Historical introduction
Insects, people, and disease: Adolpho Lutz and tropical medicine

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Adolpho Lutz spent seventeen years far from his family. Born in the Brazilian capital on 18 December 1855, the second of Gustav and Mathilde Oberteuffer Lutz’s ten children, Adolpho was two when his parents returned to Switzerland in 1857, seven years after emigrating to Brazil. In 1864, they once again relocated to Rio, leaving their three eldest children to study in Basilea. Adolpho would only be reunited with his parents in 1881, when he went back to Rio with his medical degree in hand and a solid background in medicine and biology, acquired at German-speaking universities. While he was away, the city that had welcomed these Swiss immigrants had undergone remarkable changes.

When Adolpho Lutz’s parents disembarked in Rio de Janeiro, in late 1849/early 1850, the forces that had in the previous decades resisted the Empire’s centralizing policies and Southeastern Brazil’s escalating economic hegemony had been tamed.

Divided essentially into masters and slaves, the port city was thriving as the point of contact between an expanding coffee crop – tended by slaves in the Paraíba River Valley – and the world market. Its streets bustled with slaves hired out by their masters on a daily basis for work in a wide gamut of trades. The slaves received only a small portion of the money earned and applied it towards their own sustenance: food, drink, perhaps
even a room in a tenement. But most of the earnings went to fatten the coffers of the richest slave holders or to guarantee the survival of the poorest among them, sometimes almost as poor as the working slaves themselves. Household slaves did the multiple tasks demanded by the natural economy buttressing residences based on slave labor. This meant they supplied their masters’ homes with water and removed sewage, duties soon transformed into profitable ‘public’ services in the hands of private companies.

In the ensuing decades, the second industrial revolution – the revolution of iron and steel manufacturing, of capital goods, and of the construction of railways and steamboats – would consolidate England’s world power, albeit other countries likewise revolutionized by big industry were emerging as serious competitors. Capital exports, in the form of public loans and direct investments, lent impetus to the modernization of peripheral economies like Brazil’s, equipping them to respond to the new inflow of raw materials and industrialized goods.

In the time between Gustav and Mathilde Lutz’s immigration to Brazil and young doctor Adolpho’s return voyage, other processes helped to change
the face of the Brazilian capital: abolition of the slave trade in 1850, the Paraguayan War (1864-70), demographic growth, and the gradual expansion of freed labor. In the 1870s, the Empire of Dom Pedro II and of the coffee barons seemed to be at the heyday of its grandeur and stability, and Brazil was living out its apparent destiny as a primarily agricultural nation. Rio de Janeiro was its most prosperous commercial and financial center. Matching the pace at which Paraíba Valley plantations absorbed the last contingent of Brazilian slaves through interprovincial trade, Rio made ample room for wage labor. The circulation of merchandise – basis of the urban economy – improved qualitatively thanks to these new labor relations. The production of important manufactures notwithstanding, the productive sector remained an appendix of import and export activities.

Replacing river and pack-mule transportation, the tracks of the Dom Pedro II and Leopoldina railways brought Rio de Janeiro closer to its rural rear guard. Likewise revolutionized by steam power, maritime transport increased alongside a complex of commercial and financial enterprises formed chiefly of British capital. The port itself saw its first reforms, like metal warehouses and steam-driven cranes on the Customs House pier, where cargo handling could now be done without slave labor.

In the 1860s through 1870s, foreign companies, along with some Brazilian ones, began setting up public services: gas lighting, household
water and sewer, garbage disposal, urban transit, and so on. This helped eliminate the system under which slaves provided such services and also undermined the household economy responsible for the self-sufficiency of slave-run residences. Tramway companies spearheaded extension of the urban network beyond the former perimeter of the Old City and of Rio’s latest development, the New City. Although new neighborhoods were springing up, critical problems became concentrated in downtown Rio, born of a growing incompatibility between the former material structure and the new capitalist economic relations taking root within this framework. Between rail station, docks, and the labyrinth of commerce, the center’s narrow, winding streets became crowded with a greater inflow of men and merchandise, including large iron manufactures. The populous downtown was home to a wide assortment of edifices: offices and banks, stores, repair shops, waterfront warehouses, public buildings, private one- and two-story residences, grocery stores oft times servicing tenements and the like, old aristocratic mansions sliced up into tiny, dirty rooms that boarded entire working-class families. A heterogeneous, drifting multitude lived and labored in central Rio de Janeiro. In physical terms, the market where this now-freed labor force put itself up for sale was contiguous with the market where daily wages and uncertain earnings by workers were converted into the sustenance that ensured their survival.
There, every year, more or less deadly epidemics broke out. Morbidity and mortality rates varied with the biological and social synergy of those who came into contact with each other during the course of each disease. Smallpox epidemics generally occurred in the winter. Cholera struck Rio de Janeiro in 1855-56, at the close of the 19th century’s third major pandemic. In the nation’s capital and in its provinces, tuberculosis, dysenteries, malaria, and fevers known by dozens of names raged like chronic scourges.

The most serious public health issue was without a doubt yellow fever, which ‘docked’ at the Imperial capital in the summer of 1849-50, just when Gustav and Mathilde Lutz first stepped on this new land, where signs seemed to promise a civilization similar to that which had flourished in the Old World’s temperate zones.¹

Yellow fever and the beginning of Adolpho Lutz’s career

According to Bertha Lutz,² her grandparents arrived in Rio de Janeiro in January of 1850. This would have been at the peak of the severe epidemic sweeping the Brazilian capital for the first time. Adolpho Lutz (1930, p.2), however, wrote that when his parents reached Rio, it was still free of the disease, which means they disembarked shortly before 28 December 1849, the date on which Robert Christian Berthold Avé-Lallement (1812-84), a physician from Lubeck, diagnosed the first cases at Santa Casa de Misericórdia Hospital (Franco, 1969, p.35; Chalhoub, 1996, p.61; Santos Filho, 1991, p.195).

Witnesses to the epidemic associated its outbreak with the arrival of a slave ship from New Orleans, which had made stopovers in Havana and Salvador before making harbor in Rio on 3 December 1849. Crew members took up lodging around the city, and ‘yellow jack’ erupted at one of the inns where they stayed, on Misericórdia Street. By February 1850, when the Imperial Academy of Medicine finally recognized that yellow fever held the city in its grip, the illness had already spread through the beaches of Mineiros, Peixe, Prainha, Saúde, and beyond. According to estimates by Dr. José Pereira Rego (1872, p.159), it struck 90,658 of Rio’s 266,000 inhabitants, killing 4,160 according to official data or, according to extra-official sources, up to 15,000 (Chalhoub, 1999, p.61).

“Year for mangoes, year for yellow fever,” the locals said, expressing in colloquial language the relationship that doctors drew between heat,
humidity, and epidemics. Except for the period between 1862 and 1869, the disease ‘grew’ as regularly as other seasonal fruit, always during the so-called muggy season, that long period of heat and rain running from about November till March or April. Analogies with the plant world didn’t end there. It was assumed that yellow fever, like plants, took perfectly to coastal lowlands, especially port cities, where putrefying plant and animal matter provided them with ideal humus.

Writing about his parents and Rio de Janeiro’s first epidemic, Adolfo Lutz said (1930):

In the subsequent period, they had many children, all of whom ran the risk of yellow fever for varying but generally quite lengthy periods of time. My mother, who lived in Rio for over thirty years, never caught it, but my father and a brother of mine each came down with it twice and another got sick during the first Santos epidemic, that is, in 1879, meaning that family immunity can be discarded. However, many more people in the family remained free and today can be considered [to have been] protected by this imperceptible process of immunization, whose existence is as evident as its establishment is hard to accompany. Taking the third generation into account, one can say that in my family the morbidity rate of those exposed did not reach one-third, a remarkable fact when one compares it with the general morbidity rates during the first epidemics in different points around the state of São Paulo.

When he came ashore in Rio de Janeiro in 1881 with his medical degree, at the age of 26, Adolfo Lutz took up residence at his parents’ home at 33 Princeza Imperial Street, in the neighborhood of Catete, a house large enough to accommodate his nine siblings and the Girls’ School founded by his mother. His father’s business was located at 44A Sabão Street. According to advertisements published in Almanak Laemmert (p.497, 512, 549), Lutz & C. was a company of “foreign businessmen” dealing in import and export, in the latter case in collaboration with J.R. Dietiker. They were publicized as “Consignees and Commission Houses for Import and Export Goods” and “Wholesale Stores of Imported Dry Goods.” Taking advantage of the process of urban modernization then underway, Lutz & C. acquired (4 Apr. 1872) control of the assets of an industry that produced “instantaneous tubular wells,” owned by Gustavo Adolpho Wierffbain, “civil engineer, born in Germany, and resident of the Empire of Brazil.”

As we saw in the first book of The Complete Works of Adolfo Lutz, one of the first things he did was to have his diploma recognized by the Rio de Janeiro School of Medicine. In an article published in Correspondenz-Blatt
für Schweizer Aerzte in April 1882, Lutz described the bureaucracy entailed in this process and drew a portrait of medicine in the Brazilian Empire. He explained that the country’s larger cities had their advantages, but that life was thrice as expensive as in Switzerland and, in the case of Rio de Janeiro, one could add the risk of yellow fever to the city’s bothersome heat. This was one reason the young doctor decided to find a rural town where he could practice medicine. Besides enjoying the rewards of the “beauty of nature,” whoever set about building a reputable practice in such places could expect to make a small fortune, as in Europe. Income varied with the region, with less money to be earned in some places and substantial sums in others, like certain coffee-growing areas or some German settlements in southern Brazil.

Lutz remained in the capital of the Empire for eight months, waiting for his diploma to be recognized. In the first half of 1882, he tried opening an office in the nearby mountain town of Petrópolis but ended up settling in Limeira, site of a sizeable Swiss-German settlement where his sister Helena had just moved, shortly after her marriage to the German businessman Gottfried Wilhelm Luce.
Adolpho Lutz's registration at the Faculty of Medicine of Rio de Janeiro, dated 27 October 1881, to take exams required to practice the profession in Brazil (BR. MN. Fundo Adolpho Lutz).

From June 1882 to March 1885, Lutz resided in this vital center for coffee, sugarcane, and grain production in São Paulo province, its population then around 14,000.

He attended people from other settlements along the Paulista railway as well, which soon gave him a comprehensive view of the region’s diseases. In late 1882, in a note published in the Swiss periodical mentioned earlier, he recounted his first impressions of rural São Paulo and listed the topics he might analyze in future articles. He underscored the contributions he could make to medical geography. Since he treated diverse ethnic groups, he could offer rich “observations concerning the influence of climate and of
the human race on different diseases; in [his] work, [he] attended blacks, Brazilians, and German, Portuguese, and Italian immigrants, who thus provided most interesting comparative material” (Lutz, 1883, p.30).

In Limeira, Adolpho Lutz did important investigations into both clinical practice and the helminthology of domestic animals and man. It was then that he undertook his research on worms, broadening the repertoire of pathologies under study by the Bahian tropicalist school and likewise opening the door to the investigation of animal diseases in Brazil. As we saw in the second book of this collection (Benchimol and Sá, 2004), Adolpho Lutz’s interest in leprosy led him to Hamburg in March 1885, where he worked about a year at the clinic founded by Paul Gerson Unna. Under the latter’s guidance, he ventured into the territory of bacteriology, focusing on the morphology of germs linked to a number of dermatological diseases.

When he returned to Brazil in mid-1886, Lutz resumed his medical practice, this time in the city of São Paulo, while continuing to publish articles in German periodicals not only on dermatology but on helminths too. A Portuguese translation of Lutz’s work on ancylostomiasis, originally released in Volkman’s collection of lessons from medical practice (Leipzig, 1885), then came out in O Brazil-Medico, Gazeta Médica in Bahia (1887-89), and shortly thereafter in book form (1888), all of which made Lutz’s name more familiar to his Brazilian peers. In Limeira and then later in the city of São Paulo, he also explored the life cycles of Ascaris lumbricoides and Rhabdonema strongyloides. In 1888, he published a series of articles in the prestigious Centralblatt für Bakterologie, Parasitenkunde und Infektionskrankheiten on infestations of intestinal nematodes in man – ancylostomiasis, oxyuriiasis, ascaridiasis, and trichocephalosis. He
emphasized the role of the soil and feces in spreading these diseases and drew correlations between them and the immigrant population’s living and eating habits; he further called his peers’ attention to how often domiciliary infestations and family epidemics occurred.6

**Amoebae, bacilli, and dysenteries**

Intestinal diseases were a preponderant component of public health problems in Brazilian cities, which were to greater or lesser degrees then experiencing population booms and witnessing the decline of living conditions as a counterpart to the development of capitalism. Lutz started studying these diseases while practicing medicine in Limeira, and in 1891
he published a vital study on the subject, likewise in *Centralblatt für Bakteriologie, Parasitenkunde und Infektionskrankheiten* (1891, p.241-8).

The etiology of dysenteries was quite unclear back then. Amoebae had been located in the corpses of people who had succumbed to a variety of intestinal syndromes, but no one had been able to demonstrate a cause-and-effect relation with these protozoans.

When Lutz began exploring the question, existing notions about dysentery were quite ill-defined and the topic one of great controversy. Physicians searching for the cause of this malady found various microorganisms in patients' feces and body organs, and each one believed that whatever they found played the role of specific etiological agent.

Dopter (1909, p.1-2) pinpoints 1859 as the year these studies began, when Vilem Dusan Lambl (1824-95), a Bohemian physician who had obtained his medical degree in Prague and who worked at a children’s hospital there (Franz-Josefs-Kinder-Spitale), observed the presence of amoebae in the feces and, more importantly, in the intestines of a child stricken with dysentery.

According to Martinez-Palomo (1996), in an article published ten years earlier in the bulletin of Moscow’s Imperial Society of Naturalists under the title “Fragments d’helmintologie et fisiologie microscopique,” the Russian doctor G. Gros had demonstrated that organisms he called *Amoeba gengivalis* parasitized humans.

As stated by British biologist Clifford Dobell (1919), it was the surgeon Timothy Richards Lewis who in 1870 first observed that amoebae could be present as parasites in the human intestine. Lewis was then member of a commission formed to investigate cholera in colonial India.

“Today we know,” Martinez-Palomo explained, “that two types of intestinal amoebae are present in man: one non-pathogenic, observed by Lewis
(Entamoeba coli), and one pathogenic (Entamoeba histolytica), defined by Lösch. It took decades to establish this differentiation.”

This author credits Fedor Aleksandrovich Lösch, a physician and microscopist from Saint Petersburg, with discovery of the causative agent of amoebiasis.

In 1873, a young peasant named J. Markow, who was suffering from heavy diarrhea and a rectal malady, began undergoing treatment (initially successful) with quinine sulfate (and other drugs) ... Lösch provided a precise description of the amoebae found in the patient’s feces, which he christened Amoeba coli (most of the ulcerations were in the colon), but since ... the inoculation of dogs with these amoebae had no effect, he dared not categorically affirm that they were responsible for the infection.8

The Russian doctor presumed the amoebae would merely exacerbate intestinal inflammation through direct mechanical irritation (Dopter, 1909, p.2). Perhaps the physician’s doubts also derived from his reading of a paper published by Basch shortly before (1869), in which the latter had observed non-characterized spherical elements and filaments suggestive of a bacterium of the group Leptothrix in slices of dysenteric intestines. A short time later (1875), Rajewsky was to describe colonies of cocci and bacteria in the lymphatic vessels of the submucous membrane.

In subsequent years, Sonsino, Perroncito, Grassi, Calandruccio, and Blanchard corroborated the presence of amoebae in dysenteric feces, but their role remained an issue since Grassi found them in healthy individuals too, and Cunningham and Lewis, in patients suffering from cholera.
In 1883, while studying the disease in Egypt, Koch recognized that amoebae could play a specific pathogenic role, after having found them not just in human stool but also in slices of dysenteric intestines, deep within the intestinal walls. Kartulis, his disciple, confirmed these observations after examining many sick people in Alexandria and in Greece. He had found amoebae in their stools and intestines and in abscesses of the liver as well (Dopter, 1909, p.2-3), but he could not erase doubts about their pathogenic role, since experimental inoculation in laboratory animals had been inconclusive; nor could he demonstrate a relationship between the ‘tropical’ dysentery he was studying and the cases described in Europe by doctors who saw no relation with the amoebae.

From then on, the number of papers published on the topic grew steadily, some confirming and others refuting Koch and Kartulis’ hypothesis.

In Prague, in 1886, Jaroslav Hlava presented the results of a study of sixty cases. He had managed to reproduce the disease by inoculating dysenteric material in different animals, but he had isolated not only amoebae but nineteen different bacteria too. He did state that only the first were specific. The following year, however, Theodor Albrecht Edwin Klebs (1887) incriminated a bacillus isolated from the intestinal walls. Chantemesse and Widal reached a similar conclusion (1888).

At Lösch’s clinic in Kiev, Massiutin examined the stools of five patients: one with chronic dysentery,
two with ‘chronic intestinal catarrh’, one with typhoid fever, and the last with ‘acute intestinal catarrh’. All presented amoebae. In an article published in 1889, Massiutin concluded that these were not responsible for the dysentery; they had penetrated the intestinal canal through water, and their development there had been favored by existing ulcerations, which those protozoans merely exacerbated (Councilman and Lafleur, 1891, p.400-1; Dopter, p.4).

Towards the end of that year, Adolpho Lutz passed through Baltimore, in the United States, on his way to the Kingdom of Hawaii, where he was to accept a post as the Government Physician for the Study and Treatment of Leprosy. In this east coast city, then a heavily-populated junction of roads linking north and south, one of the most modern hospitals in the world had just been inaugurated: the Johns Hopkins. While there, Lutz talked about his unpublished investigation into amoebae with the head of medical services, the pathologist William Osler (1849-1919).12

Johns Hopkins (1795-1873), the hospital’s founder and patron, had been president of Merchants Bank and a director of the Baltimore and Ohio Railroad. Unmarried and a Quaker, Johns Hopkins had decided to invest part of his huge fortune in the establishment of a university and hospital for medical education and research, serving mainly the region’s indigents. John S. Billings (1838-1913) was hired to supervise construction; he was an Army doctor familiar with the Civil War’s ‘barrack hospitals’. The guiding principle behind these – prevention of contagion – was the same one underlying the hospital project, which was composed of isolated pavilions subject to rigorous standards when it came to the arrangement of spaces, beds, and services. The complex comprised seventeen pavilions connected by nearly 600 meters of hallways; construction stretched from 1877 until the institute’s inauguration on 7 May 1889. In early 1886, William H. Welch had already begun teaching microbiology and pathological histology; that same year William Thomas Councilman (1854-1933) joined the pathology laboratory headed by Welch.13 After Osler was made head of the hospital’s medical services, he put together a team of assistant physicians who resided there, dedicated full-time to teaching and research. Henry A. Lafleur was one of them from 1889 to 1891, when he transferred to McGill University in Canada, where he had earned his degree.

Adolpho Lutz began his 1891 paper (published in Germany) with a mention of his visit to this hospital, thereby underscoring that in 1889 no
studies on amoebiasis had as yet been conducted at the Baltimore establishment. In 1890, quick as can be, Osler had described a case of hepatic abscess involving the presence of amoebae, making no reference to the Brazilian scientist, which provoked the following resentful comment: “the priority of observations concerning the New World falls ... to me, whereas Osler should present these [observations] only in relation to North America.” Lutz alleged that he had delayed publication of his investigations “in the (unfortunately vain) hopes of being able to complete them with a greater abundance of material.”

Lutz’s article was based on only three cases, but it was comprehensive and precise. He provided an overview of the fragmentary facts recorded in
different countries to that date and postulated a theory that could reconcile the divergent viewpoints regarding dysenteries. In one of the first compendia later published on this syndrome (then associated with a number of reasonably well-established etiologies), Lutz’s work was characterized as “quite singular, since the ideas laid out by the author are those currently considered definitive regarding the etiology of dysentery.” Lutz believed the pathogenic role of amoebae to be “unquestionable” but he presumed that, in addition to this dysentery,

whose evolution is chronic, with the alternatives of a passing cure or exacerbations, frequently complicated by liver abscesses, there is room for another dysentery, epidemic, acute, causing diphtherial lesions in the intestines, and without ever occasioning liver abscesses. In a word, at that time Lutz had already foreglimpsed the now-recognized distinction between bacillary and amoebic dysentery. (Dopter, 1909, p.4)

The São Paulo doctor sent these observations to the *Centralblatt für Bakteriologie und Parasitenkunde* while in Hawaii serving as head of medical services at the Molokai leprosarium. He had in effect demonstrated that two morbid entities were being confused, and he established criteria for differentiating them. He showed that pathological changes known by such diverse names as ‘acute (or chronic) intestinal catarrh’, ulcerous enteritis, hepatic abscesses, and chronic or tropical dysentery all fit into the profile of morbidity caused by amoebae. He called this “enteritis with bloody stool” to distinguish it from dysentery itself, which manifested as an acute infectious disease capable of spreading across wide areas. This distinction would be proven in 1898 when the Japanese bacteriologist Shiga Kiyoshi (1870-1957) isolated the agent of bacillary dysentery (*Shigella dysenteriae*).

Lutz showed that amoebae were “true parasites” that lived inside and outside the human body but could tolerate only a limited temperature range. This was precisely one of the main problems in studying them. Once immobilized by the cold, they resembled other cells, especially in the feces, so rich in microorganisms. In order to observe amoebae at length, Lutz devised an apparatus that kept the microscope stage heated to a constant temperature (Lutz and Lutz, 1943). This enabled him to uncover their mechanisms for adaptation to human hosts, their prolonged persistence inside hepatic abscesses, and the presence of elements supplied by the host organism, especially red blood cells.
Despite both the prestige of the German periodical that published Lutz’s paper and its broad readership, many authors passed over the study, as Dopter noted in surprise (1909, p.4).

Quicker on the draw, Osler was in fact the first in the United States to describe amoebae, but involving only one case of chronic dysentery. The patient was a 29-year-old physician who had contracted the illness in Panama many years earlier and had suffered numerous attacks, consisting of fever, general malaise, and pain in the liver region. Although Osler observed numerous amoebae in the pus of a liver abscess and in the man’s feces, he concluded it was still early to incriminate these protozoans as the disease’s causative agent with any real certainty.14

It was actually Councilman who first detected amoebae in Osler’s patient. Together with Lafleur, Councilman then studied another fourteen cases. The paper they published in 1891 likewise failed to mention Lutz, although
his article had already come out. These authors knew German well, and their omission is even more startling given the pains they took to conduct a thoroughgoing inventory of the state of the art in that particular area of medical investigations.

The pathologists at Johns Hopkins Hospital endeavored to isolate and distinguish the microorganisms present in patients’ stools and intestinal tissue slices and also analyzed the tissue lesions produced by the parasitic amoebae. Based on clinical and pathogenic characteristics, they drew distinctions between dysenteries, differentiating the amoebic variety from the inflammatory and diphtherial varieties (Dopter, 1909, p.4-5).

Based on our current knowledge of protozoans, we do not have the same ability to classify and recognize the distinct species of amoebae as we have for bacilli. It is therefore impossible to state whether the amoebae found in fecal matter under certain conditions are or are not members of the same species.

We use the name Amœba dysenterice to refer to the microorganism first described by Lösch as Amoeba coli, since the latter term does not seem very characteristic, given that a good number of amoebae of various species may exist and that ... they are not located specifically in the colon. (Councilman & Lafleur, 1891, p.405)

In addition to suggesting that the human host contained different species of amoebae, some pathogenic and others not, these authors showed that amoebiasis was a disease characterized by “well-defined anatomical lesions displaying a degree of homogeneity.” They declared there to be no similarity between changes to tissues produced by bacteria or by amoebae. The latter reached the large intestine when a person ingested food or drink; they had no affect on the stomach or small intestine since conditions there were not favorable to their development (owing, for example, to the environment’s alkalinity). Ulcerations were believed to be caused by invasion of the mucous membrane. They detected amoebae in the lymphatic and blood vessels, leaving the impression that these reached the liver via such channels. They also often found amoebae in the veins of the liver and lungs “but there was no evidence that they [reached] these organs via the blood stream.” They further stated that “other organs do not suffer metastasis, which should occur were the amoebae to pass through the blood stream” (ibid, p.509, 512-4).

Following publication of Councilman and Lafleur’s paper – which Cox considers the most complete of all those produced in the late 19th century –
dysentery became a topic of note in the United States, and many doctors published reports on cases found in the areas where they worked. One of these was H. Harris,\textsuperscript{15} professor of pathology at Jefferson Medical College. In a paper dated 1898 (p.385-6), he cited Adolpho Lutz, recognizing that the latter had lodged “fair criticisms against our imprecise classifications of intestinal diseases.”

Other authors assumed a stance similar to Councilman and Lafleur’s. Dopter (1903, p.4-8) cites a long list of names,\textsuperscript{16} including one Brazilian, Francisco Fajardo, who – as we will see – had a close relationship with Adolpho Lutz in other realms of protozoology. The papers published by these authors would make a vital contribution to our understanding of the life cycle of the amoeba and its parasitism, but it would take some years for the differentiation between pathogenic and non-offensive forms to firmly establish itself.

Amoebae and cell from the margin of a liver abscess, stained and fixed by different techniques (Harris, 1898, fig. 11).
caused by a pathogenic species, *Amoeba dysenteriae*, which differed from other varieties found in the intestines of healthy individuals (Dopter, 1909, p.6-7). The German scientists Heinrich Iranaus Quincke and Ernst Roos (1893) reached the same conclusion. Although unable to correctly differentiate the amoebae (Martinez-Palomo, 1996), in a 1903 paper the protozoologist Fritz Schaudinn established the name of the pathogenic species that is still used today: *Entamoeba histolytica*. But it would be another ten years before Walker and Sellards distinguished it unequivocally from free-living amoebae in water, which did not produce dysentery (*Entamoeba coli*) (Faust, et al., 1975, p.85). Schaudinn conducted his experiments on prisoners at the Bilibid penitentiary in Manila, in the Philippines. Of the twenty men who ingested eggs of that species, seventeen became infected but none developed the disease. On the other hand, of the twenty volunteers who received capsules containing *Entamoeba histolytica* eggs, seventeen were infected by the first dose whereas it took one of them three inoculations. Only four of the eighteen who had the parasite got sick. The experiment showed that the organism could be pathogenic in some people and not produce symptoms in others. It further showed that asymptomatic carriers could transmit the pathogenic parasite to healthy people (Martinez-Palomo, 1996).

During an investigation conducted in Egypt in 1894, Kruse and Pasquale raised the hypothesis that “hot countries” were home to a kind of dysentery caused by a pathogenic species, *Amoeba dysenteriae*, which differed from other varieties found in the intestines of healthy individuals (Dopter, 1909, p.6-7). The German scientists Heinrich Iranaus Quincke and Ernst Roos (1893) reached the same conclusion. Although unable to correctly differentiate the amoebae (Martinez-Palomo, 1996), in a 1903 paper the protozoologist Fritz Schaudinn established the name of the pathogenic species that is still used today: *Entamoeba histolytica*. But it would be another ten years before Walker and Sellards distinguished it unequivocally from free-living amoebae in water, which did not produce dysentery (*Entamoeba coli*) (Faust, et al., 1975, p.85). Schaudinn conducted his experiments on prisoners at the Bilibid penitentiary in Manila, in the Philippines. Of the twenty men who ingested eggs of that species, seventeen became infected but none developed the disease. On the other hand, of the twenty volunteers who received capsules containing *Entamoeba histolytica* eggs, seventeen were infected by the first dose whereas it took one of them three inoculations. Only four of the eighteen who had the parasite got sick. The experiment showed that the organism could be pathogenic in some people and not produce symptoms in others. It further showed that asymptomatic carriers could transmit the pathogenic parasite to healthy people (Martinez-Palomo, 1996).

Following publication of Adolpho Lutz’s papers, other authors endeavored to draw a relationship between dysentery and bacteria, assigning amoebae a secondary, meaningless, or sometimes even beneficial role. One who took this line was Maggiora; he attributed the disease to the pyocyanic bacillus and to a colibacillus displaying an abnormal level of
virulence. Laveran also incriminated colibacilli. In 1895, based on case studies from Rome, Tivoli, Sienna, and even Alexandria, Celli and Fiocca concluded that a variety of this microorganism, which they called *Bacterium coli dysenteriae*, was responsible for dysentery. According to Bertrand and Baucher, dysentery was a polymicrobial infection associated with a number of different germs (septic *Vibrio*, pyocyanic bacilli, staphylococci, and colibacillus). In Saigon, Calmete incriminated the pyocyanic bacillus (Dopter, 1909, p.5-9).\(^\text{18}\)

The absence of amoebae in typical cases of dysentery, combined with their presence in healthy individuals or people with other diseases – facts pointed out by a number of authors – simply added to the confusion in this area of pathology.

As stated earlier, unification of these two lines of interpretation as proposed by Lutz in 1891 was justified nine years later by Shiga Kiyoshi (1898). He was a member of the new generation of bacteriologists who had recently joined the Institute for the Study of Infectious Diseases, inaugurated in Japan in 1892 under the direction of Kitasato Shibasaburo (1852-1931); he was also a brilliant investigator who had been one of the pillars of Koch’s group, alongside Friedrich A.J. Löffler (1852-1915), Georg T.A. Gaffky, and Emil von Behring (1854-1917). Kitasato transformed the new institute, known by the Japanese as Denken, into the main force behind transformation of medicine and public health in his country (Yoichiro, 1997).

In purifying feces from victims of a dysentery epidemic, Shiga isolated a bacillus similar to the coli and typhoid bacilli, one which did not ferment saccharides. Diagnostic tools then recently developed for use in bacteriology enabled him to recognize another important property of the microorganism: it was bound by the sera of dysentery patients but not by the sera of those suffering from other infections (Dopter, 1909, p.10-1).

Kruse (1900), in Germany, and Flexner (1901), in the United States, soon confirmed Shiga’s discovery, and dysenteries were differentiated in terms of etiology. There was one that took an acute, epidemic form, sweeping
especially through temperate countries, and another, an amoebic form, chronic in nature and endemic to the tropics. New differentiations soon followed; the form of dysentery caused by *Balantidium coli* as well as the one caused by spirilli were described (Le Dantec). The term ‘dysentery’ came to refer to a syndrome with various etiologies. “Thus the opinion formulated by Lutz, Councilman, and Lafleur in 1891-92 has now been proven,” wrote Dopter in 1909 (p.10-2), in one of the first treatises on this subject.

**Adolpho Lutz and yellow fever**

It is possible that Lutz had come into contact with yellow fever patients or individuals suspected of having the disease while practicing medicine in Limeira and in the city of São Paulo, but no such traces can be found in the documentation with which we have worked. His first documented contact with the disease took place in early 1889, in Campinas, when the city fell victim to a major epidemic that had serious impact not only on its citizens but also on public opinion around the country.

Campinas was one of the principal urban centers of Southeastern Brazil. “It competed and in many regards was actually tied with São Paulo, the capital,” in the words of Santos Filho and Novaes (1996, p.9), authors of the most thoroughgoing study on this yellow fever epidemic, which “destroyed [the city’s] vigor, paralyzed development, and crushed the city.”

Over the previous three decades, coffee had replaced the region’s sugarcane plantations, and Campinas had prospered as a dynamic commercial and financial center. Its plantation owners and businessmen held a good share of the wealth in the province of São Paulo, and they invested it in railway lines that encouraged the expansion of crops and of new urban centers in western São Paulo state, an expanding frontier for the country’s principal economic activity. Armed with a capitalist mentality that contrasted with the mentality of plantation owners in the decaying areas of the Paraíba Valley, Campinas coffee growers and businessmen invested capital in companies that provided public services and outfitted the city with the same improvements that had been transforming urban life in the capitals of the Empire and the provinces: gas lighting (1875), animal-powered tramlines (1879), and telephone lines (1884). Water and sewer services came only later, in 1891-92, largely in response to yellow fever.
Campinas became the hub of western São Paulo thanks to two railroads: the Companhia Paulista de Vias Férreas e Fluviais, founded in 1868, and the Companhia Mogiana de Estradas de Ferro. The first company inaugurated a line between Jundiaí and Campinas on 11 August 1872, four months after the second company had been founded (30 Mar. 1872). On 27 August 1875, Dom Pedro II and his entourage were aboard the train that opened the track connecting Campinas to Moji-Mirim, owned by the Mogiana (Santos Filho and Novaes, p.13-4).

The Brazilian census of 1872 informs us that 31,377 people resided in Campinas’ two parishes, Nossa Senhora da Conceição and Santa Cruz. Another source estimated that in 1871 the urban population was 10,000, while another 32,000 resided in the surrounding rural areas, 12,000 being freedmen and 20,000, slaves.19

Santos Filho and Novaes (p.13-6) provide a detailed description of the companies, urban facilities, and personalities that made Campinas “the main center of São Paulo’s rural aristocracy.” They write of the opulent mansions of the coffee barons, who even questioned whether São Paulo should remain the capital of the province. They name the retail and
wholesale businesses that supplied the entire western part of the province, with such products as iron and bronze castings, agricultural machinery, ornamental goods, and locally made construction materials.

Campinas had a cathedral, theaters, a skating rink, a hippodrome, clubs, and three large hotels: the Europa, the França, and the Grande Hotel Campineiro. It also boasted six schools (including one called Culto à Ciência, or Homage to Science) that drew students from a broad region of influence and where some important proponents of the Republic taught. The *Gazeta de Campinas* (1869) was the major newspaper but others in circulation included *Sensitiva* (1873), *Mocidade* (1874), *Diário de Campinas* (1875), and *Correio de Campinas* (1885).

Santa Casa de Misericórdia Hospital was inaugurated in 1876; two years later, Beneficiência Portuguesa Hospital was founded. Santos Filho and Novaes (p.21) also write of hospitals for smallpox victims and lepers, both funded by the Municipal Chamber. During the crisis of 1889, special infirmaries or lazarettos would be hastily created to isolate those stricken with yellow fever.

The conviction that this disease “could not move into the mountains” (Lutz, 1930) and that its sole habitat was the overcrowded urban centers of the hot and humid coastal plains had been shot down in 1876, when Dr. Valentin José da Silveira Lopes, future Viscount of São Valentin, had diagnosed two Portuguese with yellow fever, both having just arrived from Rio de Janeiro. They had checked into a small hospital of which Lopes was one of the owners. The event had no major repercussions because these were imported cases, but two months later (30 Apr. 1876), Lopes stated to the *Gazeta de Campinas* that another eight people had come down with yellow fever. Since all of them lived near the Companhia Paulista de Vias Férreas e Fluviais railroad station, in houses contiguous with land where train cars filled with coal and merchandise from the port of Santos stood parked, Lopes presumed these wagons had brought the germs from infected ships. His diagnosis alarmed the population and incited much controversy not only in Campinas but likewise among physicians, politicians, and journalists from the capitals of the province and of the Empire, since very few believed...
yellow fever could manifest itself so far from the coast (Santos Filho and Novaes, p.23-4).

Lopes submitted a special communication about this to the Imperial Academy of Medicine; in its 5 June 1876 session, the note was read and commented on by Antônio Correia de Sousa Costa (1834-89), professor of hygiene at Rio’s School of Medicine and president of the Central Board of Public Hygiene. His expert opinion was that the cases verified in Campinas were indeed yellow fever, and this was published in the *Anais Brasilienses de Medicina* (“Treatise on yellow fever in Campinas” [in Port.]). Lopes was elected a corresponding member of the Academy on 28 August 1876.21

Dr. Valentin José da Silveira Lopes’ viewpoint was endorsed by Antônio Felício dos Santos (1843-1931), Júlio Rodrigues de Moura (1839-92), João Vicente Torres Homem (1837-87) – one of the most renowned clinicians in the city of Rio de Janeiro and professor of medical practice at its School of Medicine – and Carlos Ferreira de Souza Fernandes (1829-88), author of “Yellow fever in Campinas” [in Port.] (*Anais Brasilienses de Medicina*, v.28, p.1876-7). Another ally was Augusto César de Miranda Azevedo (1851-1907), future deputy for São Paulo to the constitutional assembly (1891) and then a resident of Rio de Janeiro.

