



Adolpho Lutz and controversies over the transmission of leprosy by mosquitoes

Adolpho Lutz e as controvérsias sobre a transmissão da lepra por mosquitos

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During his years of study in Switzerland and Germany, Adolpho Lutz published his first articles on zoology, clinical practice, and therapeutics. In Limeira, São Paulo, he began studies on animal and human diseases caused by germs and parasites. In 1885-86, Lutz traveled to Hamburg to study the morphology of germs related to skin diseases, in conjunction with Paul Gerson Unna, one of Germany's foremost dermatologists. He proposed the inclusion of Hansen's and Koch's bacilli in a new genus. In 1889, Unna nominated his student as physician-in-chief of the Leper Settlement on Molokai Island, Hawaii. From then on, Lutz sustained the theory that the disease was transmitted by mosquitoes. He conducted research to prove this theory when he was head of the Instituto Bacteriológico de São Paulo (1893-1908) and, later, after he moved to the Instituto Oswaldo Cruz (1908-1940). Although this research was not successful, on commissions and at congresses in which he participated until his death in October 1940, he still held to his conviction that leprosy was transmitted by mosquitoes.

KEYWORDS: Adolpho Lutz, history of leprosy, microbiology, history of tropical medicine.

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Quando estudava na Suíça e Alemanha, Adolpho Lutz publicou os primeiros trabalhos sobre zoologia, clínica e terapêutica. Em Limeira, São Paulo, iniciou estudos sobre doenças humanas e animais causadas por germes e parasitas. Em 1885-86, viajou para Hamburgo para estudar microrganismos relacionados a doenças de pele sob a orientação de Paul Gerson Unna, um dos mais renomados dermatologistas alemães. Propôs a inclusão dos bacilos de Hansen e Koch num novo gênero. Em 1889, Unna indicou seu discípulo como chefe dos serviços médicos do Leprosário de Molokai, no Havaí. Lutz passou a defender a transmissão da doença por mosquitos. Realizou pesquisas para provar esta teoria depois que assumiu a chefia do Instituto Bacteriológico de São Paulo (1893-1908) e, sobretudo, após a transferência para o Instituto Oswaldo Cruz (1908-1940). Apesar de não terem sido bem-sucedidas estas pesquisas, sustentou a transmissão da lepra por mosquitos nas comissões e congressos de que participou, até sua morte em outubro de 1940.

PALAVRAS-CHAVE: Adolpho Lutz, história da lepra, microbiologia, história da medicina tropical.

One of the most interesting chapters of Adolpho Lutz's scientific work is the one dealing with leprosy,¹ a topic he investigated until the end of his life. By then one of Brazil's leading experts in this field, he went to his death convinced the disease was transmitted by mosquitoes. He had turned his interest to the illness during an era marked by major theoretical and practical turbulence over conflicting views on its etiology, transmission, and prophylaxis.

Among scholars of leprosy from both past and present, it is consensus that two Norwegian physicians, Daniel Cornelius Danielssen (1815-94) and Carl W. Boeck, established the disease's defining characteristics on scientific bases in 1847. While Danielssen and Boeck did not discard a possible association with dissolute and anti-hygienic living conditions or with an unhealthy environment — as upheld under the neo-Hippocratic paradigm in the case of many other diseases — they maintained that leprosy was essentially a hereditary disease.² As this belief spread, the fear long instilled by the disease came to an end, at least among doctors. Under the new assumption that leprosy was not contagious, previous concern over the need for strict isolation or segregation of its sufferers waned. Extending to the bubonic plague, cholera, yellow fever, and other diseases (Ackerknecht, 1948), this anticontagionist vogue was of short duration, and by the late 1870s it had already begun to ebb.

Leprosy was one of the first infectious diseases to be restructured in the light of microbiology, once again by a Norwegian, Gerhard Armauer Hansen (1841-1912), physician at Lazarus Hospital in Bergen. Hansen named the small rod-shaped bodies that he observed in the cells of cutaneous tubercles *Bacillus leprae*, since their constant presence on examined skin lesions made him suspect they were the specific cause of the disease. Hansen reported his discovery to the Cristiânia Medical Society in 1874, and his finding was soon after confirmed by Edwin Klebs.³ Using material provided by Hansen, Albert Neisser offered a more consistent description of the bacillus in 1879, thanks to pioneer use of the staining techniques that gained prime importance in the observation of this and other microorganisms.

As Obregón (1996, pp. 173-4) has shown, a clash then arose between two opposing sets of conceptions and 'evidence' on how leprosy is transmitted, giving birth to divergent strategies for dealing with the disease. The physicians and lay public involved in this controversy took as "ideal-types" the prevention models adopted in two different regions of the world: the "democratic" model, which got its start in Norway at a time when nationalism was on the rise and doctors were greatly interested in the study of territory, population, and epidemiological profiles; and the segregationist, colonialist model enforced in Hawaii by metropolitan administrators who were repulsed by leprosy and nourished a strong prejudice against native or Asian-blooded sufferers.

In different countries or colonies, physicians then recently converted to bacteriology were unsuccessful in their attempts to replicate Hansen's bacillus *in vitro* so as to satisfy Koch's prerequisites as postulated in the early 1880s: isolation of the microorganism in pure cultures, experimental inoculation of animals, and production of a disease whose symptoms and lesions were if not identical at least equatable to those of the disease as "typical" in man. These problems made it hard to unequivocally prove a connection between the bacillus and leprosy. Nevertheless, the 1st International Leprosy Congress, held in Berlin in October 1897, acclaimed this specific etiology, along with the thesis that the only way to keep the disease from spreading was mandatory reporting, oversight, and the mandatory isolation of its victims. Based mainly on epidemiological observations presented by doctors working in India, the Guianas, and other colonial possessions, the congress approved resolutions that affirmed the sovereignty of contagion over the heritability of leprosy, although the latter theory still had numerous proponents, foremost among these Rudolf Virchow, Ferdinand Von Hebra, and the Turkish physician Demetrius Zambaco Pacha (Obregón, 2000, p. 271; 1996, pp. 165-6).

The 'construction' of leprosy as a microbial disease spurred a worldwide movement to create leprosariums where the afflicted would be segregated. Because of the problems in obtaining a vaccine,⁴ the disease was considered chronic and incurable, reinforcing the belief that carriers of its microorganism must inevitably be segregated.

Held in Bergen, Norway, in 1909, the 2nd International Congress, chaired by Hansen, ratified the decisions made earlier in Berlin. In 1922, in Rio de Janeiro, with Carlos Chagas acting as chair, the 1st American Leprosy Congress remained firm to this tendency while nevertheless making room for a third stream of thought, led in Brazil by Adolpho Lutz: leprosy was transmitted by mosquitoes, just as yellow fever and malaria were. Lutz was honorary chairman of the event, attended by representatives of thirteen countries. The congress's conclusions stressed the need to foster scientific investigations of the disease and to create specific professorships at medical schools.⁵

Leprosy studies during 1860-80

At the time that Adolpho Lutz took up his interest in leprosy, one of Brazil's main treatment and research centers was Lazarus Hospital in Rio de Janeiro, under the auspices of the Irmandade do Santíssimo Sacramento da Candelária. From reports written by Dr. João Pereira Lopes, physician at the hospital during the period leading up to Lutz's involvement in the field, we can evaluate the state of the art which Lutz was about to help change.

In his report on the year of 1869,⁶ Lopes discussed several hypotheses concerning the etiology of leprosy; while emphasizing syphilitic,

nutritional, and climatic origins, he also did not abandon the eclectic or multicausal tendency prevalent among doctors working with this disease, often called ‘Greek’ elephantiasis (*Elephantiasis Graecorum*) or morphea in Brazil then. One vital aspect of the problem was the idiosyncrasies displayed by certain individuals — that is, the particular state of the organism (imprecisely defined) that determined a tendency to develop the disease. There were those who believed that certain professions, such as blacksmithing or mining, contributed to a predisposition. Another assumption was that climate had a notable influence on the appearance of leprosy. Many stressed the role of food, while not necessarily failing to endorse the widely held belief that this disease was similar in nature to syphilis, caused by a “virus” (understood to mean ‘poison’) that acted on the blood, disorganizing the ‘crasis’ of this humor. A related theory posited that syphilis was nothing more than a degenerate form of leprosy.

Lopes was an anticontagionist, and this seems to have been the predominant position among doctors of his day, shared as well by many members of the lay public. At least this is what his 1869 report suggests: Lazarus Hospital was “constantly visited by people from all classes and ranks, Brazilian and foreign, most especially by physicians ... drawn by curiosity or the news of a cure, announced in daily papers.” Many families were not afraid to pay a visit to the beautiful building constructed in São Cristóvão, near the imperial family’s residence, or to attend religious ceremonies there. “Long gone are the times,” wrote Lopes, “when Christian charity fled in terror ... , thanks to the progress of science, which has so clearly shown us that the idea of contagion, once so defended ... , has wholly disappeared in view of the numerous observations of noteworthy practitioners in Africa, North America, Norway, Brazil, and, lastly, France, as made by Alibert and Biett” (cited in Souza Araújo, 1946, p. 469).

Despite this optimism, which warranted bringing the disease’s victims somewhat back into the society from which they had for so long been removed, the treatment of leprosy was characterized by the “obscurity of darkness, [by] tremendous chaos” (ibid., pp. 463–4). Lazarus Hospital had long been — and to judge from reports by Lopes’ successors, would still long be — stage to endless experimentation. What is curious is that both lay people and experts seem to have wielded almost equal influence over the medicines tried out during the 1860s and 1870s, medicines which might equally well include preparations from local apothecaries, chemotherapeutics produced at European laboratories, or substances extracted from either local or foreign flora and fauna.

Lazarus Hospital had been the setting for a famous experiment with tropical rattlesnake venom (*Crotalus horridus*) that had killed the patient who served as guinea pig (Ferreira, 1996). Lopes had already tested a variety of plants supplied by physicians or lay people: roots of the *mochucho* plant; *cabeça-de-frade* (*Melocactus bahiensis*); and the milky

juices of the Barbados nut (*Jatropha curcas*), figueira-brava (*Ficus* sp.), and a type of manioc (*Manihot utilissima*).⁷ Among the populations of Brazil's *sertão* region, the "irritating, corrosive" juice of the latter tuber was known to help cure elephantiasis. Another plant extolled by them "as astonishingly efficient" was the yam, eaten or used in bathing.

Four experiments were underway at Lazarus Hospital at the close of 1868. One involved "warm baths with spiderwort [*Tradescantia* sp.] and *mamono branco*, a kind of papaya [*Carica* sp.]; a cooked mixture of barley, sarsaparilla [*japecanga*], and whey," where the tubercles would be rubbed with "large gastropods, which should be kept in grasses or the garden." The diet consisted of plants like "amaranth [*caruru miúdo*], chicory, beet, sowthistle, and, lastly, yam [*inbame branco*]; the sick person [could] eat some eggs, drink barley coffee, and even eat some very ripe oranges. Once in a while, the person should take some purgatives of trimeza [*Trimezia* sp.]" (Lopes, p. 34, as cited in Souza Araújo, 1956, p. 461).

Since leprosy was likened to syphilis, this led to experimentation with *Hydrocotyle Asiatica* (*Hydr. Asiatica*), a product that doctors Paupéau, Boileau, and Hunter supposedly employed most successfully in the treatment of scrofulas as well. It could also be used to treat the chronic rheumatism that afflicted so many of those interned at Lazarus Hospital.

Preparations of arsenic — which "Hindu physicians and those from Bengal, and also English and Anglo-American doctors" touted in the treatment of leprosy and syphilis — yielded almost no positive results. In 1869, Lopes also experimented with bromine and bromine compounds, in combination with baths of "sulphurous hepatic waters" prepared by a pharmacist from Rio de Janeiro for those who suffered from "rheumatism of the joints, paralyses, chronic syphilitic ulcers, and, lastly, scabies, which epidemically reign in this hospital two to three times a year" (cited in Souza Araújo, 1946, vol. 1, p. 461).

The hiring of Dr. José Jeronymo de Azevedo Lima to head up Lazarus Hospital in 1879 coincided with a turnabout at the level both of discourse and of curative and preventive practices. The physician began his first report (Lima, Aug. 5, 1880) by attempting to restore belief in leprosy's contagiousness. Although this idea had held sway in the past, it had become so "outside reason" since the studies of Daniëssen and Boeck, Von Hebra, Virchow, and others that, for Azevedo Lima, questioning these authors meant "risking an accusation of incompetence" (cited in Souza Araújo, 1946, vol. 1, p. 484).

There were as yet scant authorities whose names could be cited in defense of contagion. Azevedo Lima drew support from Hansen's newest studies but he admitted that these were not "certain and proven." Contemporaneous etiological theories did a better job of explaining the numerous examples of immunity observed "in relations of the greatest intimacy."

Studies on the morphology and germination of *Bacillus leprae* were still incomplete. Its presence in blood had not yet been proven, “but,” Azevedo Lima wrote, “this notwithstanding, the classic doctrine on the illness has still been ... deeply shaken, which will undoubtedly prove fruitful for practical deductions and, perhaps, come to place it among the cast of virulent affections” (ibid., p. 485).

