press, the government convened a special committee to look not only

at the narrow medical issues but more ominously at the university constitution. At issue was the matter of university financing and whether a public provincial university should continue to be controlled by a board of governors rather than directly from Queen's Park, seat of the Ontario Government.

Although the committee's report stopped short of recommending the abolition of the board of governors, it was highly critical of the Johns Hopkins model of medical education and the Rockefeller gift which, of course, depended on the university accepting this model. "The Committee has no hesitation in saying," they wrote, "that in a publicly-owned university, private endowments should not be accepted if, attached to them, are conditions which would bind the University Governors to any particular quality or course of action."[16] They could hardly have been plainer than that.

The university was saved by the provincial election of 1923 which returned a more friendly Tory government. Its leader, Howard Ferguson, although a member of the special committee, had no intention of implementing any of its recommendations.[17] Only a year after the special committee report, John Fitzgerald approached the Health Board with a request for help in establishing a school of hygiene. Fitzgerald, chair of the Department of Hygiene and Preventive Medicine in Toronto's medical faculty, had been trained in Toronto, Johns Hopkins, Harvard and L'Institut Pasteur and, would later become a member of the division's Board of Scientific Directors at the Rockefeller Foundation. The request was greeted favorably, not least because of Fred Russell's enthusiasm for the relationship between the faculty of medicine in Toronto and the Connaught Antitoxin Laboratory, which was just beginning to manufacture insulin as well as antitoxins and vaccines.

This laboratory was Fitzgerald's brainchild. From his experiences in L'Institut Pasteur, he recognized the value of laboratories in public health and had persuaded the Board of Governors of the university to open a laboratory distinct from the nearby provincial laboratory. The mass immunization of Canadian troops in World War I against typhoid and smallpox, and the production of tetanus antitoxoid for use on wounded troops, had turned the laboratory into a major wartime industry, causing serious space problems. The gift of a farm north of the city allowed for necessary expansion and in October 1917 the Connaught Antitoxin Laboratory and University Farm was opened, named after the then Governor General of Canada, the Duke of Connaught.[18]

The presence of the laboratory gave public health at Toronto a special flavor, and it appealed to Russell, then head of the laboratory service at the Health Board. It allowed teaching of undergraduates, graduate physicians and graduate nurses to go hand-in-hand with research and the preparation and distribution of biological products. The Health Board readily agreed to endow a new school of hygiene, with a division of the Connaught Laboratory to be built adjacent to it on the campus. In June, 1924, the university Board of Governors, in disregard of earlier rec-

ommendations to turn down gifts with strings attached, assured the Health Board that the university would "endeavour to cooperate in every possible way in carrying out the aims of the Foundation."[19] The cornerstone of the new building was laid in January 1926. In June of the following year the School of Hygiene was officially opened, on time, on budget and with little fuss (Fig. 14.2). The contrast with the Brazil situation could not have been more dramatic.

In 1931, and again in 1938, the Health Division agreed to furnish additional endowments.[20] It was an indication that the scientific directors agreed with John Ferrell and Thomas Parran that Toronto had joined Johns Hopkins to become one of the chief training centers for public health officers on the North American continent, worthy of their continued support.[21] Neither Parran nor Ferrell mentioned the role Toronto was initially meant to play in the Caribbean; the colonial world of the British empire was no longer important to them.

## Lighthouse Schools of Nursing

Despite the Health Board's apathy toward nursing, the drive to improve the training of public health experts by the endowment and maintenance of schools of hygiene forced it to face the issue of the public health nurse and her training. A consensus existed that whatever role the public health nurse played—and there

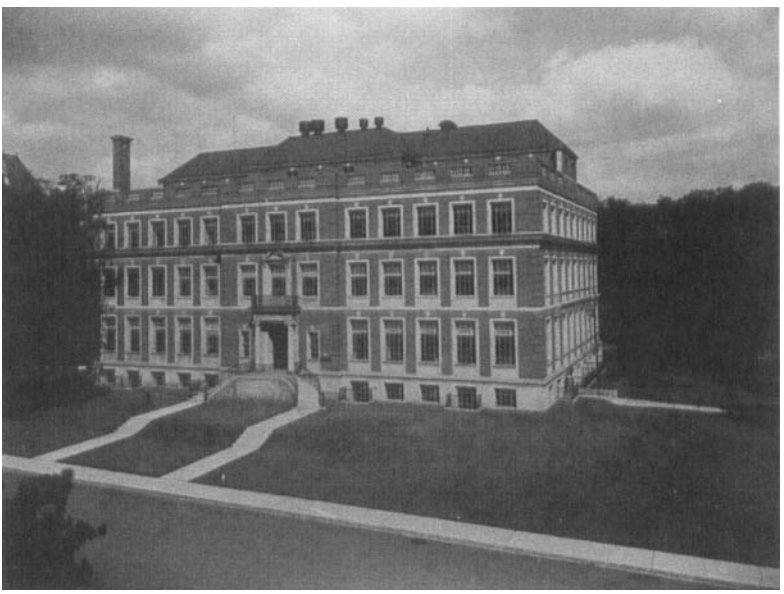

**Figure 14.2.** The Toronto School of Hygiene (courtesy of the University of Toronto Archives).

was much agonizing over that—she needed to be a fully-trained hospital nurse. It followed that any involvement of the Health Board would draw it into wider problems of hospital–and leadership-training for nurses in general—issues more in tune with the Division of Medical Education than the Health Board, and a source of constant confusion within the Rockefeller Foundation.[22]

There were upheavals in the nursing profession itself. Many intelligent and articulate nursing leaders in North America were critical of the training nurses received in hospital schools. Because student nurses served as cheap hospital labor, the training period was too long and educational needs were given low priority. To achieve a more professional status in which training by hospital apprenticeship would be replaced by independent schools of nursing, education would no longer be subservient to the service needs of hospitals. To these critics, the Anna Nery School in Rio was an antiquated model unsuitable for the training of a new cadre of nursing professionals.

The desire for professionalization became entwined with a parallel demand that nurses be recruited from a higher social class than before.[23] Even in Canada, where the standard of nursing care was certainly higher than in most countries, the author of a 1929 survey expressed alarm at the social profile of the profession. More than one-third of the nurses were daughters of farmers, while only 15% were daughters of professionals—why the daughters of farmers would make inferior nurses was not discussed other than in terms of their being undereducated, unprepared, and even, Heaven forbid, trade unionists.[24]

Nurses' leaders also argued that professional upgrading necessarily required leadership training, a concept much in tune with the thinking of the Rockefeller Foundation. Administrators and nurse supervisors required postgraduate training, either in the larger and superior hospital schools of nursing or, if possible, in university schools of nursing. In addition, because public health nurses were usually required to take some form of postgraduate training after their hospital diploma, they too were grouped among those destined for university nursing schools. By 1916 there were 15 diploma nursing schools in the United States attached to universities or colleges, but, like the hospital schools, they too lacked independence. Their nursing faculty were staff members of the local teaching hospitals, not university faculty, with prime responsibility for service, not student teaching.[25]

To further complicate matters, the board became involved in the training of public health nurses when what they were actually training for remained unclear. Were they to limit themselves strictly to preventive and educational work in the schools and homes, as favored by many physicians and practiced by the *visiteuses d'hygiène* in France? Or should they stand astride the division between curative and preventive medicine to practice bedside nursing and make early diagnoses of illness? If the former were the case, why should they require hospital training? It would be sufficient to train only "health visitors," social workers taught only to educate and demonstrate personal hygiene, as was the case in France.[26]

These issues remained unresolved when, in 1918, the foundation called together a conference on public-health nursing. It led to the appointment of Josephine Goldmark, a well-known expert in social research, to take charge of a nursing study. Her report was published in 1923.

## The Goldmark Report

With public health moving beyond community sanitation into personal hygiene, the role of the public health nurse needed to change, Goldmark said in her report. Education in personal hygiene plus the application of medical science had become the core of the new public health, and "ultimate victory" lay in direct personal contact between the public health nurse and the public. The public health nurse had become the "missionary to carry the message of health into each individual home." Goldmark believed that this teacher of hygiene should be a fully-qualified bedside nurse because, in practice, she would need to nurse home patients and detect early signs of illness.

Herein lay the dilemma. According to Goldmark, the bedside nurse was being inadequately trained and her status in society was well below where it needed to be. Nursing had to attract "young women of high capacity," but the typical hospital training school, with its drudgery, night duty, tough living conditions, and autocratic discipline failed to attract recruits of the "high type." This three-year apprenticeship system should be replaced by a two-year graded and progressive course in the theory and practice of nursing. Together with higher entrance standards, these shorter courses of higher quality would insure an increased supply of high-quality student nurses of the middle class.[27]

The Rockefeller Foundation was urged, as a matter of primary importance, to address the issue of leadership in nursing administration, teaching and public health by the generous endowment of degree-granting university schools of nursing. They were "the keystone of the entire arch," in which the elite student nurse would receive better training than was possible in hospital schools. Leadership was the issue, and the improvement of caliber a matter of "cardinal importance." This was music to the ears of those in the foundation who saw leadership training to be among their most important mandates.

But what was already taking place in France and Brazil did not accord with the recommendations of the Goldmark report. In France, Miss Crowell had opened her 10-month training schools for better qualified *visiteuses* as well as seven schools with a two year "bifurcated" program, and in Rio the first nursing class had been selected in the São Francisco de Assis Hospital.

This was the background when the trustees of the Rockefeller Foundation requested, in 1921, that Elizabeth Crowell make a survey of nursing training in Europe. In a sense her survey was in parallel with Goldmark's activities in the United States and both published their findings at much the same time. But the

difficulties were compounded when Crowell's recommendations differed significantly from Goldmark's.

## The Crowell Reports

In a formal sense Miss Crowell was probably one of the best educated of the Health Board officers (Fig. 14.3). A graduate of a private Catholic school in Ohio, she had been enveloped in European culture and languages, and was more aware than most that American methods could not simply be picked up and put down in Europe. In 1922 and 1923 Crowell visited Austria, Bulgaria, Czechoslovakia, England, Hungary, Italy, Poland, Rumania, and Yugoslavia, issuing her summary report in 1923. The picture she painted was all too familiar.[28] Very few "educated women of good social standing" were willing to become hospital nurses. Medical staff at most hospitals had a negative view of nurses, who received little social or

**Figure 14.3.** Elizabeth Crowell (courtesy of the Rockefeller Archive Center).

economic recognition. Since religious orders provided much of the nursing care in continental Europe, lay nurses were ill-paid and of the "servant class." The solution was to be found not in upgrading the quality of training within the established hospital schools of nursing, she said. Rather, a better class of woman could best be attracted to the profession by the backdoor, as it were, by offering, in all countries of continental Europe, a two-year program in special "bifurcated" schools, which had first operated in France during the Health Board's tuberculosis campaign (Chapter 3). In such schools, hospital and public health nurses would receive the same first year hospital training and only in the second year would each group "bifurcate" into the two areas. Such a program should, she argued, attract a better class of women of a higher social class, and although still minimally trained, they would replace the original poorly-trained health visitors and thereby eventually raise the general standards of nursing. No mention was made of leadership and degree-granting schools. The emphasis was on the grass-roots level with a program designed to produce large numbers of rather minimally, although better-trained public health nurses. In contrast to Goldmark, Crowell, a nurse herself, saw no point in establishing "handsome model schools" which many of the European countries would be unable or unwilling to finance.

In Britain, Crowell had to tread more carefully. Although critical of training given to public-health nurses, she was aware of "a halo that surrounds nursing traditions in England." These traditions had set the standard and created the mould for the training of hospital nurses throughout the world. "Nowhere," she wrote, "save in English hospitals will one find that universal effort to place the *patient's* physical and moral well-being above every other consideration." The intellectual content of British nurses training was small compared with the emphasis on character formation—duty, discipline, self-command, and hard manual labour. "The spirit of devotion, of service, of self-sacrifice was esteemed to be the one essential motive without which the nurse and her work could not hope to measure up to standard." But the system had defects, she warned, it had retained the traditions of Florence Nightingale without heed to modern developments. The four-year training program was far too long, and could be justified only by the demand of hospitals for cheap service; it did not give the student real nursing experience. The spirit of the times was changing, she wrote, and women in England as everywhere else were less willing to submit to discipline, military or otherwise. Some other method would have to be developed, similar to what was being done with success in the United States. Crowell nevertheless agreed that the British nurse "is disciplined, trained, and sympathetic and the world knows today no more capable, dependable hospital nurse than she has proved herself to be." The "mother country of modern nursing," she concluded, need not be considered by the Rockefeller Foundation at present; there were more pressing needs on the Continent.[29]

As was usual in such cases, the Rockefeller Foundation Trustees on receipt of the report, requested a second opinion from one of their own. The foundation's

secretary, Edwin Embree, was sent to Europe to assess the situation and suggest ways in which the foundation could play a role in nurse-training. He agreed with Crowell that "bifurcated" schools were a necessary first step. He, too, questioned the value of "exotic" or "handsome model schools" which the countries themselves would be unable to finance, and whose graduates would be unwilling to work in local conditions at feasible salaries.[30]

On December 5, 1923, when Embree's report was discussed, the foundation trustees decided to form a fourth division of the Rockefeller Foundation—the Division of Studies or "Division of Lame Ducks" as Embree once called it. It was to administer projects outside the specific programs and interests of the other divisions and would be directed by Embree himself. Of these, the nursing program was a major priority.[31] Rose and Russell, with their basic lack of interest in nursing, were only too willing to slough off this chaotic program to another division.[32] A program that would at the same time deal with elite university nursing schools in the United States, support a standard hospital school in Rio, and develop two-year bifurcated schools in France, could not be seen to be anything but chaotic.

Miss Crowell was not altogether in tune with the nurses' leadership in North America. She was herself a nurse-Anglophile and believed, with much justification at that time, that hospital nurses in Britain were superior to those trained elsewhere. Indeed, her preparation for the British nursing report seemed to have strengthened her admiration for the British nurse. She basically agreed with Lord Knutsford when he said in a letter to her that however much British methods did not jibe with those in the United States, "it cannot be overlooked that the nursing of the sick is better done in this country than in any other."[33] She did not agree with Knutsford's arguments that too much learning was dangerous for nurses in that they might question and threaten members of the medical profession, but it was not easy for her to criticize the training method that seemed to produce the best results. Knutsford's letter drew a more polite response from Crowell than would have been given by her American peers. She conceded that Americans overemphasized the development of women's careers to the detriment of patients, while the reverse was true in Britain. "Of the two positions," she wrote," yours is more logical, if one considers that the end justifies the means, and also if it *works*, for in the end any system must submit to the pragmatic test." She agreed that the British nurse was superior but attributed that to "the fibre of the woman herself," and not to her training, which, she thought, could not last much longer in its present form. In many countries she concluded rather dramatically, "they have servants doing nursing work, while in England they have nurses doing servants' work."[34] She also blotted her copybook when she told Vincent, "it makes a lot of difference if the primary emphasis is placed upon the development of a career with the care of the sick as a corollary—the trend of things in the United States seems to point that way—I believe the reverse is still true in England."[35] She realized that

this statement would be viewed as rank heresy and disloyal by many in the United States.

The heavyweights in the United States would have other ideas and Embree soon crumbled before the wrath of the U.S. nursing establishment. The crunch came during the summer of 1925 when Anne Goodrich and Lillian Clayton, directors of nursing at the degree-granting schools of Yale and the University of Pennsylvania respectively, met with Embree after returning from a trip to Europe. Both women had been heavily involved with the Goldmark report and had faced the same basic issue many times before. They saw Crowell and Embree as the enemy. Goodrich even accused Crowell of being ashamed to be associated with the nursing profession.[36] The time had come for the foundation to "blaze trails" by supporting institutions with high standards and low enrolments, which would serve as models in the field. They must do for nursing what they were already practicing in public health training for physicians.[37]

Embree told Crowell that he now agreed with what they had said. He had been opposed to "exotic" and "handsome model schools," but he now told Crowell that "we have I believe a special obligation to set standards well above the level that we may expect the country as a whole to maintain."—Embree was clearly an individual who agreed with the last person to whom he spoke! "In medical education," Crowell was reminded, "our attention has gone almost entirely to a small number of schools of the highest standard." The message was clear. "We have no obligation to assist moderately good schools of nursing," he told Crowell, "I think we would be entirely justified in reserving our assistance to a few lighthouse schools.[38] Miss Crowell was then recalled to New York for a nursing conference. She was asked by Vincent about the goal of the nursing program. Was it the production of a large number of nurses, or the training of leaders at a higher level than that to which the average school could expect to aspire?[39]

Crowell must have been unhappy to learn that Embree would be conspicuously absent from the conference, and that she was being made to stand alone on the firing line. Before his ignominious retreat, Embree wrote two memoranda to Vincent in which, while claiming to have more knowledge about nursing than any of the other officers (a good reason to be present at the meeting) he promised to "acquiesce in any unanimous decision reached by the group of directors in my absence." In the first memo he tried to give some support to Crowell; but he quickly corrected himself in the second. "I think we would be entirely justified in reserving our assistance to a few lighthouses."[40] The outcome of the conference was never in doubt. The Rockefeller Foundation did not exist to supply the rank and file of any profession, and the Division of Studies existed not to mass-produce nurses, but to train administrative heads and teachers. In 1927 the short-lived Division of Studies was disbanded, Embree left the foundation, and nursing found itself tossed hither and thither as the Rockefeller Foundation went through an important reorganization (Chapter 10).

This reorganization was of no help to the nursing program, now split between two divisions. Public health nursing went to the Health Division and nursing education became the responsibility of the Division of Medical Education. But with public health nurses viewed as hospital nurses with extra training, such a split made little sense and was not to last.

Richard Pearce, director of the Division of Medical Education, seemed unsure about his new nursing responsibilities. After some confusion, he eventually supported the lighthouse concept.[41] The fully-repentant Crowell and Pearce agreed there should be only three European lighthouse schools. One in London, affiliated with the School of Hygiene and Tropical Medicine and University College Hospital nearby, another in Austria or Germany, and the third in Lyon to serve France and French possessions, a sign that the needs of colonial countries had not been entirely forgotten in the move to Europe.[42] Of the three, only the one in Lyon was built, and that only after considerable delay: Pearce had put the nursing plans on hold in order to develop a medical school there. Money was not released until the Health Division completely took over nursing in 1931. The school opened late in 1933, after the Division of Medical Education had become the research-orientated Division of Medical Science.

The Health Division quickly made clear which schools it was prepared to support. The school had to be an integral part of a university with limited enrolment, where nursing education would be separated from nursing service, and where the principles and practice of public health were to be taught throughout the nursing program, not simply as a postgraduate afterthought.[43] When they wrote this outline, they were thinking of the Toronto School of Nursing, which was to become their true lighthouse school of the nursing world.

## Toronto. The Brightest Light

For 25 years the Rockefeller Foundation, mainly through its Health Division, kept a fatherly eye on the University of Toronto's School of Nursing. Far more than Yale and other U.S. schools, it became the model nursing institution against which all others were measured. To give but one example, in 1942 Mary Tennant sent a copy of the school's curriculum to Dr. Larraguiben, dean of the School of Medicine at the University of Chile, in an unsuccessful attempt to improve their school of nursing.[44] Toronto was, without question, the brightest of the model schools, to which were sent fully 39% of all IHD nursing fellows, nine times the number sent to Yale. These included a large number of black students from the United States.[45] This support was maintained despite the university's lack of enthusiasm for its nursing school.

There were two major reasons for the Rockefeller enthusiasm. First, the Toronto School of Nursing had a unique program that appealed directly to the Health Division. It was the only program in which the student nurse graduated with a diploma

in both hospital and public health nursing. In other programs, a diploma in public health could be obtained only after staying for an extra postgraduate year, having first qualified as a hospital nurse. But the second reason was probably the crucial one. The school's director, Kathleen Russell, came to be regarded by the officers of the Health Division as the world's finest nurse educator (Fig. 14.4). They became infatuated with her. (I do not think this is an exaggeration and it followed a Health Division pattern of judging a program by the caliber of the personnel involved). Realizing this, Miss Russell was able to gain support by playing the Health Division against the sometimes reluctant university, more or less forcing the university to continue support despite its obvious desire not to do so.[46]

Miss Russell may have been the world's best, but her message differed significantly from that of other high-profile educators in the United States. In the dichotomy between the British and American approaches, between the emphasis on service to the patient versus emphasis on the professional status of the nurse, Miss Russell seems to have stood with one foot planted firmly in the British camp and the other lightly resting in the United States. She was after all a Canadian, in the days before NAFTA and the present economic and cultural takeover. In the lead-up to the infamous 1925 meeting with poor Miss Crowell, Vincent had consulted Miss Russell, who bluntly told him that British patients received better care and that their nurses were more mature, better prepared, and more professional than their American counterparts. She was concerned lest the new American system led nurses to undervalue their ward experience because too much of their education was being secured in lecture halls. She agreed with Miss Crowell's program in France, and felt it "quixotic" to set up standards which the host country would find academic and impractical.[47]

**Figure 14.4.** Kathleen Russell (courtesy of the University of Toronto Archives).

Miss Russell had no sympathy with "pretentious aspirations on the part of the nursing profession," which found expression in the degree programs of many university schools of nursing.[48] As she told the 1936 graduating class at Toronto, what they had experienced was "not a pretentious claim for university frills," not an attempt "to dress up the art of nursing and thus to give it borrowed dignity. . . . We want no degrees. We are more content to let the other professions worry and argue about them."[49] To Miss Russell true dignity came from being well-prepared and giving satisfactory service, not from waving academic parchments. But for all that she was as adamantly opposed as her American colleagues to the usual hospital nursing schools where service took priority over training.

Kathleen Russell was destined to cast an intense light over the Health Division's nursing program. She was the third child of an Irish emigrant banker and a mother of French Huguenot descent who had met and married in Paris, Ontario, and then moved to Windsor, Nova Scotia where she was born in 1886. After graduating with a B.A. degree and the Governor General's Gold Medal from the University of Kings College, Windsor in 1904—the oldest university in the British Empire outside of Britain herself—she attended the University of Toronto for two years in order to take premedical science classes. In 1906 she was admitted to Toronto's Medical School, one of seven women admitted that year. But her ambitions were soon dashed. Forced to withdraw in 1907 because of suspected tuberculosis, she spent the next eight years at home in Windsor, including six months in the Kentville Provincial Sanatorium a few miles from her home. In those frustrating years she had to face the fact that a medical career was no longer possible for a woman with a history of tuberculosis, and in her search for a new career she turned to nursing. She entered the Toronto General Hospital's School of Nursing in 1915 as the oldest and most educated of the 164 students enrolled that year.[50] How a student with a history of tuberculosis could gain entry into nursing while being excluded from a medical school is not clear.

Working nine to twelve-hour shifts, six and one-half days each week with classes at the end of each day, Miss Russell was exposed to the typical apprenticeship training system. She remained healthy and graduated in 1918, again with the highest standing. After a one-year social service course at the university she took a position in the Toronto Public Health Department supervising home visits by public health nurses. When the Canadian Red Cross became interested in training public health nurses, and offered to finance departments of public health nursing in some Canadian universities, she was appointed head of the Department of Public Health Nursing at the University of Toronto, responsible for setting up a traditional one-year postgraduate course in public health for graduate nurses.

The program slowly expanded, and by the time the department moved into the newly-created Rockefeller-funded School of Hygiene in 1927, it offered extension courses and two diploma courses. One of these, the four-year undergraduate course, included both hospital and public health training; a graduate student was

awarded both a diploma in public health nursing and a diploma in hospital nursing from Toronto General. It was this course which attracted the attention of the Health Board. But Miss Russell felt that desirable changes in the program would require an independent school of nursing. In October 1928 she forwarded an outline of her proposals to Mary Beard, in charge of the Medical Division's nursing program, asking whether the Foundation would be interested in bringing these changes about.

She wanted, she wrote in a series of memoranda, a new building and a new residence in order to set up a school of nursing, "organized within the University," which would be independent of the Toronto General Hospital and thus free to experiment. The school would become, as she put it, "a laboratory for study of the educational problems of nursing: to experiment with new courses by means of small, selected classes. . . . a controlled experiment in nursing education."[51] In order to gain complete control over the new program, she told Beard, the school needed to be fully autonomous and financially independent, affiliated with the university but not a faculty within it. There were considerable delays brought about by the reorganization of nursing within the Rockefeller Foundation, and also by political changes in Ontario and at the university, including the retirement of President Falconer. Finally the government and the university agreed to renovate and furnish an old and beautiful university building, which opened in September 1933; the Health Division came through with a five-year annual grant sufficient to meet the yearly needs of the new school until 1937. Although the school, the scientific directors of the division noted, was doing work of a non-university character, it "should be looked upon as a laboratory for the study of the educational problems of nursing."[52] "We have in our School," Miss Russell wrote in her first annual report

> an experiment which is attracting much attention, for a reason that is very simple but profoundly disturbing in its novelty, namely, that the pupil in the School is here merely as a pupil. She is quite independent of the hospital financially, as she is paying the cost both of her living and tuition, her time belongs to herself and not to the hospital, hence this time of hers can be used wholly to study and to practise the art that she has come to acquire.[53]

By this time, Fred Russell and the Health Division seemed to be taking their nursing commitments more seriously. Russell had illustrated this new interest early in 1930 when he offered Miss Beard a position with the Health Division overseeing its nursing program (Fig. 14.5). She had turned down his original offer to be on a par with Miss Crowell, arguing that the nursing program required the position of associate director such that all projects would pass through her hands and she would have full access to Russell himself. Although some officers were opposed, Russell had no objection and in October 1930 the appointment was made—the only non-male appointment in the organization at that level. One can speculate that with nursing represented by two such powerful women, Beard in New York and Russell in Toronto, its status in the organization could not but be enhanced.[54]

**FIGURE 14.5.** Mary Beard (courtesy of the Rockefeller Archive Center).

Unfortunately, Miss Russell still faced considerable difficulty with the university, which led the Health Division to question whether the university was indeed committed to the school. But by playing the power of Rockefeller money, Miss Russell persuaded the university to play its part, and, effective July 1, 1939, the Health Division agreed to endow the school.[55] "This gift puts the school on a firm foundation," Kathleen Russell graciously thanked Sawyer in a Christmas eve letter, "and we can go on now with assurance of growth and development."[56] It must have been a fairly happy Christmas for Kathleen Russell and her Toronto school, dampened only by the war that her country now faced.