But the hypothesis that yellow fever was present in Campinas was contested by a number of professors from the Rio de Janeiro School of Medicine. Among these was José Martins da Cruz Jobim (1802-78), for many years the school’s director, professor of forensic medicine, one of the founders of the Academy of Medicine, and also a senator of the Empire. His ideas concerning the etiology of the disease underpinned the position taken by Antônio de Souza Campos (1845-1918), the first native of Campinas to earn his medical degree (1872): the latter argued that it would be impossible to transport the element which produced the disease (that is, the “infected” air of Santos, which impregnated train cargos), since the trip across the highlands would be through the mountains’ cold, healthier air (Santos Filho and Novaes, p.26).

The controversy did not dissuade rural inhabitants from their illusion that they were safe from yellow fever, “until the facts came to prove the contrary,” as Adolpho Lutz would later state (1930), having believed Lopes’ diagnosis to be correct in 1876.

The 1889 epidemic, the first to sweep through Campinas, hit the population hard and had national repercussions.
When it broke out, the population was terrified and whoever could, fled.

The plantation owners ... moved to their rural landholdings or to São Paulo ... Entire families abandoned their homes and their belongings. Those who couldn’t get their hands on a carriage or a horse went by foot, seeking refuge at nearby farms or cities. Houses, shops, grocery stores, workshops, hotels were closed ... Pharmacies could not keep up with the dispatch of prescriptions” (ibid., p.36-7).

The physician José Maria Teixeira (1854-95), about whom we will speak shortly, author of *A epidemia de Campinas em 1889* (Rio de Janeiro, 1889), was to register these words: “The city was abandoned and almost deserted! Long, straight streets with hundreds of houses closed up and not a single passerby” (cited in Simões, 1897, p.23). The remaining residents rushed to the church every day to “pray publicly *ad petendum pluviam,*” according to another witness to the crisis (ibid.), in hopes that God’s tears would wash away the sinister miasma hanging over the city.

According to a report published in the *Freie Presse: Zeitung für Deutsche in Brasilien* (Free press: a newspaper for Germans in Brazil), the “business of thieves” became quite profitable in this ghost town. One of those who fled, Mr. Felipe José, discovered upon his return that neither gold, nor silk, nor watches remained in his shop. Another businessman who had sought refuge in São Paulo came back to find his store stripped clean. “Many robberies of this type will undoubtedly come to light as the fugitives of the epidemic return to Campinas.”

In *Reminiscências* (1930), Lutz makes mention of the general opinion that three-quarters of the city’s 20,000 inhabitants had abandoned it, “leaving behind mostly men who held jobs; however, many people returned too soon and ended up contracting the infection. Of those who were not immune and lived inside the city, almost all were infected.”

According to Santos Filho and Novaes (1996) and Simões (1897), the person who carried the disease to Campinas was the Swiss woman Rosa Beck, unmarried, 24 years old, having recently arrived in Brazil with the intention of finding employment as a French teacher. It is not clear where she disembarked nor where she caught the disease – Santos or, more likely, Rio de Janeiro. She died at 2:00 a.m. on 10 February 1889. She had taken up lodging with compatriots of hers, in the same building where they ran a bakery called Padaria Suíça. From there yellow fever spread through the
rest of the town. The second death was a nine-year-old boy who shopped at
the bakery. Two days prior to the boy’s death, the attending physician, Eduardo Guimarães, “for the good of the public health,” published the news that
he was caring for a patient with a critical case of yellow fever. Such was the
incredulity that he summoned “eleven distinguished colleagues” to corroborate
his diagnosis (Correio de Campinas, 23 Feb. 1889, cited in Santos Filho and
Novaes, p.41). The fact that the victim was from Campinas and had never
left the city was frightening: some as yet unknown local cause was responsible
for his yellow fever. “Like olive oil spilled on a blotter” (Simões, 1897, p.21),
the disease spread during March; it then let up for a few days, worsened in
April (the “month of horror”), subsided again, and in early May intensified
once more, from then on waning until dying out in late June.22

According to Adolpho Lutz (1930), overall mortality was estimated at
about 2,000, “including those who were infected and passed away in other
places.” The physician Ângelo Simões (1897) says that yellow fever attacked
“over 2,000 people out of a population of 3,000 (since that is how many
remained in the city, at most), resulting in 1,200 deaths, a number which
I have most carefully verified.”

The wealthier people, who could afford to leave the city, were less
affected. Most of the victims were Brazilians with no immunity to the
disease, followed by Italians, Portuguese, and immigrants of other
nationalities.

Adolpho Lutz in Campinas

In March 1889, the Municipal Chamber converted into lazarettos some
residences in the neighborhood of Guanabara, then located a distance away
from downtown. Special infirmaries for yellow fever patients were likewise
set up in the Circolo Italiani Uniti’s building, at the Correia de Melo muni-
cipal school, at the headquarters of the Sociedade Portuguesa de
Beneficiência, and also at Santa Casa de Misericórdia Hospital. In 1890,
the Lazareto de Fundão would be inaugurated in the vicinity of the
cemetery; it was later transformed into an isolation hospital, with two
wooden pavilions for patients, lodgings for the physician and caretaker,
and facilities for a pharmacy, sterilizer, and so on (Lapa, 1996, p.261).

Santos Filho and Novaes (p.44-6) described the hygiene measures
adopted by the Chamber:23
Public thoroughfares were coated with pitch and watered down almost every evening, while barrels of tar burned day and night on the corners of the main streets. Bonfires of fragrant herbs were lit. It was believed that the smoke ... would clean the air of harmful miasmas ... All furniture and objects found in the rooms of those who perished from the fever were destroyed and burned ... To keep the city’s inhabitants safe from contagion, the corpses were buried at night.24

Since it was mostly the poor who had remained in town, the Chamber enacted measures to help this population, vulnerable not just to the disease but also to hunger and neglect. It paid for public vehicles to transport doctors who made house calls for free, distributed food and clothing, and authorized pharmacies to provide medicine at the expense of the Municipal Chamber, whose president, José Paulino Nogueira (1853-1915), fell sick with yellow fever too.

In early April, when the situation worsened, Nogueira telegraphed the president of the province, Pedro Vicente de Azevedo (1844-1902),25 requesting that doctors be sent urgently since most of them who lived in the town – about twenty – had left with their families, and the few who remained were stretched thin.26 In Santos on assignment to the government of São Paulo, Dr. Francisco Marques de Araújo Góis headed to Campinas. He arrived on 5 April. Astonished by the extent of the crisis and the precarious aid provided to its victims, he endorsed the request for medical reinforcements. Towards the end of that month, he returned to Campinas as head of the Provincial Aid Commission, which comprised some thirty-five people, including physicians, fifth-and sixth-year students from the Rio de Janeiro School of Medicine, pharmacists, disinfectors, and staff who would provide other services.27

Adolpho Lutz was a member of the commission, together with Drs. Claro Marcondes Homem de Melo (1866-1924), Irineu de Sousa Brito Junior, Aristides Franco de Meireles, Bráulio Gomes, and Luis Felipe Jardim (the last two came down with yellow fever but survived). In Reminiscências (p.128), Lutz recalls the weeks he spent in that city:

In 1889, when I was urgently called from São Paulo to Campinas, where there were no more doctors, I encountered a pandemic rightfully said to be yellow fever ... After four to five weeks, colleagues had arrived from Rio de Janeiro, and the epidemic was waning in Campinas.

Lutz makes mention of two other medical commissions, one sent by the Ministry of the Empire and the other by the Comissão de Imprensa
Fluminense (Rio de Janeiro State Press Commission), the latter comprising several Rio de Janeiro newspapers (Gazeta de Notícias, O Paiz, and Jornal do Commercio, among others).

The commission sent by the central government, then under the control of the Conservative Party, was headed by Dr. José Maria Teixeira (1854-95), mentioned earlier, who was professor at the Rio de Janeiro School of Medicine in medical subjects and pharmacy. It seems the first members of the group reached Campinas on 7 April, bearing 100 beds and an “ambulance” for which the pharmacist Joaquim T. Soares da Câmara was responsible. Besides Teixeira, other members included the physicians Eufrásio José da Cunha, Francisco Custório Pereira de Barros, Francisco Corrêa Dutra, João de Deus da Cunha Pinto, Fernando de Barros, and Luís Manuel Pinto Neto (Santos Filho and Novaes, p.54-5). The Imprensa Fluminense sent the physicians Clemente Miguel da Cunha Ferreira (1857-1947), as head, and João Batista da Mota de Azevedo Correia (1854-?). They arrived in Campinas on 20 April, bringing with them a substantial quantity of medicine, clothing, and foodstuffs that had been donated to the victims of the Campinas epidemic at events held in Rio de Janeiro,
including a charity party, a horse race, and a concert. They stayed in town until 28 May, when the epidemic was already on the ebb. In addition to the physicians on assignment, Drs. Baltasar Vieira de Melo and Domingos José Freire Júnior came to Campinas of their own accord, the first having signed two death certificates (Santos Filho and Novaes, p.58-9, 61, 63).

These physicians, especially those from the São Paulo commission, made daily house calls to the sick in the city’s four districts. They inspected swamplands, latrines, wells, and deposits of used water, closing some down and spreading large amounts of antiseptic agents (mainly ferrous preparations, sulphurous gas, and carbolic acid) over all possible sources of yellow fever, including victims’ caskets. The Municipal Chamber even threatened to break into the houses of those who had fled in order to disinfect them (Santos Filho and Novaes, p.45).

“A great deal of money was spent (not only in Campinas but in other towns as well) on disinfectants that were poured into latrines, and drinking water was brought in from other places, while mosquitoes were allowed to breed as they liked,” Lutz would later write, when views on transmission of the disease had already changed radically (Lutz, 1930, p.142).

At that time, water was incriminated as a possible etiological agent. As water lines had yet to be laid in Campinas, starting on 17 April, the Companhia Paulista de Estradas de Ferro shipped 24,000 liters per day from Valinhos to the tanks that fed the city’s public water fountains, and the Municipal Chamber provided water trucks to transport it to outlying areas. According to Santos Filho and Novaes, the São Paulo commission signed twenty-seven death certificates in April and May, a very small number in comparison with the overall number of lives claimed by the epidemic. This total displayed the signatures of only four physicians, most (i.e., fifteen) signed by Adolpho Lutz, “who attended the greatest number of patients.” The cited authors argue that the first number was so low because those doctors’ job was “essentially prevention and disinfection.” They also note that the number of patients admitted to hospitals was low as compared to those treated at home; people were terrified of hospitals and, as in many other towns, regarded them as “death chambers.” Consequently, some doctors made “about ninety house calls a day” (Santos Filho and Novaes, p.10).

As the epidemic wound down, only Góis, Homem de Melo, and two disinfectors remained in Campinas. Of the group sent by the central government, those who stayed behind were Correia Dutra (who had taken
over as head of the commission following the return of José Maria Teixeira) and Eufrásio José da Cunha, as well as the academics Vito Pacheco Leão and Alberto de Castro Meneses (Santos Filho and Novaes, p.57). They were the last to leave the town, in late June or early July. The district judge reopened the court that month, and on 15 July classes resumed. Campinas “arose from the ashes, and the risen phoenix was purposely chosen as the city’s symbol”.31

Yellow fever spreads through Southeastern Brazil

In the modern history of yellow fever in Brazil [wrote Adolpho Lutz (1930)] the most ill-fated year was 1889, when it hardly ever rained for three months during the hot season and the temperature reached highs not seen other years. In Rio and in Santos, severe and rapidly fatal pyrexias appeared in epidemic form. Never seen before, these fevers were generally classified as “pernicious fits” or, more rarely, as “fulminating yellow fever.” Only after much discussion was it recognized that the fevers were caused solely by the high temperatures – called heat stroke in the vernacular.

That same summer ... in Santos, where there had been no yellow fever for ten years, the epidemic was very severe. In fifteen days, the German Club lost one-fourth of its members.

In the month of March alone, the city saw 580 deaths due to yellow fever, according to Santos Filho and Novaes (p.38), or 650 according to Freire (1890a), 500 of whom were recent immigrants. In Rio de Janeiro, 2,408 people perished in 1889 (1,926 immigrants), according to this same source. The first authors put the number of deaths at 2,156. The figure rises if we include deaths in Niterói, across the bay from Rio: 177, of whom 63 were immigrants.

Lutz (1930) makes mention of the appearance of yellow fever in Rio Claro and Belém do Descalvado. Other cities invaded by the disease were Cataguases (MG); Barra Mansa, Vassouras, and Resende (Rio de Janeiro); Desengano (Espírito Santo); and Serraria (Rio de Janeiro), once again, part of the population in these places fled, including physicians.32

The outbreaks in these cities did away once and for all with the notion that yellow fever was specific to intertropical coastal regions. It was not without reason that physicians searched so persistently for its chemical or biological agent in the holds of ships, comparing these to the unhealthy housing of port cities.
Accompanying the tide of immigration that swept into Brazil following the collapse of slavery and the upsurge in economic prosperity during the passage from monarchy to Republic, the spread of yellow fever placed on the agenda a controversy that would only grow more heated with each successive epidemic peak in the 1890s: was this “American typhus” an illness deeply entrenched in Brazil or a constantly imported ‘pestilence’, a disease that could be yanked out by the roots through drastic environmental intervention, or a contagious illness that would only respond to palliative measures or attenuating agents?

“Back then,” wrote Lutz (1930), “many people still believed in direct contagion, and black vomit was deemed a kind of essence of yellow fever.” No other infectious disease was so easily transmitted via inanimate bodies, stated João Batista de Lacerda. Dr. Antônio Caio da Silva Prado, president of the province of Ceará (1888-1889), represented the paradigmatic case
of this viewpoint: he died on 25 May 1889, after receiving letters and newspapers from infected Campinas. This and similar cases would often be posited as unquestionable proof that the yellow fever germ was transported by a wide gamut of objects: the infective matter clung to clothing, to coal, to fruit peels, to stationery; entering the holds of ships and train cars, it could travel great distances (Lacerda, op. cit., p.16-30; Brazil-Mé
dico, 8 Jun. 1899, p.212-4).

Proponents of the idea that yellow fever was transmitted by mosquitoes would later have to work hard to refute the “monolith that is constantly hurled against the new doctrine” – the case of Caio Prado and similar others (Quinto Congresso, p.36, 150).

Adolpho Lutz’s Reminiscências was written when mosquito transmission had already become established. For him, propagation of the disease after being ‘exported’ from Rio de Janeiro to Campinas could be explained by the following set of factors: Stegomyia fasciata had been disseminated, and infected people were moving into or through regions inhabited by people lacking immunity to the disease.

Campinas saw outbreaks in 1890, 1892, 1896, and 1897. The worst took place in 1896, but “it no longer elicited the same manifestation of solidarity. Everyone accepted things. There was no longer a stampede of physicians or any charity drives” (Santos Filho and Novaes, p.35, 10).

For Adolpho Lutz (1930), infected mosquitoes carried by rail lines accounted for the 1889 outbreaks in Rio Claro and Belém do Desalvado, and also for a number of isolated cases among mail carriers and railroad workers, including “a lady in Valinhos, who was married to the stationmaster and lived in the stationhouse. She had never been to Campinas while yellow fever was there.” In subsequent years, in the words of Lutz, the disease:

invaded the state of São Paulo, primarily following the Paulista railway. It ravaged Limeira, where I had maintained a private practice for five years without ever seeing Stegomyia but it spared Araras and Jundiaí, where the villages are located quite far from the train stations. After the Paulista [railway], the Mogiana was invaded, and only much later came the Sorocabana’s turn. What unquestionably kept it from spreading faster was that the numerous cases among those who fled the centers of epidemics could not spread the yellow fever, without the prior or simultaneous import of Stegomyia.

Back when these outbreaks were erupting, the explanations that were viewed as the most innovative no longer associated the disease with miasmas
inseparably tied to certain environments; instead, it was believed to be associated with microorganisms that could travel through different environments and from person to person, airborne or by other means. The overriding belief was that the germ underwent some kind of transformation in the external environment before it became infective, yet only in some geographical zones and during a certain season of the year.

In search of the yellow fever microbe

When Adolpho Lutz began his career in the early 1880s, the issue of microbes was kindling heated discussions over the disease that was then Brazil’s number-one public health problem. December was the month when the merciless sun of the ‘muggy season’ beat down on the streets and houses of Rio de Janeiro, or when these were submerged under torrential rains. It was in that month in 1879, that Dr. Domingos José Freire, who held the chair in organic chemistry at Rio’s School of Medicine, announced in the papers his discovery of a microbe which he believed to be the cause of yellow fever. In the “humors” of sick people he had found granules and Vibrio that took the form of black corpuscles as they developed. He contended that the detritus and countless spores released by them lent the patient’s vomit its characteristic black color (Gazeta de Noticias, 23 Feb. and 29 Feb. 1880).

In the first half of 1883, using techniques recently discovered by Louis Pasteur, Freire developed a vaccine against the disease by attenuating the microscopic algae that he called Cryptococcus xanthogenicus. The only other prophylactic measure of this kind then applicable in fighting human diseases was the smallpox vaccine. Pasteur’s achievements in this area were still limited to vaccines against chicken cholera (1880) and anthrax or haematic carbuncle (1881). Pasteur’s debut in the realm of human pathologies, with the anti-rabies vaccine, would involve complex social and technical constraints overcome only in 1886, as Geison (1995), Debré (1995), Salomon-Bayet (1986), and others have shown.

A crisis involving the smallpox vaccine in Bahia catapulted Freire to the presidency of the Central Board of Public Hygiene in late 1883, facilitating dissemination of his vaccine against yellow fever among Rio de Janeiro’s tenement dwellers. He enjoyed a surprisingly warm welcome among immigrants and then among native-born Brazilians, not only
because the disease had incited such fear but also thanks to support from republicans and abolitionists with whom he had ties.

Reactions were contradictory both in the press and at the Imperial Academy of Medicine, especially after the vaccine received the tacit support of Emperor Dom Pedro II and the enthusiastic backing of one of Pasteur’s ‘disciples’, Claude Rebourgeon, a French veterinarian hired by the Brazilian government to begin production of an animal vaccine against smallpox in
Rio Grande do Sul. Rebourseon presented Freire’s discovery before the academies of medicine and of science in Paris (Freire and Rebourseon, 1884). Important figures in French medicine responded favorably, including the pathologist Alfred Vulpian and the veterinarian Henry Bouley.

It should be mentioned in passing that both Pasteur’s 1881 efforts to identify the yellow fever microbe as well as Emperor Dom Pedro II’s efforts to entice him to Brazil to decipher the disease met with failure (Vallery-Radot, 1951).

Between 1883 and 1894, at least 12,329 immigrants and native-born Brazilians in Rio and other cities around the country were inoculated with Freire’s vaccine, which reached Puerto Rico, Jamaica, the Guianas, and other French colonies too (Benchimol, 1999, p.119-68). The dissemination of this invention was partly due to the increasingly complex web of relations entwining the Brazilian bacteriologist with a number of other actors: colonial and business interests, other microbe hunters, medical and scientific associations, and the authors of treatises systematizing achievements in microbiology.

During his stay in Europe between December 1886 and July 1887, Freire submitted two special communications to the Academy of Sciences in Paris, co-authored with Rebourseon and with Paul Gibier, a researcher from the Natural History Museum in Paris (Freire, Gibier, and Rebourseon, 1887a and b). This and other events in the French capital made a strong impact in the Brazilian capital, and upon his return Freire was received as a hero of Brazilian science by students and professors at technical schools and universities in Rio de Janeiro, Minas Gerais, and São Paulo, by journalists from a variety of periodicals, and by militant members of republican clubs and abolitionist societies. Weeks later he traveled to Washington to participate in the 9th International Medical Congress, which approved a resolution recommending his vaccine for all countries afflicted with yellow fever.

A measure of the vaccine’s social and geographic reach is apparent in the quantity and quality of the names that certified Freire’s statistical data or that reported on post-vaccine symptoms, endorsing the immunizing action of his prophylactic liquid; it is further reflected in the number of physicians and authorities involved in its dissemination.

During 1883-84, a handful of people from Vassouras were vaccinated. On 6 February 1889, when the city’s Municipal Chamber met to decide
what means should be used to keep the disease from invading once again (the city had already been hit twice), the decision to employ the vaccine was unanimous. The terms of the letter to Freire show how far his reputation had come. They entreated him to give the frightened population access to “individual means of prevention, unquestionably more reliable than general means, and whose discovery and popularization are due to ... your immeasurable talent, [as you knew how] to earn not only the blessings of the fatherland ... but likewise an eminent place among humanity’s benefactors.”

Joaquim Caminhoá (son), Freire’s assistant, vaccinated 199 people there. The appeals sent by the Chambers of Pomba, Macaé, and Niterói were no different. The first city requested a shipment of the vaccine liquid in June, after the disease had struck neighboring Cataguases (MG), where vaccination was performed by Dr. Araújo Lima. In Macaé, Dr. Carneiro Mattoso, physician for the Chamber and public hygiene delegate, undertook the task. In Niterói, 163 vaccinations were administered, a good share by Freire himself, right inside the Chambers in the presence of its president, Prosper David, and of the physician Victor David and the city aldermen. The hygiene inspector for the province of Rio de Janeiro, Dr. Henrique Baptista, was responsible for going from residence to residence. At the end of the campaign, the Chamber offered use of one of its rooms so the scientist could continue his “humanitarian work” throughout 1889. In Juiz de Fora and Serraria (MG), a local physician, Dr. Avelar Andrade, was responsible for vaccinating 30 and 50 people, respectively. The 54 inoculations in Resende (Rio de Janeiro), all involving native-born Brazilians, were also performed at the initiative of two local physicians, Drs. Gustavo Gomes Jardim and Carlos Augusto de Oliveira e Silva. In Desengano (Espírito Santo), the epidemic was combated by Dr. Henrique Baptista, with two other physicians assisting: Rodrigues Guião, public hygiene delegate, and Edmundo Lacerda, local clinician. Of the city’s 425 inhabitants, 102 received what was allegedly an ‘attenuated yellow fever virus’ (24%).

The disease reached Santos. At the Municipal Chambers, under the eyes of the town’s most prominent citizens, 133 people were inoculated in February 1889 by Dr. Barata Ribeiro, professor from Rio’s School of Medicine.

On 9 March, José Paulino Nogueira (mentioned earlier), president of the Campinas Municipal Chamber, asked that the Inspector of Public
Angelo Agostini uses humor to decry water shortages, which became acute in the Summer months, yellow fever season. Yellow Fever observes a discussion of “microbe vaccination” between the president of the Board of Hygiene (Freire) and the health commission. Agostini makes fun of the precautions against contagion from Brazil taken by neighboring countries on the Rio de la Plata. Passengers are sprayed with phenol and...
Labarraque’s solution and left to dry on aromatic plants and disinfectants. Prisoners in shackles and those with death sentences open letters from Brazil and show them to their addressees from a distance. When a northern wind blows, alarm bells ring so inhabitants can run for cover. They jump over bonfires to purify themselves of miasmas (Revista Ilustrada, 1889, year 8, n.362, p.4-5. Arquivo Geral da Cidade do Rio de Janeiro).
Hygiene for the Province of São Paulo send, “as urgently as possible, a quantity of vaccine fluid against yellow fever, which has been requested by some municipalities” (Santos Filho and Novaes, p.46).

Campinas was site of the largest number of vaccinations: 651. This was because the public health crisis there was quite serious and the city lacked efficient resources for treating victims, but most certainly also because of the support Freire enjoyed among the city’s republicans and liberals. He was there with Clemente Ferreira and Azevedo Correa, the physicians on the commission sent by the Imprensa Fluminense. Records in fact indicate that republicans held a banquet in the physicians’ honor in June 1889. At the height of the epidemic, vaccinations were performed by Soares da Camara, a pharmacist assigned by the central government, and mostly by Dr. Angelo Simões (1860-1907), clinical director of Santa Casa de Misericórdia and a physician at Benficiência Portuguesa Hospital; in the opinion of Santos Filho and Novaes (p.81-2), he was a “preeminent figure” in Campinas medicine in the 1880s. Simões was to stand among the most faithful followers of Freire. His data supporting the vaccine were endorsed by the president of the Chamber, the police commissioner, a priest, a canon, the director of the newspaper Correio de Campinas, and the editors of the Gazeta de Campinas and Diário de Campinas.40

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Source: Freire (1896).
From December 1888 through June 1889, when yellow fever spread throughout these rural parts – many affected for the first time – a total of 3,576 people were vaccinated, 988 of whom were immigrants and 2,537, Brazilian (70.94%). Average mortality among those vaccinated was 0.79%, a figure that fell to zero in Santos, Resende, Serraria, and Cataguases. “Given these clear successes ... who would dare to doubt the efficaciousness of our prophylactic measure and the truth of the fruitful doctrines formulated by our eminent master, Pasteur?” asked Freire (op. cit., p.22, 25).

The more the urban middle layers accepted his new invention, the more he was vulnerable to criticism from his adversaries, since the statistical gap was widening between the vaccinated population – native-born, blacks, and immigrants considered ‘acclimated’ and thus immune to yellow fever – and the population still susceptible to the disease, made up mostly of recent immigrants. Changes in the social composition of those vaccinated reflect changes in how the vaccine was distributed. During an era of heightening skepticism not only about yellow fever remedies but also about the feasibility of sanitizing the Brazilian capital, this biological product became a most welcome component of the doctor/patient relationship and of relations between philanthropical institutions and the needy.

There is no way of knowing Adolpho Lutz’s opinion of Freire’s vaccine. It is certain that he witnessed its use in Campinas, and all indications are that he began analyzing the question, with his typical painstaking rigor, in an unsigned article attributed to him by the organizers of his archive, his children Bertha and Gualter Adolpho Lutz. “On the question of the efficaciousness of vaccination against yellow fever” [in Port.] would appear to be the first article in a series, but we were unable to locate any subsequent editions of the periodical in which it was published and therefore unable to ascertain whether Lutz continued the analysis initiated in São Paulo on 11 May 1889.

The article came out in the Freie Presse: Zeitung für Deutsche in Brasilien, a periodical founded that same year and put out three times a week. It had representatives not only in Campinas (Guilherme Trippe, resident and/or with a business at 99 Lusitana Street) but in nine other cities in São Paulo too (Araras, Botucatu, Piracicaba, Pirassununga, Ribeirão Preto, Rio Claro, Santos, São Carlos do Pinhal, and Sorocaba), as well as Curitiba, Joinville, and Leipzig, home to Germany’s major publishing houses.
In 1889, Freire and his allies endeavored to convince the central government to back generalized use of the vaccine. On several occasions, in an effort to lend legitimacy to a final test on the entire population, allusion was made to the resolution approved in Washington. Vaccination was pushed heavily by the abolitionist and republican movements, its organized groups, and its broad network of supporters. The backing of Rio’s Society of Medicine and Surgery did not reflect the authorities’ favorable acceptance of the vaccine so much as doctors’ adoption of it in their contact with patients.

The Proclamation of the Republic in November of that year coincided with the outbreak of another yellow fever epidemic, and during the new government’s negotiations over the federalization of health services, the vaccine issue led to creation of a new governmental institution: the Domingos Freire Bacteriological Institute. The microorganism attenuated by Freire was, however, not the only one incriminated as a yellow fever agent. The transition from monarchy to republic actually coincided with a transition between two cycles of theories about the etiology of the disease, a transition not limited to Brazil and in no way related to the processes leading up to a regime change there. Nevertheless, the bacteriological institutes that came to life in Rio de Janeiro and in São Paulo during the early years of the new regime were to take stands on opposite sides of this issue.

Manoel Carmona y Valle, a Mexican, was one of the Brazilian bacteriologist’s opponents; using a fungus he found in sick people’s urine, he developed a vaccine that was put to wide use in his native country. In Cuba, Carlos Juan Finlay used mosquitoes infected by yellow fever sufferers as live immunizers against the disease; he suggested an alternative to Freire’s alga: the Micrococcus tetragenus.

In Brazil, Freire’s chief opponent was João Batista de Lacerda, who served as director of the Museu Nacional in Rio de Janeiro from 1895 through 1915. In 1883, when Freire was finalizing preparation of the vaccine, Lacerda incriminated Fungus febris flavae as the true yellow fever agent. The latter microorganism and other, similar ones then described had one characteristic in common: polymorphism, that is, the ability to change shape and function in response to their environment, above all to climatological factors. Zoologists, botanists, and bacteriologists had differing opinions on the matter. Pasteur and Koch felt that polymorphism occurred only in the absence of rigorous experimental procedures and was incompatible with
the notion of etiological specificity (to put it simpler, with the notion that one single microorganism could be the agent of each disease), but many investigators recognized this property in the fungi, algae, and bacteria they were studying. The issue had to do with the still problematic classification of these “infinitesimal” entities. The generic term ‘microbe’ had in fact just been coined, precisely with the purpose of addressing the confusing taxonomic categories then being used in scientific texts, which complicated discussions of germ theory among non-specialists, including physicians and hygienists (Benchimol, 1999, p.191-200).

Polymorphism was the cement Lacerda used when drawing up his most comprehensive theory on “The pathogenic microbe of yellow fever” [in Port.], read before the National Academy of Medicine and the Panamerican Medical Congress in 1892-93. All descriptions presented up until then only accounted for different phases or forms of an extremely protean fungus.

The cycle of discoveries of these microplants began to ebb when a number of microbe hunters were converted to the hypothesis apparently formulated by Robert Koch that yellow fever was produced by a bacillus similar to the cholera bacillus, discovered by him in 1883 in Egypt and in India. His reasoning was that the prime symptom of yellow fever – so-called black vomit – was also located in the intestine.

At the same time that Lacerda was defending the ever-morphing fungus, George Sternberg, chairman of the American Public Health Association and Surgeon General of the United States, was conducting refutative fieldwork and laboratory research into the theories and vaccines then in fashion in the Americas (United States Marine Hospital Service, 1890). He was also searching for evidence in support of a microorganism similar to the Vibrio comma. He wiped the slate clean in a field then pervaded by fungi and algae, opening the way for bacilli to compete in the 1890s for a place as the causative agent of yellow fever.48
The Pasteur Institute, which had been prudently holding back, corroborated the U.S. investigation (*Annales de l’Institut Pasteur*, no. 4, 25 Apr. 1890, p.253), and a large part of the international scientific community saw this as proof that the South Americans had failed in their attempts to isolate the microbe and produce an efficacious vaccine. Yet other sources show that Sternberg was so inept in conducting his research in Rio de Janeiro that he helped strengthen Freire’s prestige among Brazilian nationalists, positivists, and republicans.

One of the microbe hunters trying to prove Koch’s hypothesis was Felix Le Dantec. A physician with the French navy, he had been in Cayenne in 1887 during an epidemic of *vomito*. The culture from this organic liquid provided him with three varieties of bacilli, which he rediscovered in stomach mucous and in the large intestine. So he raised the question: “Is yellow fever a microbial disease located in the stomach? Are these stomach microbes merely by-products?” Le Dantec knew he had not complied with Koch’s postulates for proving a causal relation between the microbial agent and the disease, so he limited himself to affirming “the substantial role played by the digestive tract in the evolution of yellow fever” and to declaring that this in itself afforded “valuable indications regarding treatment” (cited in Bérenger-Féraud, 1890, p.733).

On 15 December 1892, Le Dantec took office as director of the Bacteriological Institute of São Paulo, one of the divisions of the state’s Sanitation Service inaugurated six months earlier. His name had been suggested by Pasteur himself, through the French scientist Henrique Gorceix, director of the Ouro Preto School of Mines. Le Dantec promised to implement an ambitious program inaugurating bacteriology in São Paulo; however, less than a year later (5 Apr. 1893), he returned to Europe “without having done anything” – Cesário Mota Junior, secretary of the interior, observed caustically – “other than making some preparations on the topic of yellow fever, which he took with him when he went, leaving the impression that this was his only intention in coming to Brazil” (Lemos, p.16-9).
When Le Dantec returned to France, Adolfo Lutz, appointed under-director on 18 March 1893, took charge of the small team comprising three aids and two helpers who were to assist him in tackling some demanding missions:

the study of microscopy and bacteriology in general and, particularly, as regards the etiology of the state’s most common epidemics, endemic diseases, and epizooties; the preparation and packaging of the products needed for preventive vaccination and any therapeutic applications that may be indicated; the microscopic examinations needed to clarify clinical diagnoses. (Lemos, 1954, p.16)

Sworn into office only on 18 September 1895, Lutz remained in this position for 15 years, until transferring to the Oswaldo Cruz Institute in November 1908. With the aid of his assistants, he carried out valuable research on the infectious diseases that were endemic to the state or erupted in the form of epidemics. He also faced harsh opposition from a majority of those within the medical field and from other social actors.

Yellow fever was one of his topics of study, guided by the conviction that the causative organism would “quite likely” be found in the digestive tract.

At first Lutz followed in his predecessor’s footsteps. Histological and bacteriological research from 1893, which had used material collected during the autopsies of five yellow fever victims, led him several times to “a bacillus of the gastrointestinal tract, which [seemed] to be the one that Le Dantec
isolated earlier from black vomit in this same laboratory.” Lutz and his aids found very similar types in the stomachs and intestines of corpses; they cultured these organisms and analyzed them at length in an attempt to differentiate them. As director of the Bacteriological Institute, Lutz wrote:

We can state that so far Le Dantec’s bacillus has not been found except in cases of yellow fever. But only another epidemic of this ailment can provide us with enough material to clarify this important question once and for all.50

In early 1894,51 the Isolation Hospital in the capital of São Paulo admitted a number of cases, mostly from Rio de Janeiro, Santos, and Porto Ferreira. Autopsies were performed on eight corpses from São Paulo, one from Belém do Desalvado, and another from Santos.

The examination of tissue slices from the viscera of these corpses and also of sick people’s vomit and feces almost always produced negative results. Lutz gave no importance to the bacteria found, assuming they had invaded the tissue after death. Numerous cultures were made with blood from the viscera and with the contents of the stomach and intestines, but no germ new to science was detected. Lutz in fact concluded that Le Dantec’s bacillus – always absent – was “introduced via certain food (milk?) or medication.” Colonies similar to Bacterium coli commune were isolated on several occasions.
Because there were not enough sick people in São Paulo, studies only proceeded in February 1895, at the Isolation Hospital in Santos, where Arthur Vieira de Mendonça stayed for ten days and Lutz, five. There they could observe people just coming down with the disease and could culture material from the organs of those who perished.

They once again failed to find any “constantly present” parasite either through direct microscopic examination or after subjecting preparations and histological slices to known staining processes, as well as to some recently discovered ones, “used for the first time to study yellow fever.” Inoculated into a number of culture media, the blood most often would remain sterile for over fifteen days. “On rare occasions, varieties of white or yellow sarcines would develop, which can be considered accidental contaminations.” Cultures made from the blood of yellow fever sufferers in the final stage of the disease would at times produce microorganisms resembling Bacillus coli communis and also streptococci and other varieties of cocci “not yet determined.”

In tissue slices from the stomach and intestinal walls and also in the mucous membrane of the duodenum and the ileus, “a large number of bacilli [were often verified to be] invading the epithelium (which displayed a greater or lesser degree of necrosis) and not rarely the submucous membrane as well.” These were mainly the Bacillus mesentericus vulgatus, Bacillus subtilis, and Bacillus albuminis.” The scientists concluded that this constituted a

post mortem invasion by common species whose resistant spores are always present in the stomach and that develop chiefly where the mucous membrane suffers peptic softening... We have encountered similar processes in illnesses that differ entirely from yellow fever.