In the absence of any means for effectively combating the disease, there was no alternative but “a more or less rational empiricism,” and Azevedo Lima experimented with a good number of medicines with different effects and properties. The basis of his treatment was to “boost or maintain organic forces by modifying nutrition, through good-quality food, ... regular exercise of skin functions, etc.” (cited in Souza Araújo, 1946, vol. 1, pp. 485-8). Disinfection of infirmaries became routine, and to the list of medications in use was added phenic acid, a well-known antiseptic employed internally and externally against many other microbes inculcated as disease agents by followers of Pasteur and Koch. Azevedo Lima began treating leprosy patients with chaulmoogra oil extracted from the seeds of plants native to Southeast Asia of the genus *Hydnocarpus*, family Flacourtiaceae. According to Obregón (1996, pp. 164-5), this substance and its products, administered orally or hypodermically, were the only even minimally efficient treatments available up until the 1942 introduction of Promin, a sulphone derivative developed by Guy H. Faget.

In mid-1886, Azevedo Lima reported to the purveyor of the Irmandade do Santíssimo Sacramento da Candelária on fruitless experimental attempts to transmit leprosy to humans and animals; he also presented an evaluation of the results obtained with the treatment proposed in 1885 by Paul Gerson Unna, one of the world’s leading authorities on leprosy. Presupposing that the bacillus is oxygen starved and that it would be possible to destroy it by means of likewise oxygen-starved substances, he had proposed using such reduction agents as “pyrogallol,” “ichthyol,” “chrysarobin,” and resorcin, *intus et extra* (cited in Souza Araújo, 1946, vol. 1, p. 488).

That same year, Adolpho Lutz released his first paper on the leprosy microbe, which appeared in a Leipzig publication edited by Unna (1886). Lutz began his studies on this and other dermatological diseases in 1880 when he set up office as a physician in Limeira, a city in rural São Paulo state. By late 1888, he estimated having treated 200 to 250 lepers, “of which 50 would be followed for a long time” (Corrêa, 1992, p. 146). He judged that there were then 5,000-10,000 sufferers in Brazil, most in São Paulo, which he felt was one of the hardest-hit states.

In March 1885, Lutz left Limeira to work for about a year at the clinic Unna had founded in Hamburg. Under his orientation, Lutz ventured into the terrain of bacteriology, dedicating himself to the morphology of germs related to different dermatological diseases, mainly leprosy.⁸

Microbiologists were struggling to obtain pure cultures of Hansen's bacillus *in vitro*. Lutz tried in Hamburg but failed. Nor was he successful in transplanting it from humans to animals, so that the latter would develop a "typical" disease. Study of the microorganism's structure was facilitated by a staining technique developed by Lutz and refined by Unna. Thanks to this process and the method discovered by Ehrlich, Lutz was able to distinguish the leprosy agent from other microorganisms, except for the tuberculosis agent, then recently discovered by Koch. "This is quite an interesting fact," Lutz wrote, "that two illnesses so similar from the anatomical side ... are also produced by parasites that only differ. They can be distinguished in all certainty neither by shape nor by dye reactions" (Lutz, 1887, cited in Souza Araújo, 1946, p. 492).

In his paper published in 1886, Lutz endeavored to show that leprosy "schizomycetes" did not belong to the category of "legitimate bacilli, formed by one or more cylindrical cells," since they were found in three different forms in tissue: small spherical or oval corpuscles, isolated rods, and larger shiny masses that contained a large portion of the first two elements. A comparative analysis of this microorganism with that of the tuberculosis microorganism prompted Adolpho Lutz to disagree with their classification in the genus *Bacillus* and to propose calling the Hansen microorganism *Coccothrix leprae*. His suggestion found no echo in the scientific community and was supplanted by Karl B. Lehmann and R. O. Neumann's 1896 proposal that the agents of leprosy and tuberculosis be classified in the genus *Mycobacterium*.⁹

According to his daughter, Bertha Lutz (*Lutziana*); Arthur Neiva (1941, p. iii); and others of his Brazilian biographers, the scientist had presented enough evidence for *Coccothrix* to be considered the valid name, in a paper published ten years earlier. In 1936, Lutz himself was still complaining (pp. 373-81): "The germ that is judged to cause leprosy is generally called *Bacillus leprae* or Hansen's bacillus. But in fact it is not a true bacillus. In 1886, I proposed that the genus term *Coccothrix* be applied to this and to the germ of tuberculosis, which takes precedence over the name *Mycobacterium*, generally used."¹⁰

Lutz's claim about precedence was negated in a decision by the Judicial Commission that originated from the 1st International Microbiology Congress, held in Paris in 1930. At that time a Bacteriological Nomenclature Committee had been created, and it had in turn drawn up the commission, entrusting it to prepare a nomenclature code for bacteria in order to settle the many disputes pending in this area. The code was approved by the 1947 International Microbiology Congress, in Copenhagen, and revisited by decision of the following congress, held in Rio de Janeiro in 1950. The International Code of Nomenclature of Bacteria and Viruses approved in Rome in 1953 was subject to further revisions in accord with decisions made by the Judicial Commission. In 1958, the commission resolved that the genus *Coccothrix* Lutz 1886 had not been properly published, in that the author had

failed to use the genus name in conjunction with the species he was including within this genus (i.e., leprosy and tuberculosis bacilli) and, further, because he failed to provide descriptions of these species, previously published under other names (Lessel Jr., 1960, p. 117).¹¹

For Otto Bier (1963, p. 538), the granulations found in the leprosy bacillus should be designated “Lutz granulations” since the Brazilian had studied them quite thoroughly in 1886. Although Lutz had also underscored similarities between the Koch and Hansen bacilli and postulated the inclusion of both within the same genus, only a scarce few references to his work can be found in the literature on leprosy. One reason may be the likening of these microorganisms and cocci: ‘Coccothrix’ derives from the Greek *kokkos*, which means ‘grain’ or ‘seed’, and *thrix*, which means ‘hair’, suggesting a string of cocci. Lutz described the leprosy agent in these words: “small, round, coccoid cells that divide without the co-participation of the cellular membrane, in one direction only, being found, consequently, isolated or in strings. Staining of the deeper layers of the membranous-gelatinous outer covering reveals larger cells, in part oval and bearing a double border, sometimes free, sometimes at the ends of the cellular strings” (Lutz, 1886, p. 22 of the translation).

Problems in cultivating the microbe and replicating it in animals made contact with sufferers indispensable in order to guarantee an ongoing source of organic matter for the preparations used in microscopic studies both of the microorganism’s morphology and biology and of how the microorganism was distributed within lesioned organs and limbs. Because it involved exploring cadavers and the bodies of the ill, internally and externally, this second line of investigation required a hospital, and this is undoubtedly what led Lutz to Rio de Janeiro’s Lazarus Hospital in 1887.¹²

That same year he moved from Limeira to São Paulo, the state capital, resumed his private practice, and continued to publish numerous articles, mainly in Germany, not only on dermatology but on helminthology as well.¹³ It was then that the Portuguese translation of his work on ancylostomiasis, originally published in Leipzig (1885), came out in *O Brazil-Médico*, which was a series of articles published in Bahia’s *Gazeta Médica* (1887-89); soon after, it appeared in book form (1888), making Adolpho Lutz better known among his peers in Brazil. In 1889, in the prestigious *Centralblatt für Bakteriologie und Parasitenkunde* (Jena, Germany), he published his first studies on protozoans, the myxosporidia found in the gall bladder of batrachia, an order of animals to which he would return at the end of his life.

Lutz’s trip to Hawaii

In a letter dated October 13, 1886 (BRMN Fundo Adolpho Lutz, pasta 255, maço 2), Paul Gerson Unna informed Adolpho Lutz of the

arrival in Hamburg of the pathologist and bacteriologist Edward Arning, who had worked with lepers in Hawaii from November 1883 through July 1886. The archipelago's Board of Health had hired Arning to conduct a scientific investigation of the disease. Starting in September 1884 and continuing thereafter for four consecutive weeks, Arning inoculated Keanu — a native prisoner sentenced to death — with Hansen's bacilli, but it was only 25 months later that the prisoner developed nodular leprosy. Because the disease took so long to appear and because it was suspected that members of the prisoner's family might also have it, the result of the experiment was a matter of controversy. In late 1885, Arning reported on his disappointing attempts to cultivate the bacillus in artificial media and to locate it in the air, water, and food. The Board of Health fired the English physician,¹⁴ despite his desire to continue with the experiments. As Obregón (2002, pp. 143-7) has shown, other leprologists were to try to study the disease in Hawaii but their relations with the local sanitary authorities invariably became strained, as a result of the "many inconveniences, obstacles, and hardships" placed before them.

Let us return to Unna's letter to Lutz. Consul Weber, who represented German interests in Hawaii, had told Unna that the Kingdom's Board of Health no longer intended to support "fruitless scientific experiments with leprosy" but was most interested in sponsoring "practical experiments aimed at finding a cure." Unna believed it possible, with the help of the consul, to arrange for a trip to Hawaii, where his therapeutic method could be tested on victims there. In his letter, Unna asked Lutz if he would be interested in making the trip.

According to Corrêa (1992, p. 146), the president of the Board of Health of the Kingdom of Hawaii, Dr. N. B. Emerson, drew up a formal invitation to the Brazilian physician on March 22, 1888. For the rest of the year, the terms of the contract were discussed via correspondence.¹⁵ The requirement that Lutz reside on Molokai Island was the subject of lengthy epistolary disagreements, since the Brazilian physician intended to set up a private practice in Honolulu and would not agree to large-scale experimentation with Unna's treatment.

In September 1888, the Leper Settlement on Molokai Island sheltered 881 afflicted individuals. Emerson calculated that an equal number were to be found in the kingdom's various provinces. Fearing the disease would spread by contagion, the government segregated sufferers and it expected to have all of them confined within a little over a year. The settlement stretched over some 5,000 to 6,000 acres (20,235 to 24,282 km²) on a peninsula on the island's north side, the most exposed to wind. The only edge of the premises not on the oceanfront was blocked by a sharp ridge of mountains some 3,600 feet high, running down the island's back. "This wall, or '*pali*', cannot be crossed, save for a path that leads up the mountains to the ranch of Mr. R. W. Meyer, a

German who is a Board of Health agent on Molokai Island and interim superintendent of the settlement.”¹⁶

In July 1889, Adolpho Lutz traveled to Hamburg to ready everything he would need in Hawaii, with Unna’s help. He reached Honolulu on November 15, right when the Brazilian monarchy was being deposed. In January 1890, he was appointed Government Physician for the Study and Treatment of Leprosy. His work was to be carried out at the Kalihi Receiving Station, future site of the U.S. Leprosy Investigation Station, while broader treatment, at the settlement, was assigned to the resident physician, under Lutz’s supervision.

In his first report to the Board of Health director (Lutz, 1890), dated April 1, 1890, Lutz described the condition of the patients taken to Kalihi and the problems encountered in commencing the new treatment because the station lacked necessary hospital facilities. A nurse had just arrived to help him — the Englishwoman Amy Marie Gertrude Fowler, whom he would marry the following year (Benchimol, 2003, pp. 13-83). Lutz’s second report, dated June 30, 1890, describes the evolution of patients treated with salol (phenyl salicylate), sodium salicylate, chaulmoogra oil, guaiacol, ointments of chrysarobin, and topical salves of anthrarobin, pyrogallol acid, goldenseal, chrysarobin, and *veratrum*.¹⁷

There are conflicting versions regarding the circumstances that led Adolpho Lutz and Amy Fowler to resign their positions at the Molokai settlement. According to Corrêa (1992, pp. 150-1), in August 1890, Amy punished a Hansen’s disease sufferer who was an employee at the Kalihi Station, Charles Hahalehile, because of the “malevolent comments he made to other patients about the psychotic states displayed by two of them, which he blamed on the medication used by Lutz.” Hahalehile appealed to the Board of Health, who set up an inquiry. Questioned aggressively, Lutz “proudly rebuffed the insult and showed that a simple request for information would have cleared matters up.”

In *Lutziana*, Bertha Lutz (1971) tells a different story. She claims her father — like other physicians before him — left the Molokai settlement owing to the “interference of lay people.” There was a white man living with a native woman, and his brother — one of the missionaries hated by Lutz — had him interned as a leper. The missionary apparently felt no scruples about resorting to this means to cover up the family scandal. Upon ascertaining that the native woman’s lover did not in fact suffer from the disease, Lutz proposed to the Board of Health that he be released. But the missionary put pressure on certain members of the board, and so when it met to vote, Lutz failed to gain the majority. Indignant, he handed in his resignation, with Amy Fowler following suit. “The unfortunate man, brother of the zealous missionary seeking to save his soul and put a stop to his *mésalliance*, committed suicide. Then some of the major U.S. newspapers showed up on the scene, asking to interview Dr. Lutz. He didn’t believe it was proper ethics for a physician to discuss hospital matters with the press, and so he refused

to receive them. Mrs. Amy Fowler, however, gave the interviews and the entire matter was cleared up, as it should be.”

Adolpho Lutz’s proud letter of resignation to the Hawaiian Board of Health, dated September 3, 1890 — less than a year after his arrival — has been transcribed in its entirety by Corrêa (1992, p. 151). It seems to confirm the first version of the facts. It may be that both episodes — punishment of the troublemaking subaltern and the story of the influential missionary — were linked by a more complex web of events, including the anticontagionist opinions Lutz expressed in a paper suggestively entitled “Leprophobia,” published in the *Journal of Cutaneous and Genito-Urinary Diseases* (1892) and also in the *Revista Medica de São Paulo* (1898).

