

## ONCE UPON A TIME

### PRESIDENTIAL ADDRESS

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Justice Frankfurter has said there are two myths which have always captivated the human race, that of a Utopia in the unforeseeable future, and that of a Golden Age in some remote epoch of the past. In the dynamic field of world health we foster the same myths but shorten the time span. To a Society like ours made up in its majority of young, imaginative and forward-looking persons, the Utopia is just around the corner—a happy if somewhat overcrowded world freed forever from malaria, tuberculosis, smallpox, yaws, leprosy and syphilis. But I, who have reached the age of retrospection, find pleasure in looking back to a Golden Age no longer ago than my own youth, when public health, and especially international public health, were at the starting line, and progress was about to make a great leap forward, as the Chinese say, into a new, rich and virgin field. So I think it more appropriate tonight to review the record than to attempt to scan the future—more appropriate and more circumspect.

My first chief, Wickliffe Rose, who in 1913 organized the International Health Commission of the Rockefeller Foundation, was impressive in action and endowed with extraordinary vision and courage, but he was modest and self-effacing in his public relations, and disliked predictions. The sanguine neophytes on his staff, with all the confidence of youth and inexperience, never doubted their ability to conquer yellow fever, suppress malaria, and purge the entire "wormy world." But Rose was prudent. "We are only safe," he would admonish them, "in talking about the past." Rose was forgetting that these young doctors fresh from medical school had no past; they had nothing to talk or think about but the future. I was one of those youths, among the first to be appointed to the staff of the International Health Commission, and since that fateful conversation with Mr. Rose in April, 1914, I have accumulated enough past to talk about. My purpose tonight is to delve among the roots of the great, spreading international health movement to uncover its modest beginnings in Rose's International Health Commission, the

IHC, known in successive epochs as the International Health Board (IHB) and the International Health Division (IHD).

Rose had a program for action so simple and so reasonable that it is astonishing it never had been attempted before. The world had always been defenceless against recurring invasions of certain great and destructive diseases whose forays and infiltrations from their endemic seedbeds were a constant threat to every country on earth, in spite of all the barricades and quarantines which were erected in times of trouble, but which were always as ineffectual as they were harsh and oppressive. Rose's design was to wage a coordinated mass attack on some of these dangerous but preventable diseases on their home grounds, wherever they were found to be endemic and uncontrolled. Rose proposed to offer any government in this situation the technical and financial collaboration of the IHC, a proposition without parallel in history, and one which had its remarkable aspects, considering the disparities in age, size and wealth between the two parties. Dr. William H. Welch, in accepting a position on the Commission, said, "both the purpose of the gift and the opportunity it creates are unique in the annals of preventive medicine."

Actually, the IHC was not without a prototype in its philanthropic plan to extend a helping hand around the world to fight the aggressive diseases. Charles IV of Spain despatched a commission to disseminate the knowledge and application of Jenner's discovery of cowpox inoculation for the prevention of smallpox. This procedure had reached France, Spain and the U. S. by 1800, and Charles IV undertook to send out a scientific mission to go completely around the world carrying the protective virus "to all Spanish lands and all other races and peoples en route." The royal order was given August 3, 1803, and a military surgeon, Dr. Francesco Javier de Balmis set sail from La Coruña for America on November 30, accompanied by 3 doctors, 2 assistants, 3 male nurses and the Directress of the local orphan asylum in charge

of 22 nursing babies to conserve the virus during the long journey, passaging it from arm to arm. All arrived safely in Venezuela, where the expedition divided in two, one part going south to cover South America while de Balmis himself, after attending to the needs of the Caribbean area and Central America, crossed Mexico and sailed from Acapulco to the Philippines, where he arrived with the virus still alive and potent. Continuing on around the world he reached Lisbon August 15, 1806, two years and nine months after leaving La Coruña. The other half of the expedition was not so fortunate. It was shipwrecked at the mouth of the Magdalena River, but all the babies were saved. The group visited Colombia, Ecuador and Peru, and was preparing to go on to Chile and Buenos Aires when they learned in Lima that the vaccine had already reached the Rio de la Plata.

This story was given to me by Dr. Rafael Schiaffino, the medical historian of Uruguay, and I am sorry I cannot give you the further details we should all like to know about this picturesque expedition—how the 3 male nurses were able to provide nourishment for 22 nursing babies, what happened to the little ship and its crew as this gang of young delinquents reached the mobile and destructive age of three, and how the virus was kept alive for almost 3 years with only 22 passages. In any case, the fact is that a scientific commission had encircled the globe to promote a general attack on one of the great pestilential diseases of the age and to instruct backward and isolated peoples in the use of a new preventive measure of the first importance.