And that seemed to be that, as far as the Health Division was concerned. With the upcoming retirement of Mary Beard thoughts were being expressed that all existing nursing programs should be terminated. But that did not happen with the Toronto school. The school became a major postwar headache to the Health Division, but they stuck by Miss Russell and her school to the very end (see Chapter 16).

This funding for schools of hygiene and, to a much lesser extent, schools of nursing, dramatically changed the way the Health Board and Division distributed their money. When Frederick Russell took over the Health Board from Wickliffe Rose in the spring of 1923, approximately 70% of the budget had been allocated to the four major diseases: hookworm, malaria, yellow fever and tuberculosis, with far lesser amounts spent on public health education and county health units. By the time Russell retired 10 years later, the budget picture had changed quite dramatically. During the Russell years the disease budget shrank to 27% of the total budget while that allocated to public health education (including fellowships) increased from 8% under Rose to 35%. In the Sawyer years this pattern was maintained with both disease and public health education taking up each about one-quarter of the total budget (Table 1.2).

The decision to open schools in Europe was partly responsible for a swing in funding away from the impoverished countries of the tropics, where the Health Commission began its work, towards the relatively more affluent countries of North America and Europe. The awarding of fellowships to study at these schools shows a similar scenario. By the time the Health Division closed, 1877 fellowships had been offered in public health, by far the largest number of them (1284) in public health administration. But despite claims to be an international organization, over 500 of these fellows were citizens of the United States and another 177 were Canadian.[57]

## Notes

1. Minutes of IHB May 20, 1919. RAC. RG.1.1 S.401 B.1 f.8.
2. For details, L. Wilkinson & A. Hardy, *Prevention and Cure. The London School of Hygiene and Tropical Medicine: A 20th century Quest for Global Public Health* (New York: Keegan Paul, 1999); A. G. McBride, *The History of the Dreadnought Seamen's Hospital at Greenwich* (Greenwich: Seamen's Hospital Management Committee, 1970); G. C. Cook, *From the Greenwich Hulks to Old St. Pancras. A History of Tropical Disease in London* (London: Athlone Press, 1992); D. Haynes, *Imperial Medicine. Patrick Manson and the Conquest of Tropical Disease* (Philadelphia: University of Pennsylvania Press, 2001), Chap. 6; J. Farley, *Bilharzia, A History of Imperial Tropical Medicine* (New York: Cambridge University Press, 1991); *London School of Tropical Medicine and Hospital for Tropical Disease: Miscellanea, 1899–1927.* Library Archives, London School of Hygiene and Tropical Medicine; R. Acheson and P. Poole, "The London School of Hygiene and Tropical Medicine: A Child of Many Parents." *Med. History* 35 (1991): 385–408; D. Fisher. "Rockefeller Philanthropy and the British Empire: The creation of the London School of Hygiene and Tropical Medicine." *Hist. Education* 7 (1978): 129–143; L. Wilkinson, "Burgeoning visions of global public health: The Rockefeller Foundation, The London School of Hygiene and Tropical Medicine, and the 'Hookworm Connection.'" *Studies in Hist. Phil. Science Part C.* 31 (2000): 397–407.
3. *Report of the Post-Graduate Medical Committee.* (London: HMSO, May, 1921).
4. *Summary of Proceedings at a Conference between the Colonial Office and Representatives of the Rockefeller Foundation*, July 1921. RAC. RG.5.2 B.38 f.227.
5. "The Proposed School of Hygiene." Mond to Vincent, August 11, 1921. RAC. RG.1.1 S.401 B.2 f.10.
6. Minutes of IHB., October 25, 1921. RAC. RG.1.1 S.401 B.1 f.8.
7. P. Manson to W. Rose, April 1, 1922; Rose to Manson, April 15, 1922. RAC. RG.1.1 S.401 B.2 f.12.
8. "Preventive Medicine in London: the New School of Hygiene and Tropical Medicine." *Lancet* ii. 1929: 148; "London School of Hygiene and Tropical Medicine: the opening of the new building." *Lancet* ii. 1929: 175. See also Cook, *From Greenwich Hulks,* Chap. 10.
9. The full story of the school is told in P.A. Bator, *Within Reach of Everyone. A History of the University of Toronto School of Hygiene and the Connaught Laboratories* (Ottawa: The Canadian Public Health Association, 1990).
10. J. D. Rockefeller to Trustees, December 18, 1919. RAC. *Rockefeller Foundation History*, Source Material. Vol. 5.

11. P. Collier & D. Horowitz, *The Rockefellers. An American Dynasty* (New York: Holt, Rinehart: 1976). p. 116. The Ludlow massacre is discussed fully in R. Chernow, *Titan. The Life of John D. Rockefeller* (New York: Random House, 1998).
12. James Greenlee, *Sir Robert Falconer. A Biography* (Toronto: University of Toronto Press, 1988).
13. This story has been best told by M. Bliss, *The Discovery of Insulin* (Chicago: University of Chicago Press, 1982).
14. George Vincent, "The British Empire and World Health." Address to the Empire Club of Canada (Toronto: The Empire Club, 1930).
15. A. B. Macallum to Pearce, August 15, 1922. RAC. RG.1.1 S.427 B.10 f.79.
16. "Report of the Special Committee to Enquire into the Administration of the University of Toronto." University of Toronto Archives. Falconer Papers. A-67-007 B.81.
17. P. Oliver, *Public and Private Persons. The Ontario Political Culture 1914–1934* (Toronto: Clarke Irwin, 1975); P. Oliver, *G. Howard Ferguson: Ontario Tory* (Toronto: University of Toronto Press, 1977).
18. R. D. Defries, *The First Forty Years 1914–1955: The Connaught Medical Research Laboratory* (Toronto: University of Toronto Press, 1968). Defries, "The Connaught Medical Research Labs 1914–1948," *Can J. Public Health* 39 (1948): 330–344.
19. Falconer to Vincent, June 26, 1924. RAC. RG.1.1 S.427 B.24 f.231.
20. Details in RAC. RG.1.1 S.427 B.24 f.234, 235.
21. Ferrell to Fitzgerald, February 18, 1939. RAC. RG.1.1 S.427 B.24 f.235; T. Parran and L. Farrand, *Report to the Rockefeller Foundation on the Education of Public Health Personnel*, October 28, 1939. RAC. RG.1.1 S.200 B.185 f.2222.
22. Nursing and the Rockefeller Foundation is discussed by S. Abrams, "Brilliance and Bureaucracy. Nursing and Changes in the Rockefeller Foundation (1915–1930)." *Nursing Hist. Review* 1 (1993): 119–137; "Seeking jurisdiction: a sociological perspective on Rockefeller Foundation activities in nursing in the 1920s," in A. M. Rafferty et al (eds.), *Nursing History and the Politics of Welfare* (London: Routledge, 1997).
23. These issues are discussed in Barbara Melosh, *The Physician's Hand. Work, Culture and Conflict in American Nursing* (Philadelphia: Temple University Press, 1982); Susan Reverby, *Ordered to Care. The Dilemma of American Nursing, 1850–1945* (New York: Cambridge University Press, 1987); Kathryn McPherson, *Bedside Matters: The Transformation of Canadian Nursing 1900–1990* (Toronto: Oxford University Press, 1996).
24. G. M. Weir, *Survey of Nursing Education in Canada* (Toronto: University of Toronto Press, 1932).
25. B. Bullough & V. Bullough. "Collegiate nursing in the United States," *International Nursing Review* 10 (1963): 41–47.
26. These issues are discussed further by K. Buhler-Wilkerson, "False Dawn: The rise and decline of public health nursing in America (1900–1930)," in E. C. Lagemann (ed.) *Nursing History: New Perspectives, New Possibilities* (New York: Teachers College Press, Columbia University, 1983).
27. *Nursing and Nursing Education in the United States* (New York: Macmillan, 1923). The actual Goldmark report, "Report of a Study of Nursing and Nursing Education," is included in the larger volume. The preliminary run-up to this report is told succinctly in the Rockefeller Archives *History. Source Material.* Vol. 8, p. 2076 RAC.
28. "Nursing and Nurse Training in Europe. Digest of Reports of Miss E. Crowell." May 23, 1923. RAC. D.R. 275. There were also separate reports issued on Austria (RG.1.1 S.705 B.2 f18); Bulgaria (RG.1.1 S.711 B.1 f.11); Czechoslovakia (RG.1.1 S.712 B.5

f.40); England (RG.1.1 S.401 B.34 f.427); France (RG.1.1 S.500 B.9 f.100); Hungary (RG.1.1 S.750 B.2 f.21); Italy (RG.1.1 S.751 B.4 f.56–57); Poland (RG.1.1 S.789 B.3 f.31); Rumania (RG.1.1 S.783 B.3 f.22); and Yugoslavia (RG.1.1 S.710 B.4 f.37).

29. The relationship between the Rockefeller Foundation and British nursing is discussed briefly in Anne Rafferty, "Internationalising nursing education during the interwar period," in P. Weindling (ed.), *International Health Organisations and Movements 1918–1939* (Cambridge: Cambridge University Press, 1995).
30. E. Embree, "Suggestions for Foundation Cooperation in Nurse-training in Europe," 5 December, 1923. RAC. D.R. 282. Embree to Vincent, September 25, 1923. RG.1.1 S.700 B.19 f.137.
31. For details see Robert Kohler, *Partners in Science* (University of Chicago Press, 1991).
32. Russell and Rose's general hostility to nursing was apparent during a meeting on nurses training attended by Embree, Vincent and Rose, January 12, 1922. RAC. RG.3 S.908 B.15 f.163.
33. Lord Knutsford to Crowell, November 2, 1922. RAC. RG.1.1 S.401 B.34 f.427.
34. Crowell to Knutsford, November 20, 1922. Ibid.
35. Crowell to Vincent, August 27, 1922. RAC. RG.1.1 S.700 B.19 f.137.
36. E. Werminghaus, *Anne W. Goodrich, Her Journey to Yale.* (New York: Macmillan, 1950). Vickers, "Frances Elizabeth Crowell and the politics of nursing in Czechoslovakia after the First World War," *Nursing History Review* 7 (1999): p. 68.
37. Conference between Goodrich, Clayton & Embree, August 26, 1925. RAC. RG.1.1 S.700 B.19 f.139.
38. General Policy in Nursing Education. Embree to Crowell, August 26, 1925. Ibid.
39. Vincent to Crowell, September 4, 1925. Ibid.
40. Memo for Dr. Vincent. August 29, 1925. Ibid.
41. Pearce, "Suggestions for Possible Policy for Nursing in Europe." June 8, 1927; Pearce to Crowell, August 27, 1927. RAC. RG.1.1 S.700 B.20 f.141.
42. Crowell to Pearce, April 29, 1927; Pearce to Crowell, June 3, 1927. Ibid. For the fate of the English school, see Rafferty, "Internationalising nursing education."
43. IHD plans for nursing. April, 1934. RAC. RG.3 S.908 B.15 f.163.
44. M. Tennant to Dr Larraguiben, January 13, 1942. RAC. RG.1.1 S.300 B.5 f.50.
45. M. Tennant, "Summary of the nursing program." Statement prepared for the Scientific Directors, September 1, 1942. RAC. RG.3 S.908 B.15 f.166. Minutes of faculty meetings in the 1940s reveal, however, that there was a quota on black students and that the Toronto General Hospital would not accept black students on its wards, with the result that the school planned to send its black students to Detroit for hospital training! University of Toronto Faculty of Nursing, Teaching Staff Minutes, (1940–1944). University of Toronto Archives, A 73-0053/001.
46. Rondalyn Kirkwood makes this point strongly in her "Blending vigorous leadership and womanly virtues: Edith Kathleen Russell at the University of Toronto, 1920–52." *Canadian Bull. Hist. Med.* 11 (1994): 175–205. She argues that she was able to obtain her way by playing the role of the cultured, genteel lady and was much less successful when she became assertive and confrontational.
47. Memorandum of Conference with Miss Russell and Miss Gunn. September 18, 1925. RAC. RG.1.1 S.700 B.19 f.139.
48. K. Russell. "Amended plans with regard to a proposed School of Nursing." Prepared for the Prime Minister of Ontario. March 1932. RAC. RG.1.1 S.427 B.12 f.95.
49. K. Russell. Graduation Report, 1936. RAC. RG.1.1 S.427 B.12 f.99; Melosh *"The Physician's Hand"* discusses in detail the degree conflicts in the United States brought

to the fore by E. Brown's *Nursing for the Future* (New York: Russell Sage, 1948), who argued for the removal of hospital diploma schools of nursing and their replacement by baccalaureate schools, what Melosh calls the "critical divide" in nursing history; J. Farley "To Degree or nor to Degree: the International Health Division and the Toronto School of Nursing," in *Critical Issues in American Nursing in the Twentieth Century: Perspectives and Case Studies*, D. Stapleton & C. A. Welch (ed.). (Guilderland, New York: The Foundation of the New York State Nurses Association, 1994); Kirkwood, "Blending vigorous leadership." argues that her anti-degree stance was simply a tactic and part of a decision to proceed cautiously to the degree status. I do not agree. She was passionately opposed to vocational degrees in general and was extremely reluctant when the school at last agreed to award degrees in 1942. Perhaps her failure to toe the party line explains why nursing histories, even those dealing with Canada, generally ignore her.

50. Details of Miss Russell's life from Helen Carpenter, *A Divine Discontent Edith Kathleen Russell: Reforming Educator* (Toronto: Faculty of Nursing, University of Toronto, 1983). Canadian universities each present a yearly Governor General's Medal to the graduating student with the highest standing.
51. K. Russell, "Proposal regarding a School of Nursing in the University of Toronto," October, 1928; " Proposals regarding a School of Nursing." September, 1929; RAC. RG.1.1 S.427 B.12 f.92, 93.
52. Scientific Directors Meeting. Minutes, June 13, 1932.
53. K. Russell, *Annual Report of the School of Nursing of the University of Toronto,* 1933–34. RAC. RG.1.1 S.427 B.12 f.97. The School is described in Helen Carpenter, *A Divine Discontent.* Also J. Farley, "Building a school of nursing with Rockefeller money: Three Nova Scotians at the University of Toronto. *J. Roy. Nova Scotia Hist. Soc.* 3 (2000): 85–104.
54. Details of Miss Beard's appointment in RAC. RG.3 S.908 B.15 f.163. Sarah Abrams, "Brilliance and Bureaucracy:Nursing and Changes in the Rockefeller Foundation 1915–1930," *Nursing History Review* 1 (1993): 119–137 argues that the new research focus of the Health Division led it to turn its back on nursing. I have to say that the position of nursing within the Rockefeller Foundation remained so confused that any generalizations become almost impossible to make. The Health Division certainly stood by the Toronto School of Nursing to the very end.
55. These problems are discussed in J. Farley, "Building a school of nursing with Rockefeller money."
56. K. Russell to W. Sawyer, December 23, 1939. RAC. RG.1.1 S.427 B.12 f.101.
57. *Directory of Fellowship Awards for the years 1917–1950* (New York: Rockefeller Foundation, 1951).

# 15

## Rough Seas: Prague, Rome, Tokyo, and Calcutta

If, after its experience in Brazil, the Health Board decided to devote its major effort to opening only model schools in Europe and North America, they failed. In the end they came to support a wide range of schools, more in line with Rose's earlier vision than with Welch's. According to Andrew Warren, Assistant Director of the IHD, who surveyed the schools of hygiene in 1939, they had a range of purposes. Some were service-oriented, while others emphasized the training of physicians for public health work. Research was not totally forgotten as the Health Board tried "to create centers in which scientific and routine work could be carried on in the interests of matters connected with hygiene."[1] Funding began with Prague in 1921, and others quickly followed: London in 1922; Warsaw, 1923; Budapest, Zagreb, and Toronto, 1925; Rome, 1930; and Tokyo in 1933, to name but a few of them. In 1938 the Health Division stepped into the All India Institute of Hygiene, which had been endowed a few years earlier by the Division of Medical Education. These schools differed significantly from each other. London emphasized research, with no service functions whatsoever. In Rome and Toronto, service played an important part. Others, such as Prague, Budapest, and Zagreb, were basically teaching and service institutes. Their role was also to transplant American scientific models of public health, with all their imperfections, to these and other countries. The faculty of these schools had to be trained at the three most important schools: Johns Hopkins in Baltimore, Toronto, or London.

By stumbling into the endowment of schools in Europe and Tokyo at that time, the Health Board came face to face with the rising tide of fascism. In India they found themselves caught in the midst of political crises as the country clawed its way towards independence from the British Raj, compounded by the threat posed by Japanese troops on its northeast border. The board may have found calm waters in London and Toronto; they were not to find such conditions at other institutes.

## Prague

The dark days of Czechoslovakia unfolded before their eyes, after Selskar Gunn of the Rockefeller Tuberculosis Commission in France—responding to a request from Dr. Alice Masarykovà, daughter of the Czech president and president of the Czech Red Cross—visited Czechoslovakia in 1919. He had been asked to look at the tuberculosis picture and to advise the Minister of Health on the administration of public health. Gunn believed that "the patient is obviously quite sick and presents such a complex of symptoms that no single word or term is sufficient to express the diagnosis," and was concerned over the tensions between Germans, Czechs and Slovaks. But he was much impressed by the people. He advised the foundation to consider undertaking work in the country and to develop an institute of hygiene there.[2] Negotiations culminated in the opening of the State Institute of Hygiene on November 5, 1925, with Dr. Ku'cera, rector of Masaryk University, as its first director. It was not a Johns Hopkins-like institution. It produced sera and vaccines, provided routine laboratory service and trained physicians and others for public health work.[3] But problems soon arose, so that by the early 1930s Strode was expressing disappointment with the institute; it had failed to provide leadership, and the heath-officer training was "deplorable."[4]

Despite such problems, Pearce agreed in 1930 to provide funding for the establishment of a school of nurses for public health and social welfare within the Institute of Hygiene.[5] Geared toward educating workers rather than university-educated leaders, it, in fact, represented a clear example of the type of school from which the foundation now wished to dissociate itself. The decision had come in response to a request of the Czechoslovakian National Health Council for a bifurcated nursing school in Prague, and it represented a victory for both Dr. Masarykovà and Gunn, both of whom had been pressing for greater emphasis on social problems.

Construction of the Masaryk State School of Public Health and Social Work began in 1932, when the Health Division was once again in charge of the nursing program. Directed by two women physicians, Dr. Houzvicova and her assistant Dr. Gedlickova, it was inaugurated in March 1936. Dr. Houzvicova wrote Mary Beard in her still tentative English, that the school "has come to live. Young buoyant girls are strolling through the wide and beautiful halls of the Masaryk State

School anxious, to learn all they can, what might add sometime to the future benefit of our country."[6]

Given that it did not comply with the Health Division's concept of a lighthouse school, complaints soon followed. Crowell did not like the name of the school, saying the division had agreed to help fund only a school for public health nurses, not a school for public health and social welfare work.[7] Mary Tennant, who had taken over the nursing portfolio from Mary Beard, expressed unhappiness over its "amazing curriculum" and complained that the social work group had dominated its planning.[8]

Behind these complaints lurked mounting Czech-German antagonisms. The Minister of Health, the inaptly named Dr. Czech, was one of the leaders of the German minority, dickering, in the words of Crowell's diary, for an increase in German power. Crowell was overwhelmed by his apparent willingness to cooperate, "all smiles and dripping with honey." But his honey did not run over to his Czech subordinates, and repeated attempts to make life difficult for Dr. Houzvicova and to dismiss her would probably have succeeded but for the continued support of Miss Crowell, who persuaded Houzvicova that Czech would not retain his job much longer.[9] She was correct. He was, amazing as it appears today, a Jew among a crowd of increasingly Nazi-orientated anti-Semitic Germans. By the spring of 1938 he was gone. By then, however, Crowell was reporting an increase in Jew-baiting within the school and the presence of German students wearing white not gray stockings to show their new political creed.[10] At the end of the year, with the German annexation of the Sudetenland and the beginning of the country's dismemberment, the German students left. Another threat to increase the social content of the curriculum brought Crowell back to Prague only a few months before the 1939 crisis broke. When the Germans took over the entire country, Hynek Pelc, a former fellowship-holder who had become director of the Institute of Hygiene in 1938, and his Czech assistant, were both fired, and two Germans appointed in their place. Things were to get much worse. After the war, the Health Division learned that Pelc had been executed by the Germans on the steps of the institute.[11]

If the decision to open a school of hygiene in Brazil was the first attempt in a long-term goal to train public health experts in tropical countries, the switch to Prague, with no links at all to the tropics, illustrates again the ad hoc nature of so many of the Health Board's policy moves. Why Prague? One clue comes from Rose, who, early in 1920, arrived to look over the situation. Like Gunn, he was much impressed by the Czechs but was also conscious of their potential role in combating political extremes. "The Czechs," he reported, "are by established tradition, next after the Swiss, the most democratic people on the Continent. Placed in the heart of Europe, surrounded on all sides by the wreckage of monarchies and a fermenting mass of communist chaos, they are the one surest defense against both these extremes and the best guarantee of the final triumph of order under democratic government throughout this region."[12] Perhaps that was one reason

for the switch to central Europe, for in Czechoslovakia, at least, the Health Board was able to make a stand against two enemies: German nationalism and Soviet communism.[13] They seemed to have drifted into expanding their role in Europe at the expense of tropical countries, without being aware this represented a fundamental shift of policy.

Having agreed to fund one school of hygiene in Europe, the way was clear to move into other European countries. Italy, where the Health Board was already engaged in malaria work, was an obvious choice. The decision was made without reference to the fact that Italy, unlike Czechoslovakia, had links to the tropical world through its African colonies.

## Rome

Early in 1928, George Strode, then stationed in Paris in charge of the Health Division's European theater, had suggested that a new institute of hygiene would be the "best entering wedge" for the development of a public health service in Italy.[14] The call had come from Italy's hygiene professors who realized that candidates for positions in Italy's health departments were few in number, and ill-trained. Research laboratories and a new school for postgraduate instruction in hygiene was needed in Rome, they said.[15]

Lewis Hackett sought the support of Italian political figures, meeting with A. Messea, director of public health, and with Paulucci di Calboli Barone, private secretary to Benito Mussolini. A Rockefeller-funded Italian study group, enthusiastically endorsed the idea but Hackett had misgivings.[16] "The Institute is not at all an end, but a means; a fine institute does not by any means guarantee a fine service." And, as he told the Italians, "having the best institute of public health in Europe does not depend on the size of the Rockefeller gift, but on subsequent developments." Hackett even suggested to Russell that funding should perhaps be delayed until other public health activities had been organized. This was the field officer speaking, of course.[17]

George Vincent was worried too, although only one month after expressing his reservations, he and Fred Russell agreed that a "very substantial sum" be appropriated for the Rome institute.[18] By January 1930 plans had been finalized and the Health Division agreed to partly fund the cost of a new building and its equipment. Opening ceremonies were fixed for October 28, 1933, the 11th anniversary of the "March on Rome" by black-shirted fascists, which resulted in Mussolini's ascent to the office of prime minister and his later assumption of dictatorial powers. Strode was enthusiastic. "I consider this project one of the most important and significant that it has been my privilege to transmit," he told Russell. "It is a logical sequence to five years of IHD activities in Italy and the material evidence of a new public health spirit which our organization has helped to foster.

In the Institute rests the hope of the future."[19] After some delays, the great day arrived on April 21, 1934—the anniversary of the founding of Rome—and in November, Hackett's *Stazione Sperimentale per la Lotta Antimalarica* moved into the new building to complete what had always been planned as a teaching, analytical and now research institution. Hackett's misgivings had been quite forgotten (Fig. 15.1a).