Written in a harsh tone, Lutz’s letter left no bargaining room:

You will remember that in accepting my position with the Board of Health, I was careful not to bind myself to any given time. As my task could only be carried out if I found the necessary support where I had the right to look for it, I desired to provide for all emergencies. I am now satisfied by public facts that as a body, you not only refuse that support, but show yourselves very slow, if not absolutely reluctant, to do even common justice, sanctioning by your silence the disgraceful conduct of an inferior employé. After that, I think it unnecessary to enter into the numerous indiscretions and indelicacies, as well as the system of spying and reporting which the President and the Agent of the Board of Health seem to consider necessary to the fulfillment of their duties; nobody familiar with the circumstances will be astonished to learn that I refuse to go on exposing my life and my health meeting with such unfair treatment. If I have not resigned long ago, it is only because I would not have my resignation misconstrued; the unanimous vote of sympathy from all my patients satisfies me that my endeavors have been recognized where I most cared that they should be. I shall therefore give up my position as physician of the Kalihi Hospital at the end of the month at the latest, presuming that this time will suffice for my further arrangements. I expect retribution for my expenses for drugs and instruments, brought for and sent for from Europe, as well as of the sum stipulated as compensation for my journey home. Adolpho Lutz, M.D.

Through mid-1892, the Brazilian physician maintained his private practice in Honolulu, where he treated mostly European residents on the island, “among which, many Portuguese” (Bertha Lutz, 1971), and continued his research on leprosy and other topics related to dermatology. One of his favorite pastimes was to travel about the islands to study their flora and fauna (ibid). He published important papers in epistolary form (Sept. 1891-Aug. 1892) in the *Monatshefte für Praktische Dermatologie*. This correspondence was to continue during the second half of 1892, when he moved to San Francisco, California. In one of his letters, he described nodosity in the joints for the first time, characterizing these as syphilitic lesions. In 1912, they would be studied

this way “as a new approach, by Jeanselme” (Neiva, 1941, p. iv; Portugal, 1944).

Lutz remained interested in helminthes and used his stay in Hawaii to advance in his research on worms in humans and domesticated animals. According to a text by the Lutz Centennial Commission (1956, p. 9), it was there that he began the entomological observations that were to ground his later work as a sanitarian. He had already formulated his hypothesis that leprosy is transmitted by the mosquito. According to Albuquerque (1950, pp. 13-4), this conviction was to grow stronger over the years, but based on the following observations made in Hawaii:¹⁸

Although he had never avoided direct contact with the lepers, he had not caught the disease, nor had the young nurse in whose tender arms many of them crossed the doorways of life into death. However, among the sick who entered the settlement, many had never before seen another leper. There had been a time, and not so long before, when neither leprosy nor mosquitoes had existed in Hawaii. The native language had no terms designating either ‘leprosy’ or ‘mosquito’, and it dubbed morphea ‘the Chinese disease’ since it had only appeared with the arrival of the Chinese and their rice-growing. This crop was, as customary, grown in ditches irrigated constantly with water, where mosquitoes, also coming from abroad, found an excellent microhabitat.

Lutz and the theory of transmission of leprosy by mosquitoes

Lutz’s first speculation on the role of blood-sucking insects is found in ‘Estudos sobre lepra’ (Studies on leprosy), written in Limeira in 1885-86 and published at the time of his first stay at Unna’s clinic in Hamburg. The paper came out in a journal then edited by Unna, Von Hebra, and Lassar, called the *Monatshefte für Praktische Dermatologie* (1887), now *Dermatologische Wochenschrift*. This publication was then the most important international forum for clinical and laboratory experiments on skin diseases. In describing the primary lesions of nerve leprosy, which could be an “entryway to infection,” Lutz deemed it “remarkable that the first location of nerve leprosy occurs almost always in those parts of the body kept uncovered and exposed to insect bites and other traumatism” (Lutz, 1887, p. 24).

As to transmission of the disease, Lutz analyzed the inconsistencies in the theory of heritability and although he was already fascinated by Hansen’s bacillus, his stance differed from that taken up by proponents of the new microbial paradigm: “From my observation of the disease, I have no hesitation in stating that leprosy is less contagious than tuberculosis and in labeling the brusque expulsion of lepers from the heart of society ... as not only inhuman but also hardly efficient and, moreover, incoherent: and this because leprosy offers no greater

danger to another's life than does tuberculosis, nor are its perspectives for a cure any darker."

Infection of each new case depended upon the pre-existence of another, within a certain period of time, but "the conditions necessary for a new case to appear are so complex and singular that only rarely will they be met within the immediate vicinity of lepers."

Seeing leprosy with the eyes of a parasitologist, Lutz proposed an analogy with ancylostomiasis, topic of a study he published in Leipzig around the same time. Like leprosy, ancylostomiasis presupposed the existence of other cases but "someone who lives in a country where hookworm is found can contract the disease from muddy water without ever having come near someone suffering from it, while living in contact with the ill can be totally harmless, as long as rigorous cleanliness is observed regarding drinking water and wastes."

Lutz believed leprosy was a disease that was hard to transmit, "in which only very rarely [would] direct transmission be demonstrable and in which for this very reason infection by contact within the family household plays only a lesser role." In his opinion, congenital transmission played a "wholly insignificant" role and morbidity was maintained "chiefly by the sporadic occurrence of new cases within the heart of families spared until that point."

Once it had been learned how to distinguish leprosy from other, similar dermatological diseases, like mycosis, no new cases of infection were transmitted in unaffected countries, even when visited by a sufferer. This fact also argued against the idea of direct contagion: "I myself know of some ten lepers who have left for Germany in recent years."

To explain the peculiarities of indirect transmission, Lutz acknowledged hypothetically that the sufferer's blood or mucous secretions, containing the infectious agent, might require "a period of maturation at a lower temperature in order to develop communicability (for example, by means of spores or forms of resistance, or another stage in its evolutionary cycle), or perhaps ... exposed direct inoculation is also indispensable (for example, through biting insects)."

At the end of the paper written in Limeira and published in Germany in 1887, Lutz added the following observation: "Given the isolated situation in which I find myself, I have been obliged to completely relinquish any possibility of taking the existing bibliography into thorough account." He had not had access to Leloir's new book, "and only by chance [had he] received a review of the same, written by Unna." Lutz stated, "I see with satisfaction that many of our observations coincide, and I hope the reader will take as corroboration any involuntary repetitions."

Henry Leloir (1886) seems to have been one of the first to consider transmission of the leprous "virus" by mosquitoes. Although Edward Arning was author of a crucial experiment favoring the idea of leprosy by contagion, in 1891, he had — like Lutz — drawn a correlation

between the rather concomitant appearance of the disease and of mosquitoes on the Hawaiian Islands. Hallopeau, Chantemesse, Sommer, Leboeuf, Noc, Scott, Joly, Blanchard, and, a little later, the Colombians Juan de Dios Carrasquilla and Guillermo Muños Rivas were other names associated with the hypothesis that leprosy is transmitted “by arthropods, particularly acarines and insects, and above all mosquitoes.”¹⁹

Of those mentioned above, the name Raphael Blanchard is of special interest here. Physician and parasitologist, Blanchard seems to have been the ‘Manson’ of French tropical medicine.²⁰ He was a central figure in the network that linked zoologists and parasitologists from around the world, who were increasingly focused on medical topics (in this regards, see Sanjad, 2003, pp. 85-111; Caponi, 2003, pp. 113-49). Blanchard was a founder and secretary-general (1876-1900) of the Société Zoologique de France and, together with Alphonse Milne-Edwards, he organized the international zoology conferences that defined more precise rules for zoological nomenclature, the first, in 1889, and subsequently every three years. Blanchard chaired the Permanent International Commission on Zoological Nomenclature starting in 1898, the year in which the *Archives de Parasitologie* were created. In 1902, he founded the Institut de Médecine Coloniale, which provided training in parasitology for French and foreign physicians working in the so-called warm countries.

There was great repercussion when Blanchard voiced his opinion in the *Bulletin de l'Académie de Médecine* (1900) and the *Archives de Parasitologie* (1901) that leprosy could be transmitted by mosquitoes not only in hot countries, where it was endemic, but even in Paris, which should thus gird itself against these new enemies of public health. In 1905, Blanchard published *Les moustiques: Histoire naturelle et médicale*, one of the founding treatises of medical entomology. In Lutz's words (1939, p. 477), the work presented an “excellent summary of the arguments favoring culicidian transmission of leprosy.” In it, the French parasitologist commented on the mosquito's place in zoological classification, its morphology and anatomy, its habits and metamorphoses, and its genera and species. In chapter V, before presenting prevention measures against mosquitoes, he analyzed their proven role as carriers of malaria, yellow fever, and lymphatic filariasis, and its “presumed” role in transmitting other diseases — not just leprosy (pp. 543-5) but also scurvy, dengue fever, plague, hot-climate ulcer (caused by *Leishmania furunculosa*) and Kala-Azar, warts, moles, undulant fever, and an equine epizooty from South Africa.

The 2nd International Leprosy Congress, held in Bergen in 1909, approved a recommendation by the British delegation that the problem of leprosy transmission by insects should be elucidated (conclusion VI, cited by Souza Araújo, 1952, p. 1). A number of leprologists were already examining mosquitoes that had bitten sufferers of the disease, and in some they had found acid-fast bacilli. Others

were undertaking experiments with insect bites but had not come up with any convincing results. According to Adolpho Lutz (1939, p. 476), this was because the experiments were not “conducted using rigorous methods. ... In addition to other errors, the interval needed for the germ to incubate in the mosquito’s body was not taken into account.”

In a letter sent to Lutz in June 1905, Blanchard asked him what studies he had already published on the transmission of leprosy by mosquitoes; he apologized for his inquiry by explaining that “unfortunately, papers published in Brazil are not very accessible here.” Although Lutz’s stay in Hawaii had reinforced his conviction that the culicidian hypothesis was the most appropriate way of explaining the transmission of leprosy, since he was “unable to present positive proof” he had published nothing on the topic: “I merely ... , upon the occasion of a congress, requested that professor Unna, in my name, call leprologists’ attention to the matter” (cited in Souza Araújo, 1956, p. 130). The Brazilian zoologist and bacteriologist sent mosquitoes for Blanchard’s entomological collection, “which did not include, so to speak, any South American type.” He also sent him a brochure on yellow fever.²¹

During Adolpho Lutz’s time as head of the Instituto Bacteriológico de São Paulo (1893-1908), leprosy was the subject of epidemiological and laboratory studies but it took second stage to other, more burning questions in the realm of state public health, such as diphtheria, typhoid fever, cholera, amoebic and bacillary dysentery, bubonic plague, malaria, and yellow fever. In point of fact, Lutz gathered only scant results on leprosy, according to the reports he wrote during those years. In 1893, still as interim director, he repeated the experiments he had performed earlier at Unna’s laboratory in Hamburg and at Lazarus Hospital in Rio; these attempts to cultivate Hansen’s bacillus proved equally frustrating (see Lutz, 1895, pp. 207-8).

Leprosy was only mentioned again in his report on the year 1898, wherein Lutz highlighted events surrounding the consolidation of tropical medicine in England and other countries. The scientist hailed the inauguration of London’s school of tropical medicine and the forthcoming creation of another one, in Liverpool, as well as the launching of the *Journal of Tropical Medicine*, published in the British capital by James Coultie and W. L. Simpson. Lutz rejoiced over publication of two “excellent” treatises, one by Manson on *Tropical diseases* (1898) and another by Botto Scheube (1853-1923), entitled *Die krankheiten der warmen länder* (1898). He also made mention of a session dedicated to tropical diseases, inaugurated at the 66th meeting of the British Medical Association, held in Edinburgh in July of that year. Of special note among the papers presented there was Manson’s work on research by Ronald Ross, tending “to prove the theory that mosquitoes play an important role in spreading malaria.” Robert Koch had organized an expedition to several countries to investigate transmission of that disease, then being studied both by Lutz and his team in São Paulo and also by

Francisco Fajardo, Oswaldo Cruz, and some other bacteriologists in Rio de Janeiro.

The 1st International Leprosy Congress, held in Berlin in October 1897, was included by Lutz among events surrounding this establishment of tropical medicine and the strengthening of a medicine grounded on “precise observations” made possible through the natural sciences. Lutz lamented the fact that the São Paulo state government had not sent him or any other delegate to that congress. Disease transmission by haematophagous insects was the overriding idea that served as his touchstone in assessing the papers presented in Berlin, “generally of little import.” Hansen’s bacillus had come out strengthened while the theory of hereditary transmission of leprosy lost force, Lutz pointed out. The serum developed by the Colombian Juan de Dios Carrasquilla, already rejected in tests conducted at the Instituto Bacteriológico de São Paulo,²² found “rare supporters,” and yet Lutz still lamented that the assembly had “not more energetically condemned these absurd syllogisms and observations holding to no criteria” (Lutz, 1898, pp. 5-6). In his report, Lutz did not mention the transmission of leprosy by mosquitoes but he certainly had this in mind when he commented that in Berlin “the danger of contagion [was] somewhat exaggerated by those who have observed the illness less” (*ibid*, p. 5).

The summary of research conducted at the Instituto Bacteriológico de São Paulo from 1892 through 1906 condenses to a few paragraphs experiments dealing with leprosy. The disease had been the reason behind only three autopsies during that entire period, a number that contrasts with the many dozens performed because of the diseases mentioned above, epidemics then sweeping across the state. Although he was unable to cultivate the leprosy bacillus, Lutz saw no problem with laboratory exams meant to corroborate clinical exams: he performed twenty on soldiers from the Força Pública. Hansen’s bacillus was easily found in the “juice of tubercles and in ulcerations of the nasal mucous, as well as in the lymph glands corresponding to the affected region. Due to its shape and way of reacting to staining, it could only be confused with Koch’s bacillus, but such a mix-up would be almost impossible given the two illnesses’ differing symptomologies and also the grouping characteristic of Hansen’s bacilli” (Lutz, *Rev. Med. de SP*, 1907, p. 81).