The great epidemics continued to ravage Europe and America, and often swept around the world, but I cannot find that any coordinated action was ever again taken to curb them until Rose appeared on the international scene. The great powers, including the U. S., did not, it is true, suffer these blows in silence; after each major visitation of plague, cholera, yellow fever, smallpox or typhus, they held an international sanitary conference, usually in Paris, and kept up this practice for over 50 years without visible effect on the incursions of the epidemics. It was like the first installation of foghorns on the California seaboard, which the coastal inhabitants are reputed to have called a futile gesture and a waste of money, since the fog kept rolling in just as before. The governments of the 19th

century could not agree on any effective plan for a joint campaign against these common enemies because any suggestion that an international agency, even of their own creation, should be permitted to make inspections and carry out measures behind their frontiers was considered an intolerable derogation of national sovereignty and an affront to national pride. But now a private organization, whom no one had ever heard of, with no foreign experience and as yet without a staff, assumed that these same governments would invite it to enter their countries and help them combat certain diseases which they had failed to curb, and thus establish a united front against them all over the world. Rose was resolved to cooperate only with governments, and this was breaking ground in a number of fields; it was new to philanthropy, to international diplomacy and to public health. No one has ever accused Rose of being either naive or foolhardy, but the prospect in 1913 was one which might have daunted any valiant soul. For not only, as it turned out, was there all too scanty knowledge of the epidemiology of the diseases to be attacked, but as these gradually became better understood, the drugs and tools and other measures available proved very ineffective to control, not to speak of eradicating any of them.

But perhaps Rose's most serious handicap was the lack of any trained and experienced personnel, and this was so because there was no career in public health; practicing physicians doubled as health officers in their spare time. Rose had foreseen this major obstruction to his program long in advance. When the IHC was no more than a scheme presented to the Trustees of the Rockefeller Foundation (RF) at their first meeting, he asked the General Education Board to survey existing facilities in the U. S. for the postgraduate education of doctors in the principles and techniques of hygiene, a training which he needed not only for his own staff, but for all the countries where its work would lead to constructive developments in public health. It turned out that there were no such facilities; evidently a new kind of professional school was needed which did not exist. The RF offered at once to assist universities to develop such institutions for training and research in public health. The first of these, planned and organized by Dr. Welch was the Johns Hopkins School

of Hygiene and Public Health which enrolled its first class of 17 students in the fall of 1918. This was followed by grants to Harvard, Toronto, London and in time to a large number of schools all over the world. A majority of the students in the early years were foreign fellows sent by the IHC and its successors, and this was one of the most acceptable and constructive forms of international collaboration the RF ever undertook. The fellowship program acquired volume as the projects multiplied, until in the end the International Health Division had spent some 7 million dollars on what Rose called an investment in leadership. These great schools and the thousands of health workers whom they trained, constituted an educational force which had a profound influence on the well-being of mankind. The IHC thus set on foot a movement which resulted in a new profession of full-time health officers, in the multiplication of health services to employ them, and in the creation of schools to train them. Rose had set his sights as high as this from the beginning and always obtained the support he needed from the RF, running into many millions of dollars. He wrote at the end, "Public enlightenment, government machinery, and technical education and research are bound up in a sure sequence."

In 1914, when the work got started, this evolution had not even begun. Finding personnel was no small task. The U. S. was soon in the war and he had to scrape the barrel for candidates. These could be given at that time no proper field training in the U. S. to prepare them for a foreign program; they were equipped with only the conventional medical education of 50 years ago, before Abraham Flexner's report of 1910 had had time to improve it. Rose did not, for all this, slow down the expansion of his foreign program; within 5 years he had made agreements for projects with 42 governments, and 3 years later, when he resigned to take up other work, the number had increased to 63. Recruitment of staff never caught up in Rose's time with the invitations which came flooding in from all over the world. Since he could not expect training or experience, Rose chose his men for character and bent. They were sent overseas as soon as possible and with a minimum of briefing to some previously established station, and then on to their own destination in a land whose language, politics and ways were no less strange to them than its

diseases. When I look back upon this picture, I marvel at Rose's serene confidence both in his ballooning program which seemed always about to get out of hand but never did, and in his young doctors who knew very little of the world and its peoples, or of tropical medicine and hygiene, but who rarely let him down.

The field staff learned by doing; education is the accumulation of experience, not of facts, and as Oscar Wilde said, experience is the name we give our mistakes. I feel now that our ignorance and inexperience were paradoxically an advantage in the beginning when there was not a great deal of existing knowledge to be drawn upon from any source. My early experience was in Latin America, and when the local doctors with whom we worked found out that we did not look down upon them as ignoramuses but eagerly listened to what they had to tell us, their suspicions and defense armor soon melted away and a happy relationship of genuine and fruitful reciprocity was established. They taught us their language, politics and folkways, and what they knew about tropical disease; they got from us our kind of practical approach to problems, our lack of frustration in the face of hypothetical difficulties, and the funds to carry out demonstrations and experiments on a significantly large scale. We were all empiricists, learning what we could from observation and experience, and Rose and the rest of us refused to be dismayed by presumptive obstacles.