In his February 1895 report, Adolpho Lutz stated that he still thought it likely that the yellow fever agent resided in the digestive tract, like the cholera agent, but so far he had failed to isolate any organism different from those found in other diseases.

We paid the price of much thankless work to obtain these results, most negative, in perfect agreement with Sternberg’s. It is our opinion that the yellow fever problem not only still awaits solution but will prove quite challenging... Either its germ is so poorly characterized that it blends in with common germs, or it can only be found in significant abundance during a very limited time period, or, lastly, we as yet lack any method for recognizing and culturing it.
In 1897, the incidence of yellow fever dropped off in the state of São Paulo, with only forty-one patients admitted to the Isolation Hospital during the first five months of the year. Lutz and his aids autopsied seven of the twenty-eight people who died there; they came from Rio Claro (three), Dois Córregos (two), Araraquara (one), and Leme (one). Another five cases were cured and provided material for bacteriological examinations of the contents of the stomach and intestines, and also of the bile, urine, spleen, blood, and other organic liquids (Lutz, 1898).

The results were as frustrating as before.

Sanarelli’s bacillus on the agenda

On the façade of the Domingos Freire Bacteriological Institute in Rio de Janeiro waved a tri-color flag, black symbolizing vomit; yellow, jaundice; and red, hemorrhaging. This emblem brought home the image of a bulwark erected to defend a discovery coveted by so many. The flag was hoisted at the moment when the press was stirring up expectations that yellow fever’s secrets were about to be revealed by Giuseppe Sanarelli, a renowned bacteriologist hired by the Uruguayan government to establish and direct the Institute of Experimental Hygiene, inaugurated in Montevideo on 16 March 1896. Sanarelli, 31, had previously worked in Camilo Golgi’s laboratory at the University of Pavia, at Max von Pettenkofer’s lab in Munich, and at Elie Metchnikoff’s lab at the Pasteur Institute, in Paris. There he had conducted studies on immunity in typhoid fever and cholera, earning himself a respected name within the international scientific community (Brazil-Medico, no. 24, Jun. 1896, p.218).

Three months after inauguration of the Montevideo institute, the Italian bacteriologist came to Rio de Janeiro in quest of the organic matter he needed in order to study yellow fever. In January 1897, his caldron was bubbling. In the salons and cafes of the Brazilian capital, the topic of the day was the news that Sanarelli had completed his research and had sent a pli-cacheté (sealed communication) to the Academy of Medicine in Rome to guarantee his place as the first discoverer. From the time he took this initiative until the discovery was finally disclosed (postponed more than once), a number of other postulants to the title of discoverer of the cause and/or cure of yellow fever made their way onto the public stage.
Wolf Havelburg announced his partial results at a conference on 22 April 1897, at Rio’s Leprosy Hospital (which, it should be mentioned, had one of the best-equipped bacteriological laboratories in the city). The bacillus he found in the stomachs and intestines of people suffering from yellow fever had produced a typical disease when inoculated in rabbits and guinea pigs. After he injected the blood of convalescing patients into these animals, they displayed resistance to subsequent injection with the deadly culture, proving the feasibility of serum therapy. Havelburg’s conference (1897) was attended by representatives from all of Rio’s medical institutions and by government figures, journalists, and other gentlemen likewise interested in keeping abreast of advances in science, which at that point were overwhelmingly complex. The event sparked further research into yellow fever.

Eduardo Chapot Prévost (26 Jan. 1897, Recortes/COC) decided to break the silence he had been keeping; he had been reluctant to release information on studies he deemed “not sufficiently corroborated to warrant publication, especially in regards to such a knotty, controversial, difficult matter.” He had conducted his investigations in collaboration with Francisco Fajardo,
either passing whole days at São Sebastião Hospital performing autopsies or chemical and microscopic examinations of blood, vomit, urine, and feces; making cultures of liquids, humors, and fragments of organs in different media, in plates and in tubes ... or accompanying the march of the illness at the patient’s bedside ... or spending night after night, until two or three in the morning, studying under the microscope preparations readied during the day.

Prévost and Fajardo also verified the constant presence of a bacillus, isolated it in pure culture, and inoculated it into guinea pigs, dogs, and chickens that displayed lesions analogous to those of human yellow fever when they died.

Another to disclose incomplete studies, in March 1897, was Johannes Paulser, assistant at the Bacteriological Institute of São Paulo. In hopes of identifying those germs that appeared on a regular basis, he endeavored to isolate all that he found in the organs of people claimed by the epidemic which had swept through the state in the summer of 1895-96. He eventually incriminated one species of mold and two species of yeast; these had thrived in his cultures and had been found in the twenty-five corpses he autopsied with the help of Arthur Vieira de Mendonça (*Jornal do Commercio*, 8 Mar. 1897; *Brazil-Medico*, Mar. 1897, p.78-86).

Paulser’s mold gave João Batista de Lacerda the opportunity to put back in circulation the question of the polymorphous fungus that he had presented before the National Academy of Medicine and the Panamerican Medical Congress in 1892-93 (*Jornal do Commercio*, 10 Mar. 1897). Adolpho Lutz dismissed Paulser two months before he released his findings to the press. “Indeed,” wrote *O Paiz* (18 Mar. 1897), “Dr. Paulser’s work had made a poor impression on the group of physicians devoted to bacteriology in Rio, and hence we now rejoice to see him removed from under the responsibility of Lutz’s brilliant name.”

We now know that this note was written by Fajardo, who on the same date sent the page from *O Paiz* to his “Friend and Master ... with the place of publication marked in blue.” And he inquired: “Could you please tell me whether you agree?” In this same letter, the bacteriologist from Rio informs
Lutz that he had received news from Sanarelli: “He is going to make his announcement next May; he is decisive about the microbe. If I had time, I’d go to Montevideo before then.”

In January 1897, the press in Rio de Janeiro started raising the issue. One journalist expressed his “serious anxiousness” to learn the results already achieved by the “wise man from the state of Rio” (11 Jan. 1897, Recortes/COC). But, the journalist went on to state, Lutz was a most cautious investigator and was certainly the most averse to publicity.

One indicator of how seriously yellow fever was taken at the institute headed by Lutz are the data on post-mortem examinations: from 1893 to 1908, the lab’s bacteriologists cut open 121 corpses of yellow fever victims and 92 of typhoid fever victims (cholera morbus ranked third, at 62). The figures are even more impressive when we focus only on the 1894-97 period: 86 autopsies performed on victims of yellow fever and 67 on those who died of typhoid fever. In 1896, 43 victims of the first disease were autopsied and only 18 of the second.\(^5\)

In a mid-1897 communiqué addressed solely to the director of the São Paulo Sanitation Service, Lutz took stock of his studies on the etiology of yellow fever. He had not found any germ that could be incriminated either in the organs of those who had perished from well-characterized yellow fever or in blood drawn during life.

> The few positive results can be perfectly well accounted for by the possible invasion of secondary-infection germs or by the commencement of corpse decomposition ... The cultures that have been made independently by our laboratory staff from many autopsies of yellow fever victims have revealed forms that correspond to species known to produce secondary infections in other illnesses (as, for example, typhoid fever), mainly microbes similar or identical to *Bacillus coli commune* and to pyogenic cocci. (Lutz, 1897)

Lutz steered clear of any discussion of “certain work undertaken using defective methods and also in the absence of information considered indispensable.” Instead, he limited himself to observing that “the supposed positive results alleged in these works have not been verified by other investigators.” In an article published in the *Revista Médica de São Paulo*, he expressed the opinion that the bacillus described by Havelburg appeared to be merely “one of the countless varieties of *Bacillus coli communis*, which, as we have verified, is often quite virulent for animals, when isolated from cases of yellow fever” (Lutz, 1898).
In a report written earlier, Lutz had pointed out that the anatomical lesions observed in the viscera by advocates of the etiological theories then in circulation had been “quite exaggerated, since the authors had examined poorly conserved specimens or ones removed from the corpse long after death. Thus, changes owing to decomposition, which occurs very rapidly in yellow fever, were grouped together with changes caused by the illness.” In kidneys suffering such conditions, Lutz always saw “coagula of albumin and cylinders in the lumen of the renal tracts, but in well-preserved specimens the epithelia [were] all in their proper place, and the nuclei [were] easily stained.” The same thing was the case with the liver: “the cells always contain many yellow pigment granules; using certain staining methods, these hold the color of the dye, but only a careless observer could confuse them with microorganisms.” Clinical observation showed that the viscera quickly resumed their functions once the critical phase of the disease had passed, which seemed to demonstrate that the lesions weren’t very deep: “everything indicates that this is caused by intoxication by toxins rather than the localization of the infectious process inside the parenchyma of the viscera.”

This line of anatomical-pathological investigation led Adolpho Lutz to provide guidance to one of his assistants, Dr. José Martins Bonilha de Toledo, who was studying urine in yellow fever. Bonilha de Toledo, who had specialized in biological chemistry in Belgium, conducted in-depth chemical and microscopic analyses of the urine of thirteen victims of the disease. He concluded that renal lesions were a constant, and directly proportional to the seriousness of the case, confirming the suspicion that they were caused by a toxin secreted by the unknown microbe responsible for producing yellow fever. In January 1897, Bonilha de Toledo published his observations on renal changes and on variations in the chlorides, phosphates, and urea of yellow fever sufferers’ urine. He left only a few clues when it came to solving the etiological question, but he clarified some still obscure points concerning diagnosis and prognosis of the disease.

Adolpho Lutz, the “wise man from the state of Rio,” boasted credentials as solid as those of Giuseppe Sanarelli, the “wise Italian.” Thanks to Lutz’s circumspection and prudence, and also to the capacity of the laboratory he headed – superior to those operating in Rio – he distinguished himself as the foremost “verifier” of the microbes, sera, and vaccines proposed by these zealously competing bacteriologists. Above all, was someone who sought
victory by following the most unusual pathways, going farther than all others in his handling of the press, in his pursuit of economic advantages, and in mobilizing remarkably strong allies throughout his period in the spotlight.

On 11 November 1896, Felipe Pereira Caldas, a physician from the state of Rio Grande do Sul, spoke before the National Academy of Medicine about the smallpox serum he had developed (Brazil-Medico, Nov. 1896). The newspapers that day made much ado about the news (denied) that Sanarelli had just unveiled the secret of yellow fever. Days earlier, imitating the Italian’s action, Caldas had presented the Academy with a sealed communiqué in order to guarantee priority in the discovery of a serum against yellow fever. In January 1897, when Havelburg added fuel to the race to discover the yellow fever microbe, Carlos Seidl tested the first doses of Caldas’ serum at São Sebastião Hospital in Caju, which he directed. The Rio Grande do Sul lobby, led by Dr. Severo Macedo, beseeched Manuel Victorino Pereira, vice-president of the Republic and a physician from Bahia, to sponsor Caldas on a new trip to Rio de Janeiro, arguing that it was essential for a Brazilian scientist to assert priority for the discovery.

Felipe Caldas and his prophylactic reached the height of fame in mid-1897, precisely when Giuseppe Sanarelli, in Montevideo, announced discovery of the icteroid bacillus and the continuation of his efforts to obtain a curative serum. In a message read on 26 June, Brazilian President Prudente de Morais informed the National Congress that the Academy of Medicine, by a unanimous vote, had requested that the government submit Caldas’ serum to large-scale testing in the capital of the Republic and in other places during the next yellow fever epidemic (Diário Oficial, 1 Jul. 1897; Brazil-Medico, 1 May 1898, p.145). The message was accompanied by a statement from Nuno de Andrade, director of Public Health, in which he declared that

Dr. Caldas’ yellow fever serum is not the fantasy of a visionary discoverer, but a medicine that can easily become part of yellow fever therapy, and whose full benefits can only be definitively measured once its usefulness has been verified in a considerable number of cases.

In August 1897, Adolpho Lutz, Arthur Mendonça, and Bonilha de Toledo signed a scathing report on Caldas’ findings. Conducted solely in the laboratory, the verification contrasted sharply with the results of clinical studies by physicians from Rio. Lutz placed quotation marks around phrases
used by Caldas, to underscore questionable aspects of both method and product: the serum’s supposed dual-purpose nature (preventive and curative); the possible contamination of material collected in Rio de Janeiro and transported to the city of Rio Grande; and the non-determination of the microbe correlated to yellow fever.

Asked if he used one of the known methods for isolating the germ, he answered in the negative, since he ‘only had liquid serum available, in which – in his opinion – the only germs born are yellow fever ones.’ Asked if the inoculated animals displayed yellow fever symptoms, he said no. (Lutz and Toledo, 1897)

Although this report by the Adolpho Lutz team did not eliminate the discovery made in Rio Grande do Sul (it would find a place in newspaper headlines on and off until 1902), it did clear the way for a more enthusiastic acceptance of the discovery of the icteroid bacillus announced by Giuseppe Sanarelli on 10 June 1897, at Montevideo’s Teatro Solis.

Adolpho Lutz and Arthur Vieira de Mendonça traveled to Uruguay’s capital in June as delegates of the São Paulo government. They were soon joined by João Batista de Lacerda, Brazilian government representative, along with Francisco Fajardo, Chapot Prévost, and Virgílio Ottoni, delegates for Rio’s Society of Medicine and Surgery and for the Union of Hospital Interns.59

A parenthetical comment: On his way back to Brazil, Lutz stopped in Buenos Aires to meet the pathologist Robert Johann Wernicke (1854-1922) and his disciple Alejandro Posadas (1870-1902). There he closely examined the microscopic preparations and clinical cases that had prompted Wernicke to publish a special communication in the Centralblatt für Bakteriologie und Parasitenkunde (v.12, p.859-61, 1892) under the title “Ueber einen Protozoenbefund bei mycosis fungoides,” and Posadas to publish “Un nuevo caso de micose fungoidea con psorospermia” in the Círculo Médico Argentino (v.15, p.585-97, 1892). Lutz’s visit was spurred by his studies then underway in São Paulo, which were to culminate in his 1908 publication of a paper that would eventually be recognized as his chief contribution to Brazilian dermatology: the discovery in Brazil, for the first time, of a disease characterized by serious mouth lesions, today known by a variety of names, including Lutz disease or Lutz mycosis.60

In Montevideo, Lutz attended a conference that Sanarelli held for “a mixed public of professionals and the lay.” The audience included the president of
the Republic, the upper echelon of government administration, and Uruguay’s high society, along with medical delegations from a number of American countries. Lutz made several visits to the Italian bacteriologist’s laboratory to collect supplementary technical information on the facts he had announced. In Lutz’s August 1897 report to the director of the São Paulo Sanitation Service (1897a), he stated that these facts could not be negated “save on the basis of other observations and, in the absence of these, they should be accepted on the author’s guarantee. His capacity and skill have been proven by his earlier works, and I have had ample opportunity to verify them personally.”

Sanarelli had confirmed the frequency of the secondary infections verified by Lutz. “He even assigns a more important role to them in this disease’s high
mortality rate than would to us appear justified based on anatomical and clinical observations” (Lutz, 1897a).

The Italian scientist had isolated his bacillus from blood drawn from the organs of corpses of yellow fever victims, which for Lutz had always yielded negative results. The latter accounted for this as follows: it was so hard to find the germ that Sanarelli had only managed to isolate it in half the examined cases, “a very unfavorable proportion when compared to other microbial illnesses” (Brazil-Medico, 15 Dec. 1898, p.416-7). Having a “common” form, it could not be differentiated from the abdominal typhus bacilli or Bacillus coli communis. Under normal conditions, it did not even produce colonies on gel plates and therefore investigations using this culture (considered the best means for differentiating microbial species) had all met with failure. Furthermore, the presence of other bacteria hindered the development of the bacillus incriminated by Sanarelli: “The author himself deems it quite fortunate that he found it in almost pure culture in the second case he observed. He also feels that isolation of this germ is still very difficult today and, in certain cases, even impossible” (Lutz, 1897a).

Since there was so little to characterize it, he would not have noticed it “were it not for his extensive and truly colossal study of the pathogenic action of all the bacteria then isolated” (Brazil-Medico, 15 Dec. 1898, p.416-7).

In compliance with Koch’s postulates, Sanarelli had verified the pathogenic action of the icteroid bacillus in a number of mammal species. He had managed to induce

morbid processes typical for each species, but different from each other. These processes reproduce ... part of the phenomena observed in man when stricken with yellow fever. We watched a certain number of these experiments and saw anatomical parts from the cadavers of others; we thus had an opportunity to verify the exactitude of Sanarelli’s descriptions in various regards, and, as the other facts were witnessed by qualified people, they can be deemed established. (Lutz, 1897a)

In Lutz’s opinion, the probability that this bacillus was the cause of yellow fever had become a certainty following five experiments on human beings.

I cannot speak from personal observation, but the doctors who have had an opportunity to observe them recognize the symptoms typical of yellow fever. The temperature curves for these cases certainly correspond in every way to those of spontaneous yellow fever. Moreover, in Montevideo not one single voice has been raised to question these experiments, which have been deemed fully corroborative and decisive. (Lutz, 1897a)
On 22 July 1897, the delegates from Rio de Janeiro stepped up to the podium before the National Academy of Medicine to report on this discovery to an “especially” large audience, comprising physicians, students, and spectators from “various social classes” (Brazil-Medico, Aug. 1897, p.283-5). Lacerda spoke of the Uruguayans’ pride in the Institute of Hygiene, “unrivaled in South America.” He described the research studies that had led to the isolation of an extremely virulent bacillus and the subsequent inoculation experiments, conducted in an order of increasing zoological complexity: rodents, ruminants, simians, and, lastly, human beings. In closing, he reported on the counter-evidence he had obtained at the laboratory of the General Directorship of Public Hygiene, with the help of Dr. Emilio Gomes and the assistance of Chapot Prévost, Affonso Ramos, Zacharias Franco, and Antonio Pimentel. At the same session of the Academy and some days later at the Society of Medicine and Surgery, Francisco Fajardo presented equally positive results on the inoculations he had conducted with Miguel Couto at the Military Bacteriological Laboratory.

The similarity between the icteroid bacillus and common bacilli, together with problems encountered in isolating and culturing it, hampered the search for a secure method of differential diagnosis, something vital for clinicians and hygienists, particularly during epidemics. But Sanarelli’s work resulted in some useful public health guidelines, which Adolpho Lutz summarized: First, Sanarelli’s work had lent legitimacy to the prevailing practices of destroying the bacillus through chemical means and with heat. It was rare to find the bacillus in the blood and secretions of the ailing, which explained the reduced danger of direct contagion and even of indirect transmission during less serious infections and during the first period of the disease, which formed the bulk of cases. On the other hand, one serious or fatal case, especially during the stage of hemorrhaging, would create a “veritable epidemic.” If the disease were recognized in time and if the necessary isolation and disinfection measures could be enforced, it would be possible to keep the homes of the sick from turning into epidemic foci. The germ that escaped with the blood rarely ever became totally desiccated and could therefore remain alive for long periods in clothing, mattresses, or other objects, or mixed with dust and dirt, especially in the cracks of old flooring, waiting for a favorable moment to turn infectious and invade human beings susceptible to the disease. Lutz believed the icteroid bacillus
could be transmitted through excrement and vomit, and likewise through air and water.

Contrary to press claims, Sanarelli had categorically denied in Montevideo that he had solved the question of serum therapy, which, in his opinion, presented “major challenges.” His studies took this direction in the second half of 1897. In February 1898, he commenced field work in São Paulo. He had already sent doses of the serum to physicians in South and North America, asking in exchange that they provide him with notes on cases treated. His chief recommendation was that the serum be applied only in the early stages of the disease, since during advanced stages it would neither prevent death nor repair the serious anatomical and functional changes caused by the accumulation of “yellow fever venom” in the organism. The serum’s inefficacy during advanced stages of the disease had been verified when Seidl, Fajardo, Miguel Couto, and Paretti de la Roca undertook the first experimental inoculations with human beings at São Sebastião Hospital in December 1897.

A commission headed by Joaquim José da Silva Pinto, director of the São Paulo Sanitation Service, supervised field work in São Carlos do Pinhal. Members included Adolpho Lutz, Vital Brazil, Arthur Mendonça, and Cândido Espinheira (director of the Isolation Hospital in the city of São Paulo), along with the sanitary inspectors Ferreira, Vieira de Mello, Estevão Leão Bourroul, and a Mr. Rodrigues Souza, who was not a physician.

They found the streets of this coffee-growing center deserted and many houses abandoned. Just like in Campinas in 1889, almost all the inhabitants had fled. The isolation hospital was deserted, since most of the ill, “imbued with the traditional foolish prejudice against the pesthouse,” preferred to convalesce or die in their own homes. Small doses of the serum were given to two children, who recovered from the disease, but results from the first series of inoculations, involving another six patients in the first phase of the disease, were not satisfactory (Revista Médica de São Paulo, 15 Mar. 1898, p. 22-4). A new series was initiated on 17 February 1898, involving fourteen seriously ill patients, four of whom died. One of these was a Portuguese boy “rebellious against all treatment, violent against staff,” according to medical reports by the doctors. “After much effort and using force, we managed to apply a few subcutaneous injections.” Sanarelli lost a total of six of the twenty-two people inoculated, yielding a rather discouraging mortality rate of 27.27%.
Adolpho Lutz and his aids confirmed the presence of the icteroid bacillus in the blood of the ill, and they used this fact to counter objections still raised by doctors faithful to the old belief that the disease would not abandon the coast to take up residence eight hundred or nine hundred meters above sea level. Lutz thus corroborated the first part of Sanarelli’s discovery, but he was categorical in relation to the second: “the treatment of yellow fever does not lie in serum therapy” (Revista Médica de São Paulo, Jun. 1898, p.84-7). The same conclusion was reached by Affonso Ramos, head of the bacteriological laboratory of the General Directorship of Public Health, in Rio de Janeiro (Brazil-Medico, 1 Aug. 1898, p.256, 258).

On other occasions, the icteroid bacillus would provide a path used to dispel doubts about the identity of fevers that could be confused with yellow fever, especially typhoid fever and malaria. On 17 April 1898, three months after the unsuccessful experiments with Sanarelli’s serum, Lutz was summoned to present a report on the disease that was sweeping through the Campos Salles immigrant settlement, established the previous year by the state government next to the rail lines linking Campinas to Fazenda Funil, along the banks of the Jaguari River. In order to execute its planned venture, the Companhia Carril Agrícola Funilense had obtained a loan, authorized by the Campinas Municipal Chamber (Law no. 47, of 4 Jan. 1896), which in counterpart required the railroad to purchase land contiguous to the right-of-way and then cede it to the state so European immigrants could settle there. The co-existence of immigrant communities of diverse nationalities in Campos Salles accounts for the name given first to the railroad station (1905), later to the district of Campinas (27 Nov. 1906), and, lastly, to the autonomous municipality of Campinas (30 Nov. 1944): Cosmópolis, or “city of the world.”

In 1898, the town was just beginning its life. Adolpho Laufer, an immigrant in charge of the settlement, rendered accounts to the Inspector of Lands, Settlement, and Immigration for the State of São Paulo. In August, he obtained a permit to establish a German School on one of the settlement’s lots. Most of the settlers spoke German, and this was most certainly the language that the director of the Bacteriological Institute used when asking the newly arrived Swiss for news about the land of his genealogical roots. In 1897, the families of José Fumstein, Gothelf Lucker, and José Pfeifer moved to Campos Salles. In 1898, the families of Antonio Blaser, Roberto Maerki, Gustavo Epprecht, Alberto Fertz, and Heinrich
Mauer were to arrive, bringing total inhabitants to forty-two. They had been drawn to this colony by information publicized through Swiss newspapers, which informed potential immigrants that they should request help to cover the price of their ticket (the government paid for the immigrant’s trip to Brazil). In January 1900, the settlement’s population would reach 53 families, or 280 people, and then rise to 132 families (624 people) by 31 December of that same year, with German-speakers accounting for 68.41% of the total. They produced honey, corn, beans, potatoes, manioc, and rice for local consumption and ‘exported’ the first three products, as well as wax and butter. It was only in March 1899 that residents of the Campos Salles settlement had a physician (Dr. João Francisco Pereira) to take care of the fevers that so often afflicted them.

Lutz and Arthur Mendonça first examined two ailing people from Campos Salles who were hospitalized in the capital, one at the Isolation Hospital and the other at Santa Casa. They suspected the latter had yellow fever. On the afternoon of 18 April 1898, they headed to Campinas with all the equipment needed for microscopic examinations. They spent the next day visiting patients who lived in houses located far from each other.
They examined an Italian man displaying symptoms of yellow fever in its final stages: jaundice, hemorrhaging from nose and mouth, black vomit, a comatose state. A young Swiss boy had also fallen ill with the disease; some days earlier, he had been on a ranch inhabited by Portuguese settlers, among whom two cases of fever had occurred, one fatal.
The blood from these sufferers was inoculated into tubes containing agar and lactose broth; it displayed a clear “clumping power on the Sanarelli bacillus cultures” isolated in São Carlos do Pinhal. In all the cases that they examined from Campos Salles, Lutz and Mendonça were careful to confirm that the blood was free of the Laveran haematozoon and thus to exclude a diagnosis of malaria.

Public health in the 1890s

In his report on his assignment in Montevideo (1897a), Lutz pointed out two characteristics of the Sanarelli bacillus that helped differentiate it from other, similar ones. The first was the form taken by the colonies in agar culture; these resembled wax seals, a form not yet observed in other microbes. A second unique characteristic was that the colonies only developed in the gel when a mold was growing in it. “One can say,” the Italian bacteriologist wrote, “that it exercises a kind of radius of influence and only within its orbit is the development of icteroid colonies possible.” He went on to affirm that this peculiar parasitism was “the main cause of yellow fever aboard ships” (Sanarelli, 1897, t.63, p.190-1). Lutz imagined that this symbiosis derived from a modification of the nutritional medium occasioned by the mold, which favored the bacillus.

Upon his return from Montevideo, João Batista de Lacerda began exploring the symbiotic life of Sanarelli’s microbe and the mold he classified as *Aspergillus icteroide*. He verified that its spores would detach from the mycelial filaments and float along in the air, acting as “crutches” that allowed the bacillus to leave confined environments and spread far and wide (Lacerda, 1900, p.16-30; *Brazil-Medico*, 8 Jun. 1899, p.212-4). Holding in his hands the “key to Rio de Janeiro’s public health problem,” he left the laboratory and went to look for the mold and the bacillus in homes recently occupied by the sick. In the neighborhood of Laranjeiras, he found them living in symbiosis but always on fly excrement. “This, gentlemen, is the terrible and frightening perspective to which these observations have led us,” he declared to the Academy of Medicine on 27 July 1899. Rio’s housing provided the germ with a vast seedbed. Since it was also carried by flies, this encompassed not just the city’s “filthy” tenements but equally its “clean, aristocratic” homes.”68 Draining the soil, fixing the sewers, constructing a broad dock along the city’s shoreline, paving and planting trees along streets
and parks, opening new thoroughfares – in short, all the plans laid out under the federal capital’s urban renewal project proposed in the 1870s and repeated in 1896 (Abreu, 1998; Benchimol, 1992) would certainly beautify the city but would not rid it of yellow fever.

Instead of fixing our sewers, let us thoroughly fix our houses. We do not need to go as far as the Americans went in Cuba, burning them down. Such a procedure ... could only be used in a conquered nation. Experience has already shown us where and how the germ of this disease harbors itself inside homes; let us go inside to search them out and destroy them. (ibid., p.53)

This was the guiding principle behind public health initiatives in Rio de Janeiro and the cities of São Paulo at the close of the 19th century. A good example of this was the epidemic that swept through Campinas in 1897, the fifth and final of the 1800s.

Everything had been tried in the effort to rid the city of the disease. The installation of water and sewer lines in 1891 and 1892 did not keep yellow fever from erupting in 1892 and remaining endemic in the following years. The municipal authorities then adopted new public hygiene measures, starting by filling in some 3,000 ditches and wells that were no longer needed since the new lines had been laid (Santos Filho and Novaes, p.251).

The city was divided into three sanitation districts, each under the responsibility of a municipal hygiene delegate. Streams were drained, trees were planted along streets, tenements were closed down. A domiciliary inspection service began requiring that houses be whitewashed twice a year, and it disinfected those where yellow fever victims had died. Nevertheless, a new epidemic broke out in 1896, with 1,700 cases reported. The state government then took over sanitation in this and other towns hit by the disease.69

On 23 July 1896, during the post-epidemic period, Dr. Emílio Marcondes Ribas came to Campinas as head of a Sanitation Commission made up of Drs. Theodoro Bayma, E. C. de Souza Brito, Eduardo Lopes da Silva, and Las Casas dos Santos. On 4 August, superintendent Dr. Manuel de Assis Vieira Bueno formally handed the direction of sanitation services over to him. The Lazareto do Fundão was made into an isolation hospital, and the spacious building that housed the central market was adapted to serve as the Central Disinfecting Station.70
Concomitantly, a statewide Sanitation Commission, headed by the engineer Francisco Saturnino Rodrigues de Brito (1864-1929), undertook a new water supply project in Campinas, channeling the streams that cut through the city and also proposing that a new sewer main be built and garbage be incinerated.71

Such measures failed to avert another epidemic. It began on 5 January 1897, reached its peak in April, and died out in early July. This time 694 cases and 325 deaths were reported (Lapa, 1996, p.259-60). “The population was not expecting it,” Santos Filho and Novaes observe (p.262) – so much so that Carnival celebrations took place as scheduled in early March “in all splendor.” This time, few people fled the city. At the height of the epidemic, the Municipal Chamber considered using Felipe Caldas’ ‘vaccine’, and it was then that Adolpho Lutz began examining it at the Bacteriological Institute.

Emilio Ribas’ initiatives (1896/1897) were aimed primarily at the interior of dwellings, which, in his report of 31 January 1898, he compared more than once to “the old holds of ships ... filled with newly arrived foreigners.” Sanarelli’s and Lacerda’s studies had convinced him that

permanent foci of yellow fever only form inside dwellings, and that the yellow fever germ’s durability and tenacity was due to the presence of molds, which usually occurs in houses built on poorly prepared land, which
most often lack adequate slope to allow for the drainage of the waters that seep through the joints of the wooden floors – houses consisting of a series of alcoves, with a naturally inadequate supply of air, light, and ventilation.

The “first foci” that had formed in various locales within rural São Paulo had multiplied and spread, generating the repeated epidemics that sanitary authorities could not defeat.

Ribas placed priority on sanitary inspections of homes and on the meticulous disinfection of their interiors, based on his observations of other places, mainly Jaú. When yellow fever struck there in 1896, it had been triggered by the “revival of germs left in 1892, and it had not been imported, as people said.”

Ribas estimated that Campinas had at least 4,200 buildings in 1897, not counting those in outlying neighborhoods. The city center and environs were re-divided into five districts in such a way that each sanitary inspector could systematically visit the entire district once a month, ‘policing’ some 900 houses.

From nine in the morning till six in the evening, sanitation commission physicians saw to their districts; at night, one of them would remain on duty, with two teams of disinfectors plus the personnel needed to transfer the ill and to handle burials. As the sanitary inspectors visited homes, disinfection crews would spray the interiors with corrosive sublimate at 2 parts per 1000; they used phenol solution (2%) on metallic objects; in latrines, sewer drains, and the like, the antiseptic was a solution of copper sulphate (3%) or limewater. In stables, carriage barns, and parks, they sprayed solutions of cresyl or of phenol acid (4%). Between July 1896 and January
1898, no less than 12,089 articles of clothing passed through the large sterilizer installed at Fundão Hospital and, later, at the Central Disinfection Station (by the German company Geneste & Herscher).

From inside houses to under the ground: where does yellow fever live?

“If any of the ministers of the ex-monarchy ... had taken a file to one end of the bronze chain that was shackling and still shackles public health,” Dr. José Lourenço wrote in 1893, “the hex would be broken ... and the unpatriotic procrastination of the most pressing social issue would have ceased.” This viewpoint stood in contrast with the generic, far-reaching program that hygienists subscribed to at a moment when they were attempting to target if not all at least many of the links in the chain of urban insalubrity associated with the production of miasmas. In the 1880s and especially 1890s, hygienists and bacteriologists in Rio de Janeiro and São Paulo began to differ ever more radically in their conceptions about how yellow fever epidemics arose and spread and, consequently, of what measures would be appropriate or top priority in sanitizing cities. However, on one point all could agree: the disease was the key to urban unhealthiness, which was nearly synonymous with Brazilian unhealthiness. The line of thought then in greatest evidence advocated the theory proposed by Bavarian hygienist Max von Pettenkofer in regard to cholera and typhoid fever: Rio de Janeiro’s soil and Brazil’s national epidemic shared the same relationship that connected Munich’s soil to these diseases.

As Koch’s chief adversary in Europe on the etiology of cholera, Pettenkofer was a renowned representative of that line of thought which Rosen (1994) called “contigent contagionism.” According to the “Boden Theorie” (soil theory), four factors were necessary for an epidemic to occur: in addition to the germ, certain conditions had to be met regarding place, time, and people. In and of itself, the germ did not produce the disease, thereby excluding direct contagion. Individual susceptibility mattered but variables related to climate and to the soil were indispensable in explaining both
how some individuals and regions developed the disease and how others displayed immunity. These variables had an effect on the germ, which matured and became infective matter, just as a seed transforms itself into a plant.

Pettenkofer’s theory was very well suited to yellow fever’s most notable characteristics: its seasonal nature and its geographic specificity.

The bacteriologists exploring the question were convinced that the germ – whatever it was – completed an important stage of its life cycle in its surrounding environment. An unknown combination of factors would keep it latent during certain periods and make it virulent during others. This supposition was one of the cornerstones of theories formulated during this period. In varying proportions, the equations roughly involved telluric factors (soil, putrefying organic matter, stagnant water, hills, ditches, and so on), climatic factors (atmosphere, humidity, heat, rainfall, ozone, barometric pressure, and so on), and social factors (ships, housing, cemeteries, slaughterhouses, markets, streets, sewer lines, etc.).

For Pettenkofer supporters in Rio de Janeiro, the city’s unhealthiness derived from the “smothered swamp” that lay beneath it, overflowing with putrefying organic matter. When summer rains prompted changes in the water table, the germs deposited there would become activated and epidemics would break out (Benchimol, 1999, p.249-98; Hume, 1925, p.350-93).

During the transition from monarchy to republic, much was written and said about the dangers inherent to Rio de Janeiro’s water table, all in hopes of convincing the public and the authorities that drying out the soil by draining the deep groundwater would kill off yellow fever. This was the first item on the agenda of proposals voted on at the Second Brazilian Congress of Medicine and Surgery, in 1889, seeking a response to the following question: How could the epidemics that develop during summer months in Rio de Janeiro and other cities around the country be prevented or lessened (Gomes, 1957, p.234-6)?

The intensity of the controversies depended upon how serious the epidemics were and also upon the power games involving high-stake investments in the urban soil. Pettenkofer enjoyed his greatest popularity in Brazil around the time of the Encilhamento, a term describing a period when the country’s commercial and financial capital, and the companies and liberal professionals involved in civil engineering, were madly running after opportunities for investment or speculation in the city of Rio.
In 1892, Floriano Peixoto, head of the provisional republican government, tried to hire Pettenkofer to wrench yellow fever from the city’s soil. He likewise contacted Émile Duclaux, Pasteur’s successor as head of the Paris institute; Rubner, director of Berlin’s Institute of Hygiene; Friedrich Löffler, discoverer of the diphtheria bacillus; and the sanitary engineer Edmund Alexander Parkes, author of a well-known *Manual of Practical Hygiene* (1864), in which he systematized experience gained in the sanitation of English and Indian cities.