This report is the only one that mentions studies on transmission of this germ by mosquitoes. Lutz had ascertained that the germ did not move into the insect’s stomach, even when the tubercles themselves were pricked. “The opposite must happen during periods of fever, when the bacilli are circulating in the blood, but there has been no opportunity to verify this fact” (*ibid*, p. 81).

In 1901-2, Adolpho Lutz arranged to repeat in São Paulo the experiments that a U. S. mission headed by Walter Reed had just completed in Cuba in an effort to prove Carlos Juan Finlay’s theory on

the transmission of yellow fever by *Stegomyia fasciata* (currently *Aedes aegypti*). The goal of both Lutz and Emilio Ribas, director of São Paulo's Sanitation Service, was to win over to their new prevention strategy physicians and lay people who believed in the miasmatic etiology and especially in the bacilli and fungi inculpated by Domingos Freire, Giuseppe Sanarelli, and other hunters of the yellow fever microbe. This 'testing ground' arranged by Lutz and Ribas helped clear the way for the campaign against *Stegomyia fasciata* led by Oswaldo Cruz in the city of Rio de Janeiro in 1903-5, as part of other sanitation and urbanistic measures aimed at "regenerating" and "civilizing" what was then Brazil's capital (Abreu, 1987; Benchimol, 1992).

After his 1908 move to the Instituto Oswaldo Cruz, Adolpho Lutz, who had been studying carriers of yellow fever and malaria for some time, resumed his research on haematophagous insects that might be able to host the leprosy microorganism. The disease was moving farther up on the agenda of sanitary concerns, and at Manguinhos Lutz found both the time and laboratory facilities needed for his return to a line of research that had been smothered by the burdensome routine of public health in São Paulo. Our study of primary sources has not yet allowed us to precisely chart Lutz's experimental course from 1908 till his death in 1940. All indications are that his results were inconclusive. Nevertheless, for the first time he publicly and with great emphasis upheld the theory of leprosy transmission by mosquitoes, and in this campaign brought to bear both his sparse experimental results and all the weight of his scientific authority.

The 1915 controversy and its outcomes

Leprosy stood among the diseases that had to be reported to authorities, along with such others as yellow fever, bubonic plague, cholera, small pox, and diphtheria, according to the *Regulamento Sanitário* (Sanitation Code) drawn up by Oswaldo Cruz after his 1903 appointment as director-general of Public Health. Despite the contagionist assumptions underlying this code, and perhaps thanks to Lutz's influence, the "specific instructions regarding each one of the illnesses that must be reported" considered that "mosquitoes and other human parasitic insects (fleas, bedbugs, etc.)" were to be suspected of "carrying and transmitting the leprosy bacillus."²³

In a report written at the peak of Rio de Janeiro's sanitation campaign, Oswaldo Cruz brought leprosy to the limelight: twenty-three cases had been reported in 1904, and the disease was raging through the city. The chronic nature of the illness made it unfeasible to intern victims at the isolation hospitals intended for people with acute infectious diseases. Therefore, the ill should be "sequestered" in "leper colonies" where sufferers would find, together with indispensable treatment, the elements needed for continuing with their activities, still quite useful." Oswaldo

Cruz (1905, p. 67) considered transforming the pesthouse on Ilha Grande into the first of these settlements, with “all the elements of comfort required in accord with the habits of the various social classes.”

The idea of quarantining victims on an island, as in Molokai — or as had been the case in Bom Jesus, right in Rio’s Guanabara Bay —²⁴ was put forward by three São Paulo physicians: Alberto Seabra and two of Adolpho Lutz’s assistants at the Instituto Bacteriológico, Ulysses Paranhos and Adolpho Lindenberg. At the 4th Brazilian Congress of Medicine and Surgery, held in that state during September 1907, their motion to that effect was approved.

In an interview published in *O Imparcial* on July 3, 1913, four years after leaving his post as head of public health, Oswaldo Cruz reintroduced the project to quarantine the afflicted in a farm settlement to be built on Ilha Grande, an island in Sepetiba Bay. His warnings about the danger of the spread of leprosy, “Death’s oldest daughter,” found echo in the federal Senate, where São Paulo’s representative, Francisco Glicério, sponsored a budget amendment allocating 170:000\$000 to the proposed leprosarium (Souza Araújo, 1956, p. 117).

In mid-1915, a commission was set up to study the “most terrible of the epidemics that have been developing in frightening fashion in recent times.”²⁵ This use of dramatic language is in perfect tune with Obregón’s description of the situation in Colombia during the same period. However, the similarity in discourse does not mean that leprosy acquired the same importance here as it did in that country when it came to the professional legitimization of physicians and sanitarians. According to Obregón (1996, pp. 172-3), when Colombian doctors joined the international movement to set up leprosariums, they exaggerated the magnitude of the disease and spread panic among the public and governmental authorities because they needed to convince them, first, that charitable institutions would be unable to handle the disease and, second, that it constituted a much more serious public health concern than imagined, controllable only by those with the necessary qualifications, that is, by physicians. The “medicalization” of leprosy was therefore a predominant dimension in the professionalization of Colombian medicine. Its leaders sought to forge a “national medicine,” focused on local illnesses — and from then on Colombia was viewed as one of the world’s major settings for leprosy.

Although associated with one particular disease, thereafter considered a great threat to urban populations, Brazil’s mobilization against leprosy reflected a deeper movement aimed at redirecting the country’s sanitation agenda towards so-called rural endemic diseases. Between 1917 and 1920, new legal and institutional policy bases were laid down (see Hochman, 1998). But what topped the list on Brazil’s public health agenda, and within the social and professional dynamics of the medical field, were hookworm, Chagas disease, malaria, and yellow fever.

Like leprosy, tuberculosis and syphilis left the orbit of philanthropy and of spontaneously born organizations of civil society, moving into the arena of public health policy.

When Oswaldo Cruz passed away on February 11, 1917, the institute christened in his honor was the center of gravity for a combative group of physicians who were calling for the modernization of Brazil's sanitation services. The key leaders were Carlos Chagas, Oswaldo Cruz's successor as head of Manguinhos (from 1918 until his death in 1934), and the indefatigable Belisário Pena, author of vehement articles and of *Saneamento do Brasil*, a book that was to mark an era in Brazilian public health (Lima and Britto, 1996; Lima, 1999; Britto, 1995).

The Liga Pró-Saneamento (Pro-Sanitation League), inaugurated at the headquarters of Brazil's National Agricultural Society on February 11, 1918 (first anniversary of Oswaldo Cruz's death), rallied a large number of physicians and intellectuals around its banners: eradication of the endemic diseases that hampered the nation's development and that demanded a centralized, 'scientific' sanitation policy capable of overcoming the roadblocks created by state autonomy and able to safeguard health activities from the clientelistic influence of local potentates.

The oligarchic bloc in power gave in to some of these demands. On May 1, 1918, lame-duck president Wenceslau Brás signed into law a decree that created the Rural Prophylaxis Service, and granted its head, Belisário Pena, one thousand *contos* to establish more health posts in the Federal District.

When the Spanish flu invaded Brazil late that year, it aggravated the already troublesome effects of the disagreements between oligarchies common during times of presidential transfer of power. The winning candidate, Rodrigues Alves, fell victim to the flu before taking office. Epiácio Pessoa was then elected, from Paraíba, a state lying outside the region of the so-called *café com leite* (coffee with milk) pact, formed by São Paulo, Rio de Janeiro, and Minas Gerais. This favored calls for greater power to the national public health sector to the detriment of the states' autonomy. On November 22, 1919, Epiácio Pessoa sent the federal legislature a message proposing the reform of health services: "Whether a new ministry is created or the current organization is maintained, what is indispensable is to expand the sphere of our sanitary defense" (cited in Hochman, 1998, p. 23). In January 1920, the National Public Health Department was created, still under the auspices of the government's most political ministry, the Ministry of Justice and the Interior. Chagas's appointment as director (a post he held until 1926) reestablished the Instituto Oswaldo Cruz's umbilical link with a more autonomous and better-equipped public health service.

Created together with the Rural Prophylaxis Service, on May 1, 1918, the Official Medications Service fortified Manguinhos' industrial side. The main object of this service (also set up at São Paulo's Instituto

Butantã) was to prepare and distribute quinine. In addition to developing this malaria-prevention medicine and also tartar emetic, used in treating leishmaniasis, the Instituto Oswaldo Cruz's applied chemistry division developed other "official medications": in 1921, "sorosol," for syphilis, and in 1924, sodium salts in gelatin capsules and esters made from chaulmoogra oil to treat leprosy. Analysis of vegetable oils from Brazilian botanical species led to the preparation of oil of the *charpotcochis* plant (*Carpotroche brasiliensis*), used by Souza Araújo to treat the same disease.

Regarding this 'rural' endemic disease, viewed perhaps as the greatest health threat to urban centers, Oswaldo Cruz's July 3, 1913, interview to the *O Imparcial* ignited a movement that came to encompass all of Rio's medical societies. The Bahian physicians Belmiro Valverde and Juliano Moreira proposed to the Rio de Janeiro Medical-Surgical Association that a Leprosy Prophylaxis Commission be organized. The Rio association nominated Paulo da Silva Araújo and Henrique de Beaurepaire Rohan Aragão to be part of it as well. The National Academy of Medicine nominated Emilio Gomes, Alfredo Porto, and Henrique Autran. Other members of the commission included Eduardo Rabello, Werneck Machado, and Guedes de Mello, from the Society for Medicine and Surgery; Sampaio Vianna, Silva Araújo Filho, and Oscar D'Utra e Silva, from the Medical Society of Hospitals; and Fernando Terra, Juliano Moreira, and Adolpho Lutz, from the Brazilian Society of Dermatology. Carlos Pinto Seidl, director-general of Public Health, was chosen to head the commission, and he appointed moderators for the subgroups that were to analyze specific aspects of leprosy in Brazil.

The commission's work ran from 1915 through 1919 and yielded a number of reports and public statements, compiled and transcribed in whole or part by Souza Araújo (1956). These included Silva Araújo and Valverde's observations on leprosy and marriage; Werneck Machado and Emilio Gomes' on leprosy and occupation; and Adolpho Lutz and Henrique Aragão's on leprosy and immigration. Eduardo Rabello and Silva Araújo Filho studied the disease's relation to domicile, and Juliano Moreira and Fernando Terra, its relation to isolation.

A survey of epidemiological data was to provide the basis for the prevention offensive. Despite the dramatic adjectives used to describe the spread of this disease, Oswaldo Cruz himself admitted in his 1913 interview that Brazil's public health officials did not really know how many sufferers there were in the federal capital and the rest of the country.

The debates leading up to the formulation of government guidelines in the fight against leprosy, starting in the 1920s, rekindled the old controversy between heredity and contagion. The inarguable hegemony of the latter proponents was challenged by Adolpho Lutz, leader of a third line of thought which seems to have enjoyed greater visibility in Brazil than elsewhere. Although moderator of one specific topic, Lutz's prestige as a scientist and leprologist assured him the privilege

of espousing his views at a conference given on November 5, 1915. His lecture was attended by many physicians and medical students, and also by the Minister of Justice and the Interior, Dr. Carlos Maximiliano, honorary chairman of the Leprosy Prophylaxis Commission.

The *Jornal do Commercio* (Nov. 7, 1915) transcribed the conference in its entirety (partially transcribed in Souza Araújo, 1956, pp. 124-7) and summed up the orator's position in these words: "through exclusion of other blood-sucking insects as possible carriers of leprosy, the mosquito should be identified as the sole cause of transmission of this illness (whether *Culex fatigans* or *Stegomyia fasciata*), when it sucks in, and only in this case, the blood of lepers during febrile stages of bacillemia."

Aware of his authority, Adolpho Lutz attributed the endless disputes on the transmission of leprosy to "preconceived ideas" and to a "flawed knowledge of the literature and of the disease itself, which is the rule and not the exception among the medical classes in all countries." Brazilian physicians were not familiar with the book that Lutz considered a "veritable bible," the *Handbuch der Historisch-Geographischen Pathologie*, written by August Hirsch (1817-94), with "German patience."²⁶ They were also unfamiliar with studies released during the previous 35 years (since he had begun studying leprosy) in the *Monatshefte für Praktische Dermatologie* and in similar periodicals on skin diseases. These readings would have allowed them to observe leprosy with their "own eyes" — a remark that carries between its lines a criticism of Brazilian physicians' dependence on Francophone authors.

Lutz believed the dichotomy between heritability and contagion was false. Leprosy was indeed more common in certain families but this did not mean it was hereditary "because if that were the case, descendants could not fall ill before their ancestors, as is extremely common." The theory did not explain how multiple cases occurred in families where older generations had not acquired the disease, either because they had emigrated from unaffected regions or because leprosy did not yet exist in the place where their children and grandchildren would come to fall ill.

In Lutz's opinion, the notion that leprosy was contagious had gained ground in Europe during a time when the disease was common and tending to spread. When it had become rare, with a prevalence only of imported cases, the theory of heritability became the most plausible way to account for the victims that were to be found in but a few families.