For one thing, Rose started with hookworm disease, a sound choice as it turned out—the best he could have made. The hookworm work never led to such dramatic victories as malaria nor to such fruitful discoveries as yellow fever, but as an entering wedge for a long-term public-health program, it was far superior to either. There were no mysteries about the nature of the hookworm or the way it spread; it was large enough for the victims to see without a microscope, it infested an enormously wide belt, running clear around the earth, which contained more than half the population of the world, and it was vulnerable to treatment and sanitation. The Old World tropics were all colonial possessions in 1914 except for Liberia, Ethiopia and Siam, and the British no sooner learned of Rose's program than they invited him to begin in any or all of their tropical colonies. Thus the question as to how the great Powers would react to Rose's

initiative was answered at once to his satisfaction. Furthermore, he was not going into this thing blindly; he had just carried out from 1910 to 1914 a sort of pilot experiment in hookworm control on a large scale which had revealed both the obstacles he could expect to meet and the results he might hope to achieve. Wickliffe Rose, a layman and distinguished Southern educator, had been wisely chosen rather than a medical man to organize the hookworm campaign of the Rockefeller Sanitary Commission in the Southern States. The medical and sanitary problems were so simple that they could be handled by young doctors and sanitary engineers with a minimum of special training, but the promotional and educational aspects of hookworm control were subtle and difficult, calling for a leader experienced in methods of teaching and in the ways of the South.

This campaign, which aroused so much indignation among its beneficiaries in the beginning and so much enthusiasm in the end, was due to the propaganda and persistence of Charles Wardell Stiles, a zoologist in the Public Health Service, who in the early 1900's had uncovered the worm at the core of Southern backwardness. Having read in Hirsch's famous *Handbuch* of the prevalence of hookworm anemia in the West Indies, Stiles wondered whether it might not be present in the southern part of the U. S. The parasite had been reported once or twice as a curiosity in such unlikely places as St. Louis (1893) and Buffalo (1897), and in 1901 Dr. Allen J. Smith of Galveston sent him a few hookworms obtained from his medical students at the University of Texas. Stiles however found no specimens from man in the collections of the National Museum, Army Medical Museum or Bureau of Animal Industry. He suspected that the hookworm, as a cause of anemia, was being overlooked and he lectured on the subject at Johns Hopkins, but he says that his good friend Dr. William Osler took him roundly to task and maintained that a disease which could be so easily recognized could not have escaped detection if it were present. Stiles, however, persisted, and obtained permission from Surgeon-General Wyman of the Public Health Service to make a reconnaissance on his own account. He immediately set up a clinic in Columbia, S. C., and made excursions into Georgia and Florida. In a week he had the confirmation of his suspicion; he found to his excitement, that every anemic, listless,

pot-bellied child he examined was a victim of the hookworm. He had discovered the cause of the commonest disease in the Southern States.

Stiles reported this on December 4, 1902, to the First Panamerican Sanitary Conference, which had met in Washington to organize an international health office for the Americas, later to be called the Pan American Sanitary Bureau. By a curious circumstance, no speech he ever made caused such commotion or was fraught with such long-term consequences as this one. Stiles said that the so-called "poor white trash" in our South were not constitutionally lazy; their energy was being sapped by hookworms. A reporter from the New York Sun embroidered this in humorous vein and sent in the story that a doctor in the South had discovered the germ of laziness; loafing was just a disease. This diverted his New York office, and the Sun of December 5 headlined the discovery. The ridiculous idea was caught up by newspaper rhymesters and cartoonists, and swept the country. Stiles thought it a fortunate episode. "This reporter," he wrote afterwards, "contributed an exceedingly valuable piece of work in disseminating knowledge concerning hookworm disease. . . . It would have taken scientific authors years of hard work to draw as much attention to this subject."

Actually, the five-year campaign of the Rockefeller Sanitary Commission did not have much immediate effect on the hookworm. The task was not one which could be carried through to any conclusion by a six-weeks' campaign, which was all the time that could be devoted to any one county. To get rid of the hookworm, the standards of living and of education would have to be permanently raised. But it had an enormous effect upon the South; the Rockefeller Sanitary Commission had revealed the magnitude of the health problem, its social repercussions on the community, and the feasibility of its solution. To the Trustees of the newly-created RF, Rose's success in mobilizing the health authorities of eleven Southern States in a concerted drive against a common ill seemed capable of unlimited application. Frederick T. Gates, one of the chief architects of the Foundation and Mr. Rockefeller's personal adviser, determined to organize "a new and world-wide agency to attack hookworm and other curable and preventable diseases, and to promote universal health." That was the origin of the International Health Commission.

Although the word "eradication" had been

rashly inserted into the title of the Rockefeller Sanitary Commission, neither Rose nor Stiles thought it possible within any given time. Even before the Commission had begun its campaign, Rose wrote, "The eradication of the disease is a work which no outside agency could do for a people if it would, and one which no outside agency should do if it could." Nevertheless Rose had earnestly desired at least one complete demonstration, even on a small scale—that somewhere cure and prevention might reach 100 per cent of the inhabitants and serve as a model of perfection after the Commission had withdrawn. In September, 1913, an attempt at eradication was begun on Knotts Island, a remote fishing village on the North Carolina coast which had little contact with the rest of the world. The plan was to treat and sanitize until the job was done. Ninety-four of the 567 inhabitants had hookworms and 90 were eventually declared to be cured after many courses of thymol, a disagreeable and relatively ineffective poison which had to be given repeatedly, and administered to children by bribery or coercion. The other four stubborn old people would have nothing to do with the business. The sanitation campaign was, as usual, less successful. No inoffensive solution of the rural latrine problem had been invented by the doctors or engineers except expensive septic tank installations, and Knotts Island folk went on using "nature's way" in immutable disregard of the calamity-howlers. Attempts to persuade the recalcitrants soon ran into the law of diminishing returns and Rose was finally convinced that perfection was not worth the price. He accepted defeat gracefully for his main purpose had been accomplished, to make hookworm prevention an essential part of public health work in the South. As we learned from a paper presented at this meeting, the hookworm still prospers in many localities, and I doubt whether it has ever disappeared from any area in which it has established itself.