By then totalitarianism had reached into the schools and universities, including the new institute. University professors were forced to take an oath of loyalty to the fascist regime and swear to train students to be loyal and to refrain from associating with hostile groups. A picture of the opening ceremony shows professors were required to wear black fascist uniforms (Fig. 15.1b). The *Istituto di Sanità Pubblica* was clearly a monument to El Duce. Professor Enrico Morelli, president of the National Fascist Syndicate of Doctors, made that plain. The institute demonstrated the "incessant constructive fecundity of the fascist regime." It was "a temple in which synthesis and harmony of scientific ideas will find a worthy seat and forge for the defence of the nation's health." The impressive building, far grander than those to be built anywhere else, met with his approval. "The setting of the marbles, lights that pour through ample windows, immediately impress the visitors with their sumptuousness."[20]

Fascism did not seem to trouble the officers of the Health Division. Indeed, only Jerome Greene seems to have even mentioned the problem. "You are doubtless

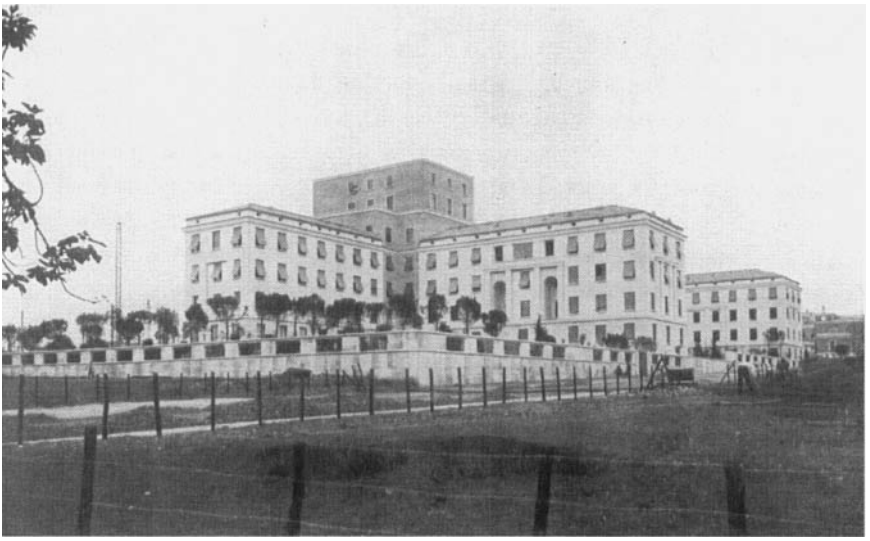

**FIGURE 15.1a.** Istituto di Sanità Pubblica, Rome (courtesy of the Rockefeller Archive Center).

**FIGURE 15.1b.** Benito Mussolini opening the Istituto (courtesy of the Rockefeller Archive Center).

aware," he wrote Mason, "that it stands as a memorial of fascist initiative and enterprise, and that as far as the public is concerned there is very little Rockefeller in the picture. This is amusing, but will doubtless do no harm."[21]

In 1939, Hackett reported to Sawyer that the institute was "far from the sort of school of public health with an organized faculty, which we had in mind." The teaching was inadequate, the institute had become divorced from the health department, and the laboratory was being used to research substitutes for imported articles, whether medical or not.[22] Nothing could be done without Mussolini's consent. But with Europe in crisis, Mussolini's mind was on other matters, and the Health Division was unable to salvage its investment.

One might conclude that the *Istituto di Sanità Pubblica* stood as a monument to the Health Division's political naivete and its indifference to the events in Italy as Mussolini gained control of the country. During the war the institute functioned as a chemical research unit and never as a school of public health. Indeed, when it opened after the war under a new director, the biochemist Domenico Marotta, it continued as a chemical institute while the Italian government opened a new National School of Public Health.

A similar scenario was to unfold many miles away when the Health Division decided to build an institute of hygiene in Tokyo. There, at least, the division was

forced to examine its involvement in a totalitarian society, something it never did in Italy.

## Japan

Japan had not been an important location on the Health Board's map, but in May 1924 a board commission arrived in Tokyo to survey the country's public health situation, with a view to building a school of hygiene there. The visit by Victor Heiser, John Grant—on loan from the Peking Union Medical College—and Frederick Russell, caused unease in the foundation. Its president, George Vincent had little regard for Japan and distrusted its rulers. It would not be stretching matters too far to say that the survey was a result of a series of almost bizarre accidents.

The Health Board was first drawn in the Japanese direction by an American, Charles Beard. In September 1922 he had been invited by Tokyo's mayor, Viscount Goto, to help lay out plans to improve the city's health service.[23] Beard passed on to Vincent the mayor's request for assistance in launching a public health program in Tokyo. "In the present state of Japanese-American relations," Beard wrote, "I think this invitation is significant. If you could come and help, the influence of your example will be profound."[24] Goto, too, made the most of the opportunity, telling Beard the idea would promote friendly relations between the two countries and would "attract the attention of nearly 200 million people to a generous act of cooperation between Japanese and American citizens."[25] Vincent was not ready to support the idea and was clearly vexed by Beard's actions. In February 1923 Wickliffe Rose seemed to put an end to the matter when he told Beard that Goto's scheme, centered on building a hospital, did not fall within the scope of the board's work. But he failed to completely close the door, hinting that at some future date the Health Board might assist in some way.[26]

That future date was not long in coming. On September 1, 1923, Tokyo was hit by a powerful earthquake, fires and a tidal wave. John Rockefeller and the Spelman Fund gave financial assistance through the Red Cross. Vincent stepped back into the picture when he told Beard that while the foundation never engaged in emergency relief, it would be willing to consider any request for help by the Japanese government, once the emergency had passed.[27] This news was passed on to the Japanese government. Slowly, like it or not, the Health Board was being pulled into the Japanese orbit.

There was considerable Japanese opposition to foreign intervention, but Goto, then minister of home affairs, was appointed chair of a Committee of Cooperation, and a request was made to foundation representatives for a meeting. The committee was particularly interested in rebuilding the Imperial Medical School which had been damaged in the earthquake. But Pearce, director of the Division of Medical Education and a major player in the Japanese story, was not keen. Pearce, author of a 1921 report of medical education in Japan, believed medical

schools needed to be reorganized on American lines. He was aware that German influence on Japanese medical education had been revived following World War I. Drastic reorganization might have been possible if the Imperial Medical School had been destroyed by the earthquake. "Unfortunately," Pearce said, this had not been the case and it would be impossible to do anything fundamental.[28]

Vincent hesitated. Finally he announced that no further action would be taken unless the Japanese government formally invited a delegation to visit Tokyo. The foundation, he told the Japanese ambassador, would only send a survey team if the government were able to give assurances that its presence was truly needed.[29] But in a letter sent the following day, Vincent told Rudolf Teusler, director of the St. Luke's Hospital of the Episcopal Church, an avid proponent of a Rockefeller-funded health program (and somewhat of an irritant to Vincent) that the foundation really wished to break off entirely any relationship with the Japanese. "We are increasingly doubtful," Vincent wrote, "as to whether any very important opportunities are offered for doing the kind of thing which we are accustomed to do."[30]

But, as was often the case, Vincent could not control the medical barons within the Health Board. In February, Heiser had prepared a memorandum instructing Grant about what should be done when the anticipated invitation arrived.[31] A month later the foundation learned that Dr. Tsurumi, Goto's son-in-law and a powerful member of the inner government, believed there was a greater need for a school of hygiene than for a medical school.[32] Thus, when the Japanese invitation finally arrived, the focus had shifted from a Rockefeller-funded medical school in the hands of the Division of Medical Education towards a school of hygiene centered on the International Health Board.

Grant's and Heiser's 1924 survey was positive in its assessment of Japan's standard of public health. No country was as well endowed with laboratories, and its undergraduate medical students were taught hygiene and public health more effectively than in the U.S.[33] The report was so glowing that one has to question yet again what the Health Board was doing there; the Japanese seemed perfectly capable of organizing their own affairs. Dr. J. Yamada, director of the Central Sanitary Bureau, latched on to the high infant-mortality rate, the only negative finding in the health survey, to request assistance in the building to train health officers and nurses, at an estimated cost of $19 million (1990).[34] Less than six months after the survey, the Health Board allocated up to $11.5 million (1990) for a new school. One gains the impression that the Health Board was being taken for a very long ride.

If the way ahead seemed clear at the time, there were clouds on the horizon which could at any time jeopardize the scheme. The traditional Japanese system of government was breaking down in the face of an increasingly powerful commercial and industrial middle class. Traditionally, influential senior officials advised the emperor on the choice of prime minister and cabinet, and were thus able to place their nominees in powerful administrative positions. By the 1920s mem-

bers of the Diet, or lower house, traditionally the center of opposition to the ruling oligarchy, had formed new political parties and were being co-opted into the cabinet. In 1924, Kato Komei, former foreign minister and ambassador to London, and an advocate of parliamentary rule, formed the first government of a political party, the Kenseikai—said by Heiser to have "socialist tendencies"—in 1925 it introduced universal male suffrage from the age 25.

This mixture of parliament and an entrenched oligarchy presented problems to the Health Board. Many Japanese politicians wished to turn away from involvement with the West and would oppose any American funding on principle. In addition, the Japanese ambassador to the United States informed Vincent that his government would appoint Prof. Mataro Nagayo, of the medical college at the Imperial University and director of the Laboratory for Infectious Diseases, as director of the planned new school.[35]

Grant, for one, was horrified to hear this and fired off a message to New York advising Heiser that such an appointment would be unsound. Nagayo would not resign his other positions and would therefore lead the new school into a subservient relationship to his laboratory, where public health officers were currently trained.[36] The issue was further complicated by the existence of two medical factions—the Kitasato and the Imperial cliques. The Kitasato Institute had opened in 1892 as a laboratory under the direction of Dr. S. Kitasato who had studied in Germany under Robert Koch. In 1899 this private laboratory had been turned over to the state to become the Institute of Infectious Diseases. But in 1914, against Kitasato's wishes, the institute had been transferred to the Imperial University where it was now run by Nagayo. The Kitasato staff then resigned en masse and opened a new Kitasato Institute, whose supporters at that time controlled the Central Sanitary Bureau. This group—much favored by Grant—included Dr. Yamada; it had been the first to recognize the need for a new school of hygiene, in contrast to the Imperial group, under Nagayo, which showed little interest in a new school. But, rather than see the opposing group secure an advantage from the Rockefeller gift, the Imperial group tried to block the negotiations—until the government promised the directorship to Nagayo.

Furthermore, Grant complained, Nagayo was the leader of the ultra-pro-German medical group that was directing Japanese medical penetration of China as part of a plan to extend influence over China. As a member of the Peking Union Medical College, Grant was exercised over this. To appoint Nagayo as the new director, would, he wrote, make it impossible for the board to use the new institute in its future work in China. Still, to oppose Nagayo's appointment was tantamount to dictating the internal policies of a foreign power, "an exceedingly dangerous action should it ever be entered upon."[37] Fred Russell expressed "mental reservations" regarding the appointment.[38] To make matters worse, Vincent learnt from Miyajima, a physician-politician, member of the Progressive Liberal Party, and Grant's choice for the directorship, that Nagayo suffered from nephritis which

kept him bedridden for weeks at a time. Nagayo was also a member of the governing Kenseikai Party, which, Vincent was told, incorrectly as it turned out, would not continue much longer in office.[39]

Grant, Heiser, Russell and Vincent were now very uneasy. The idea of appointing Nagayo, who was originally hostile to the new school, who was ill, who was a member of a minority party temporarily in power, and who was a member of the group opposed to the Kitasato group in charge of the health department, "does not auger well for the success of the school."[40] What to do? The most obvious tactic was to instigate delaying tactics until such time as the government fell and a new cabinet had been appointed—or perhaps even to abandon the project altogether, as Vincent clearly wanted.[41] But the government did not fall until the financial crisis of 1927. By then, tired of waiting, the Health Board had abandoned the Japanese project.[42] And that might have been that.

But it was not to be. Three years later Heiser accidentally met Miyajima again, this time in Geneva. The Japanese government, Heiser learned, was still anxious to establish a new hygiene institute and, having a large majority in the Diet, would remain in office for at least four years.[43] Despite what had happened before, Heiser was attracted toward work in what appeared to be a politically stable context. But, while Miyajima was correct in his first assessment, he turned out to be wrong in the second.

Following an invitation from the Japanese government, Heiser and Grant met the minister of home affairs and the director of the Central Sanitary Bureau in Tokyo, both of whom pressed for a new institute, expressed regret that previous negotiations had been terminated, and agreed, in contrast to what had happened in the 1920s, that it would have the unequivocal support of the Japanese medical profession. Heiser was further assured that the previous enmity between the Imperial and Kitasato medical groups had abated, and even agreed that Nagayo, having apologized for his previous conduct, would now be acceptable as the new director of the proposed institute. On the advice of Heiser, the government agreed to appoint a commission to study the issue and submit plans for the project.[44]

The Japanese attitude had changed, Heiser told Russell, and there is now "genuine interest" and a "keen desire" for a new school. The U.S. ambassador to Japan, Cameron Forbes, was delighted; he had become openly critical of the harsh U.S. policy towards Japan and saw support for a new school as a means to improve American–Japanese relations. The Japanese would meet every condition that Heiser suggested, he wrote, and "to say that he has been cordially received is putting it mildly. I should say they had jumped down his throat."[45] To Heiser's and Grant's consternation, the harmony and goodwill were not to last. Conflicts again broke out between the Imperial and the Kitasato group, now attached to the Keio University Medical School. With Nagayo's recurrent illness, Miyajima and Nagayo's assistant, Yonetsugi Miyagawa, representing the Kitasato and Imperial groups respectively, began vying for the director's job.[46]

In August 1931 the commission called for the establishment of an institute of public health as a training and research center in Tokyo, with public health nurses to be trained in a new St Luke's School of Nursing.[47] Two months later, the Health Division approved the expenditure of nearly $13 million (1990) and Forbes called on the foundation's president, Max Mason, to express his support "in the interests of Japanese-American friendship." And then the bomb fell. At a meeting of foundation trustees in December 1931, it was decided to postpone action because of the difficult economic and political situation. When forwarding this news to the Japanese government in late December, they avoided any reference to the political situation and alluded only to the worldwide depression as the reason for their negative decision.[48]

The reason, however, was clearly and understandably political, for the promised political stability did not occur. The Seiyukai government, which had won a narrow election victory in 1928, came into conflict with the Japanese high command; it was replaced by the Kenseikai party (now renamed the Minseito party) which, after winning the 1930 election, seemed, as Miyajima told Heiser, to be set for many years of stable government. But it too soon ran into trouble, in part through its willingness to cut naval armaments after signing the London Naval Treaty.[49] It also faced increasingly powerful conservative, antidemocratic and nationalistic groups, such as the "patriotic societies," with connections to army officers who advocated reform by force, opposed modernization and Western influences, and favored overseas expansion. Gradually these groups overwhelmed attempts at parliamentary democracy, assassinating the prime minister in November 1930. In September 1931, the Japanese army, a law unto itself, invaded Manchuria and set up a new puppet state. The government in Tokyo, faced with a fait accompli could do nothing; Manchuria had become a private empire of the Imperial Japanese Army.[50] This appears to have been the event that impelled the trustees of the Rockefeller Foundation to postpone—but not to cancel—its association with the Japanese government.

Displeasure over the decision was voiced by Cameron Forbes, Alan Gregg, Selskar Gunn now vice president of the Rockefeller Foundation, and even John Grant. Cameron Forbes accused the foundation of putting the United States in a bad position. He reminded Heiser how important it was for the oriental to "save face." "I wonder if your people want to jeopardize the whole of an enterprise of this sort," he continued "to maintain this new policy which they seem to have adopted. All that was needed was an acceptance of it in principle, a conditional acceptance, a moderate amount of cash to begin with so that the work could start up. Ponder these thoughts," Forbes urged.[51]

They clearly did. At the end of 1932, the foundation, for reasons that are not apparent, reversed its decision and decided to allocate two-thirds of the promised millions towards a new institute. This decision came at a time when yet another government leader had been assassinated during an attempted military coup, and

when the parliamentary government was being forced into making more and more concessions to the military. Now it was Raymond Fosdick's turn to complain. A Wilsonian Democrat, trustee and future president of the foundation, he was at the time with the League of Nations in Paris, talking about action against the Japanese incursion into China. He urged postponement of the grant, on the grounds that any announcement would seriously embarrass efforts to find a solution. [No solution was found, and Japan withdrew from the League in February 1933]. Meanwhile, Rockefeller Jr., still chairman of the board of trustees, urged going forward "without further conversations with outsiders." In February 1933 an uneasy Max Mason informed the Japanese of the foundation's change of heart. It was a choice between evils, he told Fosdick, made easier, perhaps, by a dramatic fall in the value of the yen. No public announcement was made; all agreed that to do so would lead to "misinterpretation."[52] Grant, meanwhile, was given permission to resume serious negotiations with the Japanese authorities.

Problems piled up on Grant's shoulders. The Japanese refused to name a director until the building had been opened, leaving room for political maneuvering between prospective nominees from the Imperial University and the private Keio University. When construction of the new building got underway in November 1934 (Fig. 15.2), competition for the directorship intensified dramatically after Nagayo, by then president of the Imperial University and front-runner for the job,

**FIGURE 15.2.** Institute of Public Health in Tokyo, under construction (courtesy of the Rockefeller Archive Center).

blotted his copybook. He played into his enemies' hands after members of his law faculty attacked the concept of the divine right of the emperor.[53]

The political situation continued to deteriorate, culminating in February 1936 in another attempted coup d'état by army officers. It failed, but it had become apparent that no government could survive without the inclusion of serving officers; by the end of the decade, the army could make and veto cabinet appointments. Grant seems to have lost his critical facilities as far as the Japanese were concerned. He excused the attempted coup and noted, without any qualms, that the army had become interested in public health after many of its troops had been invalided home from Manchuria with tuberculosis. He also approved of the powerful Minister of War who was pressing for a centralized ministry of health.[54] In January 1937 the home minister added his voice in support of public health. "The health condition of the people has the most important bearing upon the rise and fall of national prosperity and human welfare," he intoned. "For this reason the present government has taken up the promotion and betterment of public health as one of the important national policies."[55] What some of these policies were became apparent six months later when the Japanese again invaded China. Nevertheless plans for the new institute went on as if nothing had happened, with an opening date set for the first day of 1938. Because of a 200% to 300% increase in the price of steel brought about by the rush to world rearmament, the government had run out of money, and requested, with Grant's support, an additional 312,000 yen to make up the shortfall.[56]

## Totalitarianism

Caught up in this web, foundation trustees met on November 30, 1937, a few weeks after receipt of the Japanese request, and supported a confidential statement, "The Foundation versus Japan," delivered by their recently-appointed president, Raymond Fosdick.

The foundation had actually addressed the problem a few years before. In January 1934, the trustees informed the Paris office that they were no longer prepared to approve new grants to German institutions directly linked to the government, as that would be seen as a tacit endorsement of Nazi policies. They were not opposed, however, to supporting the work of individual scholars. Anxious to clarify this policy—which was directed less to the Health Division than to other divisions more heavily involved in Germany—Max Mason wrote to Strode a few months later. He said grants would not be made to any German individual if the work was likely to become "warped" in a way that would make it part of a political, partisan, or militaristic effort. "There is confusion in the public mind between the scientific work pursued by an individual and his political opinions and expressions," he wrote, but, on the other hand, he warned, "it would be unwise for the Foundation to subject itself to the criticism which would accrue if we were supporting the work of an ar-

dent pro-Nazi." He added that it would also be unwise to support the work of an individual who was persona non grata with the German government.[57] The confusion was finally clarified in 1937 when Fosdick announced that the foundation would withdraw from Germany. This was not because of disapproval of totalitarian philosophy. In answer to a lawyer's question over the foundation's role in Germany in which particular mention was made of an article in the *New York Times*, headed "Germans Studying Germ War Tactics," Fosdick wrote:

> The attempt by the German Government to impose a uniform ideology has destroyed the possibility, in a number of related fields, of objective and disinterested scholarship. . . . We have declined to make appropriations, not because of our disapproval of the totalitarian philosophy, but because that philosophy makes impossible the kind of scientific research that we want to support.[58]

This was to be the foundation's basic stand throughout the period of Fosdick's leadership (1936–1948), and it was repeated at the trustees conference on Japan. "It is an ironical coincidence," Fosdick began, "that on the very day the Japanese airplanes destroyed Nankai [a university in Tientsin, southeast of Peiping, to which the foundation had contributed toward the construction of a science building], the Treasurer's Office of the Foundation wrote a check for $74,000 towards our pledge of $1 million for the new Public Health Institute in Tokyo." This act, he said, raised the issue of future relationships with countries "whose political and social policies seem to clash with those widely accepted in this country."

"Has Japan written herself out of the orbit of our interests?" he asked. The answer, he argued, must reflect what the foundation had already done in Germany and Italy. Where government attempts to impose a uniform ideology had destroyed the possibilities of "disinterested scholarship," as had happened in Nazi Germany, then the foundation would curtail its work. Germany had ruled itself out, "by making objective scholarship impossible." But if no such action were taken, the foundation would continue to cooperate, whatever the form of government. "Scientific considerations alone have thus far provided the governing principle." Thus, the foundation had endowed the construction of an institute of public health in Rome, despite its fascist government. Did such action imply an endorsement of the government? Did offering money to Rome imply sympathy with Missolini's government? This was difficult to say, Fosdick admitted, but on balance "our reputation for scientific objectivity and detachment make it unlikely we will be charged with political bias, at least in responsible quarters." The government of Italy, he argued, had no relevance to the foundation's interest in malaria. "We do not say that we will have no relations with countries whose governmental acts we condemn."[59]

What then of Japan and its request for additional funding? Paying no heed to the memorandum he had just written, he attempted to extract the foundation from

Japan on the grounds that the request had come too late for the 1938 budget year "in spite of our earnest desire that the Institute in Tokyo should be ready for the larger opportunities which you have created for it," and thus "we regretfully find ourselves unable to take action on your request."[60]

Grant, who had supported the Japanese request, was not fooled by Fosdick's excuse. It contravened the objective policy laid down by Fosdick, he told Sawyer. To cut off Japanese-Rockefeller collaboration was, in Grant's view, "an emotional and nonobjective" response. Sawyer replied that they had not excluded future assistance, but merely drawn back because of current uncertainties. Echoing the words of Fosdick, he told Grant that the Health Division "is officially quite oblivious of governmental changes except as they interfere with progress in public health," and hoped that cooperation would be given from time to time "as evidence of our objectivity."[61]

The Rockefeller program was in disarray. If they were to follow the criteria laid out by Fosdick, support for the institute in Tokyo should have been continued on the grounds that no action seemed to have been taken against "objective scholarship," and the government was fulfilling its pledge to develop and use the new institute. Japanese actions in China should have been irrelevant to the foundation's public health policy. But in the real world, such a policy would have been impossible to maintain. As Sawyer admitted, he was worried that the Foundation would be seen as partisans of the Japanese.

In July 1938 Professor Hanio Hayashi, a 65 year-old pharmacologist and former dean of the Imperial University Medical School, was appointed director of the institute. Nagayo had become embroiled in even more serious political problems after half the College of Economics at his Imperial University had been forced to resign. He recommended Hayashi for the post, realizing that his own chances had become minimal. And because of financial and equipment problems exacerbated by the Sino-Japanese war, and the foundation's refusal to offer more money, the opening of the institute did not take place until May 11, 1940. According to Marshall Balfour, regional director of the Health Division, a tone of serious attention to research prevailed. Its equipment and facilities were equivalent to those in Rome and better than anything available in the United States.[62]

Representatives of the Rockefeller Foundation were absent from the opening ceremony, and the U.S. ambassador excused his absence on the grounds of a previous engagement. But, whether they liked it or not, the Health Division had funded an institute which was to play some small role in the health of the Japanese military—strong supporters of the new institute—and who, 18 months later, were to unleash their air force against U.S. vessels in Pearl Harbor. Japanese military action then expanded up to the borders of India, where John Grant had been appointed director of the All-India Institute of Hygiene in Calcutta.[63]

Tokyo had turned out to be a terrible mess.

## The All-India Institute of Hygiene

In 1938, the Health Division again stepped into troubled political waters after assuming responsibility for running the All India Institute of Hygiene and Public Health in Calcutta. They took over this responsibility from the Division of Medical Education. This division's representative in the Far East, William Carter, had completed a survey in 1928 and proposed a new institute of hygiene, with the understanding that the Imperial government would place its management under the Indian Research Fund Association.[64] After complex negotiations, an agreement was reached with the association and Lieutenant Colonel Alexander Stewart—formerly professor of hygiene at the Calcutta School of Tropical Medicine—was appointed as the new director.[65]

This move to involve the Indian Research Fund Association brought the All-India Institute into the quagmire of Indian politics.[66] In the India Act of 1921, eventual dominion status was more or less guaranteed after a transitional period of so-called Dyarchy. Increased Indian control was brought about by the transfer of certain government departments, such as education and public health, to provincial control. There they would be run by elected Indian ministers, accountable to elected provincial legislatures. Other departments, however, such as finance and law, would be "reserved" and left in the hands of non-elected officials of the Imperial government and provincial governors. In theory, this acted as a safety valve to turn Indian's attentions to local subjects leaving the British Raj to deal with the really significant issues.[67]

This transfer of medical education and public health to the control of provincial governments created an administrative nightmare for the All-India Institute in Calcutta. Indian ministers in Bengal Province controlled the Calcutta School of Tropical Medicine,[68] with which the institute was closely tied. A new layer of bureaucrats was imposed on the institute when its management passed to the India Research Fund Association, made up of British members and their allies in the Indian establishment, and situated in Dehli, hundreds of miles from Calcutta.[69]

Indian politicians insisted and won representation in the association, with members drawn from the legislative assembly, other medical school faculties, and the Indian Medical Association. It meant, Stewart told Carter, conferring power on a majority of non-official people who would likely vote en bloc against any proposal of the Imperial government. Lieutenant Colonel J. W. D. Megaw, director of the School of Tropical Medicine, believed that this new governing body was in violation of the agreement with the Rockefeller Foundation; the government should either return the Rockefeller funding or agree on a governing body for the All India Institute separate from the Research Fund Association.[70]

By the end of 1931, Stewart agreed to support a small and separate governing body for the institute with three elected and five ex-officio members. As many quickly realized, the key to control lay in the make up of the three elected members.