The 1892-93 epidemic and the quarantines that Argentina and Uruguay imposed on passengers and merchandise from Brazil refueled discussions of the old epidemiological question: was yellow fever an endemic disease, “residing” in the city, or an imported disease that could be combated through the triad quarantine-disinfection-isolation?73

The economic debacle that followed the euphoria of the *Encilhamento*, together with the civil war that engulfed the country after the September 1893 rebellion of the Armada, killed any projects for draining groundwater or for “hermetic sealing” of the urban soil. The results of Floriano’s inquiries were only made public in February 1897, when the Brazilian economy was beginning to recover and the question of transforming Rio de Janeiro and other ports into healthy cities was back on the order of the day.

The main threat to the city’s health was yellow fever, but lingering doubts about its etiology and transmission hampered those who wanted to fight it. Public opinion had already assimilated the notion that the disease was caused by one of the microbes on the agenda of scientific debate or, perhaps, by one not yet discovered. But the relative consensus about what should be done to make these urban centers healthy, grounded in the miasmic theory, gave way to heated controversy over the links that needed to be broken in the chain of insalubrity, with options varying according to the habitats and peculiarities of each incriminated germ.74

The new crop of germs that erupted in 1897 exasperated the social and professional groups pressing hard for the much-awaited sanitation of Rio de Janeiro. Doctors were unable to decide (intramural and inter-peer) who had hit upon the way to untie the Gordian knot of Brazil’s public health dilemma, and so Congress and the press proposed that tribunals be set up and the issue settled there. In May of that year, on the eve of Sanarelli’s conference, deputy Inocêncio Serzedelo Corrêa, a leader of the Sociedade Auxiliadora da Indústria Nacional (Society for the Assistance of Brazilian
Industry, proposed that a Pasteur Award be granted to the bacteriologist who received a favorable, unanimous endorsement from the Rio de Janeiro School of Medicine, the Koch Institute in Berlin, and the Pasteur Institute in Paris. In June, deputy Alcindo Guanabara, one of the most influential journalists in the republican movement, presented an alternative project, nationalist in nature and favoring Freire.75

The latter commission was formed, but it was not academic validation procedures that put an end to the controversy surrounding the etiology and prevention of yellow fever. As we will see ahead, it was a radical shift in the approach to the disease that would lead a new generation of bacteriologists to the public health stage, under the leadership of one of its more discrete members, Oswaldo Cruz.

**Focus on malaria**

In the summer of 1891-92, some months before creation of the Bacteriological Institute of São Paulo, the Rio de Janeiro and São Paulo state governments requested that the Domingos Freire Bacteriological Institute once again immunize the residents of a number of rural towns against yellow fever and that it clarify the nature of fevers whose diagnoses were unclear. While a commission was busy vaccinating in Niterói, Paraíba do Sul, Resende, and Barra Mansa, Freire visited Limeira, Rio Claro, Cordeiros, Piraçununga, Belém, Jaú, Campinas, Santos, and the state capital as well. Soon after, he published *Sur l’origine bactérienne de la fièvre bilieuse des pays chauds* (1892), which set off yet another controversy that garnered much press in Rio de Janeiro and involved a complex interpenetration of actors, microbes, and diseases. The purpose of this book was to differentiate yellow fever from the bilious fever of “hot countries,” which resembled each other and sometimes spread together. According to Freire, bilious fever was “one of the manifestations of malaria,” produced by a bacillus that had nothing to do with the plasmodium discovered by the French military doctor Charles Louis Alphonse Laveran (1845-1922). Freire then clashed with Adolpho Lutz and some younger bacteriologists from Rio de Janeiro who shared his interest in this haematozoon. Francisco Fajardo collaborated most closely with Lutz. They were drawn together not only by their joint involvement in the struggles surrounding yellow fever, cholera, and typhoid fever,
but also by their interest in issues brought to the fore by British tropical medicine in the late 19th century: malaria and, hence, the hematophagous insects that could serve as the hosts of microorganisms and disease transmitters. According to Arthur Neiva (1941, p.viii), Fajardo “ardently collected” blood-sucking animals of interest to Lutz in the Federal District and its surrounds. “I recall very clearly that Fajardo’s portrait was one of the few to be found in the room in Manguinhos where Lutz lived, in recognition of the collaboration offered by his unselfish friend.”

At that time, the medical press in Rio referred to Fajardo as the “discoverer” of the Laveran haematozoon in Brazil. For Freire, the French physician had explored an agent of “classic form.” “Let him come to Brazil and he will find a new world beneath his eyes. Remember that each of us is studying at a very different latitude.” The biological law postulated by the discoverer of the vaccine against yellow fever was drawn from the same theoretical pot from which Lacerda derived his arguments in favor of polymorphous fungi and from which Pettenkofer had derived the time and space coordinates that rendered the cholera and typhoid fever microbes either pathogenic or innocuous: “climatic diversity implies a diversity of infectious species and consequently a diversity of living pathogenic microelements” (Jornal do Commercio, 15 Jul. 1894; O Paiz, 20 Jul. 1894).

In a work published in 1892 (p.12), Freire raised the possibility that the bacillus he described was the one discovered by Klebs and Tomassi Crudelli, Laveran’s two chief adversaries.

As we saw in book two of The Complete Works of Adolphe Lutz, Theodor Albrecht Edwin Klebs (1834-1913) was the first to verify discovery of the leprosy bacillus by Hansen in 1874 (Bulloch, 1938, p.9, 376). In 1883, he discovered the diphtheria bacillus, cultured the following year by Friedrich
Löffler, a member of Koch’s team; it was named the Klebs-Löffler bacillus (now known as *Corynebacterium diphtheriae*). In 1878, Klebs, together with Corrado Tommasi Crudelli, began researching the malaria germ, which was endemic in the Roman Campagna. J.H. Salisbury, from the United States; Lanzi and Terrigi, both from Italy; and Pietro Balestra, another Italian, had all incriminated microscopic algae that vegetated in swamplands (Busvine, 1993, p.18). Klebs and Crudelli found the *Bacillus malariae* in the blood of feverish patients; this was a microscopic plant believed to bear a resemblance to the anthrax bacillus, whose spores Koch had just located in the ground where animals were buried; malaria spores were likewise believed to reside in the soil and float through the air. The discovery was confirmed by a number of Italian and French investigators, and it enjoyed great success at the International Medical Congress of 1884 (“Do bacillus malariae,” *União Medica*, 1881, p.82-6). Adolpho Lutz himself gave his tacit endorsement in an article he published in 1886 on the differences between algae, cocci, and bacilli (p.327-31). The paper was a by-product of his efforts at Paul Gerson Unna’s laboratory in Hamburg to demonstrate that the genus *Cocotrix* was a better fit for the bacilli of leprosy, tuberculosis, various putrefactive bacteria, and for *Bacillus malariae* as well.

Lutz so far had made no mention of the haematozoon discovered by Laveran six years earlier. Oscillaria malariae (later renamed *Plasmodium*) was a protozoan, and although both dysentery and surra had already been associated with these unicellular animals, there was no conclusive evidence they caused any important human disease. The complexity of the life cycles of animals within this subkingdom, the lack of a precise classification system, and the problems encountered in finding artificial means for culturing them all made it more difficult to demonstrate
an etiology of this nature. This helped keep Laveran’s microorganism in the shadow of Klebs and Crudelli’s for some years.

Studies by E. Richard, Camilo Golgi (1844-1926), and Ettore Marchiafava (1847-1935), which linked the microorganism’s life cycle to the clinical syndrome, were crucial in shifting support from Bacillus malariae to Oscillaria malariae in the late 1880s. Dr. Richard, stationed at Philippeville, a French Mediterranean military base, found it in 90% of clinically diagnosed cases and demonstrated that quinine destroyed it. He also proved that the main lesion produced by the parasite was destruction of red blood cells; he believed this was responsible for the characteristic anemia suffered by those with the disease and for the sharp pigmentation of the spleen and liver, verified during autopsies. It fell to Marchiafava and Golgi to clarify part of the haematozoan’s life cycle; they related it to the periodicity of fevers. Golgi showed that the release of the parasite’s progeny occurred in synchronized pulsations that corresponded to paroxysms of fever. He then raised the hypothesis that different species could be responsible for distinct clinical forms of malaria – tertian, quartan, and quotidian (or irregular) fevers.78

Insects subvert microbial theories

During these same years, there was rising interest in the means by which diseases of proven or suspected microbial etiology were transmitted. Pasteur’s and Lister’s emphasis on the ubiquity of airborne germs receded as research moved to other vehicles or carriers: on the one hand, water, sewage, food, body
wastes; on the other, dogs, cats, birds, and insects. One idea was mechanical transmission of the ‘viruses’ found in stagnant waters and putrefied material, as was the case with flies and the Eberth bacillus (Lutz, 1895, p.123). To a lesser extent, scientists considered the idea that diseases were disseminated by blood-sucking animals either directly when they bit human beings or via water contaminated with dead infected insects, which is what Patrick Manson believed to happen with the *Culex*, transmitter of *Filaria*.

In 1877-78, this physician had uncovered almost the entire life cycle of the parasite that produced filariasis, also known as elephantiasis, thereby putting together the parts of an enigma whose deciphering had begun in Brazil. In 1866, in Bahia, Otto Wücherer had attributed the disease to a microscopic nematode found in the urine of chyluric patients; Timothy Richards Lewis (1873) had then demonstrated the presence of an embryonic form of the nematode in the blood of sick people (*Filaria sanguinis hominis*); Joseph Bancroft (1876) later revealed the adult form of the embryo in a lymphatic abscess. The helminthologist T.S. Cobbold (1878) named it *Filaria bancrofti*. It was then known that the nematodes found in blood and urine were offspring of an adult worm that lodged in lymphatic vessels. Manson verified that a dog’s vessels could contain millions of embryos. If they reached adult form there, their aggregate weight would exceed that of the host itself. With the host’s death, the parasites would perish before a second generation could be born, and the species would therefore die out. This incongruity could only be explained by postulating that the embryos abandoned their host and developed outside it (Delaporte, 1989, p.37-40; Busvine, 1993, p.11-5). Their presence in the circulatory system and the fact that they had no way of leaving it led Manson to deduce that a blood-sucking animal was playing a role. He narrowed it down to the *Culex*, the most common mosquito species in regions afflicted by filariasis. In 1879, he proved that microfilaria were adapted to the mosquito’s nocturnal habits; obeying a law of periodicity, they invaded the peripheral circulation as night fell and retreated during the day. By
dissecting *Culex* during successive periods, the English physician duplicated the embryo’s metamorphism into a larva and then into the adult form of *Filaria sanguinis hominis*, ready to abandon its host and lead an independent life. At the time, he assumed that after ingesting blood the female mosquito traveled to areas near waters, where she digested the blood, laid her eggs, and died. The *Filaria* would begin their independent life in the water and through it infect humans.

Manson’s work opened the way for other discoveries involving arthropods as intermediary hosts for microorganisms pathogenic to humans and animals. In 1893, Theobald Smith and F.L. Kilborne discovered that the protozoan which produced the cattle disease known as Texas fever was transmitted by ticks (Foster, 1965, p.149-57). In 1895-96, David Bruce demonstrated that trypanosomes were transmitted by flies of the genus *Glossina*.

Much was yet to be defined about malaria and yellow fever, in part because of failures to confirm the identity of its alleged microbial agents and to locate its spores outside the human body (Worboys, 1996). Laveran’s haematozoon was found in the organisms of the ill, but no one had been able to culture it in vitro or to produce the disease experimentally.

**THE ORAL TREATMENT FOR FILARIASIS**

The polemic between Freire (spokesperson for Klebs and Crudelli’s bacillus in Brazil) and Francisco Fajardo’s and Adolpho Lutz’s teams (advocates of Laveran’s haematozoon) transpired halfway between the time that Manson formulated his hypothesis that a mosquito played host to this haematozoon before it infected humans (as with filariasis) and the time that this hypothesis was substantiated. Confirmation came thanks both to Ronald Ross, who uncovered the cycle of the bird malaria parasite in the *Culex* in 1898, and also to Giovanni Grassi, Amico Bignami, and Giuseppe Bastianelli, who the following year clarified the cycle of the human malaria parasite in mosquitoes of the genus *Anopheles*.

During this interval, the medical press in Brazil and abroad was filled with information and speculation about the role of insects in the transmission of other diseases. The spotlight was on flies, who entered the collective imagination of urban populations as an omnipresent source of danger in the midst – or in substitution – of intangible miasmas. In 1898, it was declared that flies disseminated the microbes of carbuncles, Egyptian ophthalmia, Biskara boils, pian (yaws), and glanders. In his laboratory, Yersin had verified that dead flies contained the plague bacillus and could infect drinking water. Joly (1899, 1898) confirmed that they deposited the bacilli of tuberculosis on foodstuffs and beverages, and carried them even when desiccated.

As we saw earlier, the final theory regarding the etiology of yellow fever was quite unpredictably lit upon right before Finlay’s theory was enthroned by the Brazilian public health field. When João Batista de Lacerda went into homes recently occupied by sick people in order to search for Sanarelli’s bacillus and its associated mold, he always found them on fly excrement. Many of the articles written back then leave us with this impression: it was as if the component parts of Pasteurian-based theories on malaria, yellow fever, and other diseases were ‘magnetized’ by the force field of another type of medicine, one which soon would be called “tropical.” New living pieces were fit into constructs built under the aegis of bacteriology, re-arranging them. Soil, water, air, food, houses, and people had been woven into webs traversed by alleged pathogenic microbes (mainly algae, fungi, and bacilli), and it was hard to accommodate the new players inside these nets. Connections were rebuilt, new components added, but the insects very often remained strangers in the nest. In the case of malaria and yellow fever, the logic governing state-of-the-art research in tropical medicine
seemed incompatible with the microbial theories then in the process of perishing.

Malaria and typhoid fever

Adolpho Lutz’s research on malaria tied into another polemic question that kept the press busy in the 1890s, especially in São Paulo. This was typhoid fever, a disease with intestinal manifestations that Lutz himself had contracted while on vacation in 1878, when he was studying in Leipzig, Germany. In Reminiscencias da febre typhoide, published in 1936, Lutz would classify it as “one of the Bacteriological Institute’s prime concerns.”

The disease had long remained undifferentiated, assigned to the realm of septic diseases, fevers, and diseases associated with the generic name “typhus.” The Englishman William Budd (1811-80) was the first to correlate typhoid fever with a living “virus” (which at the time meant living “poison”) – transmitted by indirect or direct contagion. In 1880, the pathologist Carl Joseph Eberth (1835-1926) described a microorganism first known as “Eberth’s bacillus” and later called Salmonella typhi; it was present in the mesenteric ganglia and spleen of bodies he had autopsied. The discovery was confirmed by Koch, who presented a more precise description of the bacillus. In 1884, Georg Gaffky (1850-1918), Koch’s assistant and successor, managed to isolate that microorganism and obtain pure cultures.

At that time, physicians in São Paulo were mistakenly diagnosing as “typhoid-malaria fever,” “remittent fever,” or “São Paulo fever” certain clinical cases that Adolpho Lutz would soon recognize as typhoid fever. The expression ‘São Paulo fever’ had been coined by “masters of indigenous medicine” to designate a disease that during the last decade of the Empire had laid waste to the capital of São Paulo, rivaling the havoc wreaked by yellow fever in Rio de Janeiro and Santos (Pestana, 1915, p.11).

In his first report as interim director of the Bacteriological Institute (2 Jan. 1894), Lutz argued against the malarial nature of fevers in the city of São Paulo, sustaining that, were this the case, Laveran’s Plasmodium malarie should be found in the victims’ blood. It was not yet clear to him what these fevers represented. Symptoms resembled those of undulant fever, which spread through the Island of Malta (a malady likewise known as brucellosis in honor of David Bruce, who in 1887 had discovered its
agent, *Micrococcus melitensis*). Lutz endeavored without success to locate this microorganism in the blood of São Paulo’s ailing. The hypothesis that the disease was “abdominal typhoid” – as the Germans called typhoid fever (Landouzy and Jayle, 1902) – was strengthened by an autopsy performed in 1894 on someone reported to have died of yellow fever.81

In the war against adversaries of this diagnosis, Lutz called to action Eberth himself, then director of the University of Halle’s Anatomical Institute. In a letter dated 1 May 1895, Eberth attested that the Brazilian bacteriologist’s cultures were “legitimate cultures of typhoid fever bacilli.”82

Most São Paulo physicians insisted that these “São Paulo fevers” were nothing more than a native or local type of malaria. In his 1894-95 report, Lutz argued against this supposition.

As always, when it is a matter of verifying mistakes, we expect protests. These were not long in coming, but were limited to articles in political newspapers, some of them anonymous, others by authors with no authority. There is no attempt to prove the facts, that is, by thermometric curves, the presentation of sick people, autopsies, or microscopic preparations. We always have our observations verified by the most qualified people we can find, and the Laboratory is open to adversaries, as much as to friends.83

During the turbulent 1890s, São Paulo fevers were diagnosed either as malaria or typhoid fever, eventually coming to rest on the side of typhoid fever, thanks to Lutz and his aids. Something similar happened with yellow fever. Some physicians reduced it to a unique manifestation of malaria, of American characteristics.84

To demonstrate that legitimate malaria did not exist in the city of São Paulo, Lutz had to produce evidence that combined clinical descriptions, autopsy reports, and, mainly, laboratory verification of the absence of *Plasmodium* in the blood. The other side of this venture was the reconnaissance of places where malaria indeed occurred under Lutz’s jurisdiction, which meant it was necessary to find sufferers who were carrying Laveran’s parasite.

In search of protozoans in birds, reptiles, and amphibians

Adolpho Lutz, his aids at the Bacteriological Institute, and his partners in Rio de Janeiro – Francisco Fajardo first and foremost – were not only in tune with studies being developed by British and Italian scientists in an
effort to firmly establish the clinical manifestations and etiology of the
disease and discover how it is transmitted. They also offered their
collaboration in these studies and pursued a research program that
preserved a relative autonomy from the more immediate, controversial issues
in public health. The mode of transmission of *Plasmodium malariae* was
the principal enigma then challenging them. Linked to it were other enig-
mas of not only medical but also zoological interest: what species of this
genus, and what other genera of this phylum of the animal kingdom, might
be involved with diseases of invertebrates and vertebrates, including
humans?

Taxonomic categories were different from today’s, and knowledge about
protozoans still quite inexact. In his 1895 report, Lutz succinctly described
the state-of-the-art in this realm of medical zoology.

Today, Laveran’s plasmodia are considered sporozoans, forming the
subdivision *Haemosporidia* together with the cytozoa of birds, reptiles,
and amphibians. Sporozoans are a group of protozoans that all lead a
parasitic existence and cause a number of diseases in man and animals,
some of which we have had the opportunity to observe in the laboratory.
We shall first mention rabbit coccidiosis, produced by the invasion of
*Coccidium oviforme* in the rabbit’s liver. This disease has killed many of
our experimental animals and must be largely responsible for the hardships
encountered by those among us who raise rabbits.

Lutz made mention of some species of myxosporidia that he had found
in other animals. He cited a pebrine called *Glugea bombycis*, which he had
observed in a species of *Lepidoptera* common in São Paulo (*Brassolis antyra*).
He had sometimes found unclassified spores of *Myxomycetes* in fish.

Adolpho Lutz’s first paper on this topic, republished in this volume under
the title “On a myxosporidium of the gall bladder of Brazilian *Batrachia*”
in Port., was conducted when he still lived and practiced medicine in
Limeira. It was first released in 1889, in the *Centralblatt für Bakterologie
und Parasitenkunde* (Bulletin on bacteriology and parasitology), a periodical
founded two years earlier in Jena by Friedrich Löffler, Oscar Uhlworm,
and Karl Georg Friedrich Rudolf Leuckart (1822-98). The latter
parasitologist, who held the chair in zoology and zootomy at the University
of Leipzig, was still Lutz’s main point of reference. As we saw in volume I
of Lutz’s *Complete Works*, Leuckart was spokesperson for the young Swiss-
Brazilian’s work on the region’s cladocera, presented before the Leipzig’s
Society of Natural Sciences (Lutz, 1878). Among the authors Lutz consulted
when he focused on protozoans some years later, Leuckart was the only one who mentioned its presence in amphibians, especially frogs. The article submitted by Lutz to the periodical edited by the German zoologist showed that they too were parasites of the gall bladder of *Batrachia*. He proposed a new genus and new species of myxosporidium: *Cystodiscus immersus*. “It is not just its new location that is of interest; this species, which would appear to be unknown, will prove worthy of study from a number of other angles.”
At the Bacteriological Institute of São Paulo, the medical-sanitary approach prevailed, and theoretical and experimental references changed, but Adolpho Lutz did not lose his zoological interest in sporozoans. His observations are found in his annual reports as director of the Institute, most of which never published.

When he went in search of Laveran’s plasmodium in 1893, its presence in the blood of malaria victims had already been demonstrated in Rio de Janeiro “by Dr. Fajardo, from whom we received splendid preparations.” But Lutz claimed priority in verifying existence of the parasite in the blood of birds: “This is the first time this fact has been verified in Brazil,” he wrote in that year’s report.

He had detected plasmodia “wholly like those found in man” in the blood of a socó bird (Nyticorax) he had purchased at the market and also in the blood of about half the “many birds” from Penha, on the outskirts of São Paulo.

His adversaries pointed out that these findings stood in contradiction with the fact that plasmodia were not found in the blood of alleged malaria victims in the city of São Paulo.

We could have said these birds come from a small farm, near the Tietê River, where there is a large swampy lake reputed to be a focus of human malaria. We chose this place for this precise reason. But ... we do not lend great importance to the fact for two reasons: first, because the identity of the haematozoa of men and of birds is far from being proven ... and, second, because, as one can know without being a naturalist, frugiverous birds move around and may have been infected elsewhere.

In all these cases, no plasmodium has been shown to have come from a sick person from the city of S. Paulo, where every month so many dangerous fevers are diagnosed, and we are still awaiting such evidence before we change our opinion.

In early 1895, Lutz requested a leave of absence and made an excursion to swampy areas around Santos, where he had already identified cases of human malaria. He examined the blood of aquatic birds and other animals, but did not find any plasmodia, which he attributed to the unfavorable season of the year. In May, back in São Paulo, he had better luck: he examined a jabirú (Mycteria americana) “so infected with plasmodia that one often saw up to ten or more pigmented bodies under the microscope ... We observed up to four small plasmodia in a single blood globule ... There were no flagellated forms. The bird’s spleen contained a large quantity of pigment.”
Lutz searched for haematozoa in amphibians and reptiles as well. Regarding frogs captured in Santos in 1893, he observed that the parasites in the red globules, “although probably belonging to the same family, distinguish themselves from human parasites by the fact that they are not pigmented and do not consume hemoglobin.” He further found *Filaria* embryos and a trichomonas in the animals’ blood. In another batch, collected from Barra de Santos two years later, there were three species of parasites: trypanosomes, *Filaria* embryos – “probably *Filaria rubella*” – and Danilewsky’s *Pseudovermiculi*, which invaded the globules like malaria plasmodia. Lutz once again found these pseudo-worms in frogs and toads from Santos, São Paulo, and Taubaté, thereby verifying that they were “common and disseminated.” He also found flagellates in the blood of rats, and a different species in the blood of *Batrachia*.

A trichomonas as well as *Filaria* embryos were found in a lizard (*Enyalius*) captured in the woods of the Serra de Santos mountain range.

These investigations of animal parasites reached their peak in the mid-1890s. Thereafter, Lutz concentrated more and more on human malaria, especially the mosquitoes that hosted its protozoans. In his 1896 report, he still included news of a series of observations on cytozoa in amphibians. The species examined were a *Boa constrictor* from Bahia, presented to him by Von Ihering, director of the Museu Paulista; a rattlesnake (*Crotalus horridus*); and five...
specimens of water snakes (*Liophis merremii*), captured on the outskirts of São Paulo.

These species represent three different families, which would seem to indicate that under favorable conditions any species of snake can be infected. I examined about twenty snakes from six different places, and found haematozoa in seven specimens caught in four different places, which indicates that the parasite is not rare. It was most often found in larger specimens, from which it can be concluded that the infection is not limited to one period of these animals’ lives.

The cytozoa in these snakes all seem to be of one species, quite similar to the *Pseudovermiculi* of frogs. They are worm-shaped, with one end rounded and the other pointed. The nucleus is rich in chromatin, and the protoplasm often contains some shiny granules. When a preparation of fresh blood is made, they remain inside the red globules quite some time; after leaving them, their movements slow. They may likewise curl and then straighten like the falciform germs of coccidia, to which they bear some resemblance.

The prime fruit of these studies was the article published in 1901 in *Centralblatt für Bakteriologie und Parasitenkunde* (v.29, no. 9, p.390-8), under the title “Über die Drepanidien der Schlangen: Ein Beitrag zur Kenntnis der Hämosporidien” – republished in this volume as “On Drepanidae in snakes: a contribution to our knowledge of haemosporidia” [in Port.]. Lutz begins the article with an overview of the previous decade’s research on sporozoans that parasitize hot- and cold-blooded beings. In humans and in birds (*Fringillidae, Ciconiae*, and *Columbae*), he had found the parasites of benign tertian fever, of quotidian fever, and also the forms *Proteosoma* and *Halteridium*. He had quite often observed cytozoa in snakes and frogs, and only sporadically in specimens of *Tejus teguixin*, *Enyalius* sp., and *Jacare nigra*. Tortoises, common limbless lizards (*Amphisbaena*, commonly known as the two-headed snake), and *Batrachia apoda* always presented negative results.

Another group of blood parasites, trypanosomes, were found in rats and frogs, while *Filaria* embryos were found in humans, dogs, birds, frogs, and reptiles (*Enyalius* sp.).

In his 1896 report, Lutz had classified the existence of haematozoa in the blood of ophidia as “still unknown.” He later verified that this fact had been observed by Billet, in Tonkin. Lutz, however, had examined over 200 individuals from some 20 species in another part of the world. In 1901, he claimed he had only included brief notes, in Portuguese, in Bacteriological
Institute reports because he wanted to further explore the study of these parasites which were of such interest to him.

The parasites of snakes ... that appear temporarily as cytozoa are undoubtedly sporozoans closely related to *Drepanidium*, which are generally included in a specific group of haemosporidia. In any case, they are quite distinct from the parasites of malaria, whereas in many aspects they resemble coccidia, that is, gregarins (*Monocystis*) and even sarcosporidia. The future will tell whether we are justified in organizing haemosporidia in one specific group and separating the genera *Danilewskia* and *Karyolysus*, based on our current, quite incomplete knowledge. I will keep *Drepanidium* as the name of the genus, since it is the oldest, and call the parasites in the blood of snakes *Drepanidium serpentium*, since I believe that I must consider the various forms I have observed as belonging to one [same] species.

The distribution of human malaria in São Paulo

Lutz’s research into human malaria focused at first on Barra de Santos and the swampy lowlands near that port. In 1893, he twice verified *Plasmodium malariae* in people who had come down with the disease there. In his 1895 report, he described three more cases: one from the port of Taboado, along the Paraná River; the other two, a mother and daughter who lived in Mogy-Guassú. There is no mention of human malaria in the 1896 report, even though this report contains the most information on the topic of haematozoans in animals. The following year, Lutz diagnosed fourteen cases in the acute stage of the disease. He attributed this increase to meteorological influences and also to his new access to patients hospitalized at Santa Casa in the city of São Paulo. But all of these came from outside the city. “We are still waiting,” he wrote, “[to find a] case of malarial fever displaying plasmodia in the blood contracted in São Paulo, [and this] should prove that the existence of paludism in the capital is no more than an old myth.”

The victims he examined in 1897 had picked up malaria along the banks of the Mogy-Guassú River (three cases, two of which in Porto Ferreira) and in Guarujá (one), Paranaguá (one), Serra Azul (one), and Motuca, near the Guariba River (one). One of the cases came from Rio de Janeiro. The largest number (six) occurred in the Serra de Santos highlands. The next year, Lutz encountered another eleven cases, distributed as follows: Porto Ferreira (one), Salto de Itú (one), Inhaim (two), and Serra de San-
tos (two, plus an unspecified number “with pigmented leukocytes”); at Santa Casa, he also examined an ailing individual from a farm near Araraquara, a “chronic case, probably Italian in origin,” plus four cases from unspecified areas of rural São Paulo.

The thirty malaria victims he had examined until then and the information provided by other physicians allowed Lutz to draw a preliminary sketch of the geography of malaria in the state of São Paulo and its surrounds. The foci were concentrated in three regions: first, along the coast (Barra de Santos, Guarujá, and, outside the state boundary, Rio de Janeiro and Paranaguá); second, in coastal highlands, mainly around Santos; and last, along the banks of rural São Paulo’s big rivers: Mogy-Guassú, Tietê, Paraná, and Piracicaba. Cases elsewhere were rare and not of great import. The malaria occurring near rural rivers only appeared when these reached a certain size; on the Tietê, for example, the malarial stretch was near the Itu falls. “Thus it is that between the coastal highlands and the banks of the big rivers there is a nearly unaffected zone, wherein lies the capital.”

Most of the cases were not serious: “They usually start as the common type and manifest in this form again during relapses. Tertian fever is more unusual and generally only observed in long-lasting cases, apparently the result of a gradual modification of the quotidian type.”

Lutz’s reports indicate that he had been putting into practice the experimental program then predominant in so-called exotic pathology, whose introduction Delaporte (1989) attributes to Manson. As the French historian has shown, Laveran’s studies had left two questions pending. The first had to do with the nature and function of certain forms displaying long motile filaments, found in extravasated blood; some believed these were bodies undergoing disintegration while others thought they represented a new stage in the parasite’s development. The second problem concerned the way malaria spread. Although it was possible to produce the disease by inoculating healthy individuals with a sick person’s blood, it did not appear to be contagious. Several hypotheses were suggested. Some believed that parasites penetrated the organism when they ingested stagnant water or inhaled the dust carried from swamplands; others thought the parasites that lived in these swampy areas infected mosquitoes, which in turn infected humans.

In 1894, Manson linked the two questions when he applied the hypothesis of the cycle of *Filaria* to motile filaments. He conjectured that
MacCallum ascertained that these cells played a role in the parasite’s biological reproduction: in studying the crow’s haematozoon, he verified that it presented itself in two forms, one masculine (hyaline bodies), and the other feminine (granule-containing bodies). At Manson’s suggestion, Ross began exploring bird malaria. To trace the development and final position of the pigmented cells within the mosquito, he performed delicate dissections, daily, and verified that up until the eighth day, the cells grew in size and then opened up and released the filiform forms that appeared only in extravasated blood were flagellated spores, which was the parasite’s first stage of life outside the human. Just as occurred in the case of filariasis, a mosquito or other blood-sucking insect would remove haematozoa from a human’s blood vessels and then transfer them to water when it died. The parasite would return to the human via water or by air. This hypothesis underlay the research program to which Ronald Ross devoted his inquiries between 1894 and 1898, while exercising his duties as an officer of the Indian Medical Service.

The object of study is infinitesimal and problematic: the extreme delicacy of the filaments makes observation more difficult, and it is necessary to find an element whose form and location are not known. Furthermore, nothing guarantees that this research is being conducted on an appropriate species. Hence the strategies Ross devised for overcoming these problems. On the one hand, [this meant] working with sterile mosquitoes, that is, from larvae kept in captivity, making it possible to avoid the complications brought by the invasion of foreign bodies. On the other, [this meant] paying close attention to the plurality of forms in which the flagellated bodies could present themselves. This implied familiarity with insect parasites and with the normal histology or pathology of the mosquito. (Delaporte, op. cit., p.96-7, 98)

In the summer of 1897, one decisive observation provided Ross with the position and characteristics of the parasite inside the insect body: he discovered pigmented cells in the stomach wall of mosquitoes fed on the blood of sick people four or five days earlier. Around the same time, MacCallum ascertained that these cells played a role in the parasite’s biological reproduction: in studying the crow’s haematozoon, he verified that it presented itself in two forms, one masculine (hyaline bodies), and the other feminine (granule-containing bodies). At Manson’s suggestion, Ross began exploring bird malaria. To trace the development and final position of the pigmented cells within the mosquito, he performed delicate dissections, daily, and verified that up until the eighth day, the cells grew in size and then opened up and released the filiform
bodies. In the end, he came across them in the insect’s salivary gland (ibid., p.99-100).

During that same period, Lutz was opening the bodies of different animals and then comparing their haematozoa with the plasmodium, in an effort to understand how they parasitized their host’s organisms. At the same time, in his reports he sought to provide detailed descriptions of the morphology and location of the protozoans found in the blood of birds and humans struck by paludism, emphasizing those aspects that scholars of the disease considered the most intriguing. In 1898, he summarized his conclusions in these terms:

The observed haematozoon almost always had the form of a plasmodium; four or five times it was accompanied by crescents, and once we found only semi-crescent shapes. Where there were crescents, lengthy observation usually revealed flagellated bodies. We never found segmented forms and only once did the plasmodia present pigment clumped together in the center, as if preparing for segmentation ... The plasmodia were always of the type described, with thin, irregularly distributed pigment in Brownian movement; they displayed not very accentuated amoeboid movements, reached or exceeded the size of a red blood cell, and did not segment in the blood of the peripheral circulation. They did not correspond precisely to any of the described types but resembled benign tertian and quartan forms.

It was only in 1897 that Adolpho Lutz quite unexpectedly turned to the issue of its transmission by mosquitoes, as a result of the enigma we are about to describe.

The discovery of forest malaria

The railroads were built in São Paulo in the second half of the 19th century as a direct result of the growth of the coffee industry. Until then goods were moved across land by caravans of mules on journeys that lasted days. As foreign trade expanded, this system became outdated and it became clear that new transportation methods had to be found to get merchandise to the coast more quickly. The solution was a railroad linking the provincial capital and the main coffee production areas to the port of Santos across the Santos or Cubatão mountains, which formed a section of the Serra do Mar mountain range.

The first studies for the railroad date back to the late 1830s. A preliminary project was submitted to English engineer Robert Stephenson, but the proposal was considered premature and was eventually dropped. In 1859,
another group of Brazilians, headed by entrepreneur Irineu Evangelista de Souza (1813-89), the Baron de Mauá, was granted a 90-year license by the Imperial Government to build and run a railroad linking Santos to the São Paulo plains and on to Jundiaí. In 1860, the São Paulo Railway Company Limited was set up in London and hired two experienced English engineers: James Brunlees and Daniel Makinson Fox. Faced with the difficulty of crossing the steepest parts of the mountain range, Fox put forward the idea of using a funicular system. It comprised four slopes, each with an 8% gradient, which were interconnected by landings. At these landings, fixed power units ran the steel cables that moved the trains up and down the mountains. Brazil’s first long-distance railroad, crossing some 140 kilometers, the São Paulo Railway was completed around 10 months before forecast and was officially opened for service on 16 February 1867.

The railroad was built by approximately 5,000 laborers, who were lodged in a campsite at the top of the mountain. When it was concluded, only a few of the workers were kept on to help its running and maintenance; the rest were laid off. The original camp spawned a village called Alto da Serra, now Paranapiacaba, which means “place with a view of the sea” in Tupi-Guarani.

With ever greater amounts of coffee being shipped to Santos and with the growth of inland towns, the railway had to be doubled and a new funicular built, called Serra Nova. Running parallel to the other line, the new line was built using an endless rope, a genial device of counterbalances that controlled the vehicle that was going down at the same time as another was going up in the opposite direction. This meant the trains could cross the mountains without interrupting their journey. The stretch over the mountains was split into five 2-km-long sections, each with a maximum gradient of 8%. Between one landing and the next, the wagons were pulled by a small locomotive called the locobreque. Construction of the second funicular started in 1895, and it was opened on 28 December 1901.92

In 1897-98, an epidemic of malaria spread among the workers building the new railway in a forest-covered section of the mountains. This environment was very different from the wetland plains usually associated with the disease. “The cases were intermittent benign quotidian tertian malaria,” wrote Adolpho Lutz in his 1897 report. “They were very frequent but not very serious and easily cured by quinine, though relapses were extremely common. They occurred both in the highest part of the mountains
The São Paulo Railway, the state’s first, built by Robert Sharpe and Sons, of London, according to a plan by British engineers James Brunlees and Daniel Mackinson Fox, hired by the Baron of Mauá. Photograph by Militão Augusto de Azevedo, 1865 (Walker & Braz, 2001, p.61).
and on the mountainsides in very steeply sloping areas destitute of swamps, and they were not observed where the line crossed the mangrove swamps.”