But while Rose was organizing the IHC for a general attack on the hookworm, his attention was drawn in a compelling way to two other diseases, both of first importance as obstacles to human progress, and each peculiarly vulnerable, according to the highest authorities, to a well-directed attack, although none unfortunately had ever been launched. These old, highly destructive enemies were malaria and yellow fever. Passing through London early in 1914 on his

way around the world to scout for projects for the IHC, Rose met Sir Ronald Ross, who had received the highest honors including the Nobel Prize for discovering the mosquito transmission of malaria, but who had been in a state of chronic indignation ever since because the knowledge had not been applied on any large scale to control the disease. Gorgas had waged war on the *Anopheles* of the Canal Zone, but at a cost prohibitive to the ordinary community, so that his methods had not been copied even in the U. S. Ross's only enthusiastic and competent disciple had been Malcolm Watson in Malaya, who had persuaded the rubber planters to finance some experiments in *Anopheles* control by drainage. Ross was becoming exceedingly unhappy over the unprogressive attitude of malariologists in general when suddenly here was Rose delivered into his hands, with what was reputed to be unlimited financial backing, and actually looking for a disease to attack. Ross was a very articulate person and Rose was impressed with his grim picture of malaria as a world problem and his exposition of the practicable measures by which it could be controlled. When Rose got to Malaya he took time out from his hookworm observations to visit Malcolm Watson, and came away convinced that the disease was one of the utmost gravity and that much could be done to prevent it. The criteria which he thought should govern the selection of a disease suitable for attack by the IHC were that it should be of global importance, that little was being done about it, and that it could be prevented at a reasonable cost. Malaria satisfied the first two conditions; the question remained, as he wrote in his report, "whether the various known measures, such as quinine treatment, screening, and drainage operations, can be effectively employed . . . at a cost which will not be prohibitive." Rose set to work at once to find out.

Always anxious to involve government at all levels in his projects, Rose asked the U. S. Public Health Service (PHS), which had been gathering data on malaria since 1912 but had no money, whether it would not like to invite the IHC to collaborate with it and the state and local authorities in some experiments in a few selected localities to find out what it would cost to protect a rural community against malaria. Under the direction of PHS officers, chiefly engineers, and at the expense of the IHC, these experiments were begun in 1916 in several Arkansas and

Mississippi villages, and succeeded beyond expectation. Malaria was greatly reduced by one or a combination of measures at reasonable cost and the villages were so pleased that they continued the work at their own expense. During World War I the same methods were used to protect Army encampments and key industrial centers from malaria, and by 1922 the anti-anopheline drive had been extended to 163 counties in 10 states with very satisfactory results, even though no major drainage operations were attempted. Rose was pleased. He had prudently sent a young engineer to Nicaragua in 1920 to see whether the measures, so successful in the Southern States, were adaptable to tropical conditions, and finding that they were, he set up a training station in Leesburg, Georgia, under Dr. Samuel T. Darling to instruct his young "malariologists" in the techniques of making a malaria survey and of applying the standard methods of obstructing the transmission of the infection. By 1922 he felt secure enough to accept invitations from Brazil and Italy to collaborate in solving malaria problems which had not yielded to the classical method of quinine prophylaxis and cure, and soon he had made similar agreements with malarious countries all over the world.

As long as we were fighting the hookworm, it took no great amount of training or epidemiological insight on the part of the staff to circumvent the parasite with its simple mode of spread; it was human nature, not the hookworm, that defeated us. Malaria proved to be a different sort of problem altogether, as I began to suspect when I was transferred to Brazil. Our principal scout in the foreign field was Dr. Mark Boyd, who was sent down to me to survey the malaria situation in the State of Rio de Janeiro. When Boyd, accompanied by the Brazilian entomologist Antonio Peryassú, disappeared into the marshes of Magé, there were only three recognized *Anopheles* in the whole region, but when they emerged months later, no one knew how many there were. They had begun that process of multiplication and division of species which has enriched entomological nomenclature and frustrated the malariologist ever since. For no one was able to tell the variants from the true species, and specimens submitted to more than one systematist would come back with more than one name, so that we cannot say even now how many species of *Anopheles* there are. The aberrant forms were all given names, usually

those of persons, like *rooti*, *evansi*, *strodei*, *osvaldoi* etc., which was considered to be an honor to the eponymous individuals. Dr. Peryassú, in a friendly gesture of hospitality and homage, created a new species, *Anopheles rockefelleri*, no specimen of which has ever been captured. What was most confusing was that the same mosquito would appear to be a notable vector of malaria in one area but quite inoffensive in another, a phenomenon which was to puzzle us later in Europe. In spite of all this, the tactics of mosquito control remained the same as in Arkansas: put the water in motion if you can't get rid of it, and *Anopheles* breeding will stop. This "secret" was our main weapon against malaria.