But it is *only under these circumstances* that someone could suggest that the illness endured solely by heritability. In other countries where the illness is common, and where there is at the same time much immigration from unaffected countries, this statement is wholly *incomprehensible*. I have for certain seen over one hundred people from unaffected places, already adult, who caught the illness

in Brazil or in another place where it is endemic, and this also proves that nationality does not indicate a predisposition, because a great number of representatives of unaffected countries fall ill (*Jorn.Comm.*, Nov. 11, 1915).

But advocates of the theory of heritability were not Adolpho Lutz's greatest opponents; rather, it was the proponents of the theory of contagion, understood here as direct transmission of the disease from one person to another. The latter invariably pinned their certainties on a historic argument: the longevity of the disease in Europe and its ebbing or virtual disappearance thanks to the isolation of sufferers in leprosariums. For Lutz, it was a mistake to assume that all of the ill had been isolated. Many cases must have gone unnoticed, given the characteristics of the disease — its slow evolution, sores that were hard to identify with precision — or because of family efforts to hide the ill.

Lutz did not question the fact that leprosy could be communicable under certain conditions, which included, necessarily, the pre-existence of other cases, but this did not prove direct contagion. He pointed out several anomalies in the theory, many of which had already been raised by those defending the theory of heritability. Individuals fell ill without having had contact with sufferers. The incubation period was sometimes short, other times lengthy. Europeans returned to their homes with leprosy picked up abroad and were interned in public hospitals, yet they did not produce foci of the disease. Paris, Vienna, and other Old World capitals remained unaffected.

To counter his adversaries, Lutz cited the repeated unsuccessful attempts to transmit Hansen's bacillus to people and to animals and also the problems in obtaining pure cultures of the microorganism. In conjunction with the infection's erratic character, such anomalies made leprosy very different from contagious diseases like tuberculosis and syphilis, and impugned the analogy with other infectious processes, like yellow fever first and foremost and also exanthemic typhus, malaria, and ancylostomiasis, where the appearance of one case depended upon the earlier appearance of another although the disease could be caught without any direct contact between victims. Studies of the bubonic plague that linked it to the fleas carried by rats had negated the supposition that diseases caused by bacteria could have nothing to do with transmission by blood-suckers.

The epidemiological characteristics of leprosy, however, made it necessary to exclude such ubiquitous species as fleas and bedbugs, mange-causing acarines, and other insects common in big cities. "Thus we are left with haematophagous Diptera," Lutz concluded.²⁷ His experience in Hawaii and his knowledge of the entomological literature equipped him to close the circle on two groups: *Culex* and, to a lesser degree, *Stegomyia*.

At the November 1915 conference, Lutz explained that the first Europeans to set foot on Hawaiian soil were some Spaniards who had been shipwrecked there in 1749. The seamen who rediscovered the archipelago in 1778, led by English navigator James Cook, christened it the Sandwich Islands, and introduced the natives to syphilis and gonorrhoea. The first cases of leprosy appeared only after 1840, at first only in very small numbers, and Lutz supposed the endemic disease had originated from a single case. It spread so much that by 1889 nearly 5% of the native population had been struck and 2.5% had already been isolated. A much smaller proportion of foreigners had fallen sick, that is, about five out of every thousand.²⁸

In the early 1820s, Lutz stated, there were as yet no mosquitoes in Hawaii. He believed that *Culex fatigans* had been introduced in 1828, “or earlier, by a ship that ran aground on the beach.” He believed *Stegomyia fasciata* had arrived later. When Lutz was on the archipelago as head of the Molokai Settlement’s medical service, only those two species existed, and they had become “extremely abundant,” partly owing to extensive aquatic plantings of taro and rice. Lutz suspected that the main carriers of leprosy were the *Culex*, that is, both the *fatigans* that existed in Hawaii as well as similar species found in cold-climate countries. The role of the *Stegomyia* was “more uncertain.” Phlebotomous flies (sandflies), maruins and mosquitos-pólvora (both members of the genus *Culicoides*), and mutuca flies — of which there were none in Hawaii — must play a secondary role in the transmission of leprosy.

“Applauded at length,” Adolpho Lutz’s conference fueled heated debates during at least two more sessions of the Leprosy Prophylaxis Commission, and stories of these circulated in the press. Among his stalwart supporters were Henrique Aragão, also from the Instituto Oswaldo Cruz, and Emilio Gomes, a long-time bacteriologist in Rio’s public health sector. Although no one failed to sing the praises of Lutz’s wisdom, he also had firm adversaries, especially Belmiro Valverde and the director of Rio’s Lazarus Hospital, Fernando Terra. The other members of the commission had no trouble reconciling certain of Lutz’s proposals with the strongly segregationist prevention recommendations inspired by a belief in leprosy’s nearly unlimited contagion.

Not a single physician came to the defense of hereditary transmission. Nevertheless, reinterpreted from the perspective of eugenics as a “predisposition,” the notion of heritability was subsumed within the contagionist program as a factor in certain specific cases of transmission.

The debate really caught fire on December 3, when Lutz presented an ‘Appendix ... to leprosy prophylaxis’ (in Port.) and Belmiro Valverde gave a paper entitled ‘Communicability of leprosy’ (in Port.; *Jorn. Comm.*, Dec. 6, 1915; *Diario Official*, Dec. 15, 1915, pp. 901-6).

The contagionists — of whom Valverde was principle spokesman — hurled against Lutz the plentiful case histories used earlier against hereditarians by Hildebrand, Kalindero, Taché, and others. They had no hesitations about retelling stories set in far-off places, veritable myths like the tale of the European lad who was playing with a small leper in Borneo and, when he saw him prick his anaesthetized skin with a knife, without feeling anything, mimicked the gesture and fell ill. Or the story of the leprous mother, with lesions on her breast, who had transmitted the disease to her son (on his face) while nursing him. Or the story of the porter who had caught leprosy when he injured his shoulder blade while carrying a leper's cadaver. The renowned bacteriologist Victor Babes, one of the experts whose name was invoked by Valverde, attributed the paucity of cases among doctors and nurses to the measures they took to avoid contagion, unquestionable even though less blatant than in the case of other diseases. The most famous victim had been Joseph de Veuster, or Father Damien, a member of the Belgian Order of Picpus, who had traveled to Oceania in 1863 to care for the lepers and who had died of the disease in Molokai on April 15, 1889 (see Obregón, 2002; Huenermann, 1953).

The authors cited by Lutz's adversaries laid heavy stress on cases transmitted arm to arm, by small-pox vaccination. This was in fact the hypothesis underlying Arning's experiment with the prisoner Keanu. In the opinion of Scheube, a German physician praised by Lutz earlier in these pages, the small-pox vaccine had played a considerable role in spreading leprosy on the Hawaiian archipelago. According to Babes, it did not attack the inhabitants of the British Indies who refused the vaccine.

A third set of evidence had to do with transmission by fomites, especially infected clothing. Valverde told of cases observed by himself in Amazonas, by Ross in India, and also by Manson, Babes, Scheube, Hansen, Lorand, and Looft, cases that made it evident that washerwomen were especially susceptible to leprosy.

All these case histories, which Lutz called "of little value," brought into the open disagreements about diagnostics and, above all, about the role of lesions in spreading the disease. For contagionists, the prime way in which infectious bacilli spread was via secretions from ulcers and nasal mucous, with the nose being the site of the first leprous lesions — and this gave new life to terrifying old ideas about contagion via the air.

For Lutz, the great number of bacilli that were eliminated through the mucous membranes and ulcerated skin lost their strength once in contact with the environment. "If they were all alive and capable of direct infection, the causes of contagion would be most numerous, which is not the case. They should also be observed all over." Calling into question his adversaries' clinical experience, he stated that leprosy generally began:

with a hyperemic spot, more or less infiltrated. ... I have seen a regular number of such cases, which are completely unknown to most doctors, ... on the back of the foot or the hand or on the face, usually on the forehead, which is a very common place. In such cases, the nasal mucous is usually not affected yet, nor is anything else in the mucous glands affected, which would be the case if the illness spread like syphilis. On the other hand, these places match entirely with what would be expected in the case of transmission by mosquitoes, something that has always struck me, right from the beginning of my studies on the topic (*Jorn. Comm.*, Dec. 6, 1915).

According to Lutz's principal champion in this controversy, Henrique Aragão (cited in Souza Araújo, 1956, pp. 137-41), it had been shown through some one hundred experimental inoculations that the germs in the nodules, ulcerations, and other spots on the body had no ability to infect. On the other hand, a number of authors, not necessarily identified with transmission by insects, had recognized that bouts of fever played a role in spreading the disease. The microorganisms that appeared in the sick person's blood during these periods had been proven to be virulent. Thus, haematophagous insects could readily become infected during such bouts, when bacillemia was easily demonstrable through Beurmann and Gougerot's process. The sufferer was thus in a position akin to a yellow-fever sufferer on the days when he could infect the *Stegomyia fasciata*, or akin to a victim of the plague, during the septicemic stage, when the flea could ingest Yersin's bacillus.²⁹

Contagionists rightfully denounced the lack of experimental data to prove the role of the mosquito as a carrier of Hansen's bacillus, a criticism that neither Lutz nor Aragão could refute.

In Aragão's opinion, it was a "widely demonstrated" fact that bacilli could be found in the digestive tubes of haematophagous insects that had bitten lepers. Cardoso Fontes, another researcher at Manguinhos, and Emilio Gomes had found acid-fast bacilli quite like leprosy bacilli in the digestive tubes of mosquitoes caught in Lazarus Hospital rooms (cited in Souza Araújo, p. 138). But Adolpho Lutz admitted that this was unusual and that multiplication of these microorganisms within the mosquitoes was not yet a proven fact. Lutz and Aragão blamed the failure of many researchers on their attempts to infect mosquitoes by making them prick leprosy nodules and patients when they were not febrile. Lutz stated:

I have, in earlier days, had occasion to verify that generally when mosquitoes bite leprosy tubercles, they do not ingest bacilli but they cannot fail to do so when they bite febrile individuals, with bacilli in the blood. Usable cases are quite rare, and experiments with *Culex fatigans*, which only bites in freedom, are difficult. Only a small proportion of mosquitoes are probably infected and of these only a small fraction, perhaps, ever transmits the bacilli. If this were not the

case, infection would be much more common, and demonstrating it, easier (*Jorn. Comm.*, Nov. 7, 1915).

The argumentation presented by Lutz and Aragão — like Rochard's, fifteen years earlier — was founded above all on epidemiological aspects of leprosy, and they in fact transformed anomalies observed in the laboratory into facts consonant with the spreading of the disease:

Transmission by the bite of a contaminated mosquito cannot be common. Yet this is precisely one of the necessary conditions, because if this were not the case, we would have serious epidemics. ... It would thus be necessary to examine thousands of mosquitoes to find the one with the power to infect (*Jorn. Comm.*, Dec. 6, 1915, cited in Souza Araújo, 1956, p. 130).

To judge from the contagionists' papers, few investigators had obtained experimental evidence contradicting transmission by mosquitoes: their results either were not conclusive or did not exclude the role of other blood-suckers. Valverde made mention of a Danish commission comprising Ehlers, With, Verdier, and Bourret that had studied the transmission of leprosy in the Antilles and that had concluded that Hansen's bacilli were found only rarely in the mosquito's digestive tube. Valverde also cited research by John Lindsay, conducted on the border between Brazil and Paraguay, where leprosy was supposedly more infectious than pulmonary tuberculosis, something the English physician attributed to unhealthy homes. Lindsay had found a large number of bedbugs in the beds and on the walls there, and he presumed they played just as important a role in transmitting leprosy as crowded, stuffy, dark housing.

Although an unwavering contagionist, Valverde allowed that insects could be involved, particularly flies, which were proven carriers of the tubercle bacillus, quite similar to Hansen's. He even cited experiments by Marchoux, who had infected rats by exposing them to flies that had fed on the soft mass of leprous tubercles.

Adolpho Lutz, who had at first discarded ubiquitous insects, altered his position at the second conference: "There is no reason not to include other haematophagous Diptera among carriers of leprosy, but there are formal indications towards mosquitoes."³⁰

As we have seen, *Culex fatigans* and *pipiens* were the principle mosquitoes inculpated by Lutz, who did not exclude *Stegomyia* and other domestic species. "As to Simuliidae, Phlebotomous flies, *mosquitoes-pólvora* [genus *Culicoides*], it can only be said that they cannot be the only carriers" (ibid.). During the debates, Lutz acknowledged that Simuliidae could account for the occurrence of leprosy in places where there were supposedly no Culicidae, like the Alps, Norway, and Ireland — often the settings of contagionist case histories — but he warned that these blood-suckers, "very common in

mountainous regions but unknown in most large cities ... , can only be of local import.”³¹

The geographic distribution of mosquitoes was the hottest topic of this controversy with the contagionists. Lutz and his allies argued that the “capricious” way in which leprosy spread and the absence of epidemics or endemicity in regions visited by victims made the involvement of mosquitoes undeniable. For the contagionists, an analogic rhetorical role was played by the argument that the mosquitoes indicated by Adolpho Lutz did not exist in regions where leprosy was endemic.

In this area, Lutz was at an advantage. He was an entomologist of renowned skill, highly respected even by the few foreign experts that the contagionists relied on. But since medical entomology was still a young discipline (it had been around for barely two decades), the comparative study of the distribution of leprosy and of mosquitoes yielded inexact results. As competent as Lutz may have been, there would never be a perfect overlapping of the two geographic maps. Beyond this, or perhaps because of this, the multiplicity of hypothetical hosts for Hansen’s bacillus in different regions of the globe was another discomfiting factor in Lutz’s theory. To uphold his theory, Adolpho Lutz, unwavering defender of the hard and fast facts obtained in a laboratory, the man who peppered his speech with “precisely’s”, had no choice but to use arguments of power in order to decide the dispute in his favor.