There were other outbreaks of malaria at worksites for other railroads. Mauá, near Rio de Janeiro, and Guarujá, near Santos, were examples that Lutz cited. In his 1898 report, he merely commented that the quantity of cases in the Santos mountain region could naturally be explained “by the concentration of workers in a normally quite deserted area.” He did not bring the subject up again in subsequent reports. Malaria still remained on the agenda after its mode of transmission was deciphered in 1898-99, but almost always with reference to its most predictable habitats, i.e., plains and swampy valleys.

In 1901, the director of the Bacteriological Institute analyzed 17 positive results, mostly from Rincão. Lutz and his assistant Carlos Meyer investigated more than 50 people with malaria, the former on trips to Conceição do Itanhaem and the latter on visits to Guatapará, Rincão, and Rebouças. They collected large quantities of samples and undertook “an extensive study using a more or less adapted version of Romanowsky’s staining method, which yielded many magnificent preparations.”
The inland journeys in São Paulo state continued into 1902, where epidemics of malaria were discovered in Peruíbe and soon afterwards in areas near Conceição de Itanhaem and Iguape. Many cases were studied in Batatais, on the banks of the Sapucaí River, and in Ribeirão Preto, where the disease was endemic.

During this period, Lutz vaguely alludes just once to the “many observations” he had made about the “coincidence of the three species of mosquito of the genus Anopheles ... to which reference will be made in a special paper on this subject.”

His study only came to light in 1903, and it was only then that Lutz revealed the complexity of the enigma he had encountered with the outbreak of malaria in the Santos mountains, to which he dedicated so many years solving. And even so, the order of events that led to the discovery of the malaria that spread through the forests is not entirely clear.

The circumstances surrounding the outbreak in question had been different from those generally encountered. Most of the railroad being built between São Paulo and Santos linked the plain slightly above sea level to a peak on the mountain range around 900 meters above sea level, which stood beside even higher peaks. Here, the railroad crossed uninhabited forestland. The sharply sloping hillside could only be crossed using the cable system described earlier. This required that not only the track bed be built but also five engine rooms and a number of tunnels and viaducts to cross the gorges and ravines which different streams ran down. Given the steepness of the slope, there were a great number of waterfalls and no still water, “in the normal sense of the word.”

The first line of the railroad, opened in 1867, was the shortest and also the steepest. Lutz had been informed by eye-witnesses that while it was being built, intermittent fever had spread among the workers, but that the problem had ceased when the works had finished. No further cases had been noted among travelers or the personnel that took the train daily, nor among the few families that lived along the railroad.

When the new line was opened, hundreds of workers received housing in the middle of the forest on ranches that were only interconnected by tracks through the forest. Many cases of intermittent fever once again broke out, initially among those who lived on the ranches in the lower, hotter area, but soon spreading up to the top of the mountain, “often affecting almost all the people on a ranch in just a few days.” Normally,
Panorama from the Cubatão Mountains (Santos), where Adolfo Lutz discovered forest malaria – 1, 2, 3, 4, 5 (BR. MN. Acervo Adolfo Lutz, caixa 23, maço 9).
Plans for the Cubatão Mountain railway (BR. MN. Acervo Adolfo Lutz, caixa 23, maço 9).
those who were affected only had to take short periods off work because the disease was mild and quinine was widely used to combat it, but relapses were nonetheless frequent.

Lutz examined blood from different workers and found that they did indeed have malaria. Intrigued by the characteristics of the epidemic, which had broken out in an environment so different from that traditionally associated with the disease, Lutz decided to spend a few nights in a house built alongside the old railroad that belonged to an engineer friend of his, whose wife had fallen ill there.

On the very first evening, which came after a terribly hot day, we were sitting around a lamp when a number of biting insects appeared. They included *Simulium pertinax* Kollár, some relatively harmless Culicidae with which I was familiar, and a species I had never before seen, which had spotted wings and an odd perpendicular position when it sucked. Though it was delicate and small, it was clear that this was a voracious blood-sucker which landed directly on the people present and on a dog that also lived there, without buzzing first. The bites of this mosquito are less painful than those of some other species. In light of all these factors, many people do not notice them, which means that this species, which mostly comes out at dusk, is easy to miss.

I instantly felt sure I had found the mosquito I was after, even though at that time nobody knew the characteristics of the malaria transmitter. Soon afterwards, when it was discovered that they are most likely to be species from the genus *Anopheles*, I realized with some satisfaction that the new species was indeed an *Anopheles*.

The final comment suggests that Lutz’s ‘revelation’ about transmission by anophelines of this as yet unknown form of human malaria was actually written prior to the publication of the ground-breaking work on this topic by Grassi, Bignami, and Bastianelli (1899).

With the suspect species identified, the next step was to find out where its larvae were hatched. He had no doubt that they were aquatic, like other mosquitoes, and it did not take long to discover that in those forests there were few pools of standing water. “The issue was then to find water deposits suitable for them to breed.” Lutz found them “soon afterwards,” his speed in so doing being thanks to his skill as a zoologist and to an important prior experience.

When in Hawaii (1889-92), Lutz had studied the Pandanaceae (*Freycinetia arnottii*), a plant that collected water in its leaves and served as a habitat for a small crustacean (*Orchestia*). He was already familiar
with the work of Fritz Müller, the first naturalist to study the relationships between animals and water-storing plants: in 1879 and 1880, he had described a small crustacean *Ostracoda* from the family Cytheridae (*Elpidium bromeliarum*), whose life cycle took place inside a bromeliad from Brazil. He tried to explain how this exclusive inhabitant of plants moved from one to another, which led Müller to present the first relationship between animals that make up part of the bromeliad fauna.96

Lutz may well have been familiar with the work of other authors who were starting to investigate this phenomenon (and if he was not, he certainly became familiar with them during his own research). In 1883, Friedenreich described a coleoptera (*Pentameria bromeliarum*) whose larvae inhabited the water in Brazilian bromeliads. The following year, another coleoptera (*Onhostygnus fasciatus*) in Mexico was described in plants of this genus (D. Sharp, 1884), while Schimper (1884, 1888) published important papers about the physiology of these plants. In the year in which Lutz started his investigation in the Santos mountain region, F. W. Kirby (1897) showed that bromeliads from Chile harbored butterflies from the family Sphingidae, genus *Castnides*. 

Camp on the plain where Lutz examined workers with malaria. Currently the site of the Piassagüera station, now abandoned. It was opened in 1902 as the first stop in the “new mountain” cog railway line (BR. MN. Fundo Adolpho Lutz, caixa 23, maço 9).
If small crustaceans and other animals could live there, “of course mosquito larvae could too,” as Lutz wrote in 1903. “As there were so many bromeliads in the forests, I set to work examining them with high hopes.”

He only found frog larvae in the bromeliads that grew on rocks. The large trees in the area bore a plentiful supply of bromeliads, but their lowest branches were at least ten meters above the ground, well out of the scientist’s reach. It would have been pointless to cut down a tree because the water in the plants that lived on them would spill out. Finally, a short while later, Lutz managed to find some more accessible bromeliads and found many larvae of the new Anopheles and of other mosquito species in both of them.

In 1903, after five years’ study, it was clear that “practically all the mosquitoes typical of the forest spend their larval phase in the water in bromeliads.”

In the time that elapsed between his first observations based on “an immediate, almost intuitive conviction” (Gadelha, 1994, p.178) and the publication of his discovery, Lutz designed suitable techniques for collecting larvae and breeding them in a laboratory. He studied the Bromeliad species and their distribution not only in the Santos mountains but also in other areas with a similar ecology. He took interest in all groups of animals that inhabited the water in these plants: tiny crustaceans (Ostracodes, Copepods, Lynceids); the larvae of Tipulidae, culicids of Corethra, Chironimus, and similar nematocers; and the larvae of aquatic coleopteras and amphibians. He noted that hylids and terrestrial planarias liked living in bromeliads that had both water and land characteristics. And above all, he studied the habits of the forest mosquitoes once they reached their winged stage, without restricting himself to the bromeliad species.
He got together a network of collectors for his program and, partly thanks to his growing interest in entomology, he soon reached remote corners of Brazil and a number of foreign countries.

“Waldmosquitos und Waldmalaria,” which means forest mosquitoes and forest malaria, was published in *Centralblatt für Bakteriologie, Parasitenkunde und Infektionskrankheiten* (v.33, no. 4, 1903, p.282-92), dated 16 September 1902, by Lutz himself. The article was the subject of a number of criticisms, and in the U.K. there was a long comment in the *Journal of Tropical Medicine* (1903, v.6, p.111-23) entitled “Forest Mosquitoes and Forest Malaria.” The Bulletin of the Pasteur Institute gave Lutz’s contribution less credit (1903, v.1, p.183); other comments were published in *Archiv für Schiffs- und Tropenhygiene* (v.7, p.339-40), *Münchener medizinische Wochenschrift* (v.50, no. 6, p.264), *Hygienische Rundschau* (v.13, no. 18, p.937-8), *Rif. Med.* (v.19, no. 15, p.418), and also in *Allgemeine Zeitschrift [für Entomologie]* (v.8, no. 18-19, p.377), the latter by Paul Gustav Eduard Speiser.

Disease-carrying mosquitoes had been studied by doctors prior to this, who had picked up the skills they needed to deal with the biology and the system of Culicidae in practice, at haste, and not always in the most suitable of ways. One of their biggest problems was a shortage of specific knowledge about this group of animals.

As in the most prolific and chaotic phase in the search for pathogenic microbes, in the 1880s and 1890s, the eagerness in the hunt for potential winged transmitters of diseases from 1890 through 1910 enhanced knowledge about Culicidae, but at the same time engendered considerable confusion about identifying and naming the same species. England took the lead in these studies after Ronald Ross’s discovery. At the request of the then Prime Minister Joseph Chamberlain, the Royal Society set up a committee to study the control of malaria in the colonies (Howard, 1930). One of its members was Edwin Ray Lankester, director of the British
Museum; he suggested a survey be made to identify the different mosquitoes that existed in the world. In response, the British consulates and other government bodies were set in motion to achieve this goal. At the same time, the Museum of Natural History put together a collection of Diptera to provide support for worldwide knowledge in this realm. Frederick Theobald, a zoologist with the South Eastern Agricultural College in Wye, Kent, was appointed by Lankester to be superintendent of the ambitious project.

Adolpho Lutz was one of the researchers in Brazil to be contacted; he received his first news of the project on 24 March 1899, from the British Consulate General. As we shall see in the following book of this volume of his Complete Works, Lutz was to play a crucial role in Theobald’s studies. He had already begun his systematic study of Culicidae in 1897 so could send his first shipment to the British zoologist as early as June 1899: “As I was then very busy with my analogous studies ... I wrote back sending all my Culicidae, which included more than forty species,” wrote Lutz in 1903. These included mosquitoes from the forest and other species that he considered new.100 Theobald confirmed the Brazilian collaborator’s suspicions, and in his honor gave the name Anopheles lutzii to the new Anopheles collected in the zones where its habitat, the bromeliad, flourished. Another species was called Anopheles albipes.

In a letter to Theobald dated 23 September 1900, Lutz commented on the classification he had made and described the Anopheles lutzii. He had found it “near Santos in forests of ... and on the mountainside, with a few, exceptional specimens near São Paulo, probably carried far from the mountainous and forest-covered area by the overflowing river.” Lutz continued:

I have also obtained some specimens from Mr. Schmalz, collected in Joinville (SC). Usually, the larva grows in bromeliads, as I suspected long ago, since the imago is encountered in forests and on steep mountainsides where there is no other [collection] of water. The larvae and nymph are brick red. The imago bites men and dogs hungrily at dusk, when the weather is hot, and enters the dwellings and huts built in the forests. It was responsible for a number of epidemics of intermittent fever among laborers employed in the construction of the railroads. It may also bite by day in the shade.101

When he named that mosquito, Theobald inadvertently gave it the same name that Oswaldo Cruz had given another species of Anopheles, also in tribute to Lutz. In a review prior to 1903, the British entomologist included A. lutzii in the genus Myzomyia, so it was then named Myzomyia lutzii.
The species previously described by Oswaldo Cruz was first included in the genus Pyretophorus by Blanchard, and later in the genus Myzorhynchella, created by Theobald.

Since the turn of the century, Oswaldo Cruz and Emil Goeldi (1859-1917) had been involved in the collection and classification of Diptera in Brazil. The former started this work even before he became director of the Manguinhos Serum Therapy Institute and Director General of public health, while the latter, a Swiss zoologist, was the director of the Pará Museum of Natural History and Ethnography in Belém, who published *Os Mosquitos no Pará* in 1905 (Sanjad, 2003). Carlos Chagas and Arthur Neiva soon joined the project. They came to publish a number of papers describing new species during the first years of the 20th century in Brazilian journals that did not have international circulation, which made the classification of the specimens more confusing.

The work by Oswaldo Cruz, the first in medical entomology, was published in *O Brazil-Medico* in 1901, the same year as Theobald’s first volume.

In 1908 (p.53), another important paper about Culicidae was published, this time by American entomologists Frederick Knab and Harrison Gray Dyar. They took the forest malaria transmitter out of the genus *Myzomyia* and put it back among the species of the genus *Anopheles*, now as *Anopheles cruzii*. “It gives us great pleasure to dedicate this interesting species to Dr. Oswaldo Cruz, renowned hygienist and bacteriologist from Rio de Janeiro. The larvae were discovered by Mr. A.H. Jennings in Panama’s canal zone, living in the water between the leaves of bromeliads, which seems to be their only habitat.” Unlike Theobald, Knab and Dyar did not recognize either *Myzomyia* or *Myzorhynchella* as separate genera of *Anopheles*. “Furthermore, we have noted the existence of *Manghinhosia lutzi* Peryassú, which will also need to be named anew should it be ascertained – as we assume it will – that the new genus *Manguinhosia* is in no way separable from *Anopheles*.”102
It is now known that *Anopheles cruzii* is the primary vector of what is known as “bromeliad malaria,” which is found in epidemics along the coast of São Paulo state and endemically from São Paulo down to Rio Grande do Sul state. It transmits malaria to man and is the only known natural vector of simian malaria in the Americas.103

But this knowledge did not emerge in a linear progression from the work published by Lutz in 1903. As with Chagas disease, there was a period of interruption and for a long while forest malaria stayed on the sidelines, with its misunderstood theories.

Lutz was not the only author to study the relationship between the epiphytic bromeliads of American forests and their fauna. An exhaustive inventory of these studies was presented in 1913 by Clodomiro Picado Twilight in *Les broméliacées, considérées comme milieu biologique*.104 The Costa Rican biologist divided them into three categories: studies whose subject was animals adapted to bromeliads, without taking into consideration environmental conditions; those that looked into only the biology and distribution of the plants; and finally, those that concerned themselves with their relationship to the fauna. Of this latter group, the work by Adolpho Lutz stands head and shoulders above the rest.

This author states, on the one hand, that the Culicidae larvae are bred by the hundreds in epiphytic bromeliads in Brazil; he considers that at least one-fifth of the known Culicidae have bromeliads as their exclusive habitat; by the same token, he states that their waste does not rot while they remain within them, but putrefies the moment they are removed from the plant and placed within a wide-mouth jar (Picado, 1913, p.220).

In tropical forests, especially in intertropical America, epiphytic bromeliads took the place of swamplands. They formed what were, to all intents and purposes, wetlands in the air, and were very different from
the environment of standing groundwater. The water in bromeliads constituted a special biological environment. Picado (p.327-9) viewed them as a permanent yet fractioned wetland above the ground, with the water coming from the daily condensation of atmospheric water, and with a cellulosic mud that did not rot, due to the plants’ own activity. It was inhabited by varied fauna, ranging from batrachus to protista, which could be split into two large groups: those animals that only inhabited bromeliads and those that also inhabited other environments. The bromeliad fauna known before Picado’s research summed around 100 species. In the list he presented afterwards, the number had reached about 250.

Many specialists helped him determine these animals, including Knab, Coquillet, and D. Keilin, who helped with Diptera, and Dyar, who helped with Lepidoptera larvae.105 The many authors listed by Picado described batrachus (Ohaus, 1900; L. Stejneger, 1911; C. Werkle, 1910); planarias (P. M. de Beauchamp, 1912, 1913); oligochaetes (W. Michaelsen, 1912); Odonata larvae like those of the genera Megaloprepus and Mecistogaster (Barret, 1900) and others (Knab, 1907; Philip P. Calvert, 1909); Orthoptera of the family Blattidae (R. Shelford, 1912) and others (A. Borelli, 1911); Diptera tipulids (Ch. P. Alexander, 1912), Rhyphidae and Eristalinae (F. Knab, 1912 and 1913), Borboridae (Knab and Malloch, 1912); Coleoptera such as the larvae of Helodidae (Knab, 1913; Picado, 1913) and others (H. Scott, 1912); Hemiptera (W. L. Distant, 1912); and butterflies (Walsinghan, 1913) and similar animals. Knab (1913) had also just described a Culicidae, the Megarhinus iris, which had been bred by F. W. Urich in Trinidad (Picado, p.221). Adolpho Lutz was the author of the most comprehensive study yet into forest mosquitoes, and Picado attributed the description of the disease-transmitting species to nobody else. It was certainly the Brazilian zoologist’s work that formed the basis for one of his conclusions, set out as follows:

Knowledge of bromeliad fauna explains the existence of certain infectious diseases (malaria, filariasis, etc.) in the regions of America that have no wetlands. The bromeliads shelter the intermediate hosts (Culicidae, copepods, etc.) of parasites, whose life cycles end with man or with some wild animal, whether simiae or not. This is how these diseases persist, in the absence even of man or of swampland. (Picado, 1913, p.329)

Adolpho Lutz’s discovery had been confirmed by other important peers, above all by those researchers at the Oswaldo Cruz Institute who had been...
asked for help by businessmen who were investing in railroad construction and hydroelectric power generation in Brazil’s malaria zones. In 1906, Carlos Chagas wrote about an outbreak of malaria at some 700 meters above sea level in a place where it seemed impossible for the Culicidae to live: “The fact caused us some surprise, which then evaporated when we discovered the *Myzomyia lutzi* in the region. This was on one of the ridges of the Serra do Mar mountain range, where the Companhia Docas de Santos set up an important electricity facility.” At the same time, in Trinidad, F.W. Ulrich, and later Lassale and De Verteuil, also highlighted the role of forest mosquitoes [*Anopheles (k) bellator*] in transmitting malaria (Gadelha, 1994).

In the same year that Lutz published his work, Galli-Valério (1903) encountered oocysts in the stomach of specimens from Paranaguá (PR); five years later, Stephens and Christopheres (1908) included *Myzomyia lutzi* among the proven malaria vectors, since they found what they called ookinetes in it, which are malaria parasites at the stage prior to the zygote, when they take the form of tiny wriggling worms.

Even though this species was considered “dangerous,” Arthur Neiva (1909, p.76) believed the evidence that it transmitted the disease was inconclusive. In his view, the poor conditions in which the species were found in Italy after being preserved in alcohol and then sent to Galli-Valério made it impossible to know whether the oocysts he had encountered were of human or bird origin, and so the finding only increased the “likelihood that this anopheles transmits malaria.” However, the ookinetes seen by Stephens and Christopheres could easily be detected in “any anopheles that has fed on gametocytes under transmission conditions; it should not be concluded from this discovery that the ookinete can always become a sporozoite.”

Such provisos signaled the need for a more comprehensive research program, but the growing acceptance of *Anopheles lutzi* as the protagonist of a special type of malaria transmission came to a sudden halt in the early years of the 1910s, and it was only in the 1940s that forest malaria reemerged as a subject of study and was targeted for sanitation control.

**The controversy with Knab and Dyar and its upshot**

Gadelha (1994) and authors he quotes (Downs and Pittendrigh, 1946, 1949; Rachou, 1946) attribute the prolonged ostracism to criticisms made in
1912 by Frederick Knab. Though these criticisms were contained in articles signed only by this zoologist from the U.S. Department of Agriculture’s Bureau of Entomology, they were endorsed by entomologist Harrison Gray Dyar.

Born in Wurzburg, Bavaria, on 22 September 1865, Frederick Knab emigrated with his family to the United States when he was eight. His father, Oscar Knab, was a printer and painter, and one of his brothers had served as a painter in the Bavarian court. Frederick Knab himself showed some artistic talent and after spending a time studying in Germany, he dedicated himself to landscape painting. His interest in natural history and insect life inspired him to take part in an expedition to the Amazon from 1885 to 1889. Later (1903-04), he worked as an illustrator for entomologist Stephen Alfred Forbes. The collaboration then set up with Leland Ossian Howard and Harrison Gray Dyar led to *The Mosquitoes of North and Central America and the West Indies*, a four-volume work published by the Carnegie Institute in Washington between 1912 and 1917. Much of the lengthy study was based on investigations performed in tropical and subtropical countries by Knab and Dyar. The former was also the author of a number of the illustrations included in the work. Dyar, for his part, took charge of the taxonomy part, and thanks to his comfortable financial status, he himself financed a number of the collection trips.

In 1906, Knab joined the U.S. Department of Agriculture’s Bureau of Entomology. After the death of Daniel William Coquillett (1856-1911), he took over the curatorship of the collection of Diptera at the U.S. National Museum.

The *Lepidoptera* section had been curated since 1894 by Dyar, who was also an assistant at the Bureau of Entomology (1904-16) and a captain in the Sanitation Department of the U.S. Army (1924-29). One of the leading taxonomists of his time, he wrote what came to be called Dyar’s Rule, by which different stages of life of a Lepidoptera could be determined by its head measurement. He published a number of articles about the North American species and also studied mosquitoes, especially in their larval stage.

When Knab died on 2 November 1918 of an undiagnosed disease he had contracted on a trip to Brazil, Dyar started researching mosquitoes in...
their adult phase. His studies into the masculine genitalia of the Culicidae were very important in classifying the group. He also studied the families Simuliidae, Psychodidae, and Chaoboridae.

Not only did Dyar often publish papers in his country’s leading entomology journals, but he also set up *Insecutor Insectiae Menstruus*, which brought out fourteen volumes between 1913 and 1927. He died on 21 January 1929, having engaged in heated controversies with some of the leading names in American entomology, including Coquillet, J. B. Smith, and Henry Skinner.

The clash with Lutz started with the publication of an article by Frederick Knab (1912a) in *Journal of Economic Entomology*, in which he analyzed the transmission of diseases by blood-sucking insects. The same theme was also the subject of a written communiqué at the same time at the Entomological Society of Washington. At this session, when Howard set Lutz’s theory about forest malaria against Knab’s argument, the latter declared together with Dyar that the Brazilian zoologist had wrongly interpreted the facts that had led him to formulate his theory.

In the article in the American Association of Economic Entomologists’ publication, Knab stated that the studies into the role of blood-sucking insects in disease transmission that had recently been started were overwhelmingly vague, chaotic, and supported by questionable data collected by investigators with little biological training. “Since the discovery that certain blood-sucking insects are secondary hosts for pathogenic parasites, practically every insect that sucks blood either habitually or occasionally has been viewed as a potential disease transmitter or suspected of being such.”

He went on to say that only insects that were closely associated to man and that regularly sucked blood repeatedly could host and transmit a parasite from human blood. It would not be enough to do this from time to time, like the forest-dwelling mosquitoes studied by Lutz. Other prerequisites were their relative longevity, and continuous feeding on blood and reproduction. Only thus would there be enough individuals for the life cycle of the parasites they hosted not to be interrupted, including the definitive host – man – who would then be struck by an endemic disease.

These criteria were equally applicable to mosquitoes and any other blood-sucking insect. As Tabanidae and Simuliidae did not meet these criteria, they could be disregarded as disease transmitters. Actually, few insects
would meet the requirements set by Knab: the *Aëdes calopus*, synonymous for *Stegomyia fasciata* and an intermediate host of the yellow fever organism; the *Culex quinquefasciatus*, transmitter of filariasis and dengue fever; and the *Triatoma (Conorhinus) megistus*, recently identified by Carlos Chagas as a transmitter of a dangerous trypanosomiasis in Brazil.  

The *Anopheles* was a genus that did not seem so well adapted to humans, but in Knab’s view, this impression arose from a dearth of knowledge about the habits of its different species. The evidence available at the time suggested that malaria transmitters inhabited areas near where man lived, while those that did not feed regularly on blood were harmless.

Adolpho Lutz’s ideas about mosquitoes and forest malaria contradicted such theories. Yet in Knab’s article published a decade after the discovery, he stated his conviction that the Brazilian had “got his facts wrong.” Most likely, the *Anopheles* he had identified had had nothing to do with the outbreak of malaria among the workers camped in the Santos mountains.

It is well known that in the tropics most people, even when they appear healthy enough, have latent malaria. When such an individual undergoes physical stress, such as fatigue, exposure to the elements, or physical exertion, the disease is manifested ... The men already carried latent malaria when they reached the area, and the exertion and exposure associated with their work caused the disease to break out.

In the rebuttal published in the *Proceedings of the Entomological Society of Washington*, Lutz (1913a) struck out against this argument. “If such an etiology of a typical epidemic was possible, which no knowledgeable person could admit, the people that lived here and that were interested in discovering the cause would not have waited until two lay people thought this up, and I would not give myself the trouble of seeking a satisfactory explanation for the enigma.” The Brazilian zoologist reminded his critic that the climate in the area in which his studies took place was not in the least tropical, nor was it in the areas where the people who had contracted the disease came from. The huge effort they had made to distinguish typhoid
fever and other fevers from malaria so as to gainsay the incorrect concept of “São Paulo fevers” had shown that the disease caused by the plasmodium discovered by Laveran was highly localized, “even in tropical countries. Actually, it is not to be found in many places where Anophelines dwell, which themselves are far from being ubiquitous.”

Lutz further argued that his findings had been confirmed by a number of scholars, in particular Carlos Chagas, “which is extremely important, since he observed a number of malaria epidemics in different places and studied the Anophelidae present there.” There had been records of other epidemics of malaria in places without wetlands but with a profusion of epiphytic bromeliads.

Observations made in different forested areas of the Serra do Mar mountain range that had characteristics similar to those described in 1903 by Lutz had led to the generally accepted belief in Brazil that large engineering projects in such environments would inevitably be accompanied by outbreaks of malaria. However, no sign of the illness had been found in worksites in dry areas in Campos and in inland forests that had no bromeliad anophelines.

It cannot be denied that some of the workers hired for these works were individuals that had chronic malaria, but they did not have a healthy bearing, and as Knab supposed, they could easily be identified and discounted.

Going further with his argument against his rival’s theoretical assumption, Lutz stated that two malaria transmitters in Brazil – Cellia albimana and, most importantly, Cellia argyrotarsis – were common in uninhabited areas, moving closer to human habitations only in swampy areas. “The fact that they neither want nor prefer human blood is demonstrated by the well-known fact that they prefer the horse to the horseman ... The same applies to all other species of Anopheles.”

People who entered areas where large animals were rare naturally attracted mosquitoes, and if they stayed there long enough,
It is in this paragraph that the crux of the matter lies. Knab held that malaria, yellow fever, and other “parasitic” diseases were transmitted only by blood-suckers that were already accustomed to human blood. Knowledge about the different species capable of performing this role grew considerably at the time, but the system Knab adhered to was conservative, almost static. Lutz had perceived the chance of humans being involved in existing or emerging cycles in the wild, and not only for malaria. In a later communiqué – the counter-rebuttal to Knab’s second article – the Brazilian scientist stated:

Messrs Dyar and Knab believe that mosquitoes that have never been in contact with man cannot transmit disease. One must place men in totally uninhabited areas to test his thesis. Overall, this is rather difficult, but it so happens that in Brazil roads and railroads have been built under such conditions, and there is almost always a malaria epidemic. Epidemics of Leishmaniasis skin sores have also been noted in absolutely deserted areas, correctly attributed to transmission by Phlebotomus. I have also witnessed a small epidemic of yellow fever among people who lived in a place where one might expect there to be forest mosquitoes. All this shows that the theoretical considerations have not been respected by the facts, and all that is needed is for the transmitter, whatever its past may be, to belong to a category in which the parasite can develop; then it must have repeated access to human beings, some infected and others without immunity. As the development process takes time, its life cannot be very short. For this reason, it is a favorable condition for it to be egg-laying (Lutz, 1913b).

In his response to Lutz’s first rebuttal read by Dyar at a session of the Entomological Society of Washington, Knab (1913a) had offered his mea culpa for the dogmatic tone he adopted in his first paper, and for his arrogance in putting forward explanations at such a distance in geographic and temporal terms from the problem studied by the Brazilian zoologist. “Personally, Dr. Lutz’s writings have inspired me greatly, and it was in no way my intention to discredit him.”

After rereading his paper and its rebuttal, he had reached the conclusion that they were in agreement on all the points except one:

I am not inclined ... to admit that a species of Anopheles which is only found in uninhabited forests, and which normally could not have access to human blood, should all of a sudden become the host for a parasite of human blood. Certainly this could occur, and maybe it is just this that is in question, but in my view it would be so unusual as to require extremely conclusive proof.
Knab and Dyar had three hypotheses to explain the outbreak observed by Lutz. First, that he had not identified the true transmitter. Second, that the disease had been transmitted by *Anopheles lutzii* from a latent case among the workers; after a period of incubation, first in the mosquito and then in man, it had spread among humans as a consequence of bites by infected *A. lutzii*. Third, that there was a form of malaria among the wild animals that inhabited the forest which was transmitted by *A. lutzii*; when humans had entered the area, they had been exposed to bites by mosquitoes that had previously been infected by wild animals, and had developed the disease originated from them.

As Knab and Dyar themselves considered their second and third hypotheses implausible, they were convinced that Lutz had made some mistake, as explained in the first hypothesis.

In a short investigation trip, Dr. Lutz found one single species of *Anopheles* at the location and immediately concluded that it was responsible for the outbreak of malaria ... He supposed that no other species of *Anopheles* could be present because it seemed to him that there was no suitable breeding ground except for the bromeliads.

In justifying his “incredulity,” Knab turned to his own collection experiences in regions whose characteristics he considered similar to those encountered by the Brazilian zoologist. He had found two anopheline species in small pools in a mountain riverbed, two of which (*A. argyritarsis* and *A. eiseni*) occurred in southern Brazil. In Cordoba, Mexico, he had collected larvae in a canyon which was ‘washed’ by flash floods after each heavy rainfall.

Knab believed that the relationship between the malaria parasites and certain *Anopheles* species depended upon a highly delicate physiological adjustment. Of those that fed on blood from the same source in a given location, some were efficient hosts for the parasites, while others simply digested them together with the blood. Generally, the most common *Anopheles* in a region was the one that served as host, but it was not always easy to estimate the relative abundance of different species in a place, since some were more easily spotted than others, some better at hiding in rooftops, for example, or camouflaging themselves with objects of the same color.

With his line of argument, he aimed to show that Adolpho Lutz had failed to notice other anopheline species in the water that existed in the
Santos mountains. The American entomologist believed there may be another source of error in his theory.

The question arises naturally: how completely and for how long were the laborers confined to the forest habitat? Did they not take holidays, either individually or in small groups, away from that area or pay night visits to taverns and places of pleasure situated nearby? What we know of the habits of *Homo* tends to give us reason to suspect as much!

The third hypothesis that may explain Lutz’s discovery seemed to Knab and Dyar no more than an interesting possibility.

There are no known material organisms that inhabit wild animals and that can be transferred to man, though it seems plausible that there may exist monkey parasites that can be transmitted by forest *Anopheles* and that man may be susceptible to this. If such a relationship does exist, it could be demonstrated by means of a suitable study, but I consider that we do not have the right to invoke it as an explanation for the present case based on no more than its possibility.

What was left, then, was Lutz’s claim that a totally “wild” *Anopheles* species had become an efficient host for the parasite of human malaria. In
Knab’s view, this seemed “so unlikely that no other evidence except for a demonstration of the presence of the parasites in the mosquito’s salivary glands would make [him] accept it.”

Like Neiva, in 1909, he considered Galli-Valério’s (1904) notification about the presence of oocysts of the malaria parasite in the walls of the stomach of specimens of *Anopheles* cited by Lutz. “Even a versed student of American mosquitoes would hesitate to positively identify such specimens, and I do not recall Galli-Valério before having given the slightest attention to American mosquitoes!”

Knab and Dyar issued Lutz a challenge: that whatever the true explanation was, “the burden of truth lies with the investigator ... we have the right to wait for the proof to be complete or to reject the explanation provided.”

In Lutz’s counter-rebuttal read by Howard in a session of the Entomological Society of Washington, Lutz (1913b), in a paragraph quoted above, refuted the assumption that mosquitoes that had never before been in contact with man could not transmit malaria, leaving open the chance that the infection of bromeliad mosquitoes could initially occur through contact with a non-human host that inhabited the forest.

Lutz then attacked the weakest links of the arguments against him made by the American entomologists.

I am accused of ignoring the fact that men are men, and it is said that they must have slipped away at night, getting infected elsewhere. I have already said that they lived in the forest some many miles away and had no means of getting around except by foot. Even if they had gotten out of control, they could not have gone far enough away in one night for them to reach anywhere where they could have found that which is suggested they may have found, nor would this have helped, because there was and there still is no malaria in those areas. If one were to take such an argument further, one could also say that the Italian sailors who contracted yellow fever on board the war vessel anchored off Rio de Janeiro some distance from the coast were attacked because they swam inland at night.

Lutz scoffed at the statement that Galli-Valério was not equipped to identify the *Myzomyia lutzi*, even though “it is an extremely characteristic species that can be differed from all others by a quick look at its scutum.” It was with justified disdain that he dealt with the accusation that he – as well as Chagas – had failed to note the presence of other anophelines in the areas where he made his studies, “even though Messrs Knab and Dyar
can see them from Washington. Yet it was to be expected that Lutz and Chagas knew their anophelines, since they had worked with them for many years and had together identified most of the Brazilian species.”

But Adolpho Lutz made no comment about his adversaries’ demand that he provide proof that the mosquito from the forest in fact hosted the malaria parasite. He had not followed the protocol established by Manson in the study of filariasis transmission and replicated in the studies by Ross and the Italians about the transmission of bird malaria by *Culex*, and human malaria by *Anopheles*.

The study program that Lutz used was a combination of exhaustive study of habitats and the distribution of *Myzomyia lutzi* and the vegetation that hosted it with field studies into the cases of malaria in humans that occurred in the environments where forest and bromeliad mosquitoes lived in great numbers. It was a program that required the use of entomology, botany, epidemiology, and medicine, but which totally bypassed the wisdom of Ross, Grassi, and other investigators who had spent their time observing the evolution of the parasite in the organisms of its intermediate hosts. Though he had demonstrated his skills in this type of experimental procedure in the parasitology studies he had published in the 1880s, Lutz would not take the route indicated by his opponents. He concluded the matter, maintaining “entirely” the exactness of his observations and emphasizing the “practical interest” inherent to them.

This is why I oppose them ... and reject the comment that most likely the gentleman made a mistake, which seems to imply that it is a habit of mine to commit errors when making scientific observations. Being as I am in less of a hurry to communicate my observations than many these days, I do not believe my quota of errors to be unusually high.

These and other equally cutting remarks by Lutz, reintroduced into the present edition of his text, were suppressed by the editor of the *Proceedings of the Entomological Society of Washington*, which triggered a sharp protest by Frederick Knab. The letter he sent to W. D. Hunter on 24 January 1914 shows Adolpho Lutz’s authority in that new field of knowledge – medical entomology – as well as the leading position he held among his peers in Brazil, especially the talented group of investigators at the Oswaldo Cruz Institute.