The thing that had made Ronald Ross indignant and had surprised Rose was that the European malariologists did not seem to know this. Rose determined to find out why, and to this end I was rapidly converted in a few weeks at Leesburg from a helminthologist to a malariologist and sent to Italy. My qualifications were that I knew my anophelines through association with Boyd, and my malaria by personally contracting a *vivax* infection in Leesburg and a *falciparum* infection in Panama. I had thus been afforded a splendid opportunity to study at first hand Dr. Bass's standard quinine treatment in various formulations and dosages, though I never got to like it. Dr. William James of the Canal Zone used to double and triple it in obstinate cases like mine and add calomel.

I think I was supposed, when I got to Italy in 1924, to talk rather severely to Grassi, Marchiafava, Bastianelli and other renowned malariologists, and show them lantern slides of the work in Arkansas. What I did of course was to keep my mouth shut and sit at the feet of the masters. What I learned was that the vector anophelines of the Mediterranean countries were more versatile than those of Arkansas—they could fly farther, they were more prone to bite human beings, and would breed in almost any kind of water—brackish or sweet, standing or moving—so that the great drainage canals themselves were a prolific source of mosquitoes. No wonder the Italians had failed to adopt the simple American measures. Their *Anopheles* were more dangerous by far than ours, and much more difficult to reduce in number below the threshold density required to maintain malaria transmission. Considering how best to spend their limited funds, the Italians had fallen back on

intensive quinine therapy, a time-honored resource with a history of 300 years of continual defeat. There were good years, as always in the malaria cycle, which they attributed to the quinine program, and there were bad years which they attributed to acts of God, but the fact was that the death rate from malaria in Italy in 1924 was higher than it had been a decade earlier. The outlook was therefore pretty discouraging to a fellow with my limited experience. There were evidently no easy victories to be won either over the *Anopheles* or over the plasmodia, and I hoped I could make the people in our New York office understand this.

However, I had another weapon up my sleeve in Paris green, which Dr. Marshall A. Barber had just discovered to be an effective anopheline larvicide. I had brought a small supply with me to Italy and tried it out on the quiet in Sardinia. Barber had experimented with it on a small scale at his laboratory in Louisiana, but this was the first time it had been used in actual combat. It worked splendidly on the Italian larvae and was an enormous improvement over anything that had ever been tried before against anopheline mosquitoes. We had no doubt from the first that it would put quinine out of the running as an antimalaria measure.

This was not immediately conceded by some of the European malariologists who were looked up to as authorities. The Second Report of the Malaria Commission of the League of Nations, appearing in 1927, still called for more intensive quinization, and contained the extraordinary statement (and how Ross must have gnashed his teeth!) that the knowledge of the mosquito transmission of malaria had, contrary to all expectation, not helped us in the struggle against the disease, and may in fact have led us away from the right path. The reported success of anti-mosquito measures in our South was considered a typical American exaggeration if not pure invention, and a commission was sent to the U. S. in 1927 to investigate the matter. It reported that no conclusions could in fact be drawn from the American experience, since malaria was obviously dying out for social and economic reasons unconnected with any specific measures taken against it. Nor was Col. James, the medical adviser to the Colonial Office, impressed by our little show in Sardinia; he wanted a more permanent solution, and thought of malaria as a social disease, perpetuated by bad housing, mal-

nutrition and inadequate medical care, and believed it would tend to disappear spontaneously if the standard of living could be significantly improved. Prof. Marchoux, the leading French malariologist, held that malaria was a disease of backward peoples and would gradually be eliminated as civilization advanced. (He politely refrained from pointing out that France had the least malaria and America the most, of the so-called advanced nations.)

I thought it would be instructive to stage a competition between quinine and Paris green. My Italian colleague, Prof. Missiroli, and I chose two neighboring small villages in Sardinia, which was the most malarious region of Italy, and instituted a treatment campaign in one and antilarva measures in the other, providing equal budgets for the two experiments. The first town had about 1,000 inhabitants and, employing a doctor and a nurse, we succeeded in treating virtually every case of malaria or of parasitemia as soon as it was detected, and we continued this therapeutic campaign for three years. The supply of quinine was unlimited and it was pressed upon the patients in quantities unnecessarily large. In fact, the average consumption per case treated was over 31 grams, three times the amount generally considered ample. It reminded me of old times in Panama. Nevertheless, the amount of malaria actually increased over the three years; the parasite rate went up from 22 to 36 per cent and the spleen rate from 75 to 94 per cent. In the last year we treated 915 individuals, almost everybody in town. The quinine did a great deal of good; it relieved suffering and reduced the death rate, but there was no evidence that it had suppressed endemic malaria in the slightest degree. In contrast, malaria diminished rapidly in the village protected from mosquitoes, and before long there developed an emigration of anemic children from the first town to the second during the worst of the summer months. The Sardinian demonstration put our modest Malaria Experiment Station on the map, nationally and internationally. I was appointed Vice President of the Malaria Commission of the League of Nations, a purely ornamental job if there ever was one, but indicating their change in attitude, and Prof. Missiroli was at last able to obtain the ear of his government, whose authority was then concentrated in the person of Mussolini. It transformed the official malaria-control measures

in Italy, and the use of Paris green spread around the world. The attack on the mosquito now had a decided edge over the attack on the parasite, which it has never lost.