Sir Leonard Rogers took the line that members of the Research Fund Association had no right to elect members to the institute's governing body; he urged, instead, that the elected three be nominated by the other ex-officio five. This, of course, was an absurd idea. The legislative assembly which controlled the financial purse, would never vote money to the governing body unless it had at least some political control over it. Thus while the India Office in London, who knew the limits of their power, supported the idea of five official members and three members to be elected by the India Research Fund, Carter took Roger's position that the three be nominated by the five.[71] But after all had agreed that two of the elected three be medical men with public health experience (an arrangement that seemed to have guaranteed additional white faces on the governing body), the All India Institute of Hygiene and Public Health was finally opened in December 1932 by Lord Willingdon, Viceroy of India.

But the battle for control had only just begun. In 1934, a threat by the India Research Fund Association to partially withdraw funding, brought an angry reaction from Carter, who had shown no sympathy toward Indian desires to control their own country. The Indians, he correctly noted, were making every effort, fair or foul, to get the British out, and the politicians of Bengal, the state in which Calcutta lay, "by their adroitness make Tammany politicians by contrast appear like bungling amateurs." Blinded by intense nationalism and hatred, Carter continued, and caring little for the quality of the Indian institutions, their only objective was to get rid of the British and replace them with "incompetent Indians." The British, he went on, are attempting to save what they can from the wreckage, but "there can be no doubt of the fact that all medical colleges in India have deteriorated in proportion to the extent to which they have been Indianized." Thus he urged the trustees of the Rockefeller Foundation not to put any more money into India in view of the fact that the grant made to the All India Institute in 1928 had been obtained under false pretences and the government of India had not kept faith with its agreement.[72] This, of course, did not happen. An American institution could hardly walk out of an increasingly democratic India while staying in an increasingly totalitarian Japan, especially when, at the time, the American government was putting pressure on the British to come to terms with the Indian nationalists.

Further complications came in September 1935 with the retirement of the institute's first director, Lt. Col. A. Stewart. He was temporarily replaced by Dr. R. B. Lal, an Indian who became acting director until a permanent replacement could be found. Here lay the dilemma. The British expressed dissatisfaction with Lal and demanded a British replacement, while the Indians wanted an Indian in charge. The situation was bad, William Jacocks, the Health Division's representative in India, told Sawyer, and the delay is only "feeding the political maw."[73] It certainly was. By then the Congress Party, following the India Act of 1935 (India's last London-made constitution), had won a landslide victory in the 1937 elections. The Indians had gained considerable power and would be even more unwilling to allow a Briton to assume the directorship of the institute.

The seeds of what was to follow were sown at a meeting in 1937, when the idea of an American compromise director was first put forward. Jacocks was probably the first to suggest that someone from the Health Division take on the job, and told Sawyer that the British had jumped at his suggestion.[74] George Strode, in charge of the Health Division's Paris office, realized that John Grant would make an ideal candidate.[75] After years of deadlock, the appointment committee agreed to a five-year appointment for Grant, during which time a suitable Indian director would be selected and trained. Grant arrived at his new post in July 1939 just in time to witness the British declaration of war on Germany on India's behalf. Perhaps, as a Canadian and a Commander of the British Empire, Grant was acceptable to the British in India, who regarded the Americans with a great deal of suspicion at that time.

It did not take long for Grant to realize just what he had stepped into. Plans were afoot to move the All-Indian Institute to Dehli, the capital, on the grounds that—while the School of Tropical Medicine was a Bengali institute—the All-Indian was supposed to be just that and should be rebuilt adjacent to a new medical school and hospital, as part of the planned University of Dehli. Grant favored the move. "The Institute cannot function satisfactorily until control has been academically rationalized," he said, and this meant either placing the institute in Delhi or transferring control to the Bengali government. "Taking the long view," Grant concluded, the transfer to Delhi seems an opportunity that cannot be ignored.[76]

Grant was overwhelmed by the "asinine system of administration, and the blind adherence to form" as practiced by the huge Indian bureaucracy. The place was full of "superclerks," he complained, "whose larger than professional salaries can be justified only through concocting delays in administrative changes." Thus Irishman Colonel Edward Cotter of the Indian Medical Service—also the public health commissioner in Dehli and a member of the India Research Fund's governing body—came to act as the de facto director of the institute, even though stationed 800 miles away. He was, according to Grant, a typical Indian-style bureaucrat who lacked anything "other than routine bureaucratic thinking or action," the "safest" and most "pussyfooting individual" Grant had ever met.[77] Affairs came to a head in 1940 when Grant decided the Malik health unit in Singur, which the Health Division had opened in 1939, should be used as the institute's rural training area. But the unit had been "transferred" to the Bengali government, creating nightmarish bureaucratic difficulties.[78] Cotter told Grant the new arrangement would require an additional fee from each student, an impossible request because it had not been announced in the institute's prospectus; to do so would require permission from both Dehli and the Bengal government. And that, Cotter told him, would require two years notice! Once again, in the words of Grant, Indian bureaucracy had "run amok."[79]

More was to follow. In March 1940, Grant told Strode 19 urgent issues had been referred to Dehli for approval and none had been acted upon. These ranged from the admission of foreign students to a pay revision for a draftsman, the ap-

pointment of a temporary stenographer, and the syllabus for part I of the Diploma in Public Health, which, Grant believed, needed drastic revision.[80]

Differences also existed between Cotter and Grant over the role of the institute. Cotter saw it as a teaching and service-orientated institute, and denied that the government of India had committed itself to support any research role for it. Grant, together with the Health Division, was anxious to see it develop as an academic institution with a research component. On those grounds he was unhappy over the quality of the students; only about 10% of them were interested in public health, he wrote in his diary, and the rest took classes simply to add letters to their names. Few entered the public health field, and of those that did, "there is not one chance in 1000 that a graduate could inaugurate the slightest departure from the norm," the system of administration being so rigid. They all have the mentality of clerks, he complained.[81]

When Grant returned from a short leave at the end of 1941, he had virtually decided it would be his last year. But 1942 was not the year to quit. Japanese armies were standing at the borders of India, which had been virtually denuded of defense capabilities when the bulk of its magnificent army was sent to fight in North Africa and Greece.[82] By March, 500,000 refugees had arrived in Calcutta from Burma, and over 800,000 non-essential people had fled the city. Furthermore, when Churchill, seen as the most imperialistic of all British leaders and the least sympathetic to Indian desires, took over, all provincial ministers of the Congress Party resigned and violence against the British increased. To further enflame Indian opinion, Churchill and Roosevelt signed the Atlantic Charter in August 1941 which promised "the right of all peoples to choose the form of government under which they will live," and piously hoped that self-government would be "restored to those who have been forcibly deprived of them." But, as Churchill told the House of Commons a few days later, this charter did not apply to India and Burma.[83]

With the rising tide of Indian nationalism and the failure of the Stafford Cripps mission, Gandhi initiated his Quit India movement, seen by the British as open rebellion. As Gandhi told the people of India, "The mantra is, 'Do or Die.' We shall either free India or die in the attempt. We shall not live to see the perpetuation of slavery." Surprisingly, none of these events drew a response from Grant. Grant had no option but to stay and run the All-India Institute which, during the war, suspended its normal work to become a blood bank and a center for the manufacture of the Cox anti-typhus vaccine.

## The Bhore Committee

This was not the end of Grant's work in India. In October 1943 the government moved to accommodate the demands of Indian nationalists by setting up the Health Survey and Development Committee, under the chairmanship of Sir Joseph Bhore, an officer of the Indian Civil Service. This Bhore Committee, as it was called,

indicated to Grant government interest in medical and health matters, as they slowly began to realize that postwar independence was a virtual certainty. The committee was empowered to survey health conditions in India and to make recommendations for future development, and shortly after it was formed Grant accepted an invitation to join.[84]

A few months earlier, Grant had published a pamphlet called *The Health of India.*[85] In it, he argued the case for social medicine. The disease-centred approach of Indian medical policy needed to change, he wrote, "as it is recognized that disease is as much a result of social and economic maladjustments as of infections." The pamphlet was filled with ideas drawn from social medicine that went far beyond the narrower concerns of socialized medicine—i.e. the provision of medical care irrespective of income or health status and at no direct cost to the patient. Proponents of social medicine realized that good health required far more than adequate medical care, although that, of course, was necessary. They called for reform in all areas inimical to good health, whether economic or social. It had gained much favor in wartime Britain, culminating in the 1942 appointment of John Ryle to the professorship of social medicine at Oxford. What constituted this new discipline was made clear by Ryle several years later in a radio broadcast over the South African Broadcasting Company:

> We know that the greatest savings of life and many of the great improvements in popular health, especially in infancy, childhood, and young adulthood, come not from medicines and surgical operations and hospitals but from improvements in sanitary, domestic, nutritional and working conditions.[86]

Thus proponents of social medicine were in the forefront of those demanding social change—social security, better food and housing, better recreational facilities and better education.[87] These ideas were promoted by the Bhore Committee.

How such policies could be made to apply to millions of illiterate and poorly-fed villagers in rural India Grant did not say. Neither did he discuss the enormous health costs of a system which tolerated such social inequalities, made even worse by the Hindu caste system. He never went as far as to suggest that some of India's major health problems would best be solved by putting their priorities into radical land reforms, minimum wage and child labor laws, stable grain prices, a massive rechanneling of educational funds into primary education, and primary health care—as the state of Kerala later managed so successfully to do.[88] Nevertheless, even Grant's views in *The Health of India*, were at odds with the message put out by the majority of the Health Division's officers.

To Grant, the Bhore Committee provided an opportunity to enlighten Indian health officials on social medicine and other international trends in health care, and perhaps to suggest a postwar agenda for the Health Division. His section of the committee's report discussed social medicine, outlined the steps being taken

in Great Britain to bring in a comprehensive health service. It praised the Soviet system not only for the provision of free medical care but for placing the control of housing and industrial conditions under the aegis of the People's Commissariat of Public Health.

The committee's lengthy report called for a state health service, and "the elimination of unemployment, the provision of a living wage, improvement in agricultural and industrial production, the development of village roads and rural communications. . . . A frontal attack upon one sector alone can only end in disappointment and a waste of money and effort."[89] The committee was concerned, too, about the poor distribution of medical personnel and by the lack of adequately-trained medical faculty in the medical colleges. They recommended the establishment of an All-Indian medical institution—what Grant privately referred to as an "Indian Johns Hopkins"—which would admit no more than 50 students a year and would "provide a steady, if limited, stream of teaching personnel of the highest quality."[90] The committee was divided in its rejection of less elaborately trained "Feldsher" practioners on the Soviet model. The final report included a "minutes of dissent" signed by 5 of its 25 members, who believed the limited resources available should not be concentrated on the production of the "most highly trained type," and argued that U.S.S.R. experience would be "a more helpful guide."[91] Grant, despite viewing himself as a medical Bolshevik, was not one of the signatories; in that sense, at least, he was a true son of the Health Division.

By the time the Bhore Committee had reported, the British Labour Party had won its postwar election victory and events quickly moved to their climax in Indian independence followed by the bloody and tragic partition two years later. Four months before that election Grant had resigned from the institute and was succeeded by Dr. C. Lakshmanan, whom Grant had earlier dismissed as yet another example of "the Cotter type of non-imaginative administrator."[92] By withdrawing in 1945, the Health Division removed itself from Indian affairs and was to play no part in shaping India's destiny as it once fondly imagined itself doing.

The All-India Institute continues to exist, but perhaps the Health Division's major impact on Indian affairs has resulted from Grant's involvement with the Bhore Committee report. It has, according to some, acted as a health blueprint for the country, even though most of its recommendations have yet to be implemented. The model is essentially western, dominated by a hospital-based curative approach.[93] "This report needs reprinting and wide dissemination," Dr. N. H. Antia recently argued, "for it should be incumbent reading for anyone concerned with the planning and implementation of the health services of our people; not forgetting the medical profession."[94] But it would not be Grant's last chance to inform the Health Division about socialized and social medicine. Immediately after leaving India he was appointed by the Health Division to look into these very issues.

## Notes

1. A. J. Warren, "Confidential Report on Institutes of Hygiene and Schools of Public Health in Europe." October 31, 1939. RAC. RG.1.1 S.200 B.185 f.2223.
2. S. K. Dunn to Rose, May 28, 1919 RAC. RG.1.1 S.712 B.3 f.9; Dunn, "Report of a Visit of Investigation to Czecho-Slovakia," June 1–14, 1919, RAC. RG.1.1 S.712 B.3 f.16; S. Gunn "Case History: The Republic of Czechoslovakia." May 6, 1921. RG.1.1 S.712 B.2 f.11.
3. Cable from Dr M. Srobar to IHB, October 1919; Heiser to Srober, June 18, 1920; Srobar to IHB, February 26, 1920. RAC. RG.1.1 S.712 B.2 f.9 and B.1 f.4.
4. These problems are discussed in many letters to and from Prague. RAC. RG.1.1 S.712 B.2 f.7; Strode's Diary February, 1933.
5. Memorandum Minister of Health to Paris Office, October 15, 1930. RAC. RG.1.1 S.712 B.5 f.41; E. Vickers, "Frances Elizabeth Crowell and the politics of nursing in Czechoslovakia after the First World War." *Nursing History Review* 7 (1999): 67–96 discusses the Czech scene at much greater depth.
6. Houzvicova to Beard, March 14, 1936. RAC. RG.1.1 S.712 B.5 f.41.
7. The Minister of Health informed Crowell that naming confusion was brought about because there was no Czech equivalent for public health nurse.
8. Memo. Mary Tennant, 29 June, 1937. RAC. RG.1.1 S.712 B.5 f.41.
9. Crowell's diary, March 18, May 1, 1937.
10. Crowell's diary. May 9, 1938. Talking to a Czech-Canadian who was in Prague at the time, I have been unable to substantiate Crowell's belief that the stockings signalled Nazi beliefs.
11. Details of these events are detailed in R.M. Taylor, "Notes on visit to Czechoslovakia, November 30 to December 3, 1938"; Newssheet from American Friends of Czechoslovakia, February 15, 1941; Paris Office letter, August 24, 1945. RAC. RG.1.1 S.712 B.2 f.7, 8
12. Rose and Gunn, "The Public Health Situation in Czechoslovakia." February 26, 1920. RAC. RG.1.1 S.712 B.3 f.16.
13. These issues are discussed in Paul Weindling, "Public Health and Political Stabilization: The Rockefeller Foundation in Central and Eastern Europe between the Two World Wars," *Minerva* 13 (1993): 253–267. In the end, institutes of hygiene were established in all East European countries except Germany, the Soviet Union, and the Baltic States. They became part of centralized government-run public health bureaucracies and experienced rapid decline in the face of political disruptions and ethnic clashes. The Warsaw Institute is discussed in Marta Balinska, "The National Institute of Hygiene and public health in Poland, 1918–1939." *Soc. Social Hist. Med.* 9 (1996): 427–445.
14. Strode to Russell, April 16, 1928. RAC. RG.1.1 S.751 B.1 f.5.
15. Minister of Interior to L. Hackett, enclosed in Hackett to Strode, January 30, 1930. Ibid.
16. "Origins of the Idea," and "Implications of the New Plan," included in Hackett to Strode, January 15, 1930. Ibid.
17. Hackett to Russell, August 9 and September 27, 1928. Ibid.
18. Vincent memo to Russell, October 11, 1928; Financial agreement, November 21, 1928. Ibid.
19. Strode to Russell, February 7, 1930. Ibid.
20. E. Morelli, "The Institute of Public Health." *Le Forze Sanitarie* 3 (1934): 737–41.

21. J. Greene to Mason, October 20, 1932. RAC. RG.1.1 S.751 B.1 f.6.
22. Hackett to Sawyer, February 7, 1939. RAC. RG.1.1 S.751 B.1 f.7.
23. Beard's report to Goto, December 5, 1922. RAC. RG.1.1 S.609 B.1 f.7.
24. Beard to Vincent, December 29, 1922. Ibid.
25. Goto's comments included in Beard's letter to Vincent.
26. Rose to Beard, February 6, 1923. RAC. RG.1.1 S.609 B.1 f.7.
27. Vincent to Beard, September 6, 1923. Ibid.
28. Pearce to Houghton , December 21, 1923. Ibid.
29. Vincent to Japanese Ambassador, January 10, 1924. Ibid.
30. Vincent to Teusler, January 11, 1924. Ibid.
31. V. Heiser, Memorandum for the Japanese Health Survey. February 4, 1924. Ibid.
32. R. S. Greene luncheon with Tsurumi, March 18, 1924. Ibid.
33. "Public Health Survey of Japan," May 12 to June 23, 1924. Ibid.
34. J. Yamada to Heiser, June 11, 1924. Ibid.
35. I. Yoshido to Vincent, November 13, 1924. Ibid.
36. Grant to Heiser, November 17, 1924. Ibid.
37. Grant to Heiser December 4, 1924. Ibid.
38. Russell to Gregg, November 18; Gregg to Russell, December 5, 1924. Ibid.
39. Vincent interview with Miyajima, January 5, 1925. RAC. RG.1.1 S.609 B.1 f.8.
40. Russell and Heiser. Interview with Japanese Ambassador, April 16, 1925. Ibid.
41. Vincent noted in his diary June 17, 1925 that he wanted to abandon the project.
42. The International Health Board abandoned the project on March 29, 1927. RAC. RG.1.1 S.609 B.1 f.8. With the government expected to fall in 1926, both the government and the Kitasato group employed delaying tactics.
43. Heiser to Russell, April 18, 1930. RAC. RG.1.1 S.609 B.1 f.8.
44. Conferences held by Drs. Heiser and Grant with Home Department, November 2–4, 1930. Ibid.
45. Heiser to Russell, April 18, November 7, 1930; Cameron Forbes to Colonel A. Woods, November 4, 1930. Ibid.
46. Grant to Heiser, May 30, 1931. RAC. RG.1.1 S.609 B.2 f.10
47. "Outline for the Institute of Public Health," August 12, 1931. Ibid.
48. Mason to Akagi, director of Sanitary Bureau. December 29, 1931. Ibid.
49. The London Naval Treaty of 1930 cut the shipbuilding and ship armaments in the world's major navies, and, in the words of C. Barnett, "completed the demolition work on British seapower." Details in C. Barnett, *Engage the Enemy More Closely: The Royal Navy in the Second World War* (London: Penguin, 1991).
50. W.G. Beasley, *The Modern History of Japan* (London: Weidenfeld, 1963).
51. Cameron Forbes to Heiser, February 16, 1932. RAC. RG.1.1 S.609 B.2 f.11.
52. Cable from Fosdick, December 30, 1932. RAC. RG.1.1 S.609 B.2 f.11; Mason to Fosdick, February 15, 1933; Gunn to Ohshima, director of Central Sanitary Bureau, February 21, 1933; Gunn to Mason, March 16, 1933. RAC. RG.1.1 S.609 B.2 f.12.
53. Leach to Russell, April 8, 1935. RAC. RG.1.1 S.609 B.2 f.15.
54. Grant to Sawyer, June 30, 1936. Ibid.
55. Speech by Home Minister during visit by Sawyer, January 1, 1937. RAC. RG.1.1 S.609 B.2 f.16.
56. Grant to Sawyer, June 7, 1937; Hazama to Sawyer, November 1, 1937. Ibid.
57. Memo to Paris Office, January 31, 1934; M. Mason to Strode, April 16, 1934. RAC. RG.1.1 S.717 B.7 f.36.

58. Fosdick to B. Johnstone, October 21, 1937. RAC. RG.1.1 S.717 B.7 f.36.
59. R. Fosdick, "The Foundation versus Japan." Special Trustees Conference November 30, 1937. RAC. RG.3 S.900 B.23 f.172.
60. Fosdick to Hazema, December 17, 1937. RAC. RG.1.1 S.609 B.2 f.16.
61. Grant to Sawyer, March 4, 1938; Sawyer to Grant, April 23, 1938. RAC. RG.1.1 S.609 B.3 f.17.
62. M. C. Balfour to Sawyer, May 21, 1940. Ibid.
63. Grant had earlier commented on Japanese military support for the Tokyo Institute. Grant to Sawyer, June 30, 1936. RAC. RG.1.1 S.609 B.2 f.15.
64. W. S. Carter, "Survey of Medical Education in India." November 24, 1927. RAC. RG.1.1 S.464 B.5 f.32. The complex story of the All India Institute is told in Shirish Kavadi, *The Rockefeller Foundation and Public Health in Colonial India 1916–1945* (Pune: Foundation for Research in Community Health, 1999), Chaps. 8, 9.
65. W. Carter to W. Pearce, March 27, 1928. RAC. RG.1.1 S.464 B.5 f.33 gives an account of these negotiations.
66. My discussion of Indian politics is taken from two excellent books by Stanley Wolpert, *A New History of India* (Oxford University Press, 1982) and *Nehru: A Tryst with Destiny* (Oxford University Press, 1996) and P. Spear, *A History of India.* Volume Two (London: Pelican Books, 1965).
67. M. Brown, *Modern India. The Origins of an Asian Democracy* (Dehli: Oxford University Press, 1985).
68. Helen Power, "The Calcutta School of Tropical Medicine: Institutionalizing medical Research in the Periphery." *Medical History* 40 (1996): 197–214.
69. The Governing Body of the India Research Fund Association, as initially constituted, consisted of the president, who was a member of the Governor-General's Council of the Department of Education, Health and Lands; the secretary, Department of Education, Health and Lands; the director general and assistant director general of the Indian Medical Service; the Public Health Commissioner of the Government of India; the director of the Central Research Institute and the officer in charge of the Malaria Bureau.
70. Heiser's Diary, April 21, 1931; Stewart to Carter, March 6, 1941. RAC. RG.1.1 S.464 B.5 f.36.
71. Details in RAC. RG.1.1 S.464 B.6 f.37.
72. W. S. Carter to R. Lambert, July 12, 1934. RAC. RG.1.1 S.464 B.6 f.38.
73. Jacocks to Sawyer, July 12, 1938. RAC. RG.1.1 S.464 B.6 f.39.
74. Jacocks to Sawyer, March 10, 1938. Ibid.
75. Strode to Sawyer, July 6, 1938; Sawyer to Jacocks, August 9, 1938. Ibid.
76. Grant, "Realities in the Situation of the All-India Institute of Hygiene and Public Health." September, 1939; "Memorandum by Dr. John B. Grant, Director, All India Institute of Hygiene and Public Health, relating to the Reorganization of the Institute, with budgetary implications;" RAC. RG.1.1 S.464 B.6 f.40.
77. Grant to Strode, March 28, 1940. RAC. RG.1.1 S.464 B.6 f.41.
78. Details in RAC. RG.1.1 S.464 B.12 f.91.
79. Grant's Diary, March and April, 1940.
80. Grant to Strode, March 28, 1940. RAC. RG.1.1 S.464 B.6 f.41.
81. Grant's Diary. June, October and November, 1940.
82. L. Allen, *Burma. The Longest War (1941–45)* (London: 1984).
83. These events are analyzed in C. Horne, *Allies of a Kind. The United States, Britain and the War against Japan* (London: Oxford University Press, 1978).

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# V

# FINALE

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# 16

## Postwar Confusion: What to Do Next?

With World War II drawing to a close, many in the Health Division assumed the organization would quickly slip back into its old grooves. As Hackett noted in his *History*, however, "finding its proper role in the postwar world was to prove a more difficult and revolutionary transformation than the adjustment to war."[1] Those who hoped for continuity felt reassured when George Strode, an associate director for many years, succeeded Sawyer as director of the Health Division in the spring of 1944.

His appointment was especially welcomed by the field workers, whose morale had suffered under Russell and Sawyer. Strode, who was more of an administrator than a scientist, was seen by this group as a Rose man who would restore the balance between public health in the field and research in the laboratory. But if the field officers were expecting any significant changes they were to be disappointed. "I believe," Strode wrote, "we shall wish to continue the study of specific diseases and I would expect no immediate additions to or subtractions from the present list."[2] As a result, the Health Division tried to proceed much as before, and the budget allocations during the Strode years were not radically different from those of Sawyer's. Money spent on specific diseases continued to slide, and, surprisingly, the money allocated to laboratory research increased from 7% under Sawyer, the laboratory man, to 10% under Strode (Table 1.2). In other words, Strode showed no desire to turn the clock back to Rose and was keenly aware of

the necessity of a field–laboratory interaction. "Strode," Hackett said, "cannot justly be accused of looking on it [the IHD Laboratory] with disfavor and of making no attempt to sustain its waning activities and morale."[3]

These figures suggest that the Health Division had become an organization with no impetus for fundamental change in a world that was demanding change and reconstruction so that the horrors of the war would not be repeated.