If we leave to one side the ethical question, the change in the tone of the letter adds a completely different nature to the controversy ... placing Dr.
Dyar and I in a very unpleasant position. The researchers in Rio de Janeiro... will naturally draw the conclusion that... we have made use of untoward means to sidestep their forceful criticisms. I fear this procedure, however trifling it may seem to you... may cause irreparable damage. It may well have caused a rupture between us and the Rio researchers that may never be repaired. I am most sorry about this, as it will seriously affect my work. For some years it has been my aim to cultivate friendly relations with these researchers so that we may work in harmony and facilitate each other’s investigations, and I had found their response encouraging. As you undoubtedly know, by far the most important medical entomological work being done this side of the Atlantic is by Lutz and his associates, and from a dipterology standpoint, above all, it would be most desirable to maintain contact with them.

In his reply dated 27 January, Hunter seemed surprised at their reaction. “The only alterations I made were to omit two sentences that in my view were too scathing to appear... In view of this, it seems to me that the publication of the letter... has served more to maintain an entente cordiale than to destroy it.”

The tone of the letter that Knab sent to Lutz two days later clearly shows his desire to restore good feelings to their relationship, which, to all intents and purposes, was sustained by a substantial, important scientific exchange for both parties. In 1911, Knab had published a detailed, complimentary criticism of two papers by Lutz (1909, 1910) about the Simuliidae of Brazil. In the American’s view, it was by far the most comprehensive study yet dedicated to this so interesting, so economically important group of Diptera... it is gratifying to see that Dr. Lutz does not adhere to the systems of the old school, but rather approaches his subject from all angles. He grants due importance to data obtained at initial stages from biology, matches them with the characteristics of the imagoes, while at the same time carefully considering any potential sources of error.

In a letter in January 1914, Knab respectfully consulted Adolpho Lutz about the habits and classification of two species of Ceratopogoninae he had been studying, and alluded to a comment previously made by his Brazilian colleague that he still had a lot of unpublished material about mosquitoes. “Why not publish it? It is hardly likely that your biological data at least will conflict with our book... Most of your observations will undoubtedly be original, and the rest, more comprehensive, thereby improving on what we have been able to present.”
Lutz’s theory in limbo

Gadelha (1994) states that the decline of Adolpho Lutz’s theory became more marked in the 1920s, when well-known malaria specialists produced work that seemed to invalidate it. He specifically refers to Darling and to Nelson Davis. The former had shown that no malaria vector came from bromeliads in Panama, thereby prompting cancellation of a costly project to eradicate these plants that had been launched in that country.

Davis investigated an epidemic in a mountain area of Angra dos Reis (Rio de Janeiro), where more than one railroad was built. Though he attributed most of the cases to relapses of chronic malaria, as Knab had about the cases examined by Lutz in the Santos mountains, Davis was forced to admit the existence of local transmission. This led him to study the infection capacity of forest malaria mosquitoes, which were now included in the genus *Kerteszia*. He obtained a very low frequency, and concluded that they were capable neither of setting off nor of maintaining an epidemic. Only when an epidemic was at its height, when there were numerous carriers of highly infected gametocytes, could the species perhaps contribute with some cases of transmission.

That very same year, Arthur Neiva (1925, p.4) produced a report for The Light and Power Company about the sanitary risks involved in introducing one of the Canadian company’s projects in the Serra de Cubatão mountain range, the same area where Lutz had discovered forest malaria. The entomologist from the Oswaldo Cruz Institute, who had considerable experience working in campaigns to fight the disease, did not hesitate in stating that the main problem was the *Myzomyia lutzii*, which accounted for 40% of all the mosquitoes that lived in the bromeliads in that region. In his report, Neiva referred to the campaign he had led against that mosquito in Iguape, São Paulo, which had mainly consisted of deforesting the area that they planned to protect.

Gadelha (1994, p.136) finds it strange that the opposing viewpoints of Neiva and Davis should not have triggered a controversy. In his view, this was not only due to the limited circulation of the report written in Portuguese, but more importantly to the fact that “Davis’s conclusions were based on lab work, while Neiva’s were founded on generic entomologic and epidemiological observations.” Based on Deane (1986, p.7), Gadelha shows that the dissection of salivary glands to check for the presence of oocysts...
was a laborious, delicate technique, in which a small number of individuals were used to check infection rates in anopheles. With the introduction of a new technique at the end of the 1930s, it became possible for entomologists to examine hundreds or even thousands of salivary glands. It was then clear that Davis had been mistaken. In Gadelha’s article, the reader will find a reliable analysis of the chain of events that granted forest malaria greater visibility as of the 1940s.

Lutz did not live to see this resurgence or the campaigns introduced by the National Malaria Service (1941) that caused the destruction of millions of bromeliads in southern Brazil.

In a paper published in 1903, he had considered it impracticable to deal with these plants. The use of chemicals to destroy the larvae sheltered in them had seemed to him “more theory than practice ... Normally, the only thing that gets results against malaria in the wild is making clearings.” Malaria in wetland plains was more easily combated using a set of preventive measures. In a report written at the turn of the 20th century, Lutz referred to studies performed in different parts of the world by English, German, and Italian committees, and stated that clear guidelines could now be established for fighting the disease that were very similar – as we shall see – to those that would soon be adopted to fight yellow fever. The director of the Bacteriological Institute stated:

1 – due to the habits of the parasites in the plasmodium discovered by Laveran, of which there are many species, malaria is only transmitted by mosquitoes of the genus *Anopheles*, which serve as hosts during an essential phase of their evolution; malaria only occurs where there is an abundance of *Anopheles*; 2 – if it is to transmit the malady, the *Anopheles* must bite people with malaria at an appropriate stage in the development of the disease. Only after a certain period of time, which varies between eight and fifteen days, will these insects be capable of transmitting the malady to other individuals by biting them.

Lutz believed malaria could be combated in any of three ways: by treating people with the disease to stop anophelines from infecting them; by destroying the mosquitoes, especially their larvae; by stopping infected mosquitoes from biting people by using mosquito nets, houses protected with screens, fumigation, and other such measures. “It is seen that these last means may stop groups of individuals who spend the hot months in areas where malaria is commonplace from being infected.”
In “Instruções sobre a profilaxia do impaludismo” (Instructions on the prevention of paludism), a previously unpublished text being released for the first time in the present volume of his Complete Works, Adolpho Lutz sets out in more detail what he calls “offensive,” “defensive,” and “specific” or “therapeutic” preventative measures.
Yellow fever from microbes to mosquitoes

Accountings of events have attributed the victory of scientific medicine over yellow fever either to the US or to Cuba, depending on the weight given two factors: Cuban doctor Carlos Juan Finlay’s formulation of the theory of the disease’s transmission by mosquitoes in 1880-81 and its experimental demonstration in Cuba by the U.S. team under Walter Reed in 1900.

According to Nancy Stepan (1978), the essential ingredients for elucidating the enigma of yellow fever had been present for some time, but social and political obstacles prevented an earlier victory over the disease. The conviction that the disease was entrenched in the Cuban soil, the Spanish metropolis’ lack of interest in science, Cuba’s prolonged war of independence (which began in 1868 and resumed in 1895), and, finally, the island’s occupation by the United States three years later all conspired toward this delay. The U.S. Yellow Fever Commission visited that island in 1879-80, and from it Finlay had picked...
up the hypothesis that yellow fever was caused by a germ that suffered a transformation outside the human body before infecting a susceptible person. A description of the intermediary host of the rust fungus (cereal disease), presented in a well-known botanical treatise by French scientist Philippe Edouard Léon Van Tieghem (1830-1914), led the Cuban doctor to conclude that the transmission of yellow fever was probably the work of an agent that existed independent of both the sick person and the microorganism of the disease.

Entomological and epidemiological studies led him to verify that the female *Culex* (later denominated *Stegomyia fasciata*, currently *Aedes aegipti*) transported a live, infecting particle from a sick to a healthy person via its proboscide. Finlay first published his theory in Spanish (“El mosquito hipoteticamente considerado como agente de transmisión de la fiebre amarilla,” 1881) and then in English (“Yellow fever: its transmission by means of the Culex mosquito,” Jul.-Oct. 1886). Stepan points out that knowing the precise species would have been sufficient for eradicating the mosquitoes and thereby controlling the disease, if there had been due interest.

On examining the events of 1900, we verify that the same arguments that had previously seemed inconclusive, or even implausible, had acquired great plausibility when the needs of the military demanded a quick solution for the yellow fever problem. The fact that the Reed commission needed only two months to confirm Finlay’s hypothesis, and that it committed many of the mistakes attributed to him, suggests that it does not suffice to blame his science to explain its inactivity ... One needs to examine the political and social context in which yellow fever was perceived in the United States, and the reasons behind such a drastic change in perception in 1900. (Stepan, 1978, p.402)

For François Delaporte (1989), Finlay and the North Americans diverged in their view of the mosquito: the first saw it as a mechanical medium of transmission and the second, as an intermediary host associated with a more complex biological process. Finlay’s decision to make the mosquito an object of study, and the time that elapsed between the proposition and
confirmation of his theory, were enigmas that would be explained by English tropical medicine, through the relations of conceptual kinship that linked the Cuban doctor to Patrick Manson, and Walter Reed to Ronald Ross. For Delaporte, Finlay’s hypothesis remained in limbo for twenty years because this was the time necessary for establishing malaria’s mode of transmission.

When Ross revealed the parasite cycle of bird malaria in the *Culex* mosquito in 1898, and Giovanni Grassi, Amico Bignami, and Giuseppe Bastinelli revealed the parasite cycle of human malaria in mosquitoes of the genus *Anopheles* the following year, it became inevitable to suppose that these insects played the same role in yellow fever – often confused with fevers caused by different species of *Plasmodium* during clinical diagnosis, as we have seen.

For Manson, the mosquito was important in that it was the host or nurturer of a parasite that completed part of the cycle necessary for its preservation as a species within the insect. Dealing with the transmission of an unknown germ, Finlay saw the mosquito bite in terms of the advantages that it brought to the insect. He verified that the female of the species engaged in not just one but frequent feedings of blood, and deduced that so much blood in such a small body could only serve to maintain optimal temperatures for maturing eggs. He concentrated on smaller-sized mosquitoes that needed several feedings and needed to lay eggs several times. A study of culicid fauna in Cuba and its cross-comparison with yellow fever’s geographic distribution led him to point to the small *Culex* as its agent of transmission. Under the light of this theory, he was able to explain a variety of intriguing epidemiological aspects of the illness (Delaporte, 1989).

**The Reed commission’s experiments in Havana**

The U.S. medical commission nevertheless did not immediately accept Reed’s theory. On 25 June 1900 in Cuba, tasks were assigned to team members according to the instructions of George Sternberg, Surgeon General for the U.S. Army and a man who had eyes only for the bacteriology of yellow fever. Investigations were brusquely redirected from the icteroid bacillus to Finlay’s hypothesis after a meeting (which will be addressed further on) with English doctors from the recently created Liverpool School of Tropical Medicine. On 11 August, Jesse William Lazear (1866-1900)
began his experiments with mosquitoes, while James Carrol (1854-1907) and Aristides Agramonte y Simoni (1868-1931) continued the bacteriological studies.

The first two cases of positive infection by *Culex* were obtained toward the end of that month. On 25 September, Lazear suffered a tragic death due to an accidental mosquito bite. Walter Reed, who was in Washington finishing up the report of a commission on typhoid fever rushed back to Havana and hurriedly wrote the “Preliminary Note” presented at the 28th Annual Meeting of the American Public Health Association in Indianapolis, held 22-26 October 1900. Although he used mosquitoes born of larvae in captivity, Lazear had not established sufficient control over the inoculated individuals to rule out the possibility of other sources of infection. Reed, who until then had not taken part in the experiments, took on himself the task of completing them.115

Three series of experiments were conducted between November 1900 and February 1901 at Lazear Field, in the vicinity of Quemados, Cuba, sheltered from the epidemics, at a place that had been drained, was sunny, and exposed to wind. Volunteers were recruited from among immigrants and U.S. soldiers, quarantined before being bitten by mosquitoes that had been previously infected by yellow fever sufferers. The aim of this first series was to confirm that the mosquito was the intermediary host of its ‘virus’. Of the six volunteers bitten, five showed symptoms of
The commission reached the conclusion that twelve days were necessary after contamination of the mosquito for the germ to travel through its stomach, reach its salivary glands, and make it capable of transmitting the infection.\(^{116}\)

The next experiment was set up in a room divided into two areas by a metal screen. Infected mosquitoes and a volunteer who let himself be bitten repeatedly were placed in one of the areas. Two witnesses remained many days in the other, protected area, without contracting the disease. The intention was to rule out the firmly entrenched notion that the air – vehicle for miasmas and germs – could transmit yellow fever. Reed wanted to demonstrate that a dwelling place could only be dangerous if it contained infected mosquitoes.\(^{117}\)

In a second series of experiments analogous to the ones carried out a century earlier by the anti-contagionists, three volunteers were confined for twenty consecutive nights in a room full of objects covered with the vomit, feces, and urine of patients who had died of yellow fever. None of them contracted the disease, thus invalidating once again the notion of contagious fomites and the procedures that resulted from this belief: disinfection of clothes and objects supposedly contaminated by contact with the sick.\(^{118}\)

During September and October 1901, the U.S. commission carried out another series of experiments related to the yellow fever germ. Blood from a diseased person was injected into four volunteers, producing three positive cases; this proved that the disease was present in the circulatory system and that it could be transmitted by the prick of a needle. The next step was to verify if this was a filterable virus, a hypothesis that had been raised by the bacteriologist Frederick George Novy and suggested to Reed by his former professor, William Welsh. Serum was extracted from the blood of a diseased person, and its inoculation produced an experimental case. The serum was subsequently heated to 55° centigrade and inoculated, without producing any results. This procedure demonstrated that the virulence could not be attributed to a toxalbumin secreted by a bacillus. Finally, after
passing it through Berkefeld and Chamberland filters, the serum was diluted and injected, provoking a clearly defined onset. It was understood then that yellow fever could be caused by such a minute microorganism that it could go through the most closely meshed filters and remain invisible to the most potent microscopes. Bacteriologists’ interest in this category of “ultra-microscopic” agents had been stirred by Friedrich Löffler and Paul Frosch’s March 1898 discovery that foot and mouth disease was induced by an agent of this kind. Ironically, Sanarelli had been one of the pioneers in studying “viruses” – a concept that was only just gaining shape in its modern sense – and had described the properties of the invisible agent responsible for myxomatosis in rabbits (Hughes, 1977).

On deciding to test this hypothesis, Reed and Carroll faced an environment that was already unfavorable to the use of ‘human guinea pigs’. The first experiments did not result in fatalities, but their reenactment by a Cuban team under the direction of José Guiteras in the summer of 1901 caused the death of three of the seven inoculated volunteers, leading to a great commotion among citizens of Havana and frightening off new candidates. Löwy (1991) assures us that this kept the Reed commission from providing new and conclusive proof that the yellow fever agent was a filterable virus.119

The anti-Culex campaign that U.S. military forces deployed in Cuba was a success. Diseased patients were isolated under mosquito nets, and mosquitoes and larvae were exterminated, breaking the disease’s cycle of dissemination and bringing yellow fever under control in just six months. The experiments carried out by the Reed commission were then confirmed inside the walls of a few laboratories and in the open field, especially in
some Brazilian cities, until being accepted as definitive at international scientific forums.

However, before we proceed to examine the developments mentioned above, let us examine one thread of the complex social and technical web in Cuba, since it is directly related to an event described earlier. The damning report that Adolpho Lutz wrote on Caldas’ serum did not mean the death of the invention by the doctor from Rio Grande do Sul. His greatest vulnerability was that the yellow fever microbe was unknown. On 28 April 1898, Felipe Caldas presented to the National Academy of Medicine yet another communication on the “transformation of the colibacillus into a bacillus that produces yellow fever.” Refuting Sanarelli, he then stated that it was a colibacillosis, in other words, a form of infection caused by the colon bacillus, which became malignant under the influence of biological and environmental factors. This saprophyte, normally present in the intestinal tract, would change its morphological characteristics in the presence of other microorganisms and become a terrible pathogenic agent. The theory was received respectfully at the New York weekly Medical News, whose editor commented:

Caldas’ communication is interesting because it represents a serious and, at least in the eyes of its author, successful attempt to prove in practice the interesting theory that bacilli can be polymorphous, as so frequently discussed in recent bacteriology – that is, that they can exist in different

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forms, each of which has a unique virulence. This stands as an important contribution to the question of whether or not a non-pathogenic virus may turn into a pathogenic virus.\textsuperscript{121}

Caldas subsequently developed a vaccine for preventive use and made his last (or second-to-last) bid in favor of his inventions in Cuba, at a time when a successful battle was being waged against the disease, in light of Finlay’s theory. He arrived in Havana in July 1901, along with his assistant, Dr. Bellinzaghi, as well as a commercial representative – “a businessman.” He proposed that his discovery be demonstrated to an official commission. When he was asked for letters of recommendation, he telegraphed the United States, and two weighty credentials arrived promptly at the desk of the Military Governor: one from the Secretary of War and the other from the Brazilian Consul General in New York. A commission was set up, the members being V. Havard, William Gorgas, Juan Guiteras, Carlos Finlay, and a certain Dr. Albertini.\textsuperscript{122} It is probable that they performed their task at the same time that Gorgas and Guiteras tested another vaccine on human volunteers, at Finlay’s suggestion, independently of the Reed commission’s endeavors. As mentioned earlier, the death of three human guinea pigs would jeopardize these studies on the ultra-microscopic virus.

On 9 August, the commission met with Caldas in the Las Animas Hospital and heard from him a description of the procedures employed in preparing his two immunizers. He explained that he first injected a dose of the serum to neutralize any strong reaction to the vaccine. The commission asked him to demonstrate the existence of the microbe and to describe the method used to isolate and culture it. Caldas refused to do so, alleging that he had made a pledge of secrecy to a company created to develop his discovery. Had they authorized him to vaccinate non-immune subjects, and if these people were bitten by infected mosquitoes without subsequently developing the disease, the causal relation between the microbe and yellow fever would have been proven, indirectly.\textsuperscript{123}

After lengthy discussions, the inquirers reached an ambiguous decision: they would not take part in the experiments, since the Brazilian was withholding data crucial to addressing the issue scientifically, but they would allow him use of the necessary facilities and assume responsibility for checking results and reporting on them afterwards. Bellinzaghy hired four non-immune immigrants and had them sign a written agreement, as
required by the commission, stipulating that they receive monetary compensation for the risks they were voluntarily assuming. Two were rejected due to a suspicion that they were immune. The other two – robust Spaniards who had recently arrived in Cuba – were admitted to the hospital and confined in mosquito-proof rooms; they were inoculated by Caldas himself. According to the Brazilian, immunization took four days. The commission thought it better to wait a week. On 22 August, two infected mosquitoes, whose history had been known from the very jar in which they were born, bit Paulino Alonso. Three days later, the signs of a typical and serious case of yellow fever developed. Caldas’ visits became more infrequent and when the commission met to establish a formal diagnosis, he was absent. He was a sore loser. Right to the very end, he denied that the disease was yellow fever, arguing instead that it was septicemia. According to Major Doctor Harvard, in charge of reporting the case, this statement put Caldas in a “strange and unenviable position,” since he believed

that the mosquitoes that bit [sic] the men with yellow fever could only transmit septic infection and yet he proposed that these mosquitoes would serve as proof of the value of his vaccine! ... The fact that he accepted the mosquitoes for the experiments makes it doubtless that, had the results been negative, Caldas would proclaim the triumph of his vaccine.\footnote{124}
The ‘Havana theory’ in Brazil

The experiments carried out in Cuba in 1900-01 have doubtlessly become a watershed in the history of yellow fever and, because of the disease’s importance in Brazil, an equally distinct dividing line in the history of the nation’s medicine and public health. The discovery of its mode of transmission made possible sanitary campaigns that were for some time able to neutralize epidemics in urban centers along the American coastline, silencing the controversy over the disease’s etiology.

Ronald Ross’s rise to fame as the “bold British successor to France’s Pasteur and Germany’s Koch” (Worboys, 1976, p.85, 90-1) was decisive in bringing to reality a project that Patrick Manson had defended in conferences at St. George’s Hospital in October 1897: to invest in training medical doctors in what he termed “tropical medicine.” In June 1899, the Liverpool School of Tropical Diseases began its existence, and in October, the larger London School of Tropical Medicine was inaugurated.

It is worth mentioning that during the same period, Brazil also witnessed an attempt to establish a chair in tropical diseases at the Schools of Medicine in Bahia and Rio de Janeiro, with Francisco Fajardo being considered for
the latter position. (The syllabus offered in Liverpool was published in Brazil-Medico, 1 Jul. 1900, p.220-1). The fourth Brazilian Congress of Medicine and Surgery (v.2, p.74) took place in the Brazilian capital in June 1900. Vitor Godinho and Carlos Seidl, representing O Brazil-Medico (22 May 1900, p.173-4) and Revista Médica de São Paulo proposed that the Legislature be asked to urgently create two new chairs at those schools, one in tropical pathology and medical practice, the other in clinical bacteriology and microscopy. The proposal was defeated by twenty-one votes to three at the 19 June session (Leão de Aquino, 1945, p.170-1).

In 1900, Drs. Walter Myers and Herbert E. Durham, from the Liverpool School of Tropical Medicine, took off on an expedition to Brazil to investigate yellow fever. Their June encounter with the North Americans was a stopover that resulted in the establishment of a long-lasting experimental center of the English school in the Amazon. Myers would eventually die in Belém, on 29 January 1901, a victim of the disease he had traveled to study.\textsuperscript{125} In Havana, they met with the members of the U.S. commission; with Major William Gorgas, who headed the Bureau of Inspection of Infectious Diseases; with Henry R. Carter, from the U.S. Marine Hospital Service; and also with Cuban doctors: Finlay, Guiteras (professor of the Havana School of Tropical Diseases), Bango, and Martinez. Durham and Myers (1900) brought with them a generic hypothesis – that a host insect transmitted yellow fever – and it gained consistency with the information gathered in Cuba. The article they published in September was skeptical of Sanarelli’s bacillus, praised Finlay’s ideas, and defined the unknowns that provided a glimpse of the contours of the hypothetical live vector. The feeling one gets from reading this article is that if the North Americans had not chosen to go down this path, his theory would have found its justification through the hands of the Englishmen in northern Brazil.\textsuperscript{126}
Studies by Ross, Grassi, and collaborators had also led to a change in the approach to yellow fever at the Bacteriological Institute of São Paulo, resulting in serious ruptures within the team. In 1898, Vital Brazil raised the first experimental objections to the icteroid bacillus, and Adolpho Lutz began his studies of the distribution of *Culex* and *Anopheles* across the nation. In February 1900, Arthur Vieira de Mendonça, another of Lutz’s assistants, left the institute. “For the medical field, the mosquito bears ridicule on its wings” was his statement to the São Paulo newspapers (Antunes et al., 1992, p.64, 67).

In the present volume, we reproduce three articles published in the *Revista Médica de São Paulo*, in which Lutz and Mendonça expound their now opposing views on the bacillus discovered by Giuseppe Sanarelli.

Headed by Walter Reed, the commission presented its findings at the Third Panamerican Medical Congress in Havana, in February 1901, at the same time William Gorgas began his campaign against the mosquito in that city (Reed, Carrol, and Agramonte, 1901). One month earlier, the sanitation commissions in the Brazilian cities of Sorocaba, Santos, and Campinas included as part of their routines the cleaning up of stagnant waters containing mosquito larvae. The battle against the mosquito played a bigger role in defining the measures adopted in São Simão. The outbreak of yellow fever began in that city in May 1902, but it was only in August that the commission appointed by Ribas was put into action; here, it also reconciled guidelines derived from the ‘Havana theory’ with disinfection measures. In Ribeirão Preto (1903), these guidelines were abandoned for good (Franco, 1969, p.64-6).

In a leaflet published in 1901, entitled “O mosquito como agente da propagação da febre amarela” (The mosquito as an agent of dissemination of yellow fever), Emílio Ribas endorsed the Reed commission’s work publicly and without reservations. Although he recognized the need for “more experimental facts to conclude, positively and confidently, in favor of Finlay’s theory,” he went on to say that

on the other hand everything leads us to believe that yellow fever is a malady that disseminates itself through mosquitoes, in the manner of paludism and filariasis ... Among us, Dr. Adolfo Lutz, director of the Bacteriological Institute, has been concerned with the topic and continues his detailed research on such an important question.
As a matter of fact, Lutz had been correlating the presence of mosquitoes to the yellow fever epidemics in the territory under his jurisdiction for quite some time, and therefore verification of the Cuban doctor’s theory had not surprised him. São Paulo’s Sanitation Service publication brought already, in the form of an inset, his first systematic observations on mosquitoes as agents of disease transmission.

Lutz’s note concerned two species of mosquitoes of wide geographic distribution usually found in homes, only one of which, however, had been positively linked to the transmission of yellow fever. It is interesting to note that both Lutz and Theobald at first had trouble correctly identifying the species. In the first letter to his Brazilian colleague, dated 28 April 1900, the entomologist from the British Museum commented that their wide-ranging distribution and variety of local names raised significant obstacles to the identification of certain species, especially *Culex taeniatus*, even in older descriptions. Ficalbi’s and Arribalzaga’s works were cited as valuable resources in this endeavor. Lutz’s answer to Theobald included a request for a sample of that *Culex* to compare with his own. A few months later, after receiving updated literature, he declared that he was already able to determine his species. In observations sent to Theobald on the material identified, he noted that *C. taeniatus* was commonly found in houses in coastal regions or inland, but not in the capital city of São Paulo. He had not yet postulated its relationship with yellow fever. It was only in a letter dated January 1901 that he informed Theobald that he had been paying particular attention to this species because its distribution coincided with that of the disease. His observations were published that same month by Ribas.

Terms such as ‘mosquito rajado’ (literally, striped mosquito) or ‘pernilongo’ (long-legged mosquito) – both colloquial names for *Stegomyia fasciata* – were commonly used to designate blood-sucking dipterous insects. Lutz had previously studied “a couple dozen species” in Brazil, most of which were only present in the wilds or in swamplands, the setting of the investigation he was finishing on the transmission of forest malaria. Only two species (*Culex taeniatus* and *Culex fatigans*) were frequent ‘tenants’ in human households. Although found in almost all regions offering the right climatic conditions, they were “extremely rare” species in uninhabited or uncultivated areas.
Letter to Adolpho Lutz from Frederick Vincent Theobald (1868-1930), of the Zoology Department of the British Museum (Natural History), dated 28 April 1900 (BR. MN., Acervo Adolpho Lutz, pasta 267, maço 2).
Zoological Department.

FROM P. V. THEOBALD.

2. *Culex taeniatus*—This occurs nearly everywhere in each country seems to have a local name. Hence the tremendous difficulty of identification in all old descriptions—In fact, I hardly think other valzopen are alone of much value—

*Most of your Anophèles, I make new*

1. I call *A. alliptes*—Very like *A. alliptis*—But with black on last hind tarsal joint.

*2*.* I call *A. Lutzii*—With 2 broad long thoracic dark stripes.

3. Your Hind tarsus in *A. alliptis* and

Amongst your Culicés, I find *Culex Biptie*—A large handsome species.

*Culex taeniatus*—Wid Thorax.

A another from Curicó Morena—*Pouphora euphala*—M. Bororo. Your Rain amounts I am placing with others in a new form, Appendix, I shall write you a full account on these finished directly. I am not a museums official and only show this neglected piece to them.
Official communication addressed there, very few species. I should be glad to see as the Trustees of the British Museum are going to issue the same as a Monograph with colored plates. I wish it to be as complete as possible.

Yours,

E. Theobalds

Private address, Wye Court
Wye
Kent
The most widely scattered is *Culex fatigans*, our common night-time mosquito, which is found almost everywhere since it is less sensitive to the cold. It transmits filariasis and certain bird haematozoa; I do not consider it a suspect in the case of yellow fever. *Culex taeniatus*, on the other hand, is the only one that can account for the distribution of yellow fever, because we know that it has been described, under various names, in almost all the places where yellow fever has reigned.

Lutz pointed out its occurrence in the United States, Cuba, Buenos Aires, on the south and west coasts of Africa, in Hawaii – in short, in all places where yellow fever had spread. He did not, however, associate it with *Culex fasciatus*, a species used in the experiments carried out by the Reed commission in Cuba. The Brazilian zoologist followed the classification proposed by Giles (1900) and the one Theobald sent him in August 1900.128

Upon creating the genus *Stegomyia* in 1901, Theobald placed in it the *Culex* species related to yellow fever transmission, including *C. taeniatus* and many of the synonyms pointed out by Lutz.129 The species was then named *Stegomyia fasciata*. It was a mosquito that “had had 36 synonyms from 1762, when Linnaeus classified it as *Culex aegipty*, until 1926, when Silver renamed it *Aedes aegypti*, as it is currently known” (Franco, 1969, p.64).

Albuquerque (1950, p.11-2) most certainly falls into an anachronism when she states that already in 1889 in Campinas, Lutz would have had a “clear intuition of the truth” on the correlation between mosquitoes and yellow fever. Lutz himself is to blame since, in an article published in 1903 (“Yellow fever and the mosquito,” in Port.), and later in his *Reminiscências* (1930), he alluded to observations made in 1889; these, however, had not been published, most probably because they amounted to little more than suspicions as yet irreconcilable with the etiological theories of the day.

“At that time, all the elements for explaining this mode of transmission were lacking,” reads an excerpt from the first article. Even so, when that coffee-growing center was devastated by yellow fever, Lutz had noted “an extraordinary abundance of the ‘striped mosquitoes’ that [he] knew so well from Rio de Janeiro, but that [he] had never observed in the interior during five years of practice in a region not too far from Campinas.” Lutz at that time correlated the “plague of the *Stegomyias* with tanks of still waters in the gardens of houses that had been abandoned by their owners. They were such a nuisance that he used a mosquito net every night and sometimes
even in the daytime “so that [he] could read in peace; naturally that did not stop [him] from being bitten on several occasions” (Lutz, 1930).

It is, however, true that his first speculations on the role bloodsuckers played in spreading disease predate this event. They are found in his “Studies on leprosy”, written in 1885-86 and published in Monatshefte für Praktische Dermatologie (1887). On describing primary lesions of the nervous form, which could act as “the entryway for infection,” he considered it “impressive that its first locus of manifestation ... is almost always in the parts of the body that are left uncovered and exposed to insect bites and other trauma” (1887, p.549). Lutz had already observed that each new case of infection required a pre-existing case within a defined area, but that the necessary conditions were of such complexity and peculiarity (much like yellow fever, we might add), that one could exclude direct person-to-person contagion. To explain transmission of Hansen’s disease, Lutz admitted, albeit hypothetically, that blood or excretions from the patient’s mucus that contained the infectious agent might require “maturation” in an external medium, or “direct inoculation that generates vulnerability (for example, through insect bites)."

This documented speculation leads us to give credit to the statement made in 1903 that the notion of mosquito transmission of yellow fever had been “always attractive” to Lutz after Campinas. “As a doctor and naturalist, I have always paid attention to these mosquitoes,” Lutz wrote, “and I knew that they were very common in Rio de Janeiro and in Santos, although much scarcer in rural São Paulo, where on rare occasions they did become a nuisance to me in the places I knew and lived in.”

In the 1903 article, he traced doubts about this hypothesis to the realm of entomology itself; his main objection concerned the epidemics of yellow fever along the Mediterranean and African coastlines, since it was believed that the Stegomyia fasciata existed only on the American continent. “It was with great surprise that, a few months later, I observed the same mosquito in the Sandwich Islands, and not only in the capital but even in far-removed plantations ... I then understood that [S. fasciata] was scattered about by seafaring vessels and that it must be found in other warm countries, although I was unaware of its presence in southern Europe. Theobald was to demonstrate this in the early 20th century.

As we have shown in the previous volume of the Collected Works of Adolpho Lutz, it was in Hawaii, where he lived from 1889 to mid-1892,
that Lutz became convinced that mosquitoes transmitted leprosy. According to Albuquerque (1950, p.13-4):

Although he had never avoided direct contact with lepers, he did not contract the disease ... and among the sick who entered the leper colony, some had never seen another leper. There had been a time, and not so long before, when neither leprosy nor mosquitoes existed in Hawaii. The native language did not have words for either leprosy or mosquitoes, and nicknamed leprosy ‘Chinese sickness’, since it had appeared only after the Chinese arrived and began raising rice. They cultivated rice, as is customary, in continuously irrigated ditches where mosquitoes, similarly of alien origin, found an excellent micro-habitat.

We thus find reasons to believe that the following statement from Lutz’s Reminiscências (1930) is legitimate:

Had I not made the previously mentioned observations on mosquitoes in Campinas, it would not have been possible for me – much later, and as soon as I received a letter with the first news of the demonstrative experiences carried out in Havana – to name the guilty mosquito among us without hesitation, a mosquito that did not exist in the city of São Paulo. It seemed probable to me that we were dealing with the same species, although I did not call it by the name the Americans used; only later was the species identified, having already been given a variety of names. I immediately informed the director of Sanitation Services, who accepted my guidance when I insisted on the importance of the verifications made in Cuba. After publishing the necessary instructions, we verified a coincidence of yellow fever epidemics with an abundance of Stegomyias in many places.

This discussion is related to the important role that Adolpho Lutz played at the turn of the 20th century in changes in how yellow fever was perceived and in the drastic change in direction taken by Brazilian public health. There is, however, another important issue underlying the text of Reminiscências, written in 1928-29. The paradigm that guided the campaigns of William Gorgas, Emílio Ribas, and Oswaldo Cruz began to fall apart at the seams, a paradigm that had led the Rockefeller Foundation to strive for the complete eradication of the disease by combating its transmitter in a few ‘key foci’ along the American and African coastlines following World War I.

For doctors at the turn of the century, yellow fever was an illness associated with ships, European immigrants, port cities, and the warm and humid coastal lowlands, which created the habitat for miasmas, then for fungi, algae, and the bacillus, and, lastly, for Stegomyia fasciata. The re-
Infestation of many coastal Brazilian cities in the 1920s prompted the recognition of anomalies in a paradigm that had been considered irreproachable. When yellow fever reappeared in Rio de Janeiro in 1928-29, it became clear that its ‘place’ had shifted to poor inland settlements, from which native migrants now brought the disease to the periphery of the great coastal cities.

The certainties that had been upheld inflexibly during Gorgas, Ribas, and Cruz’s times came crashing down in 1932-33 in the valley of Canaã, in rural Espírito Santo, when sanitarians from the Rockefeller Foundation, led by Fred Soper, confirmed the suspicion that yellow fever had one or more undetermined vectors, and that it was connected with the work of men who became infected while in the forests.

The epidemiological map that began to take shape between 1930 and 1937 ultimately changed the terms of the equation that had prevailed in the previous decades. Jungle yellow fever seemed to form not only great endemic stains that interlinked regions and nations but also waves that would from time to time, and by way of unknown mechanisms, sweep through vast regions, advancing dangerously from the wilds and forests to...
coastal cities. It seemed to be the common form of the disease, and the urban one, simply an abnormal manifestation that would tend to extinguish itself either when the mass of non-immune individuals had been consumed or when Aedes aegypti itself had been eradicated (Benchimol, 2001).

Löwy (Nov. 1998/Feb. 1999) rightly holds that the dogma of exclusive transmission via Aedis aegypti would never have been defeated had there not been tools that magnified the presence of the virus: histological analysis of the livers of the deceased and protection tests on blood drawn from the living. One must add that this change in point of view was also due to the growing skepticism of Brazilian doctors, who rebelled against the dogma maintained by the directors of the Rockefeller Foundation in the 1920s. The flames of escalating doubts were fanned when Adolpho Lutz published his Reminiscências.