It was Paris green which Drs. Soper and Wilson of the IHD used in Brazil to eradicate *Anopheles gambiae* in 1939–40. This led to another mutation in our thinking about malaria. Long before that time, eradication of malaria had been discussed, but without conviction. Dr. Frederick L. Hoffman, statistician for the Prudential Life Insurance Co. procured in 1916 the formation of the National Malaria Committee as a catalyzing agent for the eradication of malaria from the U. S. and possibly from the Western Hemisphere. It was estimated at that time that more than a million cases and 15,000 deaths occurred annually in the U. S. due to malaria, causing an economic loss which ran into the hundreds of millions of dollars. Virtually nothing was being done about this, and in 1917 Dr. Hoffman read a paper before the Committee entitled "A plea and a plan for the eradication of malaria." He was soon led to the conclusion that this would entail the eradication of the malaria vectors, for he said in 1918, "Any effort must fail to rid the community of malaria if the *Anopheles* mosquitoes are not entirely done away with." That seemed an extravagant proposal in the days of quinine and minor drainage, and the nation was not stimulated to action until Soper's and Wilson's spectacular feat in Brazil. Here was an achievement, accomplished with the tools we all had, which aroused first this country and then the world to revolt against paying annual tribute in lives and fortune to the brutality and exactions of malaria. Dr. Wilbur A. Sawyer, then Director of the IHD, a man of cool and balanced judgment not apt to be carried away by a wave of over-optimistic enthusiasm, became an advocate of mosquito eradication as the safest and most economical way of getting rid of malaria. "The success beyond all predictions," he wrote, "of the major strategy in the extermination of *A. gambiae* in Brazil has fired the imagination of malariologists everywhere." and Dr. L. L. Williams, Jr., began to elaborate the National Malaria Eradication Program which was put into effect by Congress in 1947, and which in turn influenced the WHO to enter upon its present global effort. So in malaria as in hookworm, the IHC set in motion a chain of events which we can describe, in Rose's words, as "bound up in a sure sequence."

The *gambiae* campaign in Brazil had a curious sequel which caused consternation for a brief period throughout South America, but which as far as I know, has never been reported. The last adult *gambiae* was caught in Brazil on November 9, 1940, and continued surveillance for almost two more years failed to turn up a single larva or adult. It was practically certain that every last *gambiae* had died.

But on September 5, 1945, I received a letter at the regional office of the IHD in Buenos Aires from the Rio de Janeiro office calling my urgent attention to an article in the Journal of the Museum of Natural History of Asunción, Paraguay, by a Russian entomologist named Podtiagin, the Curator of the Museum. Podtiagin stated that among some mosquito larvae collected at Colonia Independencia during a very serious epidemic of malaria, he had identified larvae of *A. gambiae*. Podtiagin had worked in Africa and knew the species well. The letter said that he had sent an adult specimen bred out from one of the larvae to the Oswaldo Cruz Institute in Rio, where it had been confirmed by Nelson Cerqueira as undoubtedly *A. gambiae*. Dr. René Rachou of the Brazilian Malaria Service had gone at once to Asunción and I lost no time in getting the news to Dr. Carlos Alvarado who was Director of the Malaria Service of Northern Argentina. Both Brazil and Argentina sent commissions immediately to examine the entire length of their frontiers with Paraguay, but no *gambiae* were found. A joint Brazilian and Argentine committee was then sent to Asunción to interview Podtiagin and review the evidence. Podtiagin suggested at least four routes by which *gambiae* might have entered Paraguay. A plane from North-East Brazil during the *gambiae* invasion might easily have continued its flight to Asunción. The Botanic Garden of Asunción had recently received jars of tropical plants from Africa which might have contained larvae. During World War II a visiting entomologist named Jörgensen, who called himself a Dane but who Podtiagin suspected might have been a German, had been studying the possibilities of "adapting exotic mosquitoes to Paraguay." He died and his widow sold his collections to Podtiagin who found *gambiae* among his live larvae. But in Podtiagin's opinion the most likely means of transport of live *gambiae* from Africa to Paraguay was in one or both of the two gunboats which Paraguay had bought in Italy just before the war and which had

stopped to take on water ballast in Senegal. A Dr. Alexander Ladkovsky, contracted by the Paraguayan government, had boarded one of these gunboats in Algiers and later, in mid-Atlantic, had caught a *gambiae* mosquito on the boat. He also found larvae in the bilge and wrote an article entitled "Mosquitoes of Paraguay" for the Revue de l'Université de Nancy (Vol. I, pp. 68-85, 1938) in which he mentioned this fact.

Podtiagin thought that *gambiae* was rather widespread in Paraguay. Dr. Schenone, a well-known Paraguayan physician, was bitten by a strange-looking mosquito in the Park in Asunción; he caught it and took it to Podtiagin, who recognized it as *gambiae*. But Schenone, a bit skeptical, sent another specimen to his brother who was Military Attaché in Paris asking him to have it classified at the Pasteur Institute. He received a report signed J. Bedoc identifying it as *A. gambiae*. In the meantime an American named Schneider, who collected for North American museums, had sent Podtiagin some adult mosquitoes from the north of Paraguay, some of which were *gambiae*.