Valverde (cited in Souza Araújo, 1956, pp. 132-4) admitted he was wholly unfamiliar with medical entomology and “leafed through the masters” to see if they supported Lutz’s theory. He consulted Giles and especially Frederick Vincent Theobald’s monograph on Culicidae, or mosquitoes, and verified that there were no *Culex fatigans* in Europe but only certain *Culex* from cold countries and *Stegomyia fasciata*. Regarding North Africa, Valverde found only the description of *Culex pipiens* and *maculiventris* in Algeria and *Culex pusillus* in Egypt. “Not a single word about *Stegomyia fasciata*! So how then does one explain the transmission of leprosy in these countries? ... Is this yet another peculiarity of leprosy ... , that it is the only disease spread by an enormous variety of mosquitoes?”

It was even harder to fit Asia into Lutz’s theory. In the central part of the continent, “there is not a single variety of *Culex*, nor of *Stegomyia*, and Theobald questioned ... the existence of *Culex cuspius*!”

In New Zealand, there were *Culex albirostris*, *pervigilans*, *aussoralis*, and *iracundus* but no *fatigans* or *Stegomyia fasciata*. On Madeira Island, a long-time focus of leprosy, only *Culex longiareolatus* had been found.

The physician from Bahia pointed to certain facts that contradicted Lutz’s theory in Brazil as well. Amazonas was the state with the greatest infestation of mosquitoes but it was also among the states with a low

rate of leprosy, and “in no way was it possible to make comparisons [...with] São Paulo and Minas, Brazil’s two main foci, where there were infinitely fewer mosquitoes.”

In this arena, the polemic with the contagionists was championed mainly by Aragão, who criticized both Valverde’s scant knowledge of the authors he had cited and the fact that he had not turned to other, equally valuable sources:

When Theobald does not cite the existence of a given mosquito in a certain locale, it does not mean it did not exist there, and from this one can deduce no more than that they have not yet been collected in these places or they are cited in other works, unknown to Theobald at the time he drew up his work. ... Moreover, in Theobald himself ... one finds references to the existence of *Stegomyia* and *Culex fatigans* in spots where its presence was denied ... as, for instance, in Northern Africa, Egypt, in various parts of China, etc. Denying that *Stegomyia* exists in Africa is absurd, inasmuch as this continent is the cradle of this species that later became cosmopolitan. ... What is also found in Theobald, and should be commented here, is that this notable, highly skilled specialist in mosquitoes has such great regard and esteem for Dr. Lutz that in his book he adopted our countryman’s classification in its entirety (cited in Souza Araújo, 1956, p. 140).³²

Advocates of the culicidian theory persistently reiterated analogies with yellow fever, not only comparing the means of transmission but also the styles of thinking and conduct displayed by adversaries of Finlay’s and Lutz’s theory. The 1915 debate was at first marked by the same adamancy that had characterized the 1903 confrontation between those who contended yellow fever was transmitted solely by *Stegomyia fasciata* and those who were “unconvinced,” acknowledging the inclusion of the mosquito in a vaster network of pathways involving direct contagion by fomites.³³ Although he still stuck firmly to his ideas on the transmission of leprosy, Lutz chose not to enter into rivalry with the contagionists within the practical realm of prophylaxis. Beyond the fact that he didn’t carry the same weight as Oswaldo Cruz at the beginning of the century, the man was now his adversary — a silent yet highly influential one.

In their report on ‘Lepra e imigração’ (cited in Souza Araújo, 1956, pp. 151-2), Lutz and Aragão proposed an accommodating approach that stood in contrast with the strict prevention measures in force in New York and other U.S. ports, measures that Brazilian sanitarians wanted to see enforced in the case of immigrants arriving in Brazil. The two scientists from Manguinhos believed there was only a minute danger of importing new cases of leprosy; it would be a bit ‘like taking owls to Athens’, since Brazil afforded better conditions for spreading the disease than the countries from which most immigrants came. The article states, “as far as leprosy, our relations with foreigners offer as much or greater danger to them as to us, and any irksome measure

would of course compel reprisals, in addition to leaving very unfavorable impressions which it would be better to avoid" (ibid, p. 151).

In the case of subsidized immigration, the government was responsible for excluding people with "defective" physical and psychological health backgrounds but without this entailing the adoption of "irksome measures." Before granting a free passage, the government could demand that the immigrant produce a certificate issued by a physician or authority from his or her place of origin, or by the physician on board, or even a "formal declaration by the head of household," but prior to disembarkation, since in normal times the government could not force passengers to submit to a rigorous physical exam.

It would also not be fitting for companies to repatriate cases that only at the end of the trip are recognized. The leper thus risks the hazard of becoming a kind of errant Jew, and it would be well to consider what should be done with the ill under these circumstances. The simplest would be to allow them to board certain steamers, where there would be a doctor, but a small number of passengers, and during the trip they would occupy a small isolation hospital, protected by a wire screen, and after their arrival they would be subject to the decisions of the local sanitation authorities. If some concessions are not made for such cases, they will always try to hide their illness.

Lutz and Aragão also addressed the question of immigration by land. Brazil's neighboring countries offered no great peril because, except for Colombia, the number of sufferers was believed not to be greater than in Brazil. It would be enough for the travelers to present a certificate to the public health authorities and to customs. The same rule should be applied to those "countrymen who want to move from one place to another, because their number must be many times greater." This certificate would have "a certain moral effect and make it possible to hold responsible those deliberately making false statements" (ibid, p. 152).

We have already seen how Adolpho Lutz felt it inefficacious and cruel to isolate victims of leprosy. During the debates, he asked his adversaries why they didn't call for equally stringent measures for sequestering the victims of tuberculosis or other diseases transmitted by means of the processes erroneously attributed to leprosy (Souza Araújo, 1956, pp. 130-1). In *The Microphysics of Power*, Foucault (1984, pp. 88-9) makes a thought-provoking distinction between the two major organizational models that held sway in Western sanitation through the close of the nineteenth century: one model, inspired by leprosy and extended to embrace madmen and criminals, called for excluding these individuals from common spaces, in the name of purifying them; under the other, applied to the plague and other contagious diseases, individuals should be interned or settled in hospitals or other niches of urban space that could be scrutinized and where the individuals

could be effectively watched. The policy that Oswaldo Cruz proposed in 1907 for carriers of the tubercle bacillus — when he felt the campaign against yellow fever had been victorious — was a Draconian version of the second model, rejected by the government (see Benchimol, 1990, pp. 49-50; Nascimento, 1999; Bertolli Filho, 2001).

Although Adolpho Lutz proposed a third organizational model for leprosy, associated with the late-nineteenth century appearance of intermediary hosts within the web formed by people, things, and microbes, he pragmatically admitted that isolation would hamper the spread of the disease if leprosariums were located well away from other housing, in environments not favorable to mosquitoes.

There is no avoiding the obligation of joining prevention measures against mosquitoes with every attempt at isolation, because individuals' freedom should not be sacrificed without a maximum guarantee that this sacrifice will yield practical results. ... I leave the dubious satisfaction of combating the new guideline to that class which endeavors to keep yellow fever among us, fighting prevention measures against mosquitoes, and who would like to repeat this opposition in regard to another, no less important question (cited in Souza Araújo, 1956, p. 130).

Expressing the opinion of most members of the Leprosy Prophylaxis Commission, Juliano Moreira and Paulo da Silva Araújo proposed that the commission sponsor the experiments needed to confirm Lutz's affirmations, and that it include protection against mosquitoes among planned prevention measures but not without urgently putting into practice the "universally adopted" ideas of direct contagion, mandatory reporting, disinfection, and isolation or exclusion of the ill (*ibid.*, p. 128).

The commission's conclusions, meant to serve as the basis for a draft law,³⁴ consisted of eleven items cast in the spirit of the contagionist program approved by the international congresses held in Berlin (1897) and Bergen (1909). The only exception was item IV, which defined the need "to undertake culicidian prophylaxis, that is, a set of efficient measures against mosquitoes able to transmit leprosy, with all due rigor, in cases of individuals confined to their homes or in leper colonies and asylums or isolated in villages and colonies."

The American Leprosy Conference, which took place in Rio de Janeiro in October 1922 and was chaired by Carlos Chagas, seems to have held to this orientation, but Lutz (1921) no longer found the support he enjoyed in 1915.³⁵ Still lacking conclusive experimental evidence, he was to come up against a greater number of adversaries, Belmiro Valverde still heading the list. The latter stated:

A number of agents have been inculcated as carriers of leprosy — mites, bedbugs, fleas, flies, mosquitoes, etc. ... Proponents of direct contagion acknowledge that ectoparasites might mechanically

transport leprosy germs, as occurs with other diseases and as simple good sense would indicate. However, those who suppose that leprosy is transmitted by indirect link are dogmatic, ... as is the case among us of Dr. Adolpho Lutz, who, although he has addressed himself to these matters for forty years, still cannot present the slightest documentation showing the correctness of his ideas. Even now, in the middle of the Leprosy Conference, when some paper was to be expected from Dr. Lutz ... in support of his theory, already defeated by critics, the illustrious sage has limited himself to reaffirming, without any evidence, without a single fact, without a single new word, that the mosquito is the carrier of leprosy, although in his assertions one no longer feels that same primitive enthusiasm as in 1915, when the great scientist made public, in much detail and with wide circulation, the ripened fruit of his reasoning (*Jorn. Comm.*, Oct. 1922).

Lutz is not alone: haematophagous insects as carriers of leprosy (1920-50)

These criticisms were founded. Still, Adolpho Lutz stuck firmly to his theory, bolstering it with arguments not much unlike those used in 1915, at the 2nd American Congress of Dermatology and Syphilology held in October 1921 in Montevideo Uruguay, at the same time that Brazil's newly created National Department of Public Health kicked off its segregationist offensive against leprosy's victims through its *Inspetoria de Profilaxia da Leprosia e das Doenças Venéreas*, an oversight agency devoted to leprosy and venereal disease prophylaxis.

In 1936, Lutz published an overview (in German, Portuguese, and English) of the literature on leprosy transmission, reviewed in French and Italian medical journals.³⁶ Letters of support for his ideas came from around the world, for instance, from Jesus M. Gomes, physician in Guindolim, a town in Goa, and from Dr. Peskcowsky, director of the Krasnodar Experimental Leprosy Colony and Clinic, in the Soviet Union, who was responsible for epidemiological research into the disease in the area just east of the Azov and Black seas. Lutz sent a paper entitled "No control of leprosy without anti-mosquito campaign" to the International Congress in Cairo (Mar. 21-28, 1938).³⁷ The disease was also the topic of the scientist's final two papers, dictated to his niece since he was already completely blind. 'A transmissão da lepra pelos mosquitos e a sua profilaxia,' read at the 7th Congress of the Pan American Medical Association in 1938, was published in *Memórias do Instituto Oswaldo Cruz* in November of the following year, while 'Regras indispensáveis de profilaxia anticulicidiana sugeridas ao Serviço Sanitário do Estado de S. Paulo' (Indispensable rules for anti-culicidian prophylaxis, suggested to the São Paulo State Sanitary Service) remained unpublished.³⁸

Lutz's prevention recommendations, especially in his first article (1939), were now much more detailed and aimed primarily at "those who do not acknowledge the mosquito as the only means of transmission of leprosy." The latter category may have included not only less unbending contagionists as well as physicians and researchers who considered that other vectors might be involved, such as the haematophagous fly *Musca sorbens* Wiedemann (Lamborn, 1937).

He recommended that there should be at least one person in every leprosarium undertaking constant prevention measures against mosquitoes. Furthermore, larger settlements should keep on staff an entomologist or physician qualified to raise the larvae found in infirmaries and lodgings, determine their species, and describe them in periodical reports to the institution. In regions where leprosy was found, a complete study of the local fauna of haematophagous Diptera and insects was indispensable in orienting anti-culicidian prophylaxis, which was "always useful, dispensing the need for justification."

Patients' medical histories should include information on their contact with mosquitoes in the places where they probably caught the infection. Those with fever or whose disease was progressing rapidly should be isolated in screened infirmaries. The patients' lodgings should also have screens, and an effort should be made to eliminate dark corners, dark painting, and other "hiding places" for mosquitoes. Even though domestic species were the most likely suspects, leprosariums should be built where there were no infestations of marshland or wildland species.

In his article published in November 1939, Lutz also described in unprecedented detail the experiments meant to prove his theory:

The common nocturnal mosquito, *Culex quinquefasciatus*, must especially be suspected of transmitting leprosy but it does not lend itself easily to experiments because it only bites in the dark. It is best not to use *Stegomyia* for a variety of reasons. It is better to use species that bite readily ... , for example, species from the genera *Mansonia*, *Taeniorhynchus*, and *Ianthinosoma*. The easiest to obtain is *Culex*, today *Ochlerotatus scapularis*, plentiful in tree-filled gardens.

The mosquitoes "should be" (or were)³⁹ infected with a variety of microorganism species, not just "of the genus *Coccothrix* (1886), a name that has precedence over *Mycobacterium*," but also of different "strains" of the tubercle bacillus — especially those associated with bovine or avian tuberculosis⁴⁰ — and Stefansky's bacillus, which in rats produced an "illness bearing similarities to leprosy."