## The Status Quo

One basic problem was the inability of Health Division officers to agree on a new set of diseases suitable for a coordinated laboratory and field approach. They examined and rejected undulant fever, yaws, schistosomiasis, tuberculosis, typhus, hepatitis, scarlet fever, and rabies. They were unsure of their abilities to handle work on nutritional diseases and although they felt influenza to be a profitable area, they knew that other laboratories had already begun working on the problem. Thus, Strode concluded, malaria and yellow fever were "virtually irreplaceable," leaving the Health Division with no option other than to continue their work on these two diseases.[4] The Health Division thus continued supporting DDT control projects against malaria and yellow fever in Albania, Bolivia, British Guiana, Ecuador, Mexico, Panama, Peru, Taiwan, and Venezuela, while research work continued in attempts to discover primate and mosquito hosts of jungle yellow fever. In addition, the commission was engaged in its Sardinia campaign.

Similarly, they continued to support their schools of hygiene and nursing that had survived the war. Strode learned, for example, that, because of staff shortages, the London school felt unable to respond to the expected postwar increase in student numbers. The commission set up a five-year rehabilitation project earmarked for student fellowships, with the hope that the best fellows would subsequently be given permanent appointments at the school.[5] After some difficulties, the dean, Dr. James MacKintosh, was able to report that all the fellows were holding posts in the United Kingdom, and four of them had been appointed to the faculty of the school. Small wonder the dean regarded the postwar Rockefeller-funded program as an "outstanding success."[6]

There were serious problems that needed to be addressed at the Toronto School of Nursing.[7] Pressures had been placed on the old school building by wartime demands for more nurses, and in 1944 the Health Division received a request to assist in the erection of a new and larger building.[8] Flattered and encouraged by the Health Division, the government of Ontario came through with part of the necessary funds to support what Hugh Smith called "one of the outstanding schools of nursing in the world."[9] The division agreed to offer financial support as soon as the university provided its share of the money and made a suitable site available for the new building.[10]

But this proved difficult. Disturbed postwar labour conditions, high costs of construction, a huge increase in student enrolment and a general shortage of university funds brought inevitable delays and conflicts over the priority of a nursing school within the university as a whole. To further exacerbate matters the university failed to find a site for the new school, with the result that many in the Health Division felt the university was not meeting its obligations. In 1947, the president of the Rockefeller Foundation attempted to put pressure on the university by noting in his annual report, that Miss Russell's "leadership, scholarly ability and insight into the community's nursing needs have produced an outstanding research program, and Toronto is one of the peaks of nursing training in the world."[11] Although the university continued to procrastinate, the Health Division agreed to extend its appropriation year after year, until eventually the university agreed to construct a new building. But, since it lacked a student residence, Ms Russell regarded it as inadequate, angrily denouncing the university for failure to live up to its bargain with the Health Division.[12] Nevertheless, on October 30, 1952, three months after Miss Russell's retirement, the cornerstone to the new nondescript classroom building was laid at its present site by the premier of Ontario.[13] The Health Division had stood by Miss Russell to the very end, indicating again the high regard they had for her and her Lighthouse School.

Many years were to pass before the Health Division heard again of the Tokyo Institute of Public Health. In 1947, Marshall Balfour, re-established as the division's regional director in Shanghai, informed Strode that the public health and welfare section of SCAP (Supreme Commander Allied Powers) was looking at the institute but had lost confidence in its director, Dr K. Nobechi. A former assistant to Nagayo, he had been heavily involved in prewar negotiations with the Health Division. Balfour suggested they might loan a staff member to act as consultant or even as its director (he was thinking of John Grant) in order to support the SCAP program, the success of which, he wrote, "is vital to the welfare of mankind and the world at large, as well as the Far East."[14] Despite a request by General MacArthur for the loan of a consultant, Strode, following traditional policy, refused to cooperate without a direct request from the Japanese government, particularly after the military authorities made plain the new director would be under army command.[15] SCAP had its way. Nobechi was forced to resign—accused of engaging in subversive propaganda—and a new director named. The institute's programs were slowly reintroduced, aided by a grant from the foundation, with Dr. O. McCoy of the Division of Medicine and Public Health appointed as consultant.[16]

## The China Program

The Health Division's work in China had never amounted to much, even though, in 1935, the foundation earmarked money for a three-year program for rural re-

construction.[17] But in 1939 the Health Division decided to initiate a malaria program in southwest China, an area which was said to offer favorable working conditions, being far from the Japanese invaders. In May 1942, before any mosquito control work could begin, the Japanese attacked southwest China, and, for the time being, the Health Division was out of China.[18]

With the defeat of the Japanese, the division planned an immediate restoration of their program. Marshall Balfour once again took up residence in Shanghai and Robert Watson put in charge of the Chinese malaria program. By then the clash between Chiang Kai-shek's Nationalists and the Red Army in the northern provinces, had turned in the Nationalists' favor, especially after they were able to capture the communist capital of Yenan early in 1947. With the scientific directors expressing confidence in China's future stability, the first malaria DDT field station was established near Nanking; and the following year, a second station was set up in Ch'ao Chow in Taiwan.[19]

Fosdick believed the foundation should withdraw from China. "Even if the Central Government should suppress the Communists," he wrote in 1946, "it is unlikely that the forces presently controlling the Kuomintang would then make significant concessions or reforms in the political, economic or social field."[20] A year later, the tide began to turn, as the Red Army began to cut to pieces the U.S.-trained Nationalists and to capture tons of its American equipment. No one except the U.S. ambassador expected the Nationalists could win, one letter stated, and the Nationalist government had become even more unpopular with graft, inefficiency, terrorism and factionalism rampant.[21]

Like Fosdick, both Balfour and Watson were deeply hostile to the Nationalists and felt that by remaining in Taiwan the Health Division would become identified with that government.[22] But, believing that a coalition government would eventually be formed and that the Nationalists would never be able to hold on to the island, "the last-ditch place of resistance of Kuomintang diehards," they decided to stay.[23] But not for long. By June, Watson reported, hundreds of thousands of disreputable and undisciplined Nationalist troops had arrived, billeted in public buildings and camped out on the streets; supplies had run out after the ports became blocked by troop ships.[24] At the end of that month, Watson told the director of the Taiwan Malaria Research Institute that the Health Division would withdraw its support by the end of the year. Most of its staff were abandoned in Taiwan, not being able to procure the necessary passports for escape from the island, although some of its leaders were able to reach Hong Kong where Balfour had set up a temporary regional office.

On February 23, 1949, with Shanghai about to fall to the Red Army, the office there was finally closed. Given the incredulous stand taken by the U.S. government, it is understandable that no attempt was made to follow up on the suggestion that the Health Division forge links with the new leaders. The divi-

sion should, Fahs (one of its field officers) argued, offer aid to the Bethune hospitals (named after the much-honored Canadian surgeon who, after service in Spain, had organized the Red Army's medical system), and give research grants to Chinese faculty.[25]

## Postwar Germany

The Health Division was drawn into Germany when the foundation became involved in problems of German reconstruction. With the end of the denazification period in 1948, the Office of the Military Government for Germany began a process of democratization. They planned to offer German fellowships and bring in American experts to survey and advise on such matters as medical education, medical research, a model school of public health and nursing, and community health centers. Would the foundation be interested in such projects, the Military Government asked?[26] The response was not particularly enthusiastic.

Robert Morison, future director of the Division of Medical and Natural Sciences, was opposed to "providing any Rockefeller money to make Germans into better scientists unless at the same time we are doing something to make them into better men."[27] Most seemed to agreed that, if they were to become involved, much of the effort had to come from the Division of Social Sciences and Humanities.

Strode and Bauer reluctantly agreed to survey six universities in the American Zone in response to a request to aid in the formation of a new school of public health. If a new school was to be built, they concluded, it would have to start from scratch. Staff would be recruited in Britain or the United States, and a new director appointed who would select candidates for fellowships in order to build up a German faculty. The Paris office, apparently on its own, wrote to Dr. Bela Johan, director of Budapest's School of Hygiene who had been dismissed and imprisoned by the Nazis, more or less offering him the director's jobs in the chosen location of Heidelberg, should the plans materialize.[28] Despite Johan's interest in the position, it was a naive idea. The Hungarian government could hardly be expected to allow him to leave and, equally, the Germans would never welcome a foreigner whom they had previously imprisoned. The idea died.

At the same time the medical faculty at the University of Heidelberg wrote to Grant requesting aid in setting up a new school for nursing instructors, in order to help solve what they called "the greatest social problem of women in her history." There was a huge surplus of women in Germany after the war, with 11.4 million women between the ages of 21 and 40 and only 4 million men. Mary Tennant, in charge of the Health Division's nursing program, was not impressed by their argument that the "medical by-professions," which employed large numbers of women, needed to be built-up.[29] So that idea died too.

But the idea of a new school of hygiene would not die. In 1948, the military government told Grant its public health branch was hoping to bring in a public health expert to survey the situation with the hope of selecting a center for the new institute, Heidelberg once again being mentioned. Strode showed little interest, but the Health Division agreed to fund a public health survey by Franz Goldmann of Harvard. Plans for a new school were again on the rails.[30]

After difficulties over an entry visa, Goldmann arrived in June 1949 with a mandate to survey public health problems, suggest improvements, and select a group of six to eight German fellows with whom he would tour Britain, Sweden, Denmark and the United States. He quickly selected nine candidates whose names were sent to the U.S. military authorities for a security check. By August he had "abandoned the project as completely hopeless." Because of personnel shortages, three of the nine were not allowed to leave their jobs, three had been declared "tainted" by the Americans and two were having difficulties obtaining U.S. visas.[31] Goldmann ran into a maze of regulations not only of the military variety, but also from the Senate Appropriations Committee which was hostile towards visits by any foreign nationals, let alone Germans, and was demanding maximum screening. This witnessed the end of the Health Division's very minimal involvement in Germany.

## South America

With Hackett in charge of the Rio de la Plata and Andean Region, the war years saw the Health Division firmly established in Argentina, Bolivia, Chile, Equador, Paraguay, Peru and Uruguay, none of which had been of great importance to the Health Division before the war. By 1945, the Chilean program, which both Hackett and John Janney held in such high regard (see Chapter 2), was running into problems and Janney began to question whether "public health can be obtained unless drastic things are done about ignorance, poverty and low standards of living."[32] Hackett remained positive, noting in 1948 that Chile was still decades ahead of other South American countries in its social legislation, and its medical and nursing schools. The country had created, he continued to insist, "a vigorous profession in the field of health which is coming to grips with the old part-time political bureaucracy so characteristic of the South American scene."[33] But, under pressure from the United States, the Chilean government moved to the right, the Communist Party was outlawed and involvement in public health matters fell by the wayside as the government faced mounting civic disorder. In 1952 General Carlos Ibañez would reappear on the scene, and that would be that.

There were other problems too. In 1950, the Health Division's last year in Chile, Janney complained that the Pan-American Sanitary Bureau, which was founded in 1924 by the U.S. government and which was building up in the area, had made financial offers to seven faculty members from the School of Public Health with-

out any consideration of Chile's needs. According to Janney, had three of them not decided to stay the school would have been forced to close.[34]

## Other Health Agencies

While the Health Division struggled to find a role in the postwar world, its turf was increasingly threatened by the appearance of other health agencies. The British had become less dependent on the Health Division after the government passed a series of colonial development and welfare acts. Pushed to some extent by American demands for colonial independence, the first of such bills was presented to parliament in May, 1940. It "breaks new ground," Malcolm MacDonald told members, "it establishes the duty of taxpayers in this country to contribute directly and for its own sake towards the development of the colonial peoples." The days of colonial self-sufficiency were coming to an end; the colonial people, with the aid of British taxpayers, were to be prepared for quasi independence within the British Empire. In the process, economic development, health and education would be given priority. More to the point, as some parliamentarians noted, the British Empire would no longer be dependent on the Health Division for monetary assistance to fight diseases.[35]

No agency had a greater impact on the future of the Health Division than the World Health Organization, which came into being in September 1948. The WHO was the wave of the future. Fred Soper realized that large-scale operations against diseases such as malaria and yellow fever, required international cooperation and that both the Pan American Sanitary Bureau and the WHO were superior instruments for achieving this. The Health Division worked only with individual foreign governments. He believed the days of the Health Division were numbered.[36]

## New Ideas

The desire to maintain the status quo had been undermined by practical difficulties in a postwar world, to which was now added the growth of competing health agencies. But even before these difficulties emerged Strode had suggested two new topics as worthy of future support: the physiology of the tropics and health insurance.

In an October 1944 meeting, Strode addressed the increasingly debated question of overpopulation and the danger of famine. Many in the Rockefeller Foundation, and elsewhere, none more vigorously than J. D. Rockefeller III, attributed the problems of overpopulation to successes against infectious diseases, which the Health Division had helped to bring about. Similarly, in 1945, the first world food survey of the Food and Agricultural Organization of the UN reported that over half the world's population was malnourished attributed primarily to a decline in mortality brought about by medical successes.[37]

While avoiding the political hot-potato of birth control, Strode believed these problems could be alleviated. Like many others, he believed the tropics to have a potential for rich and fertile living space that could accommodate surplus populations from the temperate areas. In the past they had proven impossible to settle because of disease and an unfavorable environment. Now, with tropical diseases partially conquered, or so he believed, only environment stood in the way.[38]

The history of "poor whites" in the U.S. South, the West Indies and elsewhere, showed, said Strode, that whites, had not fared well in tropical environments. They became gradually "not only lazy but at times something considerably lower than a decent native." To study the physiology of the tropics, to ascertain whether this regression was inevitable or whether "by controlling his environment through the application of new knowledge, the white man can someday live in the tropics without loss of his vital energy," seemed worthy of study.[39]

Nothing came of these ideas. The South African government had already published a Carnegie Foundation-funded study of "poor whitism" among Afrikaners in which the authors concluded the problem was not climate and disease, per se, but poverty and malnutrition. The South African government had thereupon introduced economic and political measures to increase the prosperity of the Afrikaner "bywoners."[40] In this regard, Strode was out of date.

## Social Concerns

Strode had more success when he changed direction and urged the scientific directors to consider health insurance and to appoint a staff member to study the issue. Aware of the British desire to begin a national health service, Strode believed that "we are on the threshold of applying new concepts in medical economics and medical practice." The Health Division should become involved, he urged, without, he hopefully added, "entering the many controversial matters that are involved in health insurance."[41] It was hardly likely to be noncontroversial, particularly in the United States!

Health insurance had traveled a rocky road in the United States, and Strode's suggestion did not meet with universal approval. W. Halverson, a scientific director, claimed that 35% of Californians did not want health insurance, while Harry Mustard said that "it is unfortunate that so many proponents of this idea are such unpleasant people."[42] The Health Division had touched on this issue without success. A short-lived interoffice committee on health insurance was dissolved in 1938 because Fosdick feared conflict with the American Medical Association.[43] Government proposals for compulsory health insurance had raised storms of controversy from the medical profession.[44]

But the scientific directors agreed that someone should take charge of this new field. They proposed John Black Grant, and in June 1945, he accepted. With his previous experience in the Peking Union Medical College, the All India School

of Public Health, and with his involvement with the Bhore Committee, Grant was the obvious choice to examine health insurance and related issues. A few months later Fosdick appointed Grant chair of yet another short-lived and ineffectual interdivisional committee on medical care, with one representative from each of the divisions of Medical Sciences and Social Sciences joining Grant.

Grant wanted to push the division well beyond concern with health insurance. By the mid-1920's he had developed the reputation, in his own words, "of being a bit of a medical Bolshevik." He had come to appreciate the significance of social medicine. This was an area with which neither Strode nor the scientific directors showed any interest; health insurance was as far or even further than most of them wanted to go. Surprisingly, in April, 1940, the Medical Sciences Division had appropriated a small sum of money for teaching and research in social medicine at the University of Brussels under the direction of Dr. René Sand. In November 1945, after an interruption because of the war, a new grant was approved.[45]

Social ideas were introduced in the WHO constitution, drafted at a 1946 International Health Conference in New York. It presented, according to C-E.A Winslow, a broader concept of health than ever before and was "substantially in advance of the actual practice of most health departments in the United States."[46] Health, the constitution said, "is not merely the absence of disease or infirmity, it is a state of complete physical, mental and social well-being." As Brock Chisholm, a Canadian who drafted the constitution and later became WHO's first director general, noted, health could not become a reality in the world unless the WHO tackled basic problems—namely, those of mental, social, and economic misery.[47] Even Sawyer had to concede in 1951, that "health . . . cannot flourish in an adverse socioeconomic environment."[48] Hackett, too, recognized a change. "The slashing attacks on endemic diseases, at first so rapidly successful, had disclosed social and economic roots which retarded their suppression and which could no longer be ignored."[49]

Grant's desire to push the division in that direction was thwarted by his inability to write coherently and succinctly. His reports were disorganized and verbose; one doubts whether the officers in New York took the time to read them. Even if they could follow his tortured arguments, they often found his views too extreme. His appointment might have indicated a willingness to allow him to blow off steam without themselves taking any real action. He did not retain his position for long.

Grant's "interim reflections," prepared for the first meeting of the interdivisional committee, emphasized the problem of health insurance.[50] A long progress report presented to the scientific directors in September, 1946, based primarily on his assessment of the situation in Britain, Canada, Sweden and the United States, went beyond health insurance to address wider issues of social medicine. Grant realized there were both nonmedical and medical components to health care. The former, Grant said, included family allowances, sickness and unemployment bene-

fits, retirement pensions and widows' benefits. Social and preventive medicine needed to be taught as an integral part of clinical medicine. Adequate medical care depended on bringing in group practice, health centers, payment by capitation rather than fee-for-service, and the hiring of more subsidiary health personnel, such as public health nurses and social workers. Medical care [to avoid confusion, he should have called it health care], he wrote, "is the social mechanism to promote positive health for the full physical and mental enjoyment of freedom from want."[51]

He was reflecting the views of many in Britain where a welfare state was taking shape. Members of the Labour and Liberal parties, many of them with powerful voices in the wartime coalition government, backed by influential figures in the Anglican Church, successfully pressed their case during the war. Innumerable committees provided a blueprint for the postwar welfare state, none more important than the Beveridge Report of 1942 and the Cabinet Committee on Reconstruction Priorities of the following year. With Labour's Election victory in 1945, the shape of the new society began to appear, with plans for new housing projects, welfare payments, family and child allowances, old age pensions, and passage of the National Health Service Act in July 1948.[52] Proponents of social medicine attempted unsuccessfully to embed their ideas into a new medical curriculum; they believed physicians had to be trained to promote health by paying attention to the social environment in which people lived and to become aware that sickness might have social and communal roots.[53]

Grant urged the Health Division to help finance community projects controlled by universities and geared towards field practice for medical students in the area of preventive and social medicine. "The medical school," Grant concluded, "which operates the successful pilot project . . . will become the 'Johns Hopkins' of Preventive and Social Medicine."[54]

The scientific directors brushed off the report by sending Grant to look at what was being done overseas. In 1947, Grant visited Canada, nine European countries, Australia, New Zealand, and South Africa. On his return, Grant presented his findings to the Health Division which, by then, had taken over sole responsibility for the whole area of health care—the interdivisional committee having collapsed earlier in the year with the resignation of the medical sciences representative (A curious state of affairs since this was the only division supporting work in social medicine at that time). His report was steeped in concepts of social medicine which, he urged, the Rockefeller Foundation should help establish throughout the world. "The universal establishment of this pattern of health care as a 'social science in the service of society,'" he concluded, "would usher in a new era which would be momentous in human welfare and happiness."[55]

Whether the scientific directors were able to read or take in Grant's report is unclear. They merely recorded at their December 1947 meeting that "they con-

sider the recommendations suggested by Dr. Grant appropriate for the general guidance of officers." This could only be seen as a dismissal. Social and socialized medicine were never to become a central concern for the division. They limited their involvement to covering staff salaries for the subcommittee on medical care established by the USPHS in 1943 to design a national medical care program. They also played a role in the development of the Health Insurance Plan of Greater New York, lending it $388,000 to help with expenses during its first two years of operation.[56]

Grant's 1947 promotion to the post of European director of the Health Division in the Paris office, which had been reopened in 1945, signaled the end of the line as far as health care was concerned. In the European office, Grant would be unable to bring about the serious policy change he was advocating. Two years later a disillusioned Grant lamented that the literature of the past ten years had become "almost redundant" with plans to extend medical care into health care, "yet nowhere . . . has the concept been experimented with, much less demonstrated."[57]

## Overpopulation

In 1947 the Health Division swung back to the population problem. As its scientific directors noted, "overpopulation is accomplished by poverty, ignorance and disease . . . Our primary interest is in the cause of overpopulation."[58] They were cognizant of the argument that their presumed successes in eradicating diseases had in reality led to overpopulation and worsening health conditions. It was an issue with which J.D. Rockefeller III was concerned.

As a result, Marshall Balfour, the regional director for the far east, led a 1948 survey team into Japan, Korea, China, Taiwan, Indonesia, and the Philippines. These countries faced rapidly-rising-populations resulting, they thought, from a Western-induced reduction in mortality that was not being balanced by a corresponding decrease in birthrates. They realized that as things stood, large families were needed to secure income and security; thus a "decline in fertility awaits changes that will both stimulate the desire for fewer children and improve the means of fulfilling that desire." Concentrating on disease prevention had become counterproductive, leading only to overpopulation, the survey committee found. Health organizations needed to examine reproductive behavior in rural communities, as well as requirements for cheap, safe, and effective contraception. "We doubt," they concluded, "that any other work offers a better opportunity for contributing to Asia's and the world's fundamental problems of human welfare."[59]

Others in the Health Division felt more resources should be directed toward Europe.[60] One gets the impression of an organization that doubted the value of what it had been doing, but had no clear idea where to go next.

## Review Committees

Many in the New York office of the Rockefeller Foundation, realized that both the Health Division and the foundation itself needed a new sense of direction. John D. Rockefeller III, the eldest son of John Jr., attempted to engage foundation trustees in a debate over the future—and was rebuffed. As Fosdick told him early in 1945, "I think the trustees felt that the present programs were fairly adequate." But he continued to press. Like others, Rockefeller III wished to downgrade the emphasis on research and return to the older notion of the application of knowledge already known. He was also concerned that the divisional nature of the foundation had led to a fractured and unfocused program, and that many of its resources had been located in North America and Europe to the exclusion of poverty, malnutrition and overpopulation in the poorer countries of the world.[61]

Fosdick had little sympathy with that criticism, but the trustees agreed in December 1945 to set up a four-man Special Committee on Policy and Programs, chaired by Chester Barnard and including JDR III. The views of foundation officers and many others were sought; the result was a confusing blur of concerns. The majority, with the horror of the atomic bomb still fresh in their minds, wrote about preventing war and developing a new moral law in society, although how this was to be expressed in a concrete policy remained unclear. Some wished to establish "practical machinery for eliminating war and making it clear that war will not pay as a means of settling things." Others spoke of trying "to redress the imbalance between moral and material development," of bringing nations into cooperative efforts, and of establishing world government. Many felt greater emphasis should be placed on the social and political sciences in order to "make democracy function . . . to the end that a disorganized world may not choose the alternative of communism." Members of the committee must have been horrified to learn also that a U.S. Army poll taken of American troops in Germany had revealed that 51% felt that Hitler had brought a lot of "good" to Germany, and that 31% "either approve or condone the bestial persecution practiced on the Jews by the Germans."[62]

Members of the Health Division must have been particularly disturbed to read the Committee's final report. In the past, the committee found, the foundation had placed too much emphasis on medicine and public health. Instead, work should be diverted into the social sciences to address such problems as race relations, improving democracy and the promotion of international understanding.[63] But what of practicalities and concrete policies? Here their lack of imagination stands out. The only new thing they recommended was the formation of a division of moral philosophy. Otherwise they opted not to disturb existing divisional structures, calling merely for greater cooperation. The basic inertia of the foundation was open for all to see.

Cooperation then became the key in a three-division survey of Crete in 1948, which, unlike so many others in the past, did not focus exclusively on disease.

The foundation had been asked by the Greek government, then in control of the island, to determine if, and how, the standard of living of its people could be raised. The report made clear that what mattered to the Cretans was not the control of malaria and tuberculosis, both of which were still common, but an increase in their water supply and increased agricultural productivity. This could only come about, they surmised, by the diversification of crops, better marketing and transportation, and the building up of small local industries—certainly not the usual plan of attack for health campaigns of the past.[64]

But the Health Division was not yet out of the fire. In 1950, aware that a chorus of criticism of the Health Division had been reverberating in the Rockefeller Foundation, Chester Barnard, who had taken over as president from Fosdick two years before, formed a Commission of Review to recommend a program for the future. Gordon Fair, dean of engineering at Harvard, was appointed chairman of the 22-member commission, to serve as a middleman between two rival divisions that had such overlapping concerns: the Health Division and the Division of Medical Sciences.

Once again members of the Foundation were asked to submit their ideas to the review committee, which met for the first time in May 1950. They poured out their ideas in a series of letters. Some, like Paul Russell, favored the status quo; others, like John Grant and Hugh Smith, took a broader view of public health.[65] Lindsley Kimball, vice president of the foundation, told Barnard he thought the Health Division was losing significance; Strode had tried to bring about change and adaptation, he said, but there had been so much staff inertia that nothing was accomplished. The laboratories focused on basic rather than applied research, and had become "the dumping ground for personnel not usable elsewhere." The field staff was spending too much money, and the whole organization needed to be more selective, to have a narrower geographical range, and to redirect its preoccupation with individual diseases towards "indigenous health problems." The Health Division required to be "retailored," he said. "New sailing directions appear to be in order."[66]

In November 1951, the commission submitted its final report.[67] It recommnded that programs to control specific diseases be curtailed, noting that traditional Health Division programs were in danger of being overshadowed by those of the WHO, the Pan-American Sanitary Bureau, and, more recently, the Point IV Program.[68] The Health Division had only recently realized that it had too long avoided the socioeconomic factors in public health, an approach labeled "unrealistic." The commission called on the foundation to develop "broad, integrated programs designed to study some aspect of human ecology and particularly those which would increase knowledge of the interrelationships of health, population, economic development and social change." How were such programs to be developed? Once again the conservatism of the Rockefeller officers came to the fore. They recommended merely that the two divisions—International Health and Medical Sciences—be amalgamated into a single division.