Podtiagin, however, had no *gambiae* material to show the Joint Committee, since it had all been destroyed or lost in various ways except the one mosquito sent to the Oswaldo Cruz Institute. The various witnesses too were unavailable. Dr. Schenone had died and Schneider had returned to the U. S. There had been several violent deaths among the group which struck a rather sinister note; Jörgensen was found dead in the jungle with his head bashed in, and Ladkovsky had been assassinated.

The Joint Committee soon found that Podtiagin was not much of an entomologist, but was an expert at intrigue. He spread stories about the Argentines among the Brazilians and about the Brazilians among the Argentines, and set the Paraguayans against them both, with such success that the Joint Committee almost broke up, and Dr. Barbero, President of the Sociedad Científica del Paraguay (which ran the Museum) refused to give the Committee any information. The Committee then got busy running down the secondary clues in this singular affair. Dr. Schenone's brother, the Military Attaché, was traced to Buenos Aires, and his testimony was a shock to the Committee; he had never heard of *A. gambiae*, his brother had not sent a mosquito to him in Paris, and he had never set foot in the Pasteur Institute. The Captain of the gunboat was found

and he said he had not picked up anybody named Ladkovsky in Algiers, nor had he taken on water in Senegal. In any case, both gunboats had been laid up for weeks in dry-dock in Buenos Aires to have their bottoms scraped, and could not have transported any mosquitoes to Paraguay. The Botanic Garden had received only dried plants from Africa. What then of the specimen sent by Podtiagin to the Oswaldo Cruz Institute and identified as *gambiae*? Dr. Rachou remembered that Dr. Cesar Pinto of the Brazilian Malaria Service had distributed specimens of *gambiae* to all the South American countries to help them identify any possible invaders from Brazil. He rushed to the Museum, but this specimen could not be found; it had undoubtedly been sent to Rio. The entire business in all its impressive detail had been a carefully prepared hoax. But when the Committee in a body went to charge Podtiagin with fraud, he could not be found; he had eradicated himself from Paraguay. And why did he indulge in this elaborate deception? I think he planned to put himself at the head of a campaign to exterminate a nonexistent *Anopheles gambiae* and to go down in history as the Soper of Paraguay.

I am afraid I have dwelt overlong on malaria, because this is a story I know at first hand. But Rose also selected a third disease against which to wage a war of extermination. While he was in Singapore in 1914, after his visit to Malcolm Watson, he learned that there was apprehension in India and the Far East over the opening of the Panama Canal, which was to take place in August of that year. It was feared that the shortening of the sea route from the Caribbean and Brazilian seed-beds of yellow fever would place the entire Orient at risk, where *Aedes aegypti* was prevalent in many areas. Col. James suggested establishing a quarantine station in some selected port of the Far East such as Hong Kong or Singapore, where all ships from the Canal would be rounded up for inspection before being allowed to proceed to their destinations. Rose knew that the person to see about this was Gen. William C. Gorgas who had just left the Canal Zone to become Surgeon General of the Army. Immediately upon his return Rose got in touch with Gorgas, a consultation which had momentous consequences, for it marked the inception of the largest project which the IHD ever undertook. To Gorgas, the British alarm must have seemed nothing less than providential, if it should lead to a great campaign

throughout the Western Hemisphere, a thing which he had dreamed of but had not thought it possible to realize. He was convinced that yellow fever could remain endemic only in a very few large seaports—not more than five or six perhaps in the American tropics—where babies and immigrants would be numerous enough to provide a constant supply of new fuel. Whenever yellow fever broke out elsewhere in epidemic form, it was a blaze which always rapidly burned itself out. Gorgas's experience had been that the disease, though occasionally able to spread for thousands of miles along routes of travel, could not long withstand a well-directed attack on its "key centers." Thus the campaigns in Havana, Panama and New Orleans had freed the entire Caribbean area from yellow fever. "The IHC," he told Rose, "could not undertake a better piece of work than this. . . . The eradication [of yellow fever] would command the attention and gratitude of the world—and the thing can be done!" In fact, the IHC was the only agency which could wage such an international campaign, for the Pan American Sanitary Bureau at that epoch was an intelligence office without authority or funds. After three weeks of reflection, the Commission resolved to undertake the task. Rose thought it might take at least two years, but Henry Rose Carter, the distinguished epidemiologist of the Public Health Service, wise and cautious as he was, wrote to Rose: "I think you have rather exaggerated the difficulty of getting rid of yellow fever when you put two years as the time required. We know just what to do. There is no uncertainty in it." It had only taken Dr. Joseph H. White six weeks to end transmission for all time in New Orleans in 1905. The prudent Rose however set up the project for a five-year period. The dangers of unconditional prophecy are well illustrated by the fact that it took the IHC 34 years to complete its yellow-fever program; 76 staff members were associated with it at one time or another, and it cost the Rockefeller Foundation almost 14 million dollars.