As early as 1903, he admitted to the possibility that other mosquitoes were able to transmit yellow fever. “It is probable that this property belongs to the entire Stegomyia genus and not solely to one species. I know two more species that belong to our fauna that could probably be included in this genus, but fortunately they are wild species, are not abundant, and could only produce small disseminated foci.”

In his 1930 publication, Lutz brought attention to the importance of two foci he had observed at the end of the 19th century, foci that bore no relation to the railways or riverboats that carried Stegomyia fasciata and yellow fever inland. At these foci, disease transmission was attributed to probably ... woodland mosquitoes from the wilds, more or less related. One of the cases [I] only have information on involves a village of native Indians from Verde River. The other, which I investigated personally, was indeed yellow fever, which appeared in a few ranches built in the middle of a forest and inhabited by workers. They cut down the vegetation to prepare for building a railroad that was to link Funil to Campinas. I examined a number of the ranches from which cases of yellow fever had appeared, and found no trace of larvae or adult Stegomyia, there being no shortage, however, of forest mosquitoes. This fact proves even more interesting since transmission of the disease by mosquitoes other than our own domestic Stegomyia has recently been verified in Africa. Among us, this species will always play the most important role, while transmission by other species must be rare and the exception, but the problem of identifying other species able to transmit the virus is nevertheless interesting.

In the note published in 1901, Lutz was already certain that Culex taeniatus was found in Rio de Janeiro, Santos, Campinas, Casa Branca,
and Sorocaba. In the capital city of São Paulo, considered exempt from yellow fever, he had never spotted it in the houses where he had lived, but he had recently sighted it:

> even quite abundantly, in circumscribed points about town, which should explain certain outbreaks limited to a few streets. Although all those who lived [on these streets] would fall ill, there were no examples of contagion by the tramway personnel who continuously travel those streets. Simple pedestrians were not affected, but it was sometimes enough to spend but a few hours in the infected houses, be it by day or night, to contract the disease ... One should not, however, suppose that the incidence or scarcity of the species in a specific place should stand as a definitive and absolute cause. Quite the contrary, it may vary greatly, according to the season and the ease of procreation. [The species] can also be introduced into a place that was previously immune, as occurred in the Sandwich Islands on a recent, well-known date.

**Lutz’s and Ribas’ experiments in São Paulo**

Towards the end of that year, the President of the State, Francisco de Paula Rodrigues Alves, authorized the directors of the Bacteriological Institute and the Sanitation Service of São Paulo to reproduce locally the experiments carried out by North Americans in Cuba. The objective then was to neutralize reactions to the ‘Havana Theory’, reactions that were voiced most especially by doctors who were supporters of the Sanarelli bacillus and other microbes.
Before starting, Adolpho Lutz traveled to Rio de Janeiro more than once to obtain mosquitoes and have them bite people with mild cases of yellow fever. He would stay at 36 Mariz e Barros Street, at the school his sisters ran. In a letter to Emílio Ribas, dated 25 June 1902, he complained about the rain, the heat, and the time he lost traversing great distances by streetcar. This included the trip to São Sebastião Hospital, in the neighborhood of Cajú, where he kept track of three patients “from whom [he] obtained a few mosquitoes that had sucked. Two of them were doing regularly well and one was very badly off.” The epidemic that broke out in December 1901 continued its death toll, and “serious cases” kept on arriving. It had been a year of many mosquitoes in Rio de Janeiro but, Lutz wrote, “now they are rarer and the larvae develop slowly.” He might have been referring to a batch of mosquitoes being raised in a pavilion of Sebastião Hospital by the French medical mission (to which we will soon return), but it is not very likely, since he was not able to meet with one of its members, Paul-Louis Simond, “because of two holidays.” It is more likely that these mosquitoes were being raised by Lutz himself in the bacteriological laboratory at the General Directorship of Public Health, which operated at 56 Visconde do Rio Branco Street under the direction of Emílio Gomes. This was where Lutz’s assistant Carlos Meyer was supposed to send specimens of Stegomyia fasciata being bred at the Bacteriological Institute of São Paulo. “We are in sore need of more mosquitoes,” Lutz wrote. He was going to visit Manguinhos in the company of Oswaldo Cruz that same day, and was to stay at the federal capital “until Monday of the following week” – in other words, five more days, since the letter was written on a Wednesday.

In August 1902, Lutz returned to Rio de Janeiro and on the 30th of that month freely vented his frustration:

I no longer have any hope of finding a case in favorable conditions because there have been no more admissions in the last few days and there are only two cases in the infirmaries of cachexia following yellow fever. The case in which I applied the mosquitoes was not yellow fever, but influenza ... The weather has been consistently cool lately and mosquitoes are extremely rare; I have not been able to find a single Stegomyia ... There is nothing useful that can be done here, and it would be easier to return here when the first cases of the new season turn up, these being expected approximately a month from today. I was also unprepared for such a long absence.
In this letter, Lutz refers to the efforts he had been making to keep his specimens captured in Rio alive.

I still have quite a few, except that I was forced to feed them. This may result in a refusal to bite a case immediately when it is presented. I keep a few unfed but some of these always die. I apologize for not having been able to get anything. If you would like me to borrow mosquitoes from the Frenchmen, or to wait longer, I ask you to send me a telegram early Monday morning.

That same day, when his assistant Getulino was getting ready to return to São Paulo with some dispatches of Lutz’s, the latter received a message from Ribas referring to another question that had been asked about the French mission. Lutz reopened his own letter and added this postscript: “I have not yet spoken with Dr. Simond about the topic because I was awaiting your instructions; if no case turns up by Monday, I will make arrangements with him as mentioned, which probably will not present any problem. He has already offered me mosquitoes.”

There is a third letter from Lutz to Ribas at the Museu Nacional’s archive on Adolpho Lutz, typewritten in the 1950s at the time that Bertha Lutz organized her father’s files for the celebrations of the centennial of his birth.130 It is dated 28.II.1902, but there are clear indications that the typist mistakenly typed February when it ought to have been November. We are
led to this conclusion by one of Lutz’s comments: “The epidemic is declining but there are still cases.” In February 1902, it was reaching its apex, with a daily average of ten hospitalizations at São Sebastião Hospital.

On the other hand, if the letter was written on 28 November, it means that Lutz last stayed in Rio de Janeiro on the eve of beginning the experiments in São Paulo. It was a Friday, and he promised “to return on the Monday night train, arriving in Sào Paulo Tuesday morning.”

A disinfector with the Sanitation Service of Sào Paulo had just brought him mosquitoes that “had arrived alive but were mostly male, which do not produce any results.” Lutz hoped that Ivo Bandi would bring him more. The ones he had been able to infect were alive and well, and the bacteriologist from Sào Paulo referred to them in terms that show he expected to use them soon: “The four mosquitoes from the first case will be good in a few days; the patient has already been cured, having suffered an attack characteristic in all aspects, but not critical. We have some mosquitoes from two other cases, of which one mild, and the other fatal, and I have kept them separately.”

It was raining heavily, and the scientist once more complained that in Rio “one spends half the time traveling to and fro, or waiting for others.” During his previous visit he had tried to meet with Nuno de Andrade, the Director General of Public Health, and with the surgeon Pedro Affonso, Director of the Municipal Vaccinogen Institute and of the Manguinhos Serum Therapy Institute. He missed them again but was able to meet with Simond, from the French mission, “who seemed to me the most knowledgeable of the three” (the other two were Marchoux and Salimbeni). “They are very committed to the issue of the mosquitoes, which they award great importance.”

At the Bacteriological Institute, Lutz had begun controlled reproduction from the larvae stage of the *Stegomyia fasciata* captured in Rio de Janeiro, feeding them substances that were not blood, so as to exclude any other unwanted infections.

On 28 November, sample mosquitoes were sent to Meyer, who was at São Simão at the time, in the middle of a new outbreak of yellow fever. His mission there was to infect mosquitoes by having them draw blood from recent arrivals at the town’s isolation hospital, 730 kilometers away from the capital of Sào Paulo by rail. In a letter to Lutz dated 1 December, Meyer mentioned the clinical conditions of the patients submitted to the
procedure, the difficulties involved in having the mosquitoes bite them appropriately, and the problems in keeping the insects alive until the trip back to the capital. Received at the Bacteriological Institute on 2 December, they were kept on a diet of honey and dates for over twelve days, the minimum period of time considered necessary for making them agents of infection. Three more were added to compensate for the temperature, which was lower than that in Havana during Reed’s stay.

Experiments carried out in the Isolation Hospital in São Paulo employed mosquitoes infected on Alexandrina, Joaquim Farquinio, and Nicola Razzoti (the latter classified as a case of “long duration and serious in nature”); the records for each session always specify the day of the contamination procedure, the day the patients fell ill, and their clinical conditions expressed in two numbers: pulse and temperature. Details of these experiments were compiled in records signed by Ribas, Lutz, Carlos H. Meyer (an aid at the Bacteriological Institute), Candido Espinheira (director of the Isolation Hospital), Victor Godinho (a doctor at the same hospital), and a “commission of clinical doctors” made up of Antonio Gomes Silva Rodrigues, Adriano Julio de Barros, and Luiz Pereira Barreto.

A militant of the Republican Movement, Barreto had presided over the State Constitutional Assembly in 1891 and had been the first president of the Society of Medicine and Surgery of São Paulo. He had been a passionate supporter of the transmission of yellow fever by water. Telarolli Junior (1993, p.153-4) analyzed the heated controversy in the São Paulo newspapers in 1896, which largely mirrored the one that had taken place in Rio de Janeiro: “It was common at the time, for example, for well-to-do families traveling by rail to the interior of the state to take a stock of mineral water from Minas Gerais, in order to avoid catching yellow fever.”

Luiz Pereira Barreto was at that point the president of the State Senate, honorary professor at the Polytechnic School of São Paulo, and author of well-known articles on medicine, philosophy, politics, and religion. He exerted all the authority he had to legitimize the experiments on mosquito transmission.

The first series of experiments aimed at proving that Stegomyia were the agents of transmission of yellow fever; it involved five sessions between 15 December 1902 and 20 January 1903.

The preferences to use volunteers who had already been ‘acclimated’, to infect mosquitoes with mild cases of the disease, and to rely on an extended

maturation period for the germ within the mosquito’s organism were all aimed at producing low-risk infections in the human subjects. There was no treatment for yellow fever, and one death would spell disaster.

At eleven o’clock on the morning of 15 December in the presence of the medical commission mentioned above, statements were read and signed by Oscar Marques Moreira and Domingos Pereira Vaz (the latter a single 22-year-old from Paraná), according to which they submitted themselves to these experiments “of their own free will and solely their responsibility.” A document describing the conditions for carrying out the experiment was then read and signed by all. That done, they proceeded to have two mosquitoes bite Ribas’ arm and two others, Lutz’s arm, and only then did they perform this operation on Moreira and Vaz. “It was verified by all,” records of that session show, “that the mosquitoes bit well, given the amount of blood observed in their abdomens and the evident marks they left in the areas of bitten skin.”

The two volunteers remained under observation in the Isolation Hospital until the following session. Their case histories lead to believe that they were not immune to yellow fever, but Ribas and Lutz had probably already acquired immunity from fighting epidemics in the past.

The second session began three days later (18 Dec.), at practically the same time of day. The doctors present verified that none of the bitten subjects showed any changes in their states of health. Ribas, Lutz, and both volunteers submitted themselves to Stegomyia bites once more. The records of that session again include entries on the history of each mosquito being used and the state of health of the patients from which they had acquired their infection. During the third session, on 22 December, the human subjects once again showed themselves to be “enjoying the best of health.” Beginning at midday, the session lasted longer because the mosquitoes seemed less interested in biting. It was necessary to repeat the applications at five in the afternoon, at which time “they bit well, as was verified by all.” On 12 January 1903 (the fourth session), the human subjects continued “enjoying good health, the previous sessions therefore having had negative results as far as the Stegomyia applications of the three previous sessions.” It was 12:30 p.m. when four mosquitoes sucked Domingos Vaz’s blood. Other insects, of the same origin, refused to bite Oscar Moreira. A second fruitless attempt was made at 4:30 p.m. It was only at 7:00 the next morning that Meyer managed to get an infected mosquito to
draw blood from Moreira. Half an hour later, another bit him twice but did not draw blood. The operation was repeated at midday, unsuccessfully, even though the mosquito had not been fed during the previous fifty-three hours. The records were duly registered and certified by all present, who were surely exhausted. They decided to repeat the procedure with infected *Stegomyia* on the morning of 20 January 1903. This fifth session saw the inclusion of two new volunteers, Januário Fiori (Italian, unmarried, 23 years old, residing in Brazil for the previous eleven years) and André Ramos (“mulatto, Brazilian, 40 years old, married”). They were exposed to mosquitoes whose larvae had been brought to São Paulo from Itú, and the grown mosquitoes had then been infected in São Simão. All of them bit well, to judge from the signs left on arms and the blood contained in their abdomens.

According to Lemos (1954), three of the four volunteers contracted yellow fever, but there were no signs in the fourth one or in the two extra subjects, Lutz and Ribas.

Truth be told, the materials assembled in the current volume reveal a few discrepancies not entirely in keeping with such a ‘conclusive’ version of the facts. According to one of the manuscripts attached to the records of the experiments, concerning Domingos Pereira Vas, he was bitten on 12 January (fourth session) by four mosquitoes that had been infected by Benjamim Rosanini on 24 December. On 15 December (therefore between the fourth and fifth session), he was “bitten again by three of the same mosquitoes at 1:15 in the afternoon.” On 15 December, at 2:45 p.m., he began to feel ill, complaining of: “lack of appetite, headache, photophobia, especially in the frontal region, pain all over his body, and a strong backache. The symptoms increased progressively, matching his rise in temperature.”

The three members of the physicians commission presented this case in a report to Emilio Ribas, without, however, making any reference to the mosquito procedure of 15 December. It in fact reads that the “brave young man” had begun to feel “slightly unwell” the previous night, when he “threw up three times until 10 p.m.” Although they classified it as “a typically benign case of yellow fever,” they admitted it did leave room for doubt: “The absence of albumins in his urine could, to some more rigorous people, present a serious motive for doubting the precision of the diagnosis.”
Another equally questionable benign case was that of André Ramos. Bitten on the 20th, his legs began hurting four days later; he experienced stomach and back pains, burning eyes, and intense hyperemia in the conjunctiva and thorax. He was sick the night of the 25th, with strong headaches and stomach aches, and precordial anxiety. Despite these symptoms, urine tests “never showed the presence of albumins.”

If we read between the lines, the tone in which the members of the medical commission report on the third case reveals questionable aspects that they attempted to hide in the other two: Januário Fiori’s case “does not allow for hesitation in the diagnosis. Absolutely nothing was missing to complete the morbid syndrome of yellow fever.” Bitten the same day as Ramos, he began to feel sick on the 23rd. “He still had his 7 o’clock tea but he did not have an appetite. At 7:30, he felt a headache. He then complained of strong shivers, a headache above the eyes, and pain in his legs. At nine o’clock, hyperemia of the conjunctiva, face, and thorax was already quite visible.”

The irony of the story is that the most dramatic experimental evidence developed behind the backs of the medical commission. The reports on patients’ urine (like the one we see attached to the records, in this volume) were written by Bonilha de Toledo, who died 24 April 1903 of yellow fever, probably infected in his own laboratory (Lemos, p.74).

The second series of experiments – eleven sessions that extended from April to 10 May 1903 – intended to demonstrate “the contagion or non-contagion of yellow fever through clothing used by victims of the disease.” It involved three Italians who were kept secluded in Pavilion II of the São Paulo Isolation Hospital, in a room protected against mosquitoes with a metal screen and filled with clothes and objects soiled with the urine, vomit, and excrement of yellow fever patients. They submitted to this torment without showing any signs of the disease, which in this case was the desired outcome.

The first volunteer, Giuseppi Malagutti, son of Antonio Malagutti, was a native of the province of Emilia and a 31-year-old widowed mechanic. He had arrived in Brazil on 30 or 31 March 1902, and lived at 5 Américo Brasiliense Street. Angelo Paroletti came from Milan; son of João Paroletti, he was 43 years old, unmarried, and worked as a mason. He had been in Brazil since 15 or 20 June 1902, residing at 117 Libero Badaró Street. The third volunteer was Giovanni Siniscalchi, son of Fu Pascholi, born in
Lombardy. Married, 41 years old, he was a “technical teacher” and had been in Brazil longer: he had arrived on 30 August 1901. At the time of the experiments, he lived at 12 Conselheiro Crispiano Street. All three had embarked in Genoa as 3rd-class passengers destined for the port of Santos.

Only Malagutti turned up at the first session, on the night of 20 April 1903. Under the watchful eyes of the doctors of the Sanitation Service and the medical commission, he was taken to a room that had been purified with sulphur the day before to eliminate any potentially remaining mosquitoes. A gas heater kept room temperature above the outside environment, so that it served as a hot-house, favorable to the activity of microorganisms supposedly responsible for contagion of the disease. During this series of experiments, the temperature was monitored and recorded with the same care awarded to the conditions of infection of the mosquitoes in the previous series. Malagutti then removed from a box two bags of clothing that had been worn by recent victims of yellow fever. He next covered his bed with clothes stained with blood and vomit, and spread the rest about the floor. An employee spent the night observing him to make sure that he stayed in his polluted bed.

Giuseppe Malagutti and Angelo Paroletti took part in the second and third sessions (21 and 22 Apr.). The fourth, which occurred the next day, included the presence of Giovanni Siniscalchi. In addition to the clothes used the previous nights, they covered their pillows with cases stained with the vomit of sick people from Taubaté. Paroletti put on a contaminated jacket. In the fifth session, on 24 April, the three Italians repeated the previous nights’ ritual, with a new observer as a witness: Vital Brasil, director of the Butantan Serum Therapy Institute. The sanitary inspector Theodoro Bayma was among those present at the sixth session (25 Apr.), along with the doctors mentioned earlier. On the night of 26 April, the only variation was the requirement to wear new nightclothes that had been added to their supply of polluted clothes. During the eighth session (27 Apr.), they had to shake these vigorously before lying down. They
then opened three flasks, one containing urine from a man from Casa Branca, sick with yellow fever, and the other two, black vomit and bloody stools from Ribeirão Preto. These repugnant substances were poured over the clothing. The ninth session (28 Apr.) presented no novelties. During the tenth, observers of this series of experiments declared that the three Italians continued to be in perfectly good health and they were allowed to go, since it was considered that enough time had elapsed to exclude incubation and contagion by yellow fever fomites.

The doctors in charge of reporting on the results were categorical in their final report:

The experiments conducted by the North Americans in Havana and ours ... demonstrate that the yellow fever germ can only find the necessary conditions for its reproduction within the mosquito’s body.

It has been definitively proven and beyond any possibility of being contested that a mosquito – *Stegomyia fasciata* – can transport yellow fever across great distances and transmit it from a sick person to a healthy one. The experiment carried out here in the capital city of São Paulo forever dispels all objections. Here, we do not suffer the tumultuous aggression of the climatological and mesological factors that are found in many stricken places [and would] confound our conclusions. The admirable experiments by the North American doctors in Havana ... were not able to bring all controversies to an end, for the simple fact that that heavily populated city is a place where yellow fever has reigned endemically for over a century. Objections were raised because experimental cases observed there did not constitute absolute proof, since individuals could have contracted the infection by means other than mosquitoes. If this objection were raised in São Paulo, it would simply constitute scientific improbity. (cited in Lemos, 1954, p.73, 75-7)

Still *sub judice*, the conclusions reached by the Reed commission were checked by other commissions in places where yellow fever’s roots were as ancient as those in Cuba. The Public Health and Marine Hospital Service (created on 1 July 1902) sent to Vera Cruz, Mexico, Drs. Herman B. Parker from the service’s hygiene lab; George E. Beyer, professor of Hygiene from Louisiana’s Tulane University; and Oliver L. Pother, pathologist from the New Orleans Charity Hospital.133 We will comment later on the conclusions they reached. In the series of articles that he published in mid 1901 on the role of mosquitoes on propagating diseases (*Brazil-Medico*, Jun. 1901, p.208-10), Hilário de Gouveia referred to research carried out in the state of Pará by Liverpoolian doctors. Their initial conclusions clashed with Reed’s. According to an article published in *The Lancet* in early 1901, the
Englishmen had abandoned protozoa as agents of yellow fever, and only found bacilli in the organs of dead victims. Apart from not fitting in with bacterial diseases, transmission via the mosquito did not seem to fit certain “endemiological” characteristics observed in the state of Pará.

A German mission had also been to Brazil, organized by the Sailor’s Hospital (Seemannskrankenhauses) and the Institute for Tropical and Marine Diseases (Institutes für Schiffs-und Tropenkrakheiten), both from Hamburg. Businessmen from this German city had long maintained close ties with Brazil, and the mission was almost private in nature. For nearly five months (from 10 February to 4 July 1904), Drs. Hans Erich Moritz Otto and Rudolf Otto Neumann visited a few Brazilian cities, especially Rio de Janeiro.

Three researchers from the Pasteur Institute were already in Rio: Émile Marchoux, Paulo-Louis Simond, and A. Tourelli Salimbeni. Their trip had been sponsored by the French government, which was, like the German government, greatly interested in applying the new prophylactic strategies that would eliminate the damaging quarantines imposed on merchant ships in French colonies, especially Senegal. During their time in the Brazilian capital, the French and Germans could observe first hand the social and biological reality of the city that served as the first open-air laboratory for testing a campaign based on the Culex theory, under conditions that did not entail military occupation and without previous sanitation efforts that might cloud the results (in reality, the concomitant urban reform created problems for Oswaldo Cruz’s campaign).

The Pasteur Mission in Rio (1901-05)

After completing his medical studies in Paris in 1887, with a thesis on the typhoid fever epidemics experienced by the Navy contingent based in the East, Émile Marchoux (1862-1943) worked as a Navy doctor in the French colonies of Daomé and Indochina (1888-93); soon after, he directed the Saint-Louis laboratory in Senegal (1895-99).
Washington, D.C., June 7, 1903

Mr. Adolpho Lutz,
25 R. Rev., Santos,
S. Paulo, Brazil

Dear Sir:

Accept my thanks for your letter of April 25th for the reprints of your valuable papers which you kindly sent me. I am glad that you have been able to confirm the results obtained by our Commission in Cuba as to the transmission of yellow fever by inoculation. The experimental evidence is now so complete as satisfactory that we well-informed physicians can accept the fact. Indeed it is generally accepted in the United States and now forms the basis of our preventive measures, which, as you know, have been carried out in Havana with complete success.

With best wishes for your continued health and scientific usefulness I remain yours very truly,

[Signature]

Letter from General George M. Sternberg to Adolpho Lutz, dated 7 June 1903, expressing gratitude for offprints of articles that he had sent him, as well as praising Lutz’s experiments to confirm results obtained by the US Commission in Cuba (BR. MN. Fundo Adolpho Lutz, pasta 215, maço 1).
Paul Louis Simond (1858-1947) also joined the Navy, in 1882, after working as a natural history assistant at the Bordeaux School of Pharmacy and Medicine. He obtained a doctorate in medicine only in 1887, with a thesis on leprosy. His missions as a Navy doctor took him to Guiana, Indochina, and China, where in 1893 he had his first contact with the plague. On returning to Paris, he joined the Pasteur Institute. His first works in microbiology were on the haematozoons of paludism and on coccidia in general, and he was the first to prove the existence of a sexual cycle in these parasites. He was sent to India in 1897 to continue the anti-plague serum campaign that had been started the year before by Alexandre Yersin (1863-1943). He then made an important discovery: the role played by the rat flea in transmitting the disease. Before coming to Brazil, he was director of the Pasteur Institute of Saigon (1898-1900).

The third member of the French medical mission, Alexandre Salimbeni (1867-1942), was an Italian doctor who had graduated from the University of Siena and become a professor there, specialized in pathological anatomy. He joined the Pasteur Institute in November 1895, working initially at the laboratory of Elie Metchnikoff (1845-1916) and then as an assistant to Émile Roux (1853-1933). In 1898, together with them, he published his first paper on the cholera toxin and anti-toxin. During that same period he collaborated with Amédée Borrel (1867-1936) and Édouard Dujardin-Beaumetz (1868-1947) in a study on the peripneumonia microbe, published by Roux and Edmond Nocard (1850-1903). In 1899 he was sent to Portugal along with Alvert Calmette (1863-1933) to investigate the pandemic plague that had reached Porto and which would reach Santos that same year, brought by a ship transporting immigrants from Porto. Salimbeni helped to improve the anti-plague serum preparation technique, at the same time that the serum therapy institutes of Manguinhos and Butantan were being created in Brazil.135

Simond and Marchoux left Bordeaux on 4 October 1901, in the company of sergeant nurse Hébrard. Salimbeni left three weeks later. They stopped
briefly in Dakar, Senegal, where they gathered information about the yellow fever epidemic that was sweeping through that colony. On 3 November they arrived in the capital of Brazil. Nuno de Andrade, Director General of Public Health, and Carlos Seidl, Director of São Sebastião Hospital, put a hospital pavilion at their disposal so they could set up their laboratories. A small boat was also at their disposal to take them daily to Prainha (currently Praça Mauá), where they could catch the steamboat to the other side of the bay, where a train left for Petrópolis. Following the advice of Dr. Brissay, a doctor with the French delegation, Simond, Marchoux, and Salimeni took up residence in that city in the hills, where ever since the previous century, Brazil’s elites had taken refuge from yellow fever during the muggy season.

The date for commencement of the mission was established through a law promulgated on 7 December 1901, which did not define its duration. Pulmonary tuberculosis forced Salimeni to return to France on 7 March 1903. Concerns about the mission’s finances that year led Dr. Émile Roux, vice-director of the Pasteur Institute, to schedule his companions’ return
for July, but their resistance and the fact that the Brazilian sanitary authorities were interested in prolonging their stay led to a compromise: they would leave Brazil temporarily to return in December 1903. Simon would be away from April to December 1904 due to his wife’s state of health. Only Marchoux remained in Rio de Janeiro throughout the thirty-seven months of the mission, until its definitive departure on 3 May 1905.

In December 1901, a month after arrival of the French researchers, the first yellow fever patients admitted to São Sebastião Hospital began to be used to infect mosquitoes for inoculation experiments. During 1902, more than 800 people were hospitalized, most of them during the first six months. In February and March – the period when Adolpho Lutz made his first trip to Rio de Janeiro to capture and infect samples of *Stegomyia fasciata* – hospitalizations reached ten a day. In a letter to his friend Dr. Charrin (dated 9 Sep. 1902), Simond commented that until July the hospital had received on average over fifty yellow fever patients a week, the vast majority immigrants who had been in the city for less than a year.
It is this fluctuating population that, almost exclusively, keeps the epidemic going. One should not estimate it to exceed seven to eight thousand ... perhaps ten thousand at most. If one bears in mind that the number of hospitalized people does not represent more than one-third of the total number of sick people, one can begin to appreciate the epidemic’s intensity. At a conservative estimate, one hundred and fifty people stricken per week for six months ... This should be enough, do you not think, to discourage any foreigner from setting foot in Rio. (cited in Tran, 1998, p.58)
The research program they drew up was of broader scope than the one carried out in São Paulo. It went further than verifying the results of the Reed commission and, as we shall see, went well beyond the ethical boundaries that Ribas and Lutz had established in dealing with their volunteers. Its principal aims were the microscopic study of patients’ blood and the pathological anatomy and clinical study of yellow fever; the study of the biology and parasitology of *Stegomyia fasciata*, especially after it was infected with blood from patients; research on the behavior of this blood when injected into animals; and attempts to culture the yellow fever ‘virus’ *in vitro* and *in vivo*, including in humans, who would serve as subjects of experiments into transmission, serum therapy as a treatment, and prevention through a vaccine.

Marchoux, Salimbeni, and Simond published four reports in the *Annales de L’Institut Pasteur*, *O Brazil-Medico*, and *Revista Médica de São Paulo*. In Brazil, Marchoux and Salimbeni also published studies on “O garrotilho” (Equine adenitis, *Brazil-Medico*, 8 Oct. 1903) and “A espirilose das galinhas” (Spirillosis of chickens, *Brazil-Medico*, 15 Nov. 1903), while Marchoux published “Febre amarela e malária em Veracruz e no México” (Yellow fever and malaria in Veracruz and in Mexico, *Imprensa Médica*, 1906).
Otto and Neumann, from the German mission, visited Brazil when the French research program was about to end, at the height of the campaign against *Stegomyia fasciata* led by Oswaldo Cruz. This was also the time of other sanitary actions and the urban reform of Rio de Janeiro.

Hans Erich Moritz Otto (1869-1918) had been an assistant at the Eppendorf General Hospital in Hamburg from 1895 to 1900, subsequently joining the Hamburg’s Marine and Tropical Diseases Institute, inaugurated 1 October 1900. When he traveled to Brazil, he also introduced himself as a clinical assistant at the Sailor’s Hospital in Hamburg, which began its activities on 1 January 1901.

Rudof Otto Neumann, on the other hand, had graduated as a pharmacist in Erlangen in 1894. His talent for drawing and his perseverance impressed professor Karl B. Lehmann, who had hired him to work on the bacteriological atlas he was organizing: *Atlas und Grundriss der Bakteriologie und Lehrbuch der speciellen bakteriologischen Diagnostik*. This was a seminal work with seven printings and translations into four languages. As we saw in the previous volume of *The Complete Works of Adolpho Lutz*, in 1896 Lehmann and Neumann would include
the leprosy and tuberculosis bacilli in the genus *Mycobacterium*. Neumann had begun his medical studies late in life, graduating from Würzburg only in 1899. He had then obtained a doctorate in Kiel. His autobiography mentions various important institutes at which he worked: Heidelberg, Paris (Pasteur Institute), Liverpool, Cairo (Kasr El-Aini School of Medicine), and Hamburg (Hygiene Institute and Institute for Marine and Tropical Diseases). He investigated not only yellow fever but also schistosomiasis, ancylostomiasis, and malaria. When he arrived in Brazil, he was serving as a professor at Heidelberg University and an associate at the same hospital and institute where Otto worked.\(^{136}\)

German authorities were careful to inform Simond and Marchoux in advance of the arrival of the two German doctors, hoping to prevent a possible resurgence in that distant port city of the ceaseless strife that had engaged scientists of both nations since the Franco-Prussian war and the famous animosity between Louis Pasteur and Robert Koch. According to Tran (1998), the missions collaborated cordially, to the point that Otto and Neumann’s work was praised in a commentary published in the *Bulletin de L’Institut Pasteur*.

They first visited Pernambuco, where they did not find any yellow fever. After their stay in Rio de Janeiro (their foremost aim in the trip), they also visited the capital of São Paulo, as well as two other cities – Campinas and Santos – that they considered well known because of the epidemics of ‘American typhus’. Before returning to Germany, they also stopped in Bahia, a state that had remained free of yellow fever in recent years. They published preliminary versions of their results in a few German journals and, in 1906, a full report, over 150 pages long, including 55 maps, sketches, photographs, layouts, and seven tables printed in color.\(^{137}\)

In Rio de Janeiro, where they stayed almost three months (from 6 Mar. to end May), Otto and Neumann’s research was also conducted at São Sebastião Hospital, a good part devoted to verifying the results already obtained by the researchers from the Pasteur Institute. Both Germans were very fearful of contracting the disease since they were “highly susceptible” to yellow fever, and they even injured themselves during autopsies. They of course slept under mosquito nets every night.

The sick they examined were isolated in screen-lined compartments called “Marchoux Rooms,” which housed two beds each and were erected inside the oldest pavilions. The German researchers in fact referred to them as
“barracks” since the underlying concept used by the hospital to isolate diseases deemed contagious had been borrowed from military campaigns. They consisted of moveable units that could be easily discarded if they became contaminated. At one point, São Sebastião Hospital, inaugurated in 1889 on the eve of the fall of the monarchy, had paper machê pavilions imported from Germany.

All but three of the twenty-four sick patients examined by Otto and Neumann were past the third day of the illness, increasing the difficulty of investigating the pathogenic agent, which disappeared from human blood by the fourth day. But the high mortality rate (60%) favored autopsy studies.

They verified that yellow fever was an acute infectious disease displaying the characteristics of quickly evolving hemorrhagic septicemia. Anatomical-pathological examination did not reveal any specific characteristic, except for the absence of splenomegalia, which helped distinguish yellow fever from malaria. The prognosis was always serious, and could only be made with any degree of certainty in the second period. Diagnosis was not difficult in serious cases; differential diagnosis was important because of similarities to the hemoglobinuria form of malaria, to bilious typhoid fever, and phosphorous intoxication. The main yellow fever symptoms recorded by
Otto and Neumann were redness of the face; body odor (“similar to that of flesh from a recently slaughtered animal”); severe epigastric distress; and the appearance of albumin in the urine from the first days of the disease. In yellow fever (as with typhus), the pulse rate did not adjust to the temperature of the patient. Black vomit, nose bleeds, and bloody stools sometimes did not occur in cases that were less hemorrhagic or might retreat given signs of a kidney dysfunction. The patient’s clinical status would then become less precise, and an inexperienced doctor might take it for uremia when hemorrhaging was imperceptible and only the jaundice was intense.

*Stegomyia fasciata* was Otto and Neumann’s main object of study. They left Brazil convinced that it was the transmitter of yellow fever, but not that it was the only form of transmission possible. In Hamburg they would continue their experiments. “The initial material came from Santos, Rio, and Bahia, altogether from 30 to 40 mosquitoes, and a good quantity of larvae.” They were transported by ship in a heated compartment, and transferred to the “mosquito room” at the Institute for Tropical and Marine Diseases, a spacious room kept at a constant temperature of 27°C; it contained cages and a large aquarium with a mosquito-proof metal mesh. There they were able to follow twelve generations that were raised in the space of six months. “Regrettably, the *Culex fatigans* and *Anopheles argyrotarsus*, which had been brought together, did not reproduce.”

They had used “catching glasses” developed by Nocht to collect the mosquitoes in Rio de Janeiro, but on walls and harder-to-reach places they had used a device suggested by Adolpho Lutz. “It is a simple glass tube, somewhat curved, with a rubber tube and some cotton at one end. One places the tube easily over the mosquito and then draws it through the rubber tube until it reaches the cotton.”

A few problems had been encountered in growing *Stegomyia* in the Brazilian capital, especially the tiny ants that proved to be avid mosquito eaters.

The glasses where we raised our mosquitoes always had to be carefully placed in a dish of water. Even so, from time to time, the ants would somehow find their way into the glasses at night, passing through the thin piece of gauze and eating our *Stegomyia*. We finally greased the sides of the dishes with a sticky substance that trapped the ants.
To feed the insects in captivity they dipped wet cotton in sugar, honey, bananas, and water. Blood was needed to raise them. In the lab and during the trip from Brazil to Germany, they put canaries and white mice in the mosquito cages. “One hardly needs to mention that Stegomyia fasciata

Cages and apparatus to catch mosquitoes. Figure 5 shows an aspiration tube invented by Lutz (Otto & Neumann, 1906, p.28).
prefers human blood to any animal blood, and that it tries to obtain it every possible way.” Their experiments had provided “imperative” proof that only the females bit. Moreover, they observed that the mosquitoes seemed to prefer “white, delicate skin” to that of people of color.

In their first report, dated November 1903, Simond, Marchoux, and Salimbeni had presented a detailed analysis of *Stegomyia fasciata*. They cited Adolpho Lutz’s research and also Finlay’s and the Reed commission’s, but the mosquito’s habits and biological make-up were nevertheless still not fully understood. It was not even possible to state that this was the only species able to serve as an intermediary host to the still undiscovered yellow fever germ. It was essential to clear the issue up, for the *Stegomyia* had become the focus of a heated controversy and the cornerstone of the sanitary campaign already underway in Rio de Janeiro.