Perhaps they knew this was about to happen anyway. Before their report was submitted, the trustees of the Rockefeller Foundation decided at an April 1951 meeting to combine the two units into a new Division of Medicine and Public Health. The Commission of Review then rather lamely agreed, recommending basically that the new division carry on the programs of its two components. And that was that.

Increasingly the new division emphasised medical education. Indeed, so strong was this interest that in 1955 the division was renamed the Division of Medical Education and Public Health; its previous involvement with viral diseases was transferred to a new division of biological and medical research. Four years later, with public health and health care playing a decreasing role in their work, the foundation's activities in medical education and in the natural sciences were consolidated into the Division of Medicine and Natural Sciences. This division behaved as an academic granting agency supporting professional education, numerous faculties of medicine, and individuals and institutions in such areas as behavior, experimental medicine, genetics, biochemistry, biophysics, and virology. Public health was to be left to the World Health Organization.

It was far from a glorious ending.

## Notes

1. L. Hackett, MS. *History of the International Health Division.* Chap. 9, "End of an era." RAC. RG.3 S.908 B.5 f.36.
2. Strode, "International Health Division," in "Plans for the Future Work of the Rockefeller Foundation," November 1944. Discussed at Trustees meeting, April 1945. RAC. RG.3 S.900 B.23 f.174.
3. L. Hackett, MS. *History.* "Lab and Field. Rough Outline." RAC. RG.3 S.908 B.5 f.34.
4. G. Strode, "The IHD in the World of Tomorrow," March 7, 1944. RAC. RG.3 S.908 B.13 f.135.
5. James Mackintosh to Strode, October 13, 1944; Chas Leach to Strode, November 3, 1944. RAC. RG.1.1 S.401 B.3 f.27.
6. J. M. Mackintosh, "Rockefeller Foundation Fellowships in Public Health, 1945–52." RAC. RG.1.1 S.401 B.3 f.28.
7. More details are given in J. Farley, "Building a school of nursing with Rockefeller money: Three Nova Scotians at the University of Toronto. *J. Roy. Nova Scotia Hist. Soc.* 3 (2000): 85–104.
8. School of Nursing to IHD. "Request for financial aid toward a new building." October 6, 1944. RAC. RG.1.1 S.427 B.13 f.107.
9. Hugh Smith to Ontario Minister of Health, October 18, 1944. Ibid.
10. Minutes, Rockefeller Foundation. April 4, 1945.
11. R. Fosdick, *Annual Report for 1947* (New York: Rockefeller Foundation, 1948).
12. K. Russell to S. Smith, December 28, 1950. RAC. RG.1.1 S.427 B.13 f.114.
13. J. Farley, "Building a school of nursing," includes a photograph of the present building.
14. Balfour to Strode, January 31, 1947. RAC. RG.1.1 S.609 B.3 f.18. Balfour, born in 1896, did not gain his Harvard medical degree until 1926. After his first degree, a

B.Sc from M.I.T in 1918, he worked for the Red Cross in Czechoslovakia where he met Selskar Gunn who persuaded him to enter medicine. After working on malaria in the U.S. South and in Greece, he took over the China program in 1939.

15. Major Klous to the Rockefeller Foundation, May 7, 1947; Interview of A. Warren with Major Klous, May 15, 1947. RAC RG.1.15.609 B.3 f.18.
16. Watson to Strode, September 12, 1947. Ibid. Years later it was revealed that the Japanese Army, particularly through Unit 731 in northern China, commanded by General Shiro Ishii, had engaged in horrendous germ warfare experiments, and that the U.S. army had covered up the episode and granted immunity to the perpetuators in exchange for their data. One of the army personnel, a physician, reported that the data was a financial bargain. "Such information could not be obtained in our own laboratories," he wrote, "because of scruples attached to human experimentation. These data were secured with a total outlay of yen 250,000 to date, a mere pittance by comparison with the actual cost of the studies." R. Gomer, et al, "Japanese biological weapons: 1930–45." *Bull. Atomic Scientists* 37 (October 1981): 43–53; N. Kristof, "Japan Confronting Gruesome Atrocity." *New York Times*, March 17, 1995; Tien-wei Wu, "A preliminary review of studies of Japanese Biological Warfare Unit 731 in the United States." Internet, August 29, 2002. The question must be asked whether any of these experiments was carried out in the school of hygiene? An examination of P. Williams & D. Wallace, *Unit 731. Japan's Secret Biological Warfare in World War II* (New York: Macmillan, 1989) presents no proof but reveals some suggestive links, particularly with the Imperial University's Institute of Infectious Diseases which was at one time directed by Mataro Nagayo, the early favorite for the position of director of the school of hygiene. In 1939, one of Ishii's men was asked by the then director of the infectious diseases institute Yonetsugi Miyagawa, another name put forward to direct the new institute, to obtain the yellow fever virus from the Health Division's laboratory. He failed but a second, equally unsuccessful attempt was made six months later. None of the criminals listed in William's book, all of whom returned to an "untroubled civilian life," many in prestigious academic positions, seemed to have had any connection to the institute of hygiene. I would like to thank Margaret Humphreys for drawing my attention to these matters.
17. Details in RAC RG.1.1 S.601 B.12 f.125, 129, 130. The Health Division had supported some hookworm work, funded the Shanghai Health Department and a midwifery training school, and had loaned John Grant to the Peking Union Medical College.
18. Details of the malaria program in RAC RG.1.1 S.601 B.43 f.356, 357; RG.5.3 S.601 B.218.
19. Scientific Directors Report, September 1946. RAC RG.1.1 S.601 B.43 f.356.
20. Memo from R. Fosdick to A. Gregg etc. October 21, 1946. RAC. RG.1.1 S.601 B.12 f.127.
21. C. Fahs to D.H. Stevens, May 8, 1947. Ibid.
22. Strode to Watson, March 3, 1949. RAC RG.1.1 S.601 B.43 f.361.
23. Watson to Balfour, March 10, 1949. Ibid.
24. Warren to Strode, June 7, 1949; Warren's Diary, June 20, 1949. Ibid.
25. C. Fahs to D.H. Stevens, June 26, 1948. RAC. RG.1.1 S.601 B.12 f.127.
26. Project planned by OMGG, July 1948. RAC. RG.1.1 S.717 B.7 f.38.
27. R. S. Morison. Memo of November 20, 1947 and June 7, 1950. RAC. RG.1.1 S.717 B.7 f.37, 38.
28. C. Leach to B. Johan, June 12, 1947. RAC. RG.1.2 S.717 B.4 f.34.
29. K. H. Bauer (Heidelberg) to J. Grant, March 31, 1948. Ibid.

30. Military Government to Grant, August 16, 1948; Strode to Grant, September 3, 1948. Ibid.
31. F. Goldmann, "Final Report of the Planned Study Trip of German Health Officers," August 11, 1949. RAC. RG.1.2 S.717 B.4 f.35.
32. *Rio de la Plata and Andean Region Annual Report, 1945.* Chile. RAC. RG.5.3 S.300 B.103.
33. *Rio de la Plata and Andean Region Annual Report, 1948.* Hackett's Report. RAC. RG.5.3 S.300 B.104.
34. *Rio de la Plata and Andean Region Annual Report, 1950.* Ibid.
35. For details see J. Farley, *Bilharzia: A History of Imperial Tropical Medicine* (New York: Cambridge University Press, 1991).
36. Draft of letter to Nelson Rockefeller, 1951. Soper papers. MS. C. 359 B. 17 Nelson Rockefeller Correspondence. National Library of Medicine, Bethesda.
37. The food surveys are discussed in T. Poleman, "World food: a perspective." *Science* 188 (1975): 510–518; Kingsley Davis. "The population specter: Rapidly declining death rate in densely populated countries. The amazing decline of mortality in underdeveloped areas." *American Econ. Review* 46 (1956): 305–318.
38. G. Strode, "The International Health Division in the World of Tomorrow," March 7, 1944. RAC. RG.3 S.908 B.13 f.135.
39. G Strode. "The IHD Comments on Policy and Program," Presented at meeting of scientific directors, October 26, 1944. Ibid.
40. For a brief discussion of the five-volume report see J. Farley, *Bilharzia*, p. 139; W. Vatcher, *White Laager: The Rise of Afrikaner Nationalism* (London: Pall Mall Press, 1965).
41. G. Strode, The International Health Division in the World of Tomorrow, March 7, 1944; Comments on Policy and Program. Both documents presented to meeting of scientific directors, October 26, 1944.
42. Meeting, scientific directors, December 1944.
43. J. van Sickle, "Should the Foundation's Social Security Program be expanded to cover the cost of illness?" Memo, October, 1936. RAC. RG.3. S.900 B.25 f.192.
44. M. Hamovitch, "History of the movement for compulsory health insurance in the United States." *Social Service Review*, 27 (1953): 281–299; D. Hirshfield, *The Lost Reform: The Campaign for Compulsory Health Insurance in the United States from 1931–43* (Cambridge: Harvard University Press, 1970). For the earlier period see R. Numbers, *Almost Persuaded, American Physicians and Compulsory Health Insurance, 1912–1920* (Baltimore: Johns Hopkins Press, 1978). There were some insurance schemes at work: Greer Williams, *Kaiser-Permanente Health Plan: Why it Works* (Oakland: Henry Kaiser Foundation, 1971).
45. Details in RAC. RG.1.2 S.707 B.4 f.28–31.
46. C. E. A. Winslow, W.H.O. Program and Accomplishments. *International Conciliation*, March 1948.
47. Brock Chisholm, The World Health Organization, *British Med. Journal.* May 6, 1950. p. 4661.
48. Sawyer, W. "Medicine as a social instrument: tropical medicine." *New England J. Med.* 244 (1951): 217–224.
49. Hackett, "End of an era," *History.*
50. J. Grant, "Certain interim reflections on trends in medical care," presented to first meeting of Committee on Medical Care, January 17, 1946. RAC. RG.3 S.900 B.25 f.196.
51. J. Grant, "Medical Care," September 27, 1946. RAC. RG.3 S.900 B.25 f.197.

52. For details see, C. Webster, *Caring for Health. History and Diversity* (London: The Open University Press); J. Ross. *The National Health Service in Great Britain* (Oxford University Press, 1952). For a negative view of Britain's ability to pay for their welfare state see C. Barnett, *The Audit of War: The Illusion and Reality of Britain as a Great Nation* (London: Macmillan, 1986).
53. N. Oswald, "Training Doctors for the National Health Service: Social Medicine, Medical Education and the GMC 1936–48." In D. Porter (ed), *Social Medicine and Medical Sociology in the Twentieth Century* (Amsterdam: Rodopi, 1997), pp. 59–80.
54. J. Grant, "Medical care."
55. J. Grant, "International Trends in Health Care," presented to IHD December, 1947. RAC. RG.3 S.900 B.25 f.197. Revised and published with same title. *Amer. J. Public Health* 38 (1948): 381–97.
56. USPHS Subcommittee on Medical Care. RAC. RG.1.2 S.200 B.6. Details of the loan and plan to New York RAC. RG.1.2 S.200 B.49. That the Rockefeller Foundation's role in these issues was minimal is clear from Paul Starr, *The Social Transformation of American Medicine* (New York: Basic Books, 1982) and R. Stevens, *American Medicine and the Public Interest* (New Haven: Yale University Press, 1971), neither of which make mention of the Rockefeller Foundation, the Health Division or John Grant.
57. J. Grant, "The Place of the Health Center in Providing Adequate Health Care," June 1949. RAC. RG.3 S.900 B.25 f.198.
58. Meeting of Scientific Directors, May 1947.
59. M. Balfour et al., *Public Health and Demography in the Far East* (The Rockefeller Foundation, 1950.) These events are discussed further in J. Harr & P. Johnson, *The Rockefeller Century* (New York: Scribner, 1988).
60. IHD Staff Conference, May 19, 1948. RAC. RG.3 S.908 B.13 f.136.
61. Fosdick to John D. Rockefeller III, April 18; JDR III to Fosdick, April 23, 1945. RAC. RG.3 S.900 B.21 f.161. See also J. Harr & P. Johnson, *The Rockefeller Century.*
62. Résumé of letters. The U.S. Army poll is quoted in a letter from FMR to the committee. RAC. RG.3 S.900 B.21 f.161.
63. Report of the Special Committee on Policy and Program, submitted to the trustees, December 3, 1946. RAC. RG.3 S.900 B.24 f.178.
64. L. Allbaugh, *Crete: A Case Study of an Underdeveloped Area* (Princeton: Princeton University Press, 1953).
65. H. Carr to G. Strode. June 1, 1950; P Russell, "Some comments regarding the IHD—Past and Future." November 3, 1950; J. B. Grant, "Future—International Health Division Policy," October 25, 1950; H.H. Smith, "Comments on Future of IHD." August, 1950. RAC. RG.3 S.908 B.14 f.148.
66. L Kimball to Chester Barnard, "Preliminary agenda for the study of the IHD." RAC. RG.3 S.908 B.13 f.140.
67. Joint Meeting Policy and Program Subcommittees, February 1951; Report of the Rockefeller Commission on Review of the International Health Division, November 1951. RAC. RG.3 S.908 B.14 f.143, 147.
68. In his inaugural address of January, 1949, Harry Truman urged that the United States "embark on a bold program for making the benefits of our scientific advances . . . available for the improvement and growth of underdeveloped countries." An Act for International Development (the so-called Point-IV program) was approved in 1950, and the International Development Advisory Board, under the chairmanship of Nelson Rockefeller, established an attack on hunger and ill health as its first priority. W. Daniels (ed), *The Four Point Program* (New York: H. Wilson, 1951).

# 17

# Conclusion: Swinging Pendulums

The International Health Division did not simply disappear in 1951. It left a legacy of ideas and methods that were carried over into the WHO, particularly into its Global Malaria Eradication Program. Both the closure of the Health Division and the beginning of the malaria eradication program coincided with a mood of optimism toward the world's health problems. Publication of Paul Russell's *Mastery of Malaria* in 1955, with the message that the world was about to be relieved of this disease, only reinforced that feeling. At the same time, the First World had come to believe in an "ideology of development,"—foreign aid could narrow the gap between what were optimistically labeled "developing" and "developed" countries. Attacks on disease plus development aid were assumed to be the means by which poorer countries could progress, become modernized and thus westernized. But by the end of the "United Nations Development Decade" of the 1960s, victory in both the disease and economic spheres seemed as far away as ever and optimism waned. There was, in the words of two researchers, "a relatively brief evanescent epoch, flowering and expiring with almost equal rapidity."[1]

Attempts to improve the world's health and economic well-being can be likened to a series of pendulums, each swinging between two extremes. In the health world, the major pendulum swings from the belief that control, or even eradication, of communicable diseases is a prerequisite for socioeconomic development to the conviction that while disease control is necessary, socioeconomic develop-

ment is a prerequisite for improved health.[2] A second pendulum, closely related to the first, swings from those who believe that anti-disease campaigns can be directed "vertically" at single diseases and imposed from without, and those who argue that campaigns against communicable diseases must be directed "horizontally" at a broad band of diseases and involve at least a minimum amount of public health infrastructure; that disease campaigns cannot be imposed from without on a public health *tabula rasa*. In the field of development, a similar pendulum swings from proponents of the ideology of development to those who believe that the international system creates and perpetuates underdevelopment as poor countries "become locked into a permanent system of inequality."[3]

In the early 1950s all these pendulums were positioned at their extreme positions. Most experts not only subscribed to the ideology of development but agreed with Soper, Hackett, Paul Russell, and other members of the Health Division that the eradication of disease must precede social and economic improvement. They believed also that vertically imposed disease campaigns were the best way to achieve this. These, then, were the beliefs in place when, in 1955, the WHO started its Global Malaria Eradication Program.

## The Global Malaria Eradication Program

The global campaign was set up in accordance with the basic Health Division creed that to better the human lot, disease eradication, none more important than malaria, took priority, and that such programs could be carried out on populations whatever their level of development or whatever the level of public health service was available. Indeed, George Macdonald of the London School of Hygiene and Tropical Medicine credits Fred Soper and the Health Division with the concept of total malaria eradication, and for proving that "the eradication concept is not only theoretically, but practically possible."[4]

Soper had resigned from the Health Division in 1947 to become the Director of the Pan American Sanitary Bureau (PASB). In his new position, Soper, a man of action, was able to initiate schemes for the elimination of diseases, beginning with the eradication of their vectors. Immediately on taking office, he launched two regional DDT campaigns against *Aedes aegypti*, in the River Plate area and in northern South America, as the initial stage in eradicating *Aedes aegypti* from the Americas. For the first time, he said, "an international organization is undertaking the continental eradication of an insect vector."[5] From previous work in Brazil and Egypt, and from the ongoing Sardinian campaign, he felt confident that "no insoluble problems have been encountered, and ultimate success is anticipated."[6] At this stage Soper did not see the use of DDT as a way to interrupt transmission and thereby eventually eliminate the disease, but as a means to render species eradication less difficult; disease eradication essentially implied vector eradication.

In 1948, at its first meeting, the 18-member executive board of the WHO adopted what it called a new approach to health problems—but which could equally be called the Hackett-Soper approach. In place of quarantine and other outdated defensive concepts, it proposed a more positive, aggressive approach, an investment which would bring socioeconomic gains. By the use of three magic bullets, the BCG vaccine, penicillin, and DDT, it proposed to attack tuberculosis, venereal disease and malaria, respectively.[7] As early as the Third World Health Assembly in 1950, the foundations for a global attack on malaria had been laid with demonstration DDT campaigns at work in Afghanistan, Pakistan, Persia, Thailand, and 14 provinces of India. Even by then, the complete eradication of malaria from the world had become the WHO's aim.[8]

In 1950, at a symposium of the National Malarial Society of the United States, Soper announced nationwide malarial eradication projects in the Americas, noting that with DDT "the malariologist can legitimately join the yellow fever worker in talking of disease eradication."[9] By spraying human habitations with residual DDT, however, Soper seems to have shifted from species eradication to disease eradication. He still spoke, confusingly, of species eradication—which he had always viewed as the best long-term solution—but he was taking a different tack when he wrote, "the eradication of malaria may be considered as involving, first and foremost, the blocking of transmission by mosquitoes."[10]

This was the favored method when the 8th World Assembly of the WHO inaugurated the Global Malaria Eradication Program in 1955, and the Expert Committee on Malaria issued its sixth report in 1956. There was no longer any justification for advising vector eradication as a universal mechanism for malaria eradication, the committee said; rather, the aim was "anophelism without malaria." Anopheles mosquitoes would still exist, but there would be no parasites to transmit. Furthermore, "eradication of malaria should be looked on as an urgent measure, *outside the regular routine of health departments* (author's italics)." Basically there were to be three stages in these campaigns: (*a*) the attack phase, when DDT would be applied to houses for a number of years, (*b*) the consolidation phase when the insecticide would be discontinued, and the population surveyed to find and eliminate pockets of transmission, and (*c*) the maintenance phase, when, with eradication achieved, a constant watch would be kept for imported new infections. The expert committee acknowledge that the method of eradicating malaria from sub-Sahara Africa had not been determined.[11] Russell believed malaria could be eliminated from Africa. "This is the DDT era of malariology," he wrote. "For the first time it is economically feasible for nations, however underdeveloped and whatever the climate, to banish malaria completely from their borders."[12] And if that were possible for malaria, why not for other vector-transmitted diseases too?

As the malaria campaigns proceeded, problems arose and both health pendulums began to swing again. In 1960, the Expert Committee on Malaria noted that any malaria program required parallel development of rural health services;

it could no longer be simply imposed from outside.[13] This concept was hammered home in the committee's ninth report, two years later, and by Emilio Pampana, formerly of Rome University, who had played a major role in the WHO's malaria work. In his *Textbook of Malaria Eradication*, published in 1963, Pampana argued that malaria campaigns and elementary public health services, consisting of rural health posts, should be developed side by side, so that eventually the two services together would be able to carry out the eradication program. The original idea of a separate malaria organization now carried "disadvantages." Instead, the eradication program needed to be integrated with general health services.[14]

The second pendulum, at least, seemed to have moved back to somewhere near to where it was when the Health Commission began its work in 1914. In words that sounded much like those of Rose in the early years of the then Health Board's hookworm campaigns, the expert committee spoke of malaria as "a major incentive for the establishment of basic permanent health services," the role played by hookworm in Rose's plans. "Mass campaigns cannot be separated indefinitely from other health activities," warned a WHO study group in 1965. Such campaigns are often "indispensable in breaking the vicious circle of sickness, low productivity and poverty," the study group continued, but "they should not forget . . . that the ultimate goal must be the establishment of a permanent scheme of general health services."[15] Rose would have smiled! Soper, who had retired from the Pan American organization in 1959, was not impressed by the WHO's new position, accusing them of abandoning his original plan of malaria eradication. It was still possible, he argued, to eradicate diseases where no health services exist. "The WHO was going about it in the wrong way."[16]

By this time, the expert committee had come to realize that while the eradication campaign had brought "a sustained measure of well-being" to over 953 million people, 95% of the countries in the maintenance phase and 87% of those in the consolidation phase were in Europe, the Americas and Southeast Asia. Africa still remained exposed to malaria with 193 million people inhabiting areas which had not even started a campaign.[17] The director general of the WHO admitted, in 1969, that in sub-Sahara West Africa "no practical method of interruption of transmission . . . has been found," and thus a more flexible approach was needed.[18]

By 1970, both health pendulums had reached almost the opposite position from where they had been situated in 1950. No longer were vertical campaigns against single diseases in vogue. General health services were now deemed imperative, and any mass campaign had to be an instrument for the development of these services, similar to Rose's vision in the earliest years of the Health Commission. From their experiences in Africa, the WHO was beginning to argue that socioeconomic development was the key to the puzzle, although how it was to be unlocked was never made clear. In 1971, the Expert Committee on Malaria stressed a malaria control program needed to cooperate "with all concerned with socio-

economic development," and that "the situation can only be changed by the intensification of scientific research into improved antimalaria methods and by determined efforts *to expedite the socioeconomic development of* these areas (author's italics)"[19] In 1979, the 32nd Assembly of the WHO launched its "global strategy for health for all by the year 2000," in which they finally rejected the idea of imposing Western medicine on Third World countries and the step-by-step vertical elimination of one disease at a time; primary health care and community participation had become the new buzzwords.[20]

The goal of securing socioeconomic development in these countries was fast approaching another crisis point, as the same type of swing took place in the development pendulum. By the 1970s, despite large-scale funding from the international community, the realization dawned that poverty and unemployment had not decreased and there had been no trickle-down to the impoverished. Instead, in countries such as Brazil, India, Mexico and Nigeria there had been a trickle up to a new elite who were seen as guilty not only of exploitive behavior towards their own people, but had emigrated in large numbers to the wealthier countries of the world.[21]

## Thomas McKeown and the Social Historians

By subjecting the Health Division, either directly or indirectly, to their different interpretations, historians too have played a role in the swinging of these health pendulums. In the 1970s, with the publication of two books by Thomas McKeown, professor of social medicine at the University of Birmingham, the larger health pendulum received yet another major push away from where it had been in the 1950s.[22] In both books he attributed the post eighteenth century rise of English and Welsh populations to a reduction in deaths from infectious diseases. McKeown dealt only with the diseases of England and Wales, and not with the diseases (other than tuberculosis) with which the Health Division was traditionally concerned. These books punched home the same message; historically, medical developments, including most biomedical interventions into preventive medicine such as vaccines, have had no significant effect on the downward trend of disease mortality. In the past, "the predominant health problems—the infections—were associated with poverty, for it was the removal of influences such as malnutrition, defective hygiene and bad living and working conditions that led to their decline."[23] By implication, McKeown called into question the long-term value of the narrow, disease-focused campaigns with which the Health Division and the WHO had been associated, and the nature of the biomedical training offered in the Health Division's schools of hygiene.

More recently other social historians, sharing McKeown's social views of health, have subjected the Health Division's malaria campaigns in northern Brazil, Egypt and Sardinia to a more critical analysis, taking into account the social conditions

in which the malaria victims lived. All three locations had become important weapons in the Health Division's armory and in their belief that good health required diseases to be eliminated by the eradication of their vectors, social conditions being basically irrelevant.