Gen. Gorgas made a survey of the yellow fever situation in South America in 1916, and while he was in Bogotá, Colombia, with his Commission, inquiring in vain about outbreaks of acute disease in the interior which might be yellow fever, a telegram was received by the National Health authorities reporting an epidemic of yellow fever in the town of Muzo, an isolated spot near some emerald mines, 2,700 feet above the sea and 100 miles northeast of Bogotá. This was very pat

indeed, and the General and three of his companions got on horses and rode for several days up the mountains to Muzo. When they arrived they found no very sick people and no cases that suggested yellow fever; more than that, they found no adults or larvae of "*Stegomyia*" (the old name of *Aedes aegypti*), which clinched the case against there having been any yellow fever there. The Committee's report went on to say: "The Alcalde of the town frankly admitted that his telegram had been without foundation; that he had received a number of communications from high officials in Bogotá asking about yellow fever; that for some reason his superiors wished to have the disease in Muzo; and that it was his duty as a subordinate appointed by Government to meet the demand of his superiors; if the Government wanted yellow fever in Muzo, the Alcalde must furnish it. The Commission believes that this mining region may be dismissed from the area requiring consideration." However, jungle yellow fever was surely endemic in Muzo at that very time as later outbreaks proved. Probably the epidemic was not purely an invention of the Alcalde, but was over before the General arrived. As it was, the possibility of yellow fever in the absence of *Stegomyia* was not entertained, and the Commission came to the conclusion that in all South America the only endemic center at the time was Guayaquil.

Dr. M. E. Connor of the IHC cleaned yellow fever out of Guayaquil and all of Ecuador in six months. Unfortunately, that did not ring the knell of yellow fever in the Hemisphere. He had hardly recorded the last case in Guayaquil in May, 1918, when an epidemic was reported from Guatemala, and soon it was cropping up all over the map. In 1919 there were outbreaks in Peru, Brazil, Honduras, Salvador and Mexico, followed within a year or two by epidemics in Nicaragua and in Colombia without known antecedent cases and at points completely isolated from any possible sources of infection. Thus perished the key-center concept, but what was wrong with it was not known for many years, until finally in 1932 a rural epidemic, materializing out of nowhere, occurred in the Valle do Chanaan, in Brazil, in the complete absence of the *Aedes aegypti* mosquito, and it was discovered that yellow fever was not only a disease of man but of monkeys too, spread by forest mosquitoes not vulnerable to any conceivable method of attack.

Thus the hope of banishing yellow fever from

the earth was sunk by jungle yellow fever, but much was salvaged when that ship went down. By 1937, a vaccine had been perfected which protected people from infection by jungle mosquitoes, and a blood test for immunity—the mouse-protection test—had been evolved in 1930 which revealed past infections with the virus in man or monkey. This made it possible to map the endemic areas of yellow fever and even localize previous epidemics in time and place if they had occurred within the lifetime of the people examined, thus giving intelligence of the enemy. A survey of the world for yellow fever immunity in man was at once instituted by Dr. Sawyer in 1931, which revealed a much wider distribution of the virus in South America and Africa than had been suspected. This survey took five years, and since it led the yellow fever staff into the rain forests of both continents and among primitive and even savage peoples, it was attended by hardship and danger. Some of the stories which came out of the jungle have acquired by multiple passage from mouth to mouth happy accessions and incrustations of myth which give them almost a legendary quality.

There is one I prize about Dr. Allen M. Walcott, the dependable troubleshooter of the yellow fever crew. One of his missions took him over the wind-swept Andes of Ecuador by mountain trails into the Amazon jungle between the Pastaza and the Napo rivers, a rugged trip if there ever was one. This is the country of the warlike Jíbaros, the notorious head-hunters of the Upper Amazon, some of whose blood he hoped to obtain for the test, along with that of any of the fabled Amazons he might meet, the beautiful nude warriors on horseback. As his colleagues wrung his hand at his departure and said

goodbye, it crossed their minds that one of them, idling some day in a shop in Belém do Pará, might come upon a shrunken head as small as your fist and cry, "Good heavens, that's Walcott!" But Walcott turned up in Iquitos with his head on his shoulders and the blood of the Jíbaros in his bag. I wrote Walcott the other day to try to confirm this story, but he said it was mostly invention—that is, he had made the trip and collected a lot of blood, but had run across only three teen-age Jíbaros, who put up no resistance. He was silent on the subject of the Amazons. His version and mine are at variance only statistically, as you see, and I do not regard the difference as significant.

I will close on this note of modest claims and successful accomplishment, which was characteristic of Wickliffe Rose and in some measure, I like to think, descended to his staff, who admired and were loyal to him. It is ironical, as I look back on the three diseases selected for demonstrations of control, that hookworm disease, chosen for its simplicity as target for the first eradication campaign of them all, has turned out through defects of the human temperament to be the most intractable, and that yellow fever, which Dr. Carter gave two years to live, now appears to be immortal. But malaria, protean and tenacious, the endlessly adaptable and resourceful foe, may well be on the way to extinction. I have sketched for you the earliest flowering of international public health, the tentative efforts of a private organization to stir oppressed peoples to work for their own salvation. To paraphrase the poet, all this I saw and no small part of it I was. The International Health Division, its task as a catalyst accomplished, is no more; but its soul goes marching on.