Lutz recommended using culture from these microorganisms, mixed with fresh defibrinated blood or diluted honey, to infect mosquitoes. The insects could also suck the germs directly from people or animals carrying the disease, but this method did not yield good results. In this case it would be better to use people or animals displaying recent and

rapidly progressing pathological processes, with fever and the subsequent circulation of bacilli in their blood.

“Many years ago I performed some experiments, applying mosquitoes to leprous tubercles, which are always full of acid-fast bacilli and usually form masses in zooglea. In my experiments, acid-fast bacilli were not found in mosquitoes. However, other observers seem to have been more successful. Today I attach little importance to these negative results because I believe that while acid-fast forms are convenient for diagnostics, they represent later, not very active stages.”⁴¹

For Lutz, the first question to resolve after infection of the mosquito was how long the bacilli remained alive in its body. Infected specimens should be kept alive for some time, “preferably completely in the dark,” to give the germ time to incubate. If it did not disappear from the internal organs quickly, these mosquitoes could be used in the inoculation of animals and in cultures attempted in succession. In animals, the salivary glands and the body should be inoculated in an effort to produce a lesion: guinea pigs and rabbits were susceptible to various forms of tuberculosis; rats, to Stefansky’s bacillus; and monkeys, to human leprosy.

Experiment with bites, which may be repeated, or by means of inoculations using mosquitoes ground up in a little liquid. They can be washed in alcohol and lightly singed to disinfect the external parts. This same process can also be used to inoculate appropriate nutritive media. It would be good to repeat these experiments as often as possible in hopes of obtaining one or two positive results. It will suffice to obtain positive results with only one of these germs in order to demonstrate the possibility of mosquito transmission of *Coccolbrix* species.

Adolpho Lutz passed away on October 6, 1940, a few weeks before his 85th birthday. His research program was carried out by Heraclides-Cesar de Souza Araújo, head of the Leprology Laboratory at the Instituto Oswaldo Cruz, and by Gustavo M. de Oliveira Castro, an entomologist at the same institute who had already published a number of papers in collaboration with Lutz. Based on the results of experiments with Culidae (mosquitoes), Ixodidae (tics), Pediculidae (lice), Cimicidae (bedbugs), Pulicidae (fleas), and Triatominae (sub-family of Hemiptera, which includes the carrier of Chagas disease) conducted by them and by other researchers during the 1940s, Souza Araújo (1953, 1952) reached the conclusion that any haematophagous insect could transmit leprosy under certain conditions, and it would therefore be advisable for the public health authorities to extend the fumigation program aimed at the malaria vector to include rural and suburban leprosy foci.⁴² The leprologist from Manguinhos defended this thesis at the 10th Brazilian Congress on Hygiene, held in Belo Horizonte in October 1952, and at

the 5th International Congress on Tropical Medicine and Malaria, which took place in Istanbul in August-September of 1953.

Adolpho Lutz's ideas were defended before other audiences by his daughter, Bertha Lutz. During the seventeen months in which she held office as a legislative deputy (Partido Autonomista) for the Federal District, representing the Liga Eleitoral Independente (Independent Voters League), she urged that mosquitoes be combated as part of leprosy prophylaxis. She in fact lodged a petition with the Chamber of Deputies, requesting information on the anti-culicidian measures taken at the leprosariums and isolation hospitals then being created in different places around Brazil (Benchimol and Sá, 2003, pp. 203-50).

World War II was a watershed in treatment of the disease. As of the 1940s, the use of compounds derived from diamino-diphenyl-sulphone (Promin, by Parke Davis; Diazone, by Abbot; Sulphetrone, by Burroughs Wellcome) would bring a cure to thousands of interned patients who began receiving their treatment in dispensaries, until they could be fully released after some years (Coutinho, 1957, p. 321). These pages are not the place for a detailed investigation of all the chemical-pharmaceutical, socioeconomic, and political-cultural factors that made leprosariums and leper colonies obsolete, destined to decay or to be transformed into monuments meant for other purposes, among which preserving our memory of medical practices now fortunately left behind. But should you, the reader, happen to come across rusty screens protecting the doors and windows of one of these ghostly institutions, now you will know they represent prosaic vestiges of the ideas so fiercely defended by Adolpho Lutz. In this case, as in many others, the historian does not find the neat closing chapter always imagined to finalize scientific polemics, separating truth from error like wheat from the chaff.

NOTES

¹ Although in Brazil and other nations the term 'Hansen's disease' has been adopted to refer to this illness as part of an effort to relieve its sufferers of the stigma associated with 'leprosy', we have nonetheless chosen to apply the word that was in current use during the historical period examined in these pages. From a historiographic perspective, a 'politically correct' stance would in this case yield an unacceptable anachronism.

² See Danielssen and Boeck (1848). Heraclides-Cesar de Souza Araújo edited a commemorative centennial edition of the *Atlas de la lèpre par C. Danielssen e C. W. Boeck, Bergen en Norvège, 1847* (Rio de Janeiro, 1946).

³ Obregón, (2000, p. 266); Lutz, (1887). Originally published in *Norsk Magazin for Laegevidenskaben* (1874), Hansen's work was reprinted in 1955 by the *Intern. Journ. of Leprosy*. Obregón, currently one of the leading scholars on the history of leprosy, has just published a fine book on the topic (Medellín, 2002).

⁴ After testing it at the Instituto Bacteriológico de São Paulo, Adolpho Lutz gave his negative opinion regarding the serum produced by Colombian physician Juan de Dios Carrasquilla. The affirmation that the search for a vaccine against leprosy turned into "almost the only acceptable program of scientific investigation" does not apply to Brazil, albeit yellow fever was the object of intense research.

⁵ Delegates from Argentina, Colombia, Costa Rica, Cuba, Ecuador, the United States, Guatemala, Mexico, Panama, Paraguay, Peru, Uruguay, and Venezuela attended the conference, held in October 1922 at the National Exhibit's Festival Pavilion, as part of Brazil's Independence centennial celebrations ('Conferência Americana de Lepra,' *Jorn. Comm.*, Oct. 10, 1922).

⁶ Report on the status of Lazarus Hospital, prefaced by some considerations on morphea, its treatment, and experiments conducted at this hospital in 1869 by Dr. João Pereira Lopes' (in Port.), found in Lopes (1870) and partially transcribed in Souza Araújo (1946, pp. 463-71).

⁷ For a description of the cited plants, see Cruls (1965).

⁸ Before finishing his medical schooling in Heidelberg in 1871, Paul Gerson Unna (1850-1929) studied in Leipzig and Strasbourg. His doctoral dissertation on histology and the history of the development of the human epidermis (*Archiv für mikroskopische Anatomie*, 12, 1876, p. 665) was a precursor to original approaches in the field of skin diseases. In 1881, he founded a private dermatological clinic which three years later was moved to more modern facilities, in Eimsbüttel, a suburb of Hamburg. The institution soon attracted a large number of students from Germany and other countries. Co-editor of *Internationaler Atlas seltener Hautkrankheiten* (Hamburg and Leipzig, 1889-99), Unna did research on the skin's biochemical processes and discovered *Stratum granulosum*. He described a number of diseases and introduced new therapies. His book on histopathology, published in 1884, consolidated his prestige as one of the world's greatest dermatologists (<http://www.whonamedit.com/index.cfm>).

⁹ Neumann was a professor at the University of Heidelberg and associated with both the Seemannskrankenhaus (Sailors' Hospital) and the Institut für Schiffs und Tropenkrankheiten (Institute of Naval and Tropical Diseases), in Hamburg. In the summer of 1904, he traveled to Brazil with M. Otto to study yellow fever, shortly thereafter publishing *Studien über das Gelbe Fieber in Brasilien* (Leipzig, 1906).

¹⁰ According to Obregón (2002, p. 34), Lehmann and Neumann coined the name Mycobacterium (Myco from the Greek *Mykes*, meaning fungus), because of the fungus-like appearance of the strains cultivated in a liquid medium. Microbacteria are aerobic, alcohol- and acid-fast bacteria shaped like straight or slightly curved thin rods, sometimes branched. The genus comprises thirty species that differ from other bacteria because of a series of properties, many having to do with the quantity and types of lipids found in their walls (Bier, 1963, p. 129; Trabulsi, 1991, p. 188).

¹¹ The author lists not a few microbiologists who improperly used the terms *Coccothrix tuberculosis* Lutz or *Coccothrix leprae* Lutz: Unna (1887, p. 11); Toni and Trevisan (1889, pp. 943, 944); Vuillemin (1913, p. 527); Buchanan (1925, p. 275); Bergey *et al.* (1934, p. 536); Hauduroy *et al.* (1937, p. 291; 1953, pp. 327, 335); Reed (1939, p. 810; 1948, p. 877); Krassilnikov (1941, pp. 107, 109; 1949, pp., 179-80); Hanks (1948, p. 882). According to the International Code of Nomenclature of Bacteria, the following are synonymous with the species *Mycobacterium leprae* Lehmann & Neumann: *Bacillus leprae* Hansen 1874; *Coccothrix leprae* Lutz 1886; *Discomyces leprae* Neveu-Lemaire 1921; *Mycobacterium leprae hominis* Lowe 1937; *Mycobacterium Leprosy bacillus* Hansen 1880; *Sclerothrix leprae* Vuillemin 1921 (www.dsmz.de/bactnom/bactname.htm, 2000).

¹² "Relatório do dr. Lutz comunicado ao dr. Azevedo Lima," Rio de Janeiro, Jul. 4, 1887, in Azevedo Lima, 1887, pp. 24-32. Transcribed in Souza Araújo (1946, pp. 491-3).

¹³ See the bibliography on Adolpho Lutz edited by Herman Lent, in Neiva (1941). It was reprinted, with corrections and additions, in *História, Ciências, Saúde – Manguinhos*, 10:1, pp. 362-409.

¹⁴ Born in Manchester, England, in 1854, Arning was educated in Germany; he died in Hamburg on August 20, 1936 (Mouritz, 1943). In Honolulu, on September 20, 1884, he inoculated Keanu, "rubbing pus from a leprosy ulcer into a blister on his right arm and his ear. Right after a bout of fever, he removed a leproma from a 9-year-old girl with tuberculoid leprosy ... introduc[ing] it into a wide, deep incision in Keanu's left forearm, and secur[ing] it there with five stitches. ... In the second month, the inoculated man suffered rheumatoid pain; shortly thereafter, infarction of the cubital and median nerves of the inoculated arm set in, lasting from the fifth through the eighth months. ... By September 25, 1888, Keanu had developed a pronounced case of 'tuberculoid leprosy' and he was taken to the leprosarium in Molokai, where he died in 1889" (Souza Araújo, Sept. 3, 1936).

¹⁵ According to Marcelo Oswaldo Álvares Corrêa (1992, p. 144), prime source of information on this period of Lutz's life, the Brazilian scientist's name reached Emerson through H.W. Schmidt, who was Consul for Sweden and Norway and also had ties with H. Hackfeld & Co. On May 5, 1888, Unna wrote Lutz from Paris. He forwarded him a letter from Schmidt about his trip to Hawaii, leaving it entirely up to Lutz to do as best suited him (BRMN, Fundo Adolpho Lutz, pasta 252, maço 2). In a letter to Emerson, dated June 2, 1888, Lutz accepted the invitation contingent on certain terms: a stipend of US\$2,000 to finance preparatory studies in Hamburg; a monthly salary of US\$300; and the right to practice private medicine, all recorded in a contract with legal guarantees.

¹⁶ Letter from Emerson to Lutz, dated November 6, 1888, partially transcribed by Corrêa (1992, pp. 145-6). According to Obregón (2002, pp. 139-40), in late 1865 — at the height of Europe's and the United States' struggle over control of the archipelago — the Hawaiian Board of Health, composed mostly of foreigners, inaugurated the Kahili hospital, about two miles from Honolulu. Around the same time, it acquired the land on Molokai Island where more than 5,800 individuals would be interned between 1866 and 1905.

¹⁷ Considered a good antiseptic and antifebrile, salol was used internally against rheumatism of the joints and against intestinal and urinary-tract infections; externally, as a powder, it was quite often used to treat all types of sores. Owing to its antifebrile and antiseptic properties, sodium salicylate (C⁷H³O³Na), a derivative of salicylic acid (the forerunner of aspirin), was widely used against a number of infectious diseases, including yellow fever. Chrysarobin, the active constituent of Goa powder (C³⁰H²⁶O⁷), was employed to treat psoriasis and other diseases that manifested

themselves in the form of cutaneous erythemas. Pyrogallic acid (C¹²H⁶O⁶), obtained by distilling gallic acid, was used to treat leprosy and psoriasis, in the form of a salve. Goldenseal was an herb of the Ranunculaceae family; the North American native species (*Hydrastis canadensis*) had broad therapeutic use. One of the alkaloids contained in its rhizome, berberine, causes deep contractions of the uterine muscles and was thus used against hemorrhages of that organ. It also had tonic, antifebrile, and diuretic properties. *Veratrum* also designates several species of herbs from the Ranunculaceae family that contain a mixture of alkaloids, like veratrin, often used as a vomitory and purgative and used externally to treat cutaneous diseases (Littré and Gilbert, 1908).

¹⁸ The arguments put forward by Albuquerque have their origin in a number of Lutz's later works. Presented in this way, they do not leave it clear that observations and hypotheses occurred in Hawaii or how the theory that Lutz was to defend so resolutely as of the 1910s came to birth and developed.