Packard and Gadelha have argued that Soper failed to understand the social and economic context of the malaria epidemic in northern Brazil.[24] The narrow east coastal belt of northeast Brazil, the only region with enough rainfall and suitable soil to support a plantation economy, is worked by landless peasants, the Brazilian "pygmies." Victims of debt and peonage, malnourished from birth with so little protein in their diet that the WHO estimated their average height and brain sizes to be 40% less than the Brazilian average, these people were and are highly susceptible to malaria.[25] The dry inland regions of northeast Brazil, the Sertão, making up the states of Ceara, Paraiba, Pernambuco and most of Rio Grande do Norte, lie inside the drought polygon of the northeast. Here, where sharecroppers work the agricultural estates gathered along the river valleys and raise cattle in the huge *fazendas* of the upland areas, the periodic droughts can be catastrophic. In such conditions, sharecroppers would be unable to meet debts incurred prior to harvesting and many would be evicted, with the result that, in times of drought, many migrated to Amazonia or to the coastal region, creating even greater food shortages.[26]

Even without droughts, moderate levels of malaria exist along the coastal strip and along the river valleys during the rainy season. But with a drought, as happened in 1937 and 1938, the increased migration into the area and the subsequent increased levels of malnutrition and hunger, could provide fodder for a major malaria epidemic. In addition, agricultural laborers returning to the area from Amazonia after the drought declined, might be carrying the parasite from the highly endemic Amazon valley. Epidemics were therefore as much a result of drought, migration, famine and poverty as of mosquitoes. The arrival of *gambiae*, an extremely efficient vector, only exacerbated conditions that already existed. But Soper, with eyes fixed entirely on the mosquito, failed to recognize these factors.

Similarly, in Egypt, the human condition of the *Fellaheen* was a major factor in generating the serious epidemic of World War II. *Anopheles gambiae* was but one of many causes.[27] As in Brazil, poverty and malnutrition were major factors, in this case brought on not by drought and migration, but by the presence of over 300,000 Allied troops in the northern part of the country. To supply these troops, and because the Mediterranean was closed to commercial shipping (a major cause of the Paris green rumpus) (see Chapter 9), the British purchased huge amounts of Egyptian cereals with the result that the *Fellaheen* faced severe food shortages and increased food costs. The subsequent malarial epidemic, and the presence of an effective vector, led the population into an ever-worsening cycle. Weakened or incapacitated by the disease, the *Fellaheen* were unable to work, and food declined sharply, worsening their plight. Northern food crops that could have been

redirected to them were instead sold to Allied troops. But again, Soper remained unaware of the human dimension to the malarial outbreak.

Having assumed *gambiae* to be the single guilty party in both Brazil and Egypt, Soper necessarily believed that its eradication caused the collapse of both malaria epidemics. But again, in both Brazil and Egypt, such an assumption seems overly simplistic. In Brazil, for example, rains in 1940 brought an end to the drought and reversed the migratory trends. These events, as much as the destruction of *A. gambiae*, might have brought an end to epidemic malaria, although endemic malaria continued to exist. In Egypt, too, a host of other factors beyond the eradication of *gambiae* brought an end to the epidemic, all of which Soper ignored. Indeed one can argue that the anti-gambiae service merely brought to an end an already dying epidemic. In addition, by the end of 1944, Egypt and the *Fellaheen* were no longer at the cutting edge of British attempts to stave off possible German victory. Soper's assumptions about the decline of malaria in Brazil and Egypt merely reinforced the Health Division's basic attitude towards disease. From their perspective, the poverty and malnutrition shared by the Egyptian *Fellaheen* and the Brazilian sharecroppers and pygmies were irrelevant to the beginning and the end of the disease.

Similar comments can be made about Sardinia, an island which today has been transformed into a tourist Mecca, where "des plages de sable fin, des hotels haut de gamme, les plus beaux bateaux et de nombreuses activites sportives," will await you.[28] According to older tourist books and brochures this was brought about by the Rockefeller malaria campaign which removed the curse of malaria and made possible the subsequent economic development of the island.[29] But Peter Brown has argued recently against positing a simple relationship between malaria control and economic development.[30] Postwar changes are best understood, he argues, in terms of "political-economic variables." The history of postwar Italy substantiates Brown's claims.[31] The postwar Committee of National Liberation, in an attempt to break down the barriers between the two nations of Italy (the rich progressive "European" north and the backward, illiterate "African" south) set up five autonomous regions, including Sardinia, and undertook to break up the *Latifondo*, or large estates, with absentee landlords worked by day laborers. But the power of the political left that pushed for these reforms was quickly eroded. In May 1947, they were dropped from the government; the Christian Democrats became the court party dedicated to the preservation of the status quo and supported by northern industrialists, the Vatican, and the old political bosses of the south who had infiltrated the Christian Democrats. In addition, loans from the U.S. government, including Marshall Aid, were conditional upon the maintenance of a communist-free government. The government introduced only minor agrarian reforms in the south, and set up a social welfare scheme, *Cassa per il Mezzagiorno*. In the late 1950s, with discoveries of gas and oil deposits, northern Italy began to

boom and many of its citizens, including many migrants from the south, began working in industry there. Hydroelectric power stations, petrorefining and road building came to Sardinia; with something approaching a $50 million investment, the Aga Khan set up a tourist ghetto of luxury hotels and villas along the northeast coast of the island—the Costa Smeralda.

Continuing outmigration and capital-intensive schemes with power, ownership and profits in non-Sardinian hands, have resulted in what critics see only as a "veneer" of economic development. Most of the islanders moved to urban centers to take low-paying unskilled jobs in industry and construction, or service jobs in the tourist industry. The Sards were now mostly free of malaria; but their lot in life had improved little and only noncritical tourists wearing blinders could call what had happened progress. Removal of the malarial parasite was a necessary precursor to the development of tourism, but there are those who argue that the millions spent on eradicating mosquitoes would have been better spent on a lower-cost DDT campaign, and the surplus redirected at island development—introducing universal primary school education for example. Tourists have certainly replaced mosquito vectors in Sardinia but the indigenous population remain second-class citizens. It remains a matter of debate whether the eradication scheme had a happy ending for Sards themselves.

In the same vein, Margaret Humphreys has examined the history of malaria in the United States, where the disease had almost disappeared by World War II, the major drop occurring between 1938 and 1942.[32] This steep decline did not correspond with the major DDT campaigns against malaria which took place after the disease had almost disappeared. This hugely expensive campaign very much resembled the earliest Health Board campaigns in Mississippi and Arkansas in that the goal was to eliminate mosquitoes, not a disease, which, by that time was no longer there.[33] So why the sudden decline?

Unlike many social historians, Humphreys, who is both a physician and an historian, was not prepared to dismiss out of hand the antimalarial activities of the USPHS and the International Health Board. But she has also attributed the decline in the late 1930s more to socioeconomic events than to the actual malarial campaigns, in particular outmigration of blacks from the rural south, as they moved north and west. "The parasite's ecology [in the United States]," she writes, "was fragile to begin with; as the sharecropping cotton farmers disappeared from the rural South, malaria left as well."[34]

The message of these historians was clear. It was simplistic to attribute the beginning and end of epidemics solely to the arrival and departure of parasites and their vectors. They were not out-and-out critics of the Health Division; rather, by focusing on some of the social problems, they drew attention to the narrow focus of the Health Division's approach, and perhaps helped push the major health pendulum away from where it had been in the 1950s.

## A Capitalist Plot?

In the late 1970s a new and hard-edged group of ideologically-driven historians took center stage. These authors attacked the Health Division directly, portraying it as one of the means by which the United States attempted to develop and control the markets and resources of foreign countries in order to enhance the profits of its own businesses. Because so many in these countries suffered from hookworm, malaria, yellow fever and other diseases, programs first needed to be put in place that would enhance the health and working efficiency of these populations. This would render them more attractive for American cultural, economic and political domination and create a more favorable attitude towards American economic and political domination.[35] Public health operations of the Health Division were, in other words, a series of covert operations manipulated by the Rockefeller family to further its own capitalistic ends. Thus, as two of these historians note, this "advance team of public health doctors set out to prepare vast regions for commerce, investment, and increased productivity, motivated variously by economic, humanitarian, religious, scientific, and political goals."[36]

These authors make much of the decision of the Sanitary and International Health Commissions to begin work on hookworm, a parasite long known to be a major cause of anemia and low productivity among plantation workers across the tropical and semi-tropical world. Not for nothing was it called "the germ of laziness." Historically, hookworm had always been intimately tied to lowered productivity and reduced profits. There was nothing unique or surprising in the Health Board's link between public health and an economically viable and stable society. Workers, union leaders and property owners in most countries where such things matter at all, whether capitalist or not, now or in the past, have expressed concern over the health of their working populations. Such concerns do not necessarily indicate some diabolical plot.

Soma Hewa has argued correctly that British colonial policy enabled planters in Sri Lanka (Ceylon) to ignore the health of their workers in favor of their own economic goals. He says that the entry of the Health Board, labeled by him as John D. Rockefeller's "ideological mentors," served "the long-term economic and political interests of American industrial capitalism."[37] Apart from stating the obvious—that it would be good economics to keep Tamils healthy—Hewa fails to say how their work forwarded the interests of *American* capitalists. He nevertheless says the "Rockefeller capitalists . . . were convinced that philanthropic medicine would be a powerful instrument to expand American political and economic domination in the non-Western world."[38]

In a similar vein, Anne-Emanuelle Birn and A. Solóranzo play down the medical aspects of the Mexican hookworm campaign by arguing that the Health Board chose to focus its campaign in the province of Veracruz primarily because of its strategic value to the Mexican and U.S. governments. It was the hub of the

country's oil and foreign commerce as well as an important agricultural producer.[39] To back up their claim, they say the decision to campaign in Veracruz was taken even before Henry Carr undertook a primary hookworm survey, a survey which itself revealed only a low-level infestation, suggesting, of course, that hookworm itself was somewhat irrelevant to the Mexico campaign. But Carr found nearly 10% of males in the hookworm region to have very severe infections designated by a hemoglobin level of less than 36.5%, and over 40% had "a significant grade of severity," with a hemoglobin level between 36.5 and 60%.[40] Contrary to Birn, hookworm was a problem in that part of Mexico, and on medical grounds alone Veracruz would seem a natural place to work. Indeed, to knowledgeable members of the Health Board, or to anyone who knew anything about the disease, it was obvious "from theoretical considerations alone," as Carr himself noted, that hookworm would be concentrated along the eastern coastal strip and foothills with heavy rainfall, including Veracruz. Carr called this strip "the hookworm area of Mexico," in contrast to the dry inland plains at higher altitudes. Neither is it true to state that Carr surveyed only one-third of the Mexican states because, on political grounds, the Health Board had already pinpointed Veracruz as the site of the campaign. Carr in fact surveyed several representative areas in the east and west mountain areas, and in the dry inland central plateau. But he did so to check out what he already knew: the coastal plains and foothills, not the high altitudes and dry plains would be the site of hookworm infections.

Solórzano claims the yellow fever campaign in Mexico aimed "to create a ring around the coastal regions of Mexico," in order to stop the disease from spreading north to the United States, and neglected the interior regions. Thus the author ignored the seed-bed theory of yellow fever which necessarily focused on urban ports and required that campaigns be primarily located in coastal cities. There were, at that time, sound medical reasons to ignore the interior regions.[41]

If the Health Board was indeed concerned about political stability and the support of American oil investments in Mexico, one has to ask why it spent so little of its money there. Critics are correct in saying the Health Board spent less on a per capita basis in Mexico than in other Latin American countries. Birn seeks to explain this by pointing out that the Health Board (later Division) linked Mexico with Canada and the United States in the North American administrative division of their activity. But why should that have led them to basically ignore Mexico? No physician could possibly have equated the disease situation in Mexico with that in Canada.

Franco-Agudelo goes further in his critique of the organization, likening it to the infamous United Fruit Company, which brutalize and exploited its South American workforce. According to him, the Health Division, "concealed by its philanthropic camouflage," set about attacking malaria for exactly the same reasons as did the Fruit Company—to maximize its profit margin.[42] This argument is absurd. In my opinion, the officers of the Health Division could not possibly be

likened to the crude, jingoistic, exploitative and even ugly characters who worked for the American fruit and sugar companies; Frederick Russell was not a Sam Zmuri and Lewis Hackett was not a Guy "Machine-Gun" Molony. On the contrary, what the Health Division's archives indicate to me is an organization with its sights fixed on narrow medical concerns. With the notable exceptions of Rose and John Black Grant, it is a surprise to find political or social comment on the countries they work in. If there were ulterior motives involved, one would surely expect to see references to them in their letters and diaries.

More significantly, neither the allocation of its budget nor the way in which the Health Division operated sustain the thesis that it was an American capitalist advance guard used as a wedge for capitalist manipulation.[43]

During its years of operation (1914–1950) fully 61% of the Health Division's total budget was spent in North America and Europe, and only 28% in South and Central America and the Caribbean (Table 17.1). Indeed once Rose stepped aside, the Central American (in which I have included Mexico) share of the budget fell from 16% to under 2%. A business plan for American economic dominance would not have allocated funds that way. Why would an organization concerned with the economic and political domination of nonindustrialized countries by the United States spend less than one-third of its budget on countries in South and Central America which were most under U.S. influence and most susceptible to such machinations? And the changing geographical budget allocation simply does not square with the politico-economic interpretation. As Table 17.1 shows, the percentage of the budget spent in countries outside North America and Europe declined from a high of 52% during the Rose years to 38% under Russell, 39% under Sawyer and only 29% under Strode. The budget for the East—China, India and the Philippines—remained small throughout the organization's history, about 10% under Rose, Russell and Sawyer, dropping to 5% under Strode. More surprisingly, the budget allocated to those countries most within the compass of American influence either remained static, as in South America, or declined rapidly, as in the budgets for Central America, Mexico and the Caribbean. In total, these latter bud-

**TABLE 17.1.** Geographical distribution of the budget during the Rose, Russell, Sawyer, and Strode years

| LOCATION | ALL YEARS | ROSE (1916–23) | RUSSELL (1924–34) | SAWYER (1935–44) | STRODE (1945–50) |
|---|---|---|---|---|---|
| North America | 34% | 19% | 30% | 42% | 49% |
| Europe | 27% | 29% | 32% | 19% | 22% |
| South America | 20% | 17% | 21% | 22% | 16% |
| The East | 8% | 10% | 7% | 11% | 5% |
| Central America | 4% | 16% | 2% | 1% | 1% |
| Caribbean | 4% | 9% | 4% | 3% | 2% |
| Africa | 3% | 0% | 4% | 2% | 5% |

gets dropped from 25% in the Rose years to less than 6% thereafter. This simply does not look like the budget of an organization primarily intent on spreading the tentacles of American capitalism.

Finally, my position is supported by the North American budget; it went from strength to strength, from 19% under Rose to 49% under Strode. In 1950 nearly 60% of the *total* budget had been allocated to projects there (Tables 17.1 and 17.2). In absolute terms, while the total budget declined from about 1935 on, the amount spent in North America declined much less than that spent on the poorer countries of the world (Table 17.2). And fully one-third of the 2566 public health and nursing fellowships were granted to applicants from the United States and Canada. This was not an organization that was spending money in the nonindustrialized countries of the world to further U.S. economic goals.

Furthermore, the structure of the Health Division's governance would hinder or even completely prevent any decision taken only to further the goals of American capitalists, from running down the network of the Rockefeller family and their staffs in New York to the field staff on the frontline. Almost from its inception, the International Health Division became the richest and most powerful branch of the Rockefeller Foundation. It was run by a large team of medical barons, who ranged from the director himself, backed by a few associates and assistants in New York and a large field staff. Men like Fred Russell and Paul Russell, Sawyer, Strode, Heiser, Soper, Grant and Hackett made the decisions. This was certainly the case after 1929 when a team of scientific directors, who often seemed to have acted mainly as a rubber stamp, stood between the Health Division and the trustees of the Rockefeller Foundation. The trustees knew virtually nothing about the mysteries of modern scientific medicine, and always included either Rockefeller Jr. or his son, and such men as John Foster Dulles or Rockefeller's brother-in-law, Winthrop Aldrich. They were essentially cut off from day-to-day decisions. And the field officers, who were sometimes separated from New York by mail which might take as long as six weeks

**TABLE 17.2.** Budget allocation (in millions of U.S. Dollars—1990) and percentages distributed to various geographical areas at five year intervals

| | NORTH AMERICA | | EUROPE | | SOUTH AMERICA | | CENTRAL AMERICA/ CARIBBEAN | |
|---|---|---|---|---|---|---|---|---|
| YEAR | DOLLARS | PERCENT | DOLLARS | PERCENT | DOLLARS | PERCENT | DOLLARS | PERCENT |
| 1916 | 1.2 | 28% | 0 | 0 | 0.3 | 6% | 2.1 | 49% |
| 1920 | 1.7 | 19% | 3.5 | 39% | 1.8 | 20% | 1.2 | 14% |
| 1925 | 4.2 | 21% | 7.3 | 37% | 4.2 | 21% | 2.0 | 11% |
| 1930 | 3.2 | 28% | 4.0 | 34% | 1.9 | 16% | 0.5 | 4% |
| 1935 | 8.0 | 49% | 4.4 | 27% | 2.4 | 15% | 0.8 | 5% |
| 1940 | 4.4 | 39% | 2.7 | 24% | 3.1 | 27% | 0.3 | 3% |
| 1945 | 2.6 | 37% | 1.2 | 17% | 1.8 | 26% | 0.4 | 6% |
| 1950 | 2.8 | 59% | 1.1 | 24% | 0.3 | 7% | 0.3 | 7% |

in either direction, had to be granted a great deal of autonomy. There existed what Paul Russell called a "protective barrier," not only between the field staff and the New York staff, but between the New York staff and the trustees. This, according to Russell, worked to everyone's advantage—although it also seems to have prevented the organization from developing long-term plans. Birn was correct when she called attention to the changeable qualities of their decision making, a fact which makes it very difficult to pin them down.[44]

There can be no debate about the influence of the Health Division in disseminating a narrow biomedical approach to public health. Some critics charge it was supported by the Rockefeller Foundation *because* it "legitimizes the great inequalities of capitalist societies and the misery that results from the private appropriation of human and environmental resources."[45] That is, the Rockefeller organizations consciously decided to support the modern germ theory and the disease-centred approach to public health in order to legitimize the political and economic status quo. But, during the first half of the 20th century, people of every political shade seemed to have believed in the potential miracles of modern scientific medicine, and saw disease as roots of evil which at last seemed capable of eradication. Other than social medicine which had gone into a decline at that time, there was no alternative model.

This "medicalization of public health" certainly has resulted in the avoidance of those social and economic ills which are a natural outcome of unbridled capitalism. It does support the social status quo because, according to this interpretation, the maintenance of good health requires only a biomedical solution, without any reconstruction of society and without placing impediments on the workings of the capitalist system. But I deny that Russell and his colleagues believed in their narrow approach to public health because they were duped by the Rockefellers and became involved in some capitalist-conspired plot. I remain unconvinced that this makes Russell and Hackett mere "ideologues for capitalist society," mere tools of Rockefeller and other American capitalists. In my opinion, the conspiracy theory adds nothing to our understanding of the Health Division.

One cannot deny, however, that the medical barons brought much favorable publicity to bear on themselves, Americans in general and the Rockefellers in particular. So, in the end, perhaps one plot did succeed. The International Health Division played a crucial role in saving the Rockefeller name from almost universal condemnation. In 1913 the American public associated his name with the worst features of brutal, unbridled, and unprincipled capitalism. Today the Rockefellers are generally seen to have distributed their wealth wisely, particularly in the areas of health and medicine. It was a major reason the Health Division was so long able to survive attempts to close it. Through the Health Division in particular, the Rockefeller Foundation gained a worldwide reputation for medical benevolence and enhanced the Rockefeller name.

## Back to Square One?

Today some of the pendulums seem to be swinging back again as diseases such as yellow fever and malaria recover their energies. The WHO estimates there may be 200,000 cases of yellow fever in African children every year, and warns that with *Aedes aegypti* now once again endemic to many South American and southern U.S. cities, future epidemics cannot be ruled out.[46] The reappearance of *Aedes aegypti* carried back into many South American cities from the United States, which refused to sign on to Soper's earlier anti-*Aedes* campaign, has resulted in the appearance of another *Aedes* vectored disease, dengue fever. In 1982 the first Brazilian epidemic of dengue fever occurred and it has since become a major public health problem in Latin America and the Caribbean.[47] And if dengue fever, why not the far more serious yellow fever whose virus still circulates silently among the monkey population.[48]

The WHO is again calling for a global campaign against tuberculosis, AIDS and malaria; we seem to be back in the 1950s, without that era's optimism. As the *Washington Post* reported in 2002, "The new (*sic*) theory, supported by various economic analyses, is that the infections are not only the product of poverty; they also create poverty. Relieving a population of the burden of diseases for 15 or 20 years will be a huge boost to economic development." The newspaper quoted a WHO official who said that, "healthy people can get themselves out of poverty."[49] Neither Paul Russell nor Soper would have been particularly surprised by this state of affairs given what had happened to their single-focus vertical campaigns against malaria and yellow fever. Perhaps, after all, their approach was as successful as we can expect in this never-ending struggle?

Furthermore, the "ideology of development" and the long discredited trickle-down theory of economics seem once again to be capturing attention. Worried by hatred and terrorism, which some link to poverty, hopelessness, and despair, some rich countries are once again promising more developmental aid and are being told, not for the first time, that there is now general agreement over what is required to overcome global poverty. The new buzz word, to quote the *Washington Post*, is "selectivity." Western aid is to be delivered only to those countries that respect the rule of law and attack corruption. In a sort of carrot and stick mentality, some western experts believe that only in such countries would benefits trickle down to the poor.[50]

This view of the future as a repeat of the past is not shared by many in the WHO or the Rockefeller Foundation. Only a few years ago the WHO was congratulating itself over the steady rise in human health over the past half-century; it now reports "a dark cloud," threatening "to blot out the sun from this landscape."[51] Measuring average health parameters of whole countries had blinded health officials to what should have been obvious all along. Those at the bottom of the socioeco-

nomic continuum in every country, no matter what its GNP, not only suffer more ill-health than those at the top, but the divide has been widening. New health policies must therefore be put into place, both groups argue, to narrow this divide and enhance "health equity." In contrast to Western politicians who are offering aid and trade only to selected countries, those in favor of equity policies do not subscribe to the concept that the health of the poor can best be achieved by general economic growth from which the poor benefit by wealth trickling down. Instead, this group now favors a direct attack on this divide either by directly improving the health of the poor, or redressing health inequities.[52]

The Rockefeller Foundation has become an important agent of this movement, illustrating how far they have moved since the years of the Health Division. In 1996 they helped fund the Global Health Equity Initiative, which led to the publication of *Challenging Inequities in Health*[53] and in 1999 set in motion its Health Equity Program. As their 1999 Annual report noted:

> In the beginning of the 21st century, a striking paradox characterizes global health. Unprecedented advances in health have accompanied pervasive, growing and deeply troubling disparities in people's health. The distribution of health, not its production, is likely to become an urgent priority in the early years of the 21st century.[54]

## Afterlives

Margaret Humphreys and I share an admiration for those, who, in her words, "tramped through swamps, stared all day long through microscopes, and otherwise risked comfort and health to bring malaria under control in the American South."[55] I am sure she feels the same way about the field staff of the International Health Division who, like members of the British Colonial Service, rejected the comfortable life and opted instead for adventure, danger, and considerable satisfaction. So what happened to the medical barons, the chief actors in the story of the International Health Division, after it closed down in 1951?

Rose died in 1931. Sawyer, after his 1944 resignation, lived only until 1951.[56] Strode too, after his retirement in 1951, lived only a few more years, dying in 1958 after a long illness. In contrast, Fred Russell lived a full life after his retirement in 1935. He served as professor of preventive medicine and epidemiology at Harvard from 1936 to 1939, was alive and well when the Health Division closed, and finally succumbed to old age in 1960 at the age of 90. Victor Heiser outlived even Russell, his old nemesis. After his retirement (or was he fired?) in 1934 he became interested in leprosy and spent the rest of his life working on the disease at the WHO, the USPHS and the Leonard Wood Memorial Fund for the Eradication of Leprosy—which he helped establish. He ascribed his long life to exercise, a healthy diet and to the fact that he gave up smoking when 93 years old! I have "no regrets," he told a newspaper reporter on his 96th birthday, "it's hard for me to think of any place I haven't been, or anything I haven't done." He died on Feb-

ruary 27, 1972, a few weeks after his 99th birthday, a contented man and one of the great characters of the Health Division.

Others continued to work for the new division of medicine and public health. John Grant returned from Paris in 1951 to take charge of the division's medical care program. In 1954 he was transferred to Puerto Rico where he remained until his retirement in 1960. He died two years later. Paul Russell, who returned to the health division in 1946 after his discharge from the U.S. army, continued as the foundation's malarial expert until his retirement in 1959. "After the war," he wrote, "my work consisted largely of being loaned, making surveys, and making recommendations." Soper, after his retirement from the PASB, became a health consultant to the U.S. government and for one year acted as the director of the cholera research laboratory in Dacca, Bangladesh. He died in 1977 at the age of 83.