¹⁹ Aragão, (1915), cited in Souza Araújo, (1956, p. 137), and Obregón, (2002). We have found only *Leçons sur la syphilis, professées à l'hôpital Saint-Sauveur* (Paris, 1886) by Henri Leloir. In a document from the Adolpho Lutz Archives ('Transmissão da lepra por mosquitos,' caixa 19, pasta 74), Bertha Lutz wrote references apparently taken from Edv. Ehlers, 'Transmissibilité de la lèpre par les insectes succeurs de sang (parasites lectulaires),' in *Il Lepre Konferenz Mitteilungen u. Verhandlungen III*, (pp. 25-38). The list mentions a paper by Leloir entitled *Traité pratique et théorique de la lèpre* (1886). It also includes Ashmead (although nothing other than the author's name); Baldomero Sommer, 'La lèpre en Argentine,' *Semaine Médicale*, 1898; articles by Blanchard but with no titles specified (see bibliography); Arning (*Archiv. F. Dermat. U. Syph.*, 1891); Scott (*Brit. Med. Journ.*, Aug. 18-Sept. 29, 1900); Noc (*Annales d'Hygiène et de Médecine Coloniale*, Jul.-Sept. 1903, p. 481; Jan.-Mar. 1904, p. 11). More detailed references are provided on Chosky, author of *Report on Leprosy and the homeless Lepers Asylum Matunga at Bombay* (1901); Hallopeau, 'Leçons cliniques de l'hôpital St. Louis,' *Bulletin de l'Académie de Médecine*, Jul. 1901; Chantemesse (same publication, same session, Jul. 30, 1901); Pierre Cazamian, *Archives de Médecine Navale*, Dec. 1904, p. 452; Goodhue, *Indian Medical Gazette*, (Aug. 1906), and *Journal of Tropical Medicine*, (Sept. 15, 1906); Marchoux and Bouret, *Annales de l'Institut Pasteur*, (19, 1908, p. 389) and *Bulletin de la Société de Pathologie Exotique*, (1908, p. 288); Laveran (same periodical, same session).

²⁰ A helpful entry on Blanchard can be found at <http://www.pasteur.fr/infosci/archives/f-bio.html>. See also Brumpt (Feb. 13, 1910) and Linossier (Feb. 15, 1919).

²¹ This brochure was most likely 'The mosquito as an agent for spreading yellow fever' (in Port.) by Emílio Ribas (1901), which transcribes a note by Adolpho Lutz. Lutz must have sent some of his works on leprosy, because the Frenchman comments in surprise (Oct. 1, 1905): "I was unaware that you had already pointed out the role of insects as infectious agents of leprosy. If there is a second edition of my book, I will not fail to mention this interesting fact. I am delighted to find myself in agreement with you on this point. I am more and more convinced that this is the only reasonable interpretation" (BRMN Fundo Adolpho Lutz, pasta 255, maço 1).

²² The report presented on July 28, 1897, to Emílio Ribas, director of São Paulo's Sanitation Service, contains the results of an analysis of the material Lutz had received from Colombia shortly before his trip to Montevideo, where he attended the conference at which Giuseppe Sanarelli announced discovery of the icteroid bacillus, the presumed agent of yellow fever (Benchimol, 1999). The labels of the ten bottles were marked: "Instituto Carrasquilla – Suero antileproso [antileper serum] – Bogota — Plaza de los Mártires." With the help of Arthur Mendonça, Lutz ascertained that all contained bacilli similar to *coli*, a larger bacillus, and cocci. They injected the serum into a dog and noticed no "phenomena other than a bit of local tumefaction." Lutz judged it should not be used since it contained "several different organisms." "As to its therapeutic actions," he wrote, "we can say that the little we have found in the literature provides absolutely no scientific basis for justifying any hope in this realm. This appears to be merely a work guided by no criteria, or of commercial speculation (BRMN Fundo Adolpho Lutz, pasta 12, maço 1). Carrasquilla's serum was widely accepted in Colombia, and the government of that country gave him an institute so he could continue his research. Prepared from the blood of infected patients, his serum was criticized by the international scientific community, including a number of participants at the Berlin congress, who argued that "Hansen's bacillus is not found in the blood of patients" (Obregón, 2002, pp. 194-6).

²³ Art. 145, title II, 'Profílixia geral das moléstias infectuosas,' in Barbosa and Rezende (1909, p. 1,000).

²⁴ In 1817, Enxadas Island, a storage place for gunpowder, provided temporary shelter to the ill who were transferred from the leprosarium in the neighborhood of São Cristovão. The lepers were moved to Bom Jesus Island, where they stayed until completion of works on Lazarus Hospital in 1850. Another island, Boa Viagem, in Niteroi, was site of a pesthouse for seamen afflicted with contagious diseases. Created in 1810, shortly after the Portuguese Court moved to Brazil, this pesthouse was supported by daily fees paid by merchant marine ships docked in Guanabara Bay (Kushnir, 2002; Sarthou, 1964).

²⁵ These words were pronounced by Emilio Gomes at the July 22, 1915 session of Brazil's National Academy of Medicine. They were published in the academy's annals (vol. 81, p. 161) and reproduced by Souza Araújo (1956, p. 123).

²⁶ Hannaway (1993) names two works as the essential foundations of Germany's medical geography: Leonhard Ludwig Finke's, published in 1792-95, and the two volumes that Hirsch published in the 1860s, on the eve of the Pasteurian revolution. In these pages, the German physician compiled a remarkable amount of data on the distribution and types of diseases during different periods and locales around the world and on the relation between these diseases and the environments in which their victims lived.

²⁷ Among those defending the transmission of leprosy by fleas was Juan de Dios Carrasquilla, who advocated this theory at the 3rd Latin American Scientific Congress, held in Rio de Janeiro in 1905. In 1947, another Colombian physician, Guillermo Muñoz Rivas, won an award from Brazil's National Academy of Medicine for his work on the transmission of leprosy by fleas. Continuing with Carrasquilla's line of study, Muñoz Rivas conducted a number of experiments with human and canine fleas and ascertained that the leprosy bacilli remained in these insects' digestive tubes for up to 76 hours (Obregón, 2002, pp. 173, 317).

²⁸ Lutz attributed this discrepancy to the use of mosquito netting. No white person slept without one and even during daytime pyrethrum powders were widely used. The natives had no such habits. "On the other hand, the locals are very fond of water, and a lack of cleanliness cannot be alleged, as is always invoked, clouding the issue of disease transmission" (*Jorn. Comm.*, Nov. 7, 1915).

²⁹ On Dec. 3, 1915, Valverde questioned the analogy drawn between the processes by which leprosy and yellow fever are transmitted. "If one sole case of yellow fever, which only has infecting power during three days, is enough to produce a pandemic, how can we allow that leprosy, with such a lengthy febrile period ... , can be transmitted so slowly and capriciously? Given the number of lepers around the world and the number of mosquitoes likewise in the world, if this theory were true, the world would be transformed into one huge leprosarium" (cited in Souza Araújo, 1956, p. 132). The hardest blow against this aspect of Lutz's theory came from a scientist at the Instituto Oswaldo Cruz, leprologist Souza Araújo (1946, p. 6). After examining venous blood from dozens of sick people who visited his office at the Instituto between 1927 and 1929, he proved that "every lepromatous leper is constantly in a state of bacillemia, contrary to the classic notion defended ... by Ad. Lutz, that this bacillemia only occurs during bouts of fever."

³⁰ *Jorn. Comm.*, (Dec. 6, 1915), cited in Souza Araújo, (1956, p. 131). At another point during the second conference, Lutz stated: "It is only in recent years that experimental work on this topic has appeared, and its authors have usually grown discouraged as soon as they have run into their first problems concerning the mosquito's role. ... As a result, they have made the mistake of singling out flies and bedbugs, without taking into consideration that these could never explain the capricious way in which the disease spreads" (p. 130). Lamborn (1937) provides a fine review of the plentiful literature on the transmission of leprosy by flies.

³¹ Lutz pointed to the "*Culex fatigans* — or '*quinquefasciatus*', as the Americans would have it — and its corresponding *pipiens*; secondarily comes *Stegomyia calopus*, whose distribution is more limited" (Aragão, cited in Souza Araújo, 1956, p. 139). According to Consoli and Oliveira (1994, p. 139), *Culex quinquefasciatus* [Say, 1823] was long known as *Culex fatigans* or *Culex pipiens fatigans*. "It is considered tropical-cosmopolitan. It is basically found in meridional parts of Asia and in Africa, the Americas, and Oceania." A domestic mosquito, it was "a curse for those living in the warmer climes of the aforementioned continents. It is found ... in greater numbers in human agglomerates, inside cities or rural villages, becoming rare as housing is spread farther apart, and eventually not found at all where man has not yet arrived or where he left long ago. ... It attacks precisely during periods reserved for rest, following the hours of work or study."

³² Lutz's interest in mosquitoes led him to gather a representative collection with the help of collectors in different parts of Brazil (see BRMN Fundo Adolpho Lutz, pasta 216). He devoted himself with great enthusiasm to the taxonomic study of the group, and one of his main interlocutors was Frederick Theobald, the entomologist to whom the British Museum had entrusted the task of writing a monograph about mosquitoes of the world. Theobald used Lutz's descriptions of mosquitoes collected in Brazil. For five years, the two of them corresponded about the characteristics of the species they were studying (Sá, 2002).

³³ Regarding the debates at the 5th Brazilian Congress of Medicine and Surgery, held in Rio de Janeiro in mid-1903, see Benchimol (1999). "I am well aware that new ideas are always received with a certain caution, up to a point quite acceptable," stated Aragão in December 1915, "but a great distance lies between this and assuming an unyielding opposition, ... without taking into account the authority of who is presenting these. Unfortunately, this is what seems to be the tendency concerning the culicidian doctrine of leprosy, repeating earlier campaigns against the culicidian theory of malaria and yellow fever, and, in general, against all hypotheses about the spreading of diseases by an intermediary host" (cited in Souza Araújo, 1956, p. 137).

³⁴ Emilio Gomes presented these *Conclusões aconselhadas pela Comissão de Prophylaxia da Lepra para servir de base a um projeto de lei* to Brazil's National Academy of Medicine. They were published in the academy's *Boletim*, (vol. 2, 1919, pp. 738-40) (cited in Souza Araújo, vol. III, p. 159).

³⁵ Lutz probably presented 'Problemas que se ligam ao estudo da lepra,' read a little earlier at the Congress of Dermatology and Syphilology held in Montevideo in 1921 (BRMN Fundo Adolpho Lutz, pasta 252, maço 6; published in Eng. in *A Folha Medica*, 1921).

³⁶ 'A transmissão da lepra e suas indicações profiláticas,' published in May 1936 in *Memórias do Instituto Oswaldo Cruz* (in Port. and Eng.), is a short version of another, longer article, published in June in *Annaes da Academia Brasileira de Sciencias* (in German). It was reprinted in *Boletim da Campanha contra a Lepra* (May-Jun. 1936).

³⁷ Unpublished typewritten text, 7 pp. (BRMN Fundo Adolpho Lutz pasta 254, maço 5).

³⁸ BRMN Fundo Adolpho Lutz, (pasta 254, maço 4), together with correspondence between Bertha Lutz and Paula Souza about this contribution by Lutz.

³⁹ We hesitate when it comes to the best verb to employ: the modal seems to address these instructions to other researchers, but this surely was the protocol for the experiments Lutz himself had been undertaking, which would justify using the simple past.

⁴⁰ “Although tuberculosis generally is not spread by blood-suckers,” Lutz also stated, “there is a form probably produced by the bites of haematophagous Diptera. This is *Lupus vulgaris*, also usually found on the face and more rarely the hands. I believe most of these cases are bovine tuberculosis, which would account for the relative benignness of this process, which is predominantly chronic.”

⁴¹ “Acid fastness” is a property of *Mycobacterium* established by Paul Ehrlich in 1882. Tuberculosis and leprosy bacilli are hard to stain, but when dyed with gentian violet and saturated in an aniline and water solution, they resist discoloration by mineral acids. This feature became the main way of distinguishing them from other microorganisms (Obregón, 2002, p. 34).

⁴² Dr. Henrique Aragão and Dr. Herman Lent, protozoologist and entomologist at the Instituto Oswaldo Cruz, worked together on these experiments. José Mariano and Ruy Noronha Miranda (director of the São Roque Leprosarium in the state of Paraná) signed articles on the topic, in collaboration with Oliveira Castro and Souza Araújo. In his paper of 1952, Souza Araújo cited experiments by E. Montestruc and R. Blache (1951), in Martinique; Guillermo Muñoz Rivas (1946), in Colombia; and Celso S. C. Rossel (1947, 1946), at the Instituto Oswaldo Cruz. He also referred to observations and materials sent to the Leprology Laboratory at Manguinhos by physicians working in different leprosariums around Brazil, all of whom were convinced of the role played by haematophagous insects in transmitting the disease: Max Rudolph, clinician in Estrela do Sul (MG); Paulo Cerqueira, from the Santa Isabel leprosarium; J. A. Soares, leprologist from Espírito Santo; Dr. Orestes Dioniz and Dr. Josefino Aleixo, who, together with Souza Araújo, visited lepers living in Bambuí, Minas Gerais, just before the Centro de Estudos e Profilaxia da Moléstia de Chagas was founded there in November 1943. Further on this topic, see Souza Araújo (1941; 1942a and b; 1943a, b, and c; 1944a and b); Oliveira Castro and Mariano (1944); and Rossel (1947, 1946).

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