The final years of Lewis Hackett, to me the most likeable of all the Health Division's officers, were rather sad. In 1950, a year after his retirement, his beloved wife, Hazel, she who loved Rome so much, suddenly died.[57] It must have been a terrible blow; a friend wrote, "I could never picture one of you without the other." They had met when he was a medical student at Harvard and during his final year, when Hazel was teaching school in California, they wrote to each other every evening, planning their future together and never in their wildest dreams imagining the life they would eventually lead. His letters to her are housed today in the Rockefeller Archives and to my mind are far too intimate for the curious eyes of archivists and historians.[58] I felt I was intruding, and quickly turned them aside; they were for her eyes only. Perhaps they should be burnt and their ashes scattered in Rome, or perhaps along the Kicking Horse River in British Columbia. For there (and as a train enthusiast I simply could not ignore these letters) in 1912, the young couple spent part of their honeymoon sitting hand-in-hand in the dome of the Canadian Pacific Railcar as it climbed up from Kamloops over the Rogers Pass, slipped down from the Selkirks, across the Rocky Mountain Trench, to grind its way up the gorge to Field, and from there, to avoid the infamous Big Hill, to pass through the recently-completed spiral tunnels to the Kicking Horse Pass, the highest in the Canadian Rockies. They were both awestruck by that experience, as all should be, even though it rained the whole way. Then suddenly it was over. "So Lewis," one of their closest friends (and they had many) wrote, "you must work, as you always do, and live with your wonderful memories of companionship through so many years." He occupied most of his remaining 12 years without Hazel as professor of public health at Berkeley's School of Public Health, editor of the *American Journal of Tropical Medicine and Hygiene* between 1952 and 1957, and president of the American Society of Tropical Medicine in 1958. In these years too, Hackett put together his manuscript on the history of the International Health Division, which remained incomplete when he died in 1962. And though I find his approach to

health too narrow for my tastes, I would like to think of this book as my tribute to Lewis and Hazel Hackett.

## Notes

1. J. S. Coleman & D. Court. *University development in the Third World. The Rockefeller Foundation Experience* (Oxford: Pergamon Press, 1993).
2. Dr José Nájera, formally of the WHO, calls these two extremes the "anti-parasite group," and the "anti-disease group," respectively. J.A. Nájera, "The control of tropical diseases and socio-economic development." *Parassitologia* 36 (1994): 17–33.
3. J. S. Coleman & D. Court. *University development*, p. 6.
4. G. Macdonald, "Eradication of Malaria." *Public Health Reports* 80 (1965): 870–879.
5. A Report of the Director of PASB to Member Governments, 1947–50. PASB Official Documents 25 (1958): 1–20.
6. F. Soper, "Species sanitation as applied to the eradication of (A) an invading or (B) an indigenous species," *Proc 4th International Congress on Tropical Medicine and Malaria*. 1948, pp. 850–857.
7. WHO Newsletter. "Executive Board concludes 3-week session; approves outlines for new approach to health problems." National Archives of Canada (NAC) RG.29 Vol. 1002 f.340.
8. Third World Health Assembly. Annotated Agenda: Malaria. NAC RG.29 Vol. 1003 f.342.
9. F. Soper. "Nation Wide Malaria Eradication Projects in the Americas." *J. Nat. Mal. Soc.* 10 (1951): 183–194.
10. Ibid., p. 185. That the global malaria program was linked to the cold war mentality of that age has been discussed by Socrates Litsios, "Malaria control, the cold war, and the post war reorganization of international assistance." *Medical Anthropology* 17 (1997): 255–278; R.M. Packard "'No other logical choice': global malaria eradication and the politics of international health in the post-war era." *Parassitologia* 40 (1998): 217–229.
11. Expert Committee on Malaria. 6th Report, 1956. WHO. Technical Report Series. No 123. For a brief account of events leading up to the global eradication program, see Socrates Litsios, *The Tomorrow of Malaria* (Wellington, NZ: Pacific Press, 1996).
12. P. Russell, *Man's Mastery of Malaria* (London: Oxford University Press, 1955), p. 160.
13. Expert Committee on Malaria. 8th Report, 1960. WHO. Technical Report Series. No 205.
14. Expert Committee on Malaria. 9th Report, 1962. WHO. Technical Report Series. No 243; E. Pampana, *A Textbook of Malaria Eradication* (London: Oxford University Press, 1963). Socrates Litsios, formally a malariologist with WHO, criticizes them for these changes and how they were taken: "Criticism of WHO's revised malaria eradication strategy." *Parassitologia* 42 (2000): 167–172.
15. Integration of Mass Campaigns against Specific Disease into General Health Services. WHO Technical Report Series No. 294.
16. Soper's concern with the changing WHO program is discussed by S. Litsios, "René J. Dubos and Fred L. Soper: Their contrasting views on vector and disease eradication." *Perspectives in Biology and Medicine* 41 (1997): 138–149.
17. Expert Committee on Malaria. 13th Report, 1966. WHO. Technical Report Series. No. 357.

18. The 22nd World Health Assembly of 1969 is usually seen to have marked the death knell of the Global Malaria Eradication Program. For more details of the gradual fadeout of the eradication program, see Litsios, *The Tomorrow of Malaria.*
19. Expert Committee on Malaria. 15th Report, 1971. WHO. Technical Report Series. No 467.
20. *Global Strategy for Health for All by the Year 2000* (Geneva: WHO, 1981).
21. During that period, if my memory serves me, *Punch* produced a wonderful cartoon about this problem. It showed a white British patient just coming out of an anaesthetic, surrounded by a group of African and Asian physicians and nurses. "Where am I?" he asks.
22. Thomas McKeown, *The Modern Rise of Population* (London: E. Arnold, 1976); *The Role of Medicine. Dream, Mirage or Nemesis*? (Princeton: Princeton University Press, 1979). McKeown's work has engendered much comment. S. Guha, "The importance of social intervention in England's mortality decline: the evidence reviewed." *Soc. Social Hist. Medicine* 7 (1994): 89–113; S. Szreter, "Mortality in England in the eighteenth and nineteenth centuries: A reply to Sumit Guha." *Soc. Social Hist Medicine* 7 (1994): 269–82.
23. McKeown, *Nemesis*, p. 82.
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33. Ibid; also M. Humphreys, "Kicking a Dying Dog: DDT and the Demise of Malaria in the American South, 1942–1950." *Isis*, 87 (1996): 1–17.
34. Humphreys, *Malaria.* p. 111.
35. Anne-E. Birn, *Local Health and Foreign Wealth: The Rockefeller Foundation's Public Health Programs in Mexico, 1924–1951.* D.Sc. Dissertation, Johns Hopkins School of Hygiene, 1993; "Public health or public menace? The Rockefeller Foundation and public health in Mexico, 1920–1950." Voluntas 7 (1996): 35–56; Richard Brown, "Public Health in Imperialism: Early Rockefeller Programs at Home and Abroad." *Amer. J. Pub. Health* 66 (1976): 897–903; *Rockefeller Medicine Men: Medicine and Capitalism in America* (Berkeley: University of California Press, 1979); Saul Franco-Agudelo, "The Rockefeller Foundation's antimalarial program in Latin America: donating or dominating"? *Int. J. Health Services* 13 (1983): 51–67; A. Solórzano, "Sowing the seeds of neo-imperialism: The Rockefeller Foundation's yellow fever campaign in Mexico." *Int. J. Health Services* 22 (1992):

529–554. Soma Hewa, *Colonialism, Tropical Disease and Imperial Medicine: Rockefeller Philanthropy in Sri Lanka* (Lantham, Maryland: University Press of America, 1995).

36. A-E Birn & A. Solórzano, "Public health policy paradoxes: Science and politics in the Rockefeller Foundation's hookworm campaign in Mexico in the late 1920's." *Soc. Science & Medicine* 49 (1999): 1197–1213.
37. S. Hewa, "The hookworm epidemic on the plantations in colonial Sri Lanka." *Medical History* 38 (1994): 73–90.
38. S. Hewa, Colonialism, *Tropical Disease and Imperial Medicine*, p. 15.
39. A-E. Birn & A. Solórzano, "The hook of hookworm: Public health and the politics of eradication in Mexico," in A. Cunningham and B. Andrews (ed), *Western Medicine as Contested Knowledge* (Manchester: Manchester University Press, 1997); A-E. Birn & A. Solórzano, "Public health policy paradoxes."
40. H. Carr, "Observations upon hookworm disease in Mexico," *Amer. J. Hygiene*, 6 (July Supplement, 1926): 42–61. See also Chap. 5, n. 3.
41. A. Solórzano, "The Rockefeller Foundation in Revolutionary Mexico. Yellow fever in Yacatan and Veracruz," in M. Cueto (ed.), *Missionaries of Science. The Rockefeller Foundation and Latin America* (Bloomington: Indiana University Press, 1994); A. Solórzano, "Sowing the seeds of neo-imperialism: The Rockefeller Foundation's yellow fever campaign in Mexico," p. 547. He also seems unaware that the vaccine was useless.
42. Saul Franco-Agudelo, "The Rockefeller Foundation's antimalarial program in Latin America." For details of the United Fruit Company's activities see C. Wilson, *Ambassadors in White: The Story of American Tropical Medicine* (New York: Holt, 1942); and J. Farley, *Bilharzia: A History of Imperial Tropical Medicine* (New York: Cambridge University Press, 1991), Chap. 7, "Economics of disease."
43. The geographical budget was based only on figures that could be attached to a geographical area. The total geographical budget was always less than the total budget because administrative and other allocations could not be given a geographical area and were omitted from the calculations.
44. A-E. Birn & A. Solórzano, "Public health policy paradoxes," p. 1198.
45. Brown, *Rockefeller Medicine Men*, p. 127.
46. S.Robertson, et al, "Yellow fever: A decade of re-emergence." *J.A.M.A.* 276 (1996): 1157.
47. These dengue epidemics have led to a rash of publications. These include: Schatzmayr H. G., "Dengue situation in Brazil by year 2000," *Mem. Inst. Oswaldo Cruz* 95 (Supp 1) 2000: 179–81; "Dengue in Central America: the epidemics of 2000." *Epidemiol. Bull.* 21 (2000): 4–8; Degallier N. et al "Dengue and its vectors in Brazil." *Bull. Soc. Pathol. exotique* 89 (1996): 128–136; Reiter, P. "La dengue dans les Ameriques." *Bull. Soc. Pathol exotique* 89 (1996): 95–97; Aviles G. "Dengue reemergence in Argentina." *Emerg. Infect. Dis* 5 (1990): 575–78; Guzman, M.G. "Dengue: an update." *Lancet Infect. Disease* 2 (2002): 33–42; "Killer Mosquito plagues Brazil," *The Toronto Globe and Mail*, March 1, 2002.
48. Massad et al, "The risk of yellow fever in a dengue-infected area." *Trans. Roy. Soc. Trop. Med. Hyg.* 95 (2001): 370–74; Mondet, B. "Yellow fever epidemiology in Brazil." *Bull. Soc. Pathol exotique* 94 (2000): 260–67; Monath, T.P. "Yellow fever: an update." *Lancet Infect. Disease* 1 (2001): 11–20.
49. "Global Push against 3 Diseases Urged." *The Washington Post*, January 31, 2002.
50. P. Blustein, "The Right Aid Formula This Time Around? *The Washington Post*, March 24, 2002.

51. R. G. Feachem, "Poverty, and inequality: a proper focus for the new century." *Bull. W.H.O.* 78 (1) 2000: 1.
52. D.R. Gwatkin, "Health inequalities and the health of the poor: What do we know? What can we do." *Bull. W.H.O.* 78(1) 2000: 3–16.
53. Tim Evans et al (eds.). *Challenging Inequities in Health.* From Ethics to Action (New York: Oxford University Press, 2001). In 1974 John Evans, then President of the University of Toronto and father of Tim Evans who now heads the equity program, decided to close-down the university's Rockefeller-endowed School of Hygiene and make it part of the medical school. There were many reasons for this decision but perhaps the most powerful was his hope that by doing so the medical school might give more attention to health rather than concentrating solely on medicine. But no such change of outlook took place and years later he wondered whether it would have been better to have closed the faculty of medicine and merged it into the School of Hygiene.
54. *Rockefeller Foundation Annual Report 1999.* Rockefeller Foundation website.
55. Humphreys, *Malaria,* p. 153.
56. Biographical details are basically taken from Greer William's notes which were based on oral interviews. RAC R.G.3.1 S.908 B.7 f.8694–95.
57. In 1925, Russell had sent H.N. Rector from Alabama to help Hackett. Rector's Southern-born wife quickly developed serious bouts of homesickness. Hazel Hackett who adored Italy had absolutely no sympathy. "I think the IHB," she wrote to her husband, "should consider a long time before they choose a man with a Southern wife to send into foreign service . . . It seems ridiculous to me to think of anyone in *Rome*, my beloved *Rome*, being homesick for *Alabama.*" Hazel Hackett to Hackett, February 2, 1925. RAC. Hackett Papers Series 1.3 B.8.
58. Hackett Correspondence 1902–1919. RAC. Acc 33 B.1 f.1.

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# Glossary of Names

BALFOUR, ANDREW First director of the London School of Hygiene and Tropical Medicine ((1923–30).

BALFOUR, MARSHALL International Health Division (IHD) field staff. Director of the China program (1939–42). Balfour escaped in 1942 and took over the India program; returned to China for a short time after the war.

BARNARD, CHESTER AT&T executive and Rockefeller Foundation trustee. President of the Rockefeller Foundation (1948–52).

BAUER, JOHANNES IHD staff member (1927–51).

BAYNE-JONES, STANHOPE Military surgeon. Appointed head of the U.S. Typhus Commission in 1943.

BEARD, MARY Nurse. Director of nursing program for the Division of Studies and the Division of Medical Education of the Rockefeller Foundation; associate director of the IHD (1931–38).

BEEUWKES, HENRY Director of yellow fever surveys in West Africa (1925); field director and staff member, IHD (1927–39).

BIGGS, HERMANN Leading public health figure in the United States. Organized New York City's laboratory service (1892–1914); Commissioner of Health, New York State (1914–23).

BOYD, MARK IHD malariologist. Field experiences in the American South, Jamaica, and Brazil; director of the malaria research station in Tallahassee, Florida (1931–46).

CARTER HENRY After service with the Panama Canal Commission, appointed assistant surgeon general of the U.S. Public Health Service (1915–19).

CARTER, WILLIAM Staff member of the Division of Medical Education, Far East desk.

CHRISTOPHERS, RICKARD One of the truly great malariologists. Christophers served most of his life with the Indian Medical Service; died in 1978 at the age of 105.

COLE, RUFUS Scientific director of the IHD. Director of the Rockefeller Institute Hospital.

CONNOR, MICHAEL IHD field staff (1916–1930). Worked on yellow fever in Ecuador, Mexico, and Brazil.

COTTER, EDWARD Indian Medical Service. Public health commissioner for the government of India (1939–47).

CROWELL, ELIZABETH Nurse. First appointed to tuberculosis field staff in France. Field director and staff member of the IHD (1931–1940).

DARLING, SAMUEL Head of Uncinariasis Commission (1916–18). First director of the department of hygiene at Saõ Paulo (1918–20) and director of the field station for malaria in Leesburg, Georgia (1922–25).

DOCHEZ, ALPHONSE Virologist. Worked at the Rockefeller Institute and its hospital (1908–1919); on the faculty of Columbia College of Physicians and Surgeons (1921–49).

EATON, MONROE IHD staff member (1937–47).

EMBREE, EDWIN Sociologist. Appointed secretary of the Rockefeller Foundation in 1917; director of the Division of Studies (1924–27); then held the post of vice-president of the foundation for one year before retiring to head the Rosenwald Fund.

FALCONER, ROBERT President of the University of Toronto (1907–32).

FARRAND, LIVINGSTONE First head of the Rockefeller Commission for the Prevention of Tuberculosis in France (1917–19).

FERRELL, JOHN State director of the Sanitary Commission for North Carolina; associate director of the International Health Board (IHB) and IHD (1913–1944).

FITZGERALD, JOHN Founder of the Connaught laboratory and first director of the School of Hygiene, University of Toronto; scientific director of the IHD.

FLEXNER, SIMON Pathologist. First director of the Rockefeller Institute (1903–35).

FORBES, CAMERON Governor General of the Philippines (1909–13); U.S. Ambassador to Japan (1930–32).

FOSDICK, RAYMOND Attorney. Protégé of Woodrow Wilson. President of the Rockefeller Foundation (1936–48).

FOX, JOHN Virologist and pathologist. Staff member IHD (1938–49).

FOX, LEON Briefly head of the U.S. Typhus Commission. Resigned in 1943 to become the commission's field director in Cairo.

FROST, WADE Professor of epidemiology, Johns Hopkins School of Hygiene; scientific director of IHD.

GATES, FREDERICK Baptist minister and businessman. Financial advisor to John D. Rockefeller who directed the Rockefeller fortune towards public health and medicine. Gates conceived the idea of the Rockefeller Institute and Rockefeller Foundation.

GOLDMARK, JOSEPHINE Social worker. Author of the Goldmark Report (1923).

GOODRICH, ANNIE Nurse. On the faculty of Teacher's College, Columbia University (1914–23) and dean of the Army School of Nursing in World War I; dean of the Yale University School of Nursing (1923–34).

GORGAS, WILLIAM Appointed surgeon-general of the U.S. army in 1914, having gained an international reputation for ridding Havana and the Panama canal zone from yellow fever; advisor to IHB on yellow fever.

GRANT, JOHN BLACK IHD field staff. Loaned to Peking Union Medical College (1921–34); director of the China program (1935–39); director of the All-India Institute of Hygiene (1939–44). After a few years in the New York office, Grant was transferred to Paris to direct the European program (1948–51).

GREENE, JEROME General manager of the Rockefeller Institute (1910); secretary of the Rockefeller Foundation (1913–17) placed in charge of World War I relief activities; trustee of the Rockefeller Institute and the foundation.

GREENE, WARWICK Lawyer. Director of the second Rockefeller War Relief Commission.

GREGG, ALAN Associate director of the Division of Medical Education and head of the division's Paris desk (1924–29); director of the Division of Medical Sciences (1929–51).

GUNN, SELSKAR Member of French tuberculosis commission, then European regional director of IHD in the Paris office (1922–32); director of the foundation's rural reconstruction program in China (1935–39); returned to Paris desk in 1939; vice president of the Rockefeller Foundation (1927–42).

HACKETT, LEWIS IHD field staff. After service in central America, directed the Brazil program (1916–23); chief of the Italian malaria program (1923–39); director of the Rio de la Plata and Andean Region (1940–49).

HEISER, VICTOR IHD field staff. Director of the East (1914–27); associate director of the IHD (1927–34); fired by Fred Russell in 1934.

HOWARD, HECTOR IHD field staff. Rose placed him in charge of the British Guiana program and as director of the Caribbean program; fired by Fred Russell in 1934.

JACOCKS, WILLIAM IHD field staff. Served in the Sanitary Commission and then joined the IHB in 1915. After military service in France, posted to Ceylon in 1920; director of the Indian and Ceylon program from 1927 until his retirement in 1942.

JAMES, PRICE British malariologist and member of the European Malaria Commission.

JANNEY, JOHN IHD field staff. Assistant to Hackett in South America and director of the Chilean program (1942–51).

KEININGER, LOUISE Nurse. Superintendent of nursing at the Anna Nery School of Nursing in Rio.

KENDALL, ARTHUR Bacteriologist. Rockefeller Institute (1906–09); Harvard (1909–12); Northwestern Medical School (1912–42).

KERR, AUSTIN IHD Field staff. In charge of Egyptian malaria campaign (1944); first director of Sardinia anti-anopheles campaign; resigned that post in 1947, but remained on staff until 1951.

KIMBALL, LINDSLEY Vice-president General Education Board (1950–60); vice-president Rockefeller Foundation (1949–60).

LIMA, CARALHO Director of Bacteriological Institute, Saõ Paulo.

MACDONALD, GEORGE Professor of tropical hygiene, London School of Tropical Medicine and Hygiene (1945–67). Member of the WHO expert committee on malaria.

MAGEE, JAMES Surgeon General, U.S. army.

MAXCY, KENNETH Professor of bacteriology and later of epidemiology at the Johns Hopkins School of Hygiene; involved in yellow fever vaccine investigation (1942).

MEGAW, J. W. D. India Medical Service. Director of the Calcutta School of Tropical Medicine.

MEYER, KARL Veterinary pathologist. Member of the George Williams Hooper Foundation for Medical Research at the University of California (1915–24); director (1924–54); involved in yellow fever vaccine investigation (1942).

MISSIROLI, ALBERTO Italian malariologist.

NAGAYO, MATARO Director of the Laboratory of Infectious Diseases, Tokyo. Unsuccessful candidate to direct the Tokyo School of Hygiene.

NOGUCHI, HIDEYO Virologist at the Rockefeller Institute. Mistakenly identified spirochaete bacteria as the cause of yellow fever.

NORTON, THOMAS Laboratory technician in the Rockefeller yellow fever laboratory.

OPIE, EUGENE Pathologist with special interest in tuberculosis. After appointments at the Rockefeller Institute (1904–10) and the University of St. Louis Medical School (1910–23), worked on trench fever in World War I; director of the Henry Phipps Institute at the University of Pennsylvania, and later transferred to Cornell (1923–41).

PAMPANA, EMILIO Italian malariologist. Member of the WHO malaria eradication program.

PARSONS, ETHEL Nurse. Director of the Brazilian nursing program for the IHD (1921–1931).

PAUL, GEORGE IHD field staff (1916–22). Worked on hookworm in Australia, Fiji, India, and Ceylon.

PEARCE, RICHARD Pathologist. Chair of research medicine at the University of Pennsylvania (1910–19); director of the Division of Medical Education (1919–29).

PUTNAM, PERSIS IHD Statistician (1926–48).

ROSE, WICKLIFFE Educator. First director of the Sanitary Commission (1909–14) and the International Health Commission and Board (1913–23).

RUSSELL, FREDERICK Military physician and laboratory scientist. Appointed in 1919 to develop the IHB's laboratory service; director of the IHB/IHD (1923–1935).

RUSSELL, KATHLEEN Director of the School of Nursing, University of Toronto (1920–52).

RUSSELL, PAUL Malariologist. IHD field staff (1923–51), predominantly in the Philippines and India. In 1942, Russell joined the U.S. Army Medical Corps and, after service under MacArthur, was posted to the Mediterranean region to head the malaria control program; discharged from the army in 1946; worked for the WHO and for the foundation until his retirement in 1959.

SABINE, WALLACE Dean of the Graduate School of Applied Science at Harvard. Wrote the first account of the tuberculosis situation in France.

SAWYER, WILBUR Joined IHD in 1919. After directing the Australian hookworm campaign (1919–22), Sawyer entered the laboratory service in 1924

and became head of the yellow fever laboratory (1928–35); associate director of the IHD (1927–35); director of the IHD (1935–44) and director of the Health Commission (1940–44); resigned in 1944 to become director of UNRRA (1944–47).

SIMMONS, JAMES U.S. Army Medical Corps: Chief of its preventive medicine service (1940–46). After the war, Simmons became dean of the Harvard School of Public Health.

SINTON, JOHN Malariologist, India Medical Service. Winner of the Victoria Cross in World War I.

SMILLIE, WILSON IHD field staff. Served in the Institute of Hygiene in Sao Paulo (1918–22); director of the IHB training station in Andalusia, Alabama (1922–25); scientific director of the IHD.

SNODGRASS, JOHN IHB field staff (1915–18). Posted to Ceylon.

SOPER, FREDERICK IHD field staff. Regional director of the IHD in Brazil (1927–42); member of the U.S. Typhus Commission (1943); head of the Rockefeller typhus team (1943–44); organizer of malaria control units in Italy; regional director of the IHD for Africa and the Middle East (1946); resigned from IHD in 1947 to become director of the Pan American Sanitary Bureau.

SOUZA, GERALDO Director of the Sao Paulo Institute of Hygiene.

STEWART, ALEXANDER Indian Medical Service. First director of the All-India Institute of Hygiene (1930–35).

STILES, CHARLES Parasitologist. Chief of the Division of Zoology in the U.S. Public Health Service where he introduced Frederick Gates to hookworm.

STRODE, GEORGE Joined IHB in 1920 and spent his early years in Brazil before taking over from Hackett as director of the Brazil program in 1923; posted to Paris office (1927–38); director of IHD (1944–51).

SWEET, WINFIELD IHD field staff (1921–42). Involved in the early hookworm campaigns in Australia and Ceylon.

TENNANT, MARY Nurse. Associate director of nursing for the IHD (1938–51).

THEILER, MAX South African-born virologist. Appointments at Harvard (1922–29) and at the Rockefeller Institute (1930–72); won the 1951 Nobel prize for his work on yellow fever.

TURNER, THOMAS Syphilologist at Johns Hopkins School of Hygiene; worked with the IHD on yaws in Jamaica (1932–37), and on syphilis in Baltimore (1937–49).

VINCENT, GEORGE Sociologist. Dean of the Faculty of Arts and Science at the University of Chicago; president of the University of Minnesota; president of the Rockefeller Foundation (1917–29).

WARREN, ANDREW IHD field staff. Joined the IHB in 1921; regional director in the Paris office for Europe, Africa, and the Near East (1938–39); assigned to New York as IHD assistant director (1936–44) and associate director (1945–51).

WASHBURN, BENJAMIN IHD field staff. Worked on hookworm campaigns in British Guiana (1914–16), North Carolina (1917–19), and Jamaica (1920–26); remained on staff until retirement in 1939.

WATSON, MALCOLM British malariologist who joined the Malayan Medical Service in 1900 and worked on malaria control.

WEBSTER, LESLIE Virologist. Joined Rockefeller Institute in 1920.

WILLIAMS, LINSLY Second director of the tuberculosis commission in France (1919–22).

WINSLOW, CHARLES Bacteriologist and one of the first public health experts in the United States. Served in many capacities with New York and Connecticut public health departments and with the U.S. Public Health Association of which he became president in 1926; editor of the *American Journal of Public Health* (1944–54); health advisor to the League of Nations and to the WHO.

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