French doctors investigated all the mosquitoes they were able to capture there, among them *Anopheles albitarsis* and *Culex fatigans, singulatus*, and *confirmatus* (these no longer exist as *Culex*). They verified that *Stegomyia fasciata* was the only yellow fever vector.

The insect’s geographical distribution (in principle, all regions falling between 40 degrees latitude North and 40 degrees latitude South) and the observations made in Rio de Janeiro by Simond, Marchoux, and Salimbeni pointed up its sensitivity to temperature: between 27° and 30°C was ideal for their development; below 25°C, the species lost a good share of its capacities. This could explain why Petrópolis was immune to yellow fever even when epidemics were sweeping through areas not so far away (less than 100 km today), and despite the intense daily traffic between the city in the hills and the capital.

Many researchers thought that *Stegomyia* was an insect of daytime habits. This characteristic contradicted the existing idea that yellow fever was a disease that could only be contracted at night. There were no well-known cases of ‘contagion’ in the heart of the throng that descended the mountains every day, coming from Petrópolis to Rio at nine in the morning and going back up at four in the afternoon. On the other hand, there were frequent cases of people falling ill among those who chose to spend a night in Rio, even if it was for a simple *soirée*. Being in Rio after sunset in order to catch yellow fever “is a requirement that seems almost absolute,” wrote Simond in one of his notebooks (cited in Tran, 1998, p.65).
Simond in his laboratory in the Pasteur mission building. São Sebastião Hospital, 1901-1905 (Musée de l’Institut Pasteur, PLS33).

Building used for Pasteur’s mission in São Sebastião Hospital, with Paul–Louis Simond in the foreground, 1901-1905 (Musée de l’Institut Pasteur, D2808).
This common-sense notion was justified when the French verified that females indeed bit above all at night; young, recently impregnated insects might do so in the day but they were not yet able to transmit the disease since it took twelve days from the time they drew contaminated blood for them to become infectious.

Otto and Neumann’s report reveals the intensity of the controversies surrounding the question.

According to Dr. Lutz’s data, Stegomyia usually bite during the daytime. According to George Gray, most of the bites occur between one and three o’clock in the afternoon. In Durham’s report on yellow fever in the state of Pará, the lunch hour is also indicated as the prime time for bites. Lastly, Finlay observed that Stegomyia bite by day, and Bandi, as we did, had the opportunity in São Sebastião, in Rio de Janeiro, to capture during the day mosquitoes that had just bitten.

... according to the description above, one could be tempted to declare that Stegomyia is simply a daytime mosquito, although it can also bite in the evening and during the night. On many occasions we were able to capture female Stegomyia under mosquito nets at night after they had drawn blood. They likewise bit us in our chalet at Hotel Internacional when we were still seated at the table late at night, wearing only light clothing. Ribas also observes bites without distinguishing day and night. It is indeed strange that all those who stay in Rio during the day and travel back to Petrópolis only in the early evening should be spared from yellow fever.

Marchoux, Salimbeni, and Simond think ... that once Stegomyia has drawn blood, it will not bite again during the day, but will do so at night. Bandi disagrees with this notion, saying there is no motive to stop mosquitoes from biting right away once they have finished a previous bite. Thus, Marchoux, Salimbeni, and Simond’s opinion can not be cited as proof of the mosquito theory.

For Otto and Neumann, infections took place principally at night due to a notable characteristic of Stegomyia fasciata, both in its winged form and as larva and nymph: its aversion to light. These mosquitoes had a preference for dark corners or objects. In the laboratory they were seen or captured almost always on black trousers or on the fabric that covered the photographic equipment. “Their behavior is so marked that they will not bite an arm that is placed in the cage that is suddenly illuminated by a strong electric light, but will readily attack with the lights out.”
People who lived or worked in Rio de Janeiro had not been so exposed to mosquito attacks during the day because they usually found themselves out on the street or in well-lit rooms. Outside homes, *Stegomyia* liked to lay their eggs in open-air channels, tanks, barrels used to capture rainwater, puddles, bottles, boxes of preserved food, and broken glass. Inside homes they sought out dark corners and would lay their eggs in the outlets of washbasins and tanks, in toilet water, in plates beneath potted plants, and in spittoons and similar objects. They had a predilection for humid, muggy places, such as warehouses, bars, basements, and brothels. Because of their
attraction to sweet foods, they could be found in great concentrations in beer halls, bakeries, sugar refineries, and restaurants.

They did not like drafts but could rise to heights of hundreds of meters. They did not undertake major migrations but did travel great distances in trains, streetcars, automobiles, and ships, especially in cargoes of sugar, molasses, and fruit and in damp packages. Otto and Neumann put *Stegomyia* inside suitcases of dry and damp clothing, and saw that the sturdiest females would survive up to fifteen days.

According to Tran (1998), in a retrospective view, the French mission’s most original feat was to demonstrate that these mosquitoes could transmit the yellow fever ‘virus’ to descendents through heredity (vertical or transovarian). This line of investigation was prompted by the puzzling appearance of sporadic cases, sometimes spaced out, between epidemics. The theories held back then explained that the disease was ‘imported’ to stricken areas by travelers or objects that had been infected by microbes, and these sporadic resurgences were attributed to the transformation that the germ underwent in the ground, responding to climate-related or telluric factors. The ‘Havana theory’ could not explain this adequately, since the mosquito only had a limited lifetime.

Writing a paper for the general public (1903), Lutz stated that *Stegomyia fasciata* could live “for two or three summer months as long as they were fed.” He went on:

> it is well known that not only among us but even in colder nations, they can last through an entire winter. At lower temperatures, *Stegomyia* do not bite, which halts the appearance of new infections until the heat returns. If the temperature does rise significantly in the winter, they can leave their hiding places and bite again, explaining the cases we have observed in this season.

Both sporadic cases and outbreaks that occurred independently of any outside imports were given a new explanation in 1932 when Soper’s team discovered jungle yellow fever and then demonstrated the existence of a sylvatic cycle dependent on non-human receptacles of the yellow fever virus, more especially forest-dwelling primates. At the beginning of the 20th century, in the absence of any proven animal models, suspicion could not fall on any vertebrate hosts other than humans. Walter Reed’s team had tried to verify the hypothesis of an autonomous mosquito-based circuit of transmission, but their results had been negative.
French researchers in Rio de Janeiro conducted experiments in which eggs laid by females who had bitten sick patients were raised from the larvae stage to the adult stage, but to prove that they and their descendents were infectious meant resorting to human volunteers. Attempts in 1903 were unsuccessful, but in 1905 the researchers managed to reproduce symptoms of the disease in a volunteer (Tran, 1998, p.66). Simond and Marchoux (1905) hurried to publish these observations, even before the release of the third report the next year.

The newspaper article “Mystère de la Fièvre Jaune. Les découvertes des docteurs Marchoux et Simond expliquent l’épidemia inexplicable de Panama,” published in Le Matin (1 Aug. 1905, p.1-2), raised doubts about the anti-Culex measures put into practice in Cuba and Brazil. This was because in Panama these measures did not prevent the disease from proliferating at twice its previous intensity from May to June of 1905, despite the previous year’s mosquito-fighting measures. An outbreak of the epidemic in New Orleans also led the newspaper to ask Marchoux and Simond whether yellow fever might not have a way of propagating in those places which was unlike the means used in Havana and Rio de Janeiro.

Authorized by the Minister of the Colonies, the two French doctors went public with information that would be included in their reports. They vehemently rejected the suspicions that had been raised. Their studies had ratified Finlay’s theory, but in the Brazilian capital they verified that the incriminated mosquito had two new sources of infection. One consisted of the mild cases of yellow fever that would pass unnoticed by doctors yet would still provide mosquitoes with what they needed to keep the epidemic going. The main fuel, however, was provided by babies still being breast-fed: these recent arrivals to infected regions were as susceptible as any immigrant, but they were remarkably resistant to the yellow fever ‘poison’. The other source of invisible infection was the hereditary transmission of the yellow fever ‘virus’ to the descendents of the female Stegomyia fasciata that had drawn blood from sick patients. “This fact will certainly cause great sensation in the scientific field,” the paper noted. “Its verification was based on direct experience ... It was impossible for the French mission to find another volunteer in Rio to continue the experiment, but one hopes that it will be taken up again in Panama by the American doctors.”
A newspaper article published at about the same time in *Le Journal* (2 Jul. 1905) by Fernand Hauser, also drew attention to that “frightening [thing], the microbial infection of yellow fever, which extends to the eggs of the infected mosquito.”

**The etiological enigma**

In spite of the Reed commission’s efforts to demonstrate that the yellow fever agent was a “filterable virus,” recent papers on paludism had disseminated the supposition that it was perhaps a parasite whose characteristics were analogous to the Laveran plasmodium.

In an article published in the *Revista de Medicina Tropical* (Havana, Apr. 1903), Finlay himself defended the hypothesis: this parasite would be an ultra-microscopic protozoan whose stages of development were more or less similar to those of the malaria parasite. He supposed that its sexual reproduction took place inside humans (its definitive host), while schizogony occurred in the mosquito. This inference was based on the observation that the infection of *Stegomyia* lasted indefinitely, while in humans it was short. On the other hand, in the case of malaria, infection in man was of indefinite duration while in *Anopheles* it was apparently limited.

It was during this period that the first report by Parker, Beyer, and Pothier was released (1903; also Parker, 1903). The doctors from the U.S. Navy commission did not find any specific organism in the blood of yellow fever patients or in their tissues after death. Observation of *Stegomyia fasciata*, however, proved promising. After feeding on a patient, the insect’s ovaries at first underwent hypertrophy, as would normally occur after a feeding of normal blood; they would then undergo complete atrophy. The salivary glands also displayed excessive enlargement. Three or four days after the absorption of contaminated blood a “small fusiform protozoan” (whose development was analogous to that of coccidia) would be found in the salivary glands and stomach. In future experiments, the authors expected to better determine the life cycle and etiological role of the organism they named *Myxococcidium stegomyiae*.

Parker, Beyer, and Pothier considered it a definitive scientific fact that the disease’s medium of transference was *Stegomyia fasciata*. But they could not yet tell if it was “its sole medium of transference.” They believed that “to prove this negative affirmation, appropriate conditions must be
afforded for producing the disease at will, or under constant conditions” – an indirect allusion to experimentation on human subjects.\textsuperscript{140}

Influenced by those ideas and by work he had already done on coccidia, (1897), Simond, in Rio de Janeiro, turned enthusiastically to a microsporidium of the genus \textit{Nosema}, which was similar to \textit{Myxococcidium stegomyiae} and which he had found in the \textit{Stegomyia} digestive tract in March 1902. The discovery generated great expectations in the French legation, which confidentially informed Théophile Delcassé, France’s Minister of Foreign Affairs:

Drs. Marchoux, Simond, and Salimbeni ... confided in me the fortunate result of their initial research: they would like us to maintain utmost confidentiality with respect to their experiments until the director of the Pasteur Institute has made a statement. (cited in Tran, 1998, p.56)

The director, however, dampened that enthusiasm in a letter dated 28 March 1902: The mosquito that had bitten a victim of the disease did indeed have a coccidian, but the preparations he had been sent were not convincing; Roux had not seen anything, either in the blood or in the liver, suggestive of the etiological agent of yellow fever.\textsuperscript{141}

The three French researchers would eventually recognize that the parasite was frequently found in \textit{Stegomyia}, even in those that had not bitten yellow fever sufferers.

Émile Roux, who one year later would publish “Sur les microbes dits invisibles”, thought the cause of the disease might be a microorganism of this still enigmatic group, but he considered the hypothesis of bacterial etiology more likely. He wrote to Simond:

One thing makes a great impression on me ... The Americans showed that the blood is virulent, and microscopic examination does not reveal anything that one could relate to a parasitical form. Under these circumstances, it would be necessary to shift investigations to invisible viruses. The experiment to be carried out would be to filter serum or blood diluted in water and inoculate the filtrate in a man. I know it is not possible to perform such an experiment in Rio, and I feel quite uncomfortable in suggesting an experiment that targets an invisible virus ... In brief, it is necessary to try inoculating monkeys ... It is necessary to try all the different inoculation methods: intra-cerebral, intravenous, etc. (ibid., p.57)
As Tran shows (1998), in May 1902, Simond, Marchoux, and Salimbeni began a series of experiments, at first with rabbits and later with five species of monkeys. They injected not only suspect parasites but also the bodies and salivary glands of infected *Stegomyia*; they then made these mosquitoes bite the animals. None of the experiments brought them any results, thus reinforcing the supposition that yellow fever was a disease that, apart from humans, did not have any other vertebrate host.

Otto and Neumann reiterated this suspicion and yet another of the Frenchmen’s observations: its germ could pass on to the offspring of the mosquito who had bitten a sick person, but the insect that had been contaminated by heredity could only “expel” the germ after twenty-two days (instead of the twelve days it took the mosquito that had been infected directly).

In spite of their time spent at the microscope, the two German doctors failed to determine the etiological agent. In their report (1906), they mentioned all the microbes that had been found in the blood and tissues of the sick: the Richardson, Gibier, Havelburg, and Sanarelli bacilli; the Freire, Finlay, and Delgado micrococci; the Carmona y Valle and Lacerda fungi. Although they were convinced of these microorganisms’ irrelevance (already invalidated by Sternberg in 1890), they checked for the microorganism’s appearance in material collected from the ill and from autopsies – especially the Sanarelli bacillus, which was vigorously defended by doctors in Rio de Janeiro and São Paulo. They also looked for an organism similar to the influenza organism that had been described in 1902 by Durham and Myers, and which Laveran proposed to the Biology Society of Paris that same year. The results of Otto and Neumann’s research were negative for all these microbes.

It was, however, indisputable that there was a very small organism in the blood of the sick, since it was possible to transmit yellow fever to healthy people by inoculating them with that organic liquid, even if partially defibrinated, or with serum diluted in sterilized water, after passing through a Berkefeld filter, capable of retaining the smallest bacteria. It had already been determined by the U.S. and French commissions that this effect failed to occur if the blood sample were heated to 55°C for five minutes, or if it were used after the third day of onset of illness. Simond, Marchoux, and Salimbeni had also verified that the serum would lose its virulence if exposed to air for over forty-eight hours, even if not heated. Defibrinated blood
kept from contact with the air could still produce yellow fever after five days, but after eight days it could be injected harmlessly.

Much like the French, Otto and Neumann did not obtain any results from microscopic examination of fresh and stained preparations of infected *Stegomyia*.

They then targeted a hypothesis raised by Fritz Richard Schaudinn, a protozoologist at Hamburg’s Marine and Tropical Diseases Institute who was one step away from discovering the cause of syphilis: *Treponema pallidum*. The agent of yellow fever could also be a spirochaeta. During a stage of this protozoan’s development, structures would form that were so minuscule that they might easily pass through a bacterial filter. Yet none of this barred the possibility that at a later stage they could take on visible forms not yet recognized as specific to yellow fever.

“Our main lines of research were subsequently defined according to this premise.” To conduct their investigations, Otto and Neumann had brought with them a technological novelty that piqued great curiosity among both French and Brazilian doctors: an “ultra-microscope,” one of the first to be manufactured by Zeiss. The lab received frequent visits of doctors and lay people interested in seeing the instrument; physicians from “São Sebastião Hospital, as well as Dr Marchoux, did not miss the chance to take a look at ... our results.”

Otto and Neumann had to learn to use this apparatus that had not yet been tested in German laboratories. They had great difficulty at first. Developed by the physicists Siedentopf and Zsigmondy (1908) to make ultra-microscopic particles visible, the instrument could only be used in sunlight; the manufacturers had presumed sunlight would be abundant in the tropics, and believed they would not find good enough energy sources there. For that reason, research had to come to a halt every time a cloud blocked the sun. They also had to stop during overcast days and when trees threw their shadows across the windows in the setting sun. Working at night was out of the question.
Despite these inconveniences and the instrument’s intrinsic difficulties, the German researchers were able to spot in the blood serum of sick patients a tiny corpuscle, sometimes round in shape and sometimes oval, and a hundred times smaller than a red blood cell. At times the bodies would appear in their field of vision in relatively small numbers; other times they would fill it completely. The researchers attributed this to the corpuscles’ weight, which caused them to sink to the bottom of the liquid, much like bacteria, and thus disappear from their field of vision. Only sometimes did they seem to move of their own accord.

Not convinced of the specificity of these small corpuscular elements, they examined the serum of healthy people and of patients suffering from other diseases. They found completely different corpuscles.

The unsatisfactory results of our research led us to consider looking for a fluid inside the body where we might suppose to find the pathological agent, but that, on the other hand, would be more appropriate for research than blood serum ... The only fluid that met these conditions, since it is free of salts and albumin, was cerebral-spinal fluid, which, as far as we know, was studied by us for the first time with respect to yellow fever.

This choice was certainly related to Schaudinn’s research on syphilis, a disease with well-known neurological manifestations.

They extracted lumbar fluid and repeated their ultra-microscopic examinations. In the non-centrifuged fluid, for the first time they saw tiny, very delicate bodies; unlike the forms found in blood serum, they moved about in a very lively manner, like butterflies flitting about in irregular paths, zigzagging.

They then filtered the cerebral-spinal fluid through one of the narrowest filters (Chamberland F), which Dr. Marchoux graciously ceded us for our work ... The microscope allowed us to recognize, in this case too, those minuscule elements cited. On reexamining the non-filtered and filtered blood serum taken from this patient, we were able to find them there as well. Perhaps they had escaped us previously because of their small number.

Encouraged, they proceeded to the experimentum crucis: they drew spinal fluid from a patient with smallpox and “a black person who happened to be in the hospital.” To their surprise and disappointment, the fluids of both control subjects displayed corpuscles very similar to those they had seen.

In the report published in 1906, they left the pathological meaning of their findings undefined.
At this point, it is not clear what they might mean ... Who could affirm that the particles seen in the yellow fever patients are no different from those of the patients with smallpox or from healthy people? The ultramicroscope, regrettably, is not decisive ... Any other corpuscular elements capable of provoking a certain suspicion ... could not be confirmed by it. Spirilla, trypanosomes, and similar forms could not have escaped us, because the minute spirilla of the chicken epidemic, described by Marchoux (1903) and used for comparison, were impressive for their extraordinary size.

The future will tell whether the ultra-microscope can solve the question in other diseases whose agents are still unknown or when research on yellow fever is taken up once again.

It would only be three decades later that the equipment that was to make the etiological agent of yellow fever visible would appear. By then, the agent had been unequivocally linked to a virus since 1927, when three researchers from the Rockefeller Foundation – Adrian Stockes, Johannes A. Bauer, and N. Paul Hudson – managed to infect rhesus monkeys of the genus *Macaca* in French West Africa. A rapid succession of papers by virologists from that institute would culminate in the production of an efficient vaccine in the late 1930s, and once more Brazil was to play a key role in the process (Benchimol, 2001).

Before that (1918-26), however, another germ enjoyed as much fame as Sanarelli’s: *Leptospira icteroides*, a spirochaeta incriminated by Hideyo Noguchi (1876-1928), a bacteriologist from the Rockefeller Foundation that had become famous for successfully culturing the syphilis agent *in vitro* and also author of a seminal paper on the serodiagnosis of this disease.

Let us return to Simond, Marchoux, and Salimbeni. The results of their first few months in Rio did not meet their expectations, an impression heightened by the publicity given to the results of other teams formed after Reed and his collaborators had released their papers. “All our attempts at infecting a great variety of animals in the laboratory ... were unsuccessful,” they wrote. “We had no alternative but to employ the method so brilliantly inaugurated by the Americans and continued with no less success in Brazil, that is, experimentation on humans” (cited in Tran, 1998, p.59).
The first report they published states that the volunteers were warned of the risks they ran, in the presence of witnesses, and all of them agreed to submit to the experiments. Tran (1998) questions this assertion. He found no trace of “informed consent” in the documentation collected at the Pasteur Institute. It is his opinion that these immigrants, who had recently arrived from the old continent and were for the most part extremely poor, had no sense of the dangers to which they were exposed by letting themselves be bitten by infected mosquitoes, and even if they were conscious of it, they accepted it for financial reasons. Reed paid his experimental subjects generously (up to one hundred gold dollars each). Marchoux and Simond purportedly did the same. One of Simond’s lab notebooks mentions “people to pay” on a page with a list of twenty-four names.144

It was Roux who in 1902 suggested that they perform serum therapy trials using the serum of patients recovering from the disease.

You should also make experiments on patients with blood from those who are recovering from serious bouts of yellow fever. If the serum proves active, do not hesitate to inject great quantities of virulent blood in a horse to then test its serum.

If you are at all successful in this direction, it would support the notion of a bacterial agent. In my opinion, and I repeat it yet again, the solid immunity created by a bout of the disease seems, in my eyes, to favor the notion of a bacteria ... It is a pity that you cannot carry out experiences on m. [men] of good will, as in Cuba. (cited in Tran, 1998, p.58)

Experiments with men began around March 1903. Ribas and Lutz’s experiments in São Paulo were still underway. Simond and Marchoux’s were conducted with the approval of Oswaldo Cruz and Carlos Seidl in Petrópolis, a region similar to the capital of São Paulo in that it was exempt from yellow fever; this meant that the human subjects would have no accidental contact that risked invalidating the results.

The first three cases only served to confirm conclusions reached by the U.S. commission, as well as the conclusions Lutz had communicated to Simond:

“The São Paulo experiments ... prove what you have probably observed for yourselves: that the infection is readily obtained if the necessary conditions are observed. I would be grateful if you would explain this to Messrs Metchnikoff and Roux,” the director of the Bacteriological Institute had written in January 1903.145
Simond and Marchoux soon turned to experiments concerning infection and immunization (Salimbeni had just returned to France). On 23 March 1903, Daniel Silva received an injection of contaminated defibrinated blood that had been collected fourteen days earlier; five days later he was injected with a new dose, followed by an injection of virulent serum that brought the onset of the disease. Both doctors concluded that the inoculation of old blood did not confer immunity (Tran, 1998, p.61). On 29 March, J. Ralf and M. Hofer each received three injections of serum from sick patients, heated to 55°C, and days later the virulent serum. Ralf developed benign yellow fever and Hofer remained unaffected. This experiment did not enable the researchers to establish whether the heated virulent serum had preventive properties, or if Hofer had developed immunity previously.

In April, May, and June, Marchoux and Simond tested different preparations on around twenty-five volunteers whose ages ranged from 16 to 38. They were mostly German, Portuguese, and Italian immigrants who had arrived in Brazil within the previous six months, making it highly probable that they were not immune to the disease. In addition to heated or aged virulent serum and to virulent serum filtered through various apparati, they also experimented with the serum of convalescing patients. When applied to patients in the first days of the disease, this serum seemed to improve their state of health. Simond tallied seven successes and four failures (ibid., p.61-2).

The results of these attempts at preventive vaccination were, however, more contradictory in Tran’s view (1998, p.62). At times the patients remained healthy, at times they developed yellow fever. Most of the
experiments produced simple bouts of fever that were difficult to diagnose unequivocally. If the symptoms were tenuous, should the researchers conclude that the vaccination had failed or that there was relative immunity? Interpretation of the results was made even more uncertain by the absence of control groups. Simond and Marchoux alleged that they could not allow themselves this kind of luxury with human subjects.

On the morning of 18 June 1903, Raymondo Geronimo, a 38-year-old Italian, was making his way down from Petrópolis to Rio de Janeiro on the 7:30 a.m. train. He felt very ill, and upon arriving at the foot of the mountains, he left the train with his suitcase in hand and began to walk the line in the opposite direction, as if he intended to return to Petrópolis. He walked for a few hundred meters, stopped, and passed out. Railroad employees carried him to the station, where he passed away after a few hours (ibid., p.63).

Geronimo was one of Simond and Marchoux’s ‘human guinea pigs’. Six days earlier, he had been given an injection of virulent serum heated for 15 minutes to 48°C. On the 18th, at six in the morning, S. Bordach also died; he was a 23-year-old man from Hamburg who had arrived in Brazil a month earlier. On 10 June, he had been bitten by two infected Stegomyia and had taken seriously ill four days later. The third fatal victim was Heinrich Falk, a young Bavarian, also 23 years old; he had arrived in Rio on 1 June. On 10 June, in Petrópolis, he received one cc of virulent serum that had been passed through a Chamberland filter without being diluted. He fell ill six days later and died on 20 or 21 June. There seems to have been a fourth victim, on the same date: Lippe, a 34-year-old man from Westphalia; his name appears next to that of the other three in one of Simond’s notebooks (ibid., p.62).

Neither Simond nor Marchoux made any mention of these accidents in the mission’s official reports published in the Annales de l’Institut Pasteur. The experiments were not completely discontinued because in 1905, as we saw, they still used a human volunteer to prove the hereditary transmission of the yellow fever virus by mosquitoes.

The complicity of the Brazilian sanitary authorities is proven beyond any doubt by a letter that Oswaldo Cruz sent to Emilio Ribas on 20 July 1903. Amid assessments of the French results, he commented:
Regarding human experimentation, they verified that:

*Stegomyia* transmitted the typical form of the disease. Unfortunately, there were three deaths caused by classic experimental yellow fever, with anuria, black vomit, jaundice, albuminuria, etc.; the cases were verified by autopsy, and all the characteristic lesions were found ... One of the sick men victimized by yellow fever had been bitten by only two mosquitoes, one of which practically did not draw blood. I ask of you complete confidentiality regarding these cases that ended in death and which were carried out under my sole responsibility.

You will understand, my dear friend, how our adversarial press would exploit this fact if they were informed of it. These experiments were categorical, just as the others in which the accompanying symptoms were as categorical as they could possibly be.147

In the rough seas of sanitary practices

Nuno de Andrade, Oswaldo Cruz’s predecessor at the General Directorship of Public Health, had voiced many doubts on this topic in the *Jornal do Commercio* (24 Aug. 1902) and in *Revista Médica de São Paulo* (1902). The discovery made by the North Americans added a new element to yellow fever prophylaxis, but it had failed to gather enough convincing elements to oust environmental disinfection and sanitation practices. The Havana doctrine was based on the doctrine of paludism. Neither of these accepted a microbe free in the external environment, and both restricted all the threads of the problem to humans and the mosquito. “I admit that the hypothesis that the yellow fever germ does not exist in the external environment bothers me seriously,” stated Andrade, “because scientific documents and our own observations have accumulated a world of facts that would be completely inexplicable if the deductions of American prophylaxis were accepted without restrictions.”

Andrade pointed to decisive experiments that had not been conducted and which would exclude alternative pathways of the germ. The North Americans had demonstrated that contaminated objects did not transmit the disease directly, but they had not proven that these objects did not carry the germ. No experiment had cancelled out the possibility that healthy mosquitoes could infect themselves on soiled objects and then inoculate the disease in humans. No one had investigated *Stegomyia*’s excretions for possible infective properties. In the absence of such proof, the prevention formula continued to employ the term “contaminating object” as part of the equation.
The fact that the microbe remained undetermined also left the theory of its transmission exposed to other disturbing questions. “The most impressive thing about the shadowy etiology of yellow fever is that the blood injected by the syringe transmits the disease immediately ... but if drawn by the mosquito, it only becomes infectious after about 12 days!” The North Americans provided an explanation that worked by analogy and did not rest on experimental proof. They supposed that the unknown microbe underwent transformations in the body of *Stegomyia* similar to those undergone by the malaria plasmodium inside *Anopheles*. The parallelism seemed arbitrary. The circumstantial fact that the mosquitoes were hosts of both germs did not imply identical life cycles. The microbe’s invisibility, allied to a lack of knowledge over what went on during the interval between the mosquito’s contamination and the appearance of its capacity for infection, tainted the Havana doctrine with fuzzy areas. These areas did not concern *Stegomyia* as an agent of transmission – Nuno de Andrade considered this a proven fact – but rather what preventive deductions could be derived from this fact.

Nor did he take the war on the mosquito in Cuba as evidence of an exclusive form of propagation, as postulated by the Americans. He considered this battle a kind of “touch-up work” that supplemented the material improvements made by the military authorities earlier. With discipline, severity, and implacable rigor against those who hid the sick, they had rectified the Havana coastline, dried up marshlands, built sewage systems, distributed great quantities of drinking water, installed electric lighting, cleaned prisons, built hospitals, intervened in residences, cleaned up the markets, and paved all of the city streets. It was not out of the question to suppose that those interventions had contributed in no small amount to the success of an initiative that was usually ascribed only to the war on mosquitoes.

The Havana doctrine, therefore, insofar as it excludes the yellow fever germ from the environment, is a postulate or a question mark; it does not authorize the exclusive prophylaxis that the Americans recommend, nor does it at this point have the power to impose disregard for the current processes of defensive hygiene ... The prophylactic formula must be a complex one, that is, it must embrace all the processes of prophylaxis currently in use, in addition to all those derived from the transmissibility of yellow fever by *Stegomyia*. Addition and not subtraction.
In São Paulo, too, this theory found passionate opponents. Arthur Vieira de Mendonça continued to defend the icteroid bacillus and his arguments had a marked impact on public opinion since they were expressed precisely by a bacteriologist who had, until recently, been close to Adolfo Lutz. He had worked at the Bacteriological Institute since its foundation, and had served as under-director after Lutz became head of the Institute upon Le Dantec’s departure. Moreover, Mendonça was one of the founders of the Revista Medica de São Paulo, along with Victor Godinho, and had presided the Society of Medicine and Surgery of São Paulo during the most critical period of the controversy (1903-04). Another of Lutz’s assistants, Ivo Bandi, would join the fray in favor of the Sanarelli bacillus. In 1903, when the results of the experiments conducted at the Isolation Hospital were released, Mendonça published a book (1903) containing the letter, articles, and reports written by him and by other opponents of the theory of transmission of yellow fever by mosquitoes.

If we add Finlay’s cases to those of the U.S. commission and of São Paulo, we have a great number of sick people in which the disease is not clearly defined and the symptoms are always hazy and inexpressive, and yet they want medical doctors as a class to accept the diagnosis of yellow fever ... Never has Rio de Janeiro seen such favorable conditions as now to rid itself of yellow fever. Having the engineer Passos as Mayor provides us with a sure guarantee that residences can be improved and general advances made in the city, and if this were accompanied by systematic hospital isolation and disinfections, in very little time yellow fever would be eradicated, as it was in Santos and Campinas.

Preference, however, has been given to a war on the mosquito, a war that was not efficacious in a small town like São Simão; what can one expect of it in a great city like Rio de Janeiro! (Mendonça, 1903, p.136, cited in Almeida, 2003, p.252-3)

Controversies on the Havana theory found a concentrated forum at the 5th Brazilian Congress on Medicine and Surgery, which took place in Rio de Janeiro from 16 June to 2 July 1903, right when Oswaldo Cruz was silencing facts on the deaths resulting from the French mission’s experiments. Partisans of Finlay’s theory did as much as they could to make the Congress into a tribunal that would sanction it.

The reality was, however, that no one took the stand to claim that the mosquito did not transmit yellow fever. Adversaries defined themselves as “not convinced” or as “non-exclusivists.” They were intransigent in the defense of disinfection and ground sanitation, which the exclusivists wanted
to abolish. Prominent in the first group, besides Ivo Bandi, were Jorge
Pinto, head of the Sanitation Service for the State of Rio de Janeiro, and
Pacífico Pereira, a leading figure in Bahia’s Tropicalist School, who was
absent but whose paper was read during the Meeting. The most
noteworthy “orthodox exclusivists,” or unitaristas (defenders of one sole
explanation), were Felício dos Santos, an “experienced old” general
practitioner from Bahia who owned the Casa de Saúde São Sebastião hos-
pital in Rio de Janeiro, and Drs. Plácido Barbosa and Carneiro de Men-
donça, from Rio de Janeiro’s Public Health.

Doctors from São Paulo had decisive participations. Carlos Meyer and
Arthur Palmeira Ripper read reports on the experiments conducted at that
state’s Isolation Hospital, along with a communication from director of the
state’s Sanitation Service; its conclusions guided the final vote on
deliberations. At the closing session, it was decided to schedule the 6th
meeting in São Paulo, chaired by Emílio Ribas. A delegation from the 5th
congress accompanied the São Paulo contingent to the train station, where
they were acclaimed during their departure.

The result of these negotiations of a political nature concerning an issue of
a scientific nature – the truth or error of the transmission and prevention
of yellow fever – turned Oswaldo Cruz’s strategy into the officially
sanctioned guidelines of the medical corporation. It is clear, however, that
the verdict fell short of what the commanders of the anti- Culex campaign
had hoped for, and the campaign had to be negotiated in many other forums,
inside and outside the nation: before the Congress, the press, professional
associations, international scientific institutions, various (and clashing)
levels and bodies within the government administration, and the very people
of the city who were targeted with successive pieces of “advice” drawn up
with the intent of disseminating the new beliefs and producing a new
conventional wisdom regarding the capital issues of public and individual
health. The Congress of Medicine and Surgery was therefore only a
cogwheel in the political machinery underlying the campaign against yellow
fever, which was already in the streets. Measures derived from the Havana
theory were put into practice at the same time as negotiations were
going on over legal instruments, institutional arrangements, funding,
consents, and symbolic endorsements. These negotiations took up the entire
year of 1903 and part of 1904 and were made in the wake of events of
much greater consequence than the issue of yellow fever, even though the
latter had become *sine qua non* with the sanitation of the capital of the Republic. One can say that all regions and strata, all beings, all moveable and immoveable, recent or secular elements of the city were trampled on by the initiatives of the Director General of Public Health, Oswaldo Cruz; of Mayor Francisco Pereira Passos; and of other agents of the sanitary measures and urban remodeling of the Brazilian capital, under the administration of Brazilian President Francisco de Paula Rodrigues Alves (1902-06). The clashes, injustices, demolitions, and overbearing intrusion of the public powers in the private lives of Rio’s inhabitants raised the social temperature to unprecedented heights, until the Vaccine Revolt finally erupted in November 1904.

It is not our aim here to narrate the conflicts that marked the beginning of the new century (for more on this, see Benchimol 2003, 1992; Chalhoub, 1996; Carvalho, 1987). We will content ourselves with highlighting a few changes in the repetitive pattern of events.

Defending the inclusion of mosquito-fighting measures as part of the prophylaxis formula already applied to yellow fever, in 1902, Nuno de
Andrade defined their enforcement as a “world of work and a thousand worlds of struggles!” (*Revista Médica de São Paulo*, p.325). Reducing the formula to those equations essential to breaking just some of the links in the chain of infection presupposed a different way of thinking. The word ‘vector’ was very much in vogue within Oswaldo Cruz’s group; it contained and conveyed a notion of geometry that quickly brings to mind a straight line aimed at very precise targets.

In an insightful analysis of the Pasteurian revolution, Bruno Latour (1984, 1987) replaced the supposed antagonism between the old hygiene of the miasmas and the new science of microbes with an image that entailed correcting the old actors’ course of action in the light of the strategy proposed by the new actors. This implied a reciprocal “translation” of interests that would be advantageous to both sets of actors. The old hygiene was
characterized by unlimited ambition. For every targeted disease, battles had to be waged on an extremely wide array of fronts: against the forces of nature, against the topography of cities, against the most varied components of urban life. With the microbes specific to each disease in hand, the Pasteurians pinpointed their key battles – “the mandatory points of passage” – which would carry the hygienist contingents to the victories they so ardently desired. Although this seductive image can help us to think through the issues, it is the product of a distillation of accidents, mistakes, and contradictions that make the actual battles much more confusing and unruly – battles whose outcomes were not always favorable.

The efforts by Pasteur's followers in Brazil to address yellow fever from the perspective of specific microbes did not eliminate the plural nature of strategies against the disease, even for Domingos Freire, who aimed at yellow fever a syringe armed with his prophylaxis. One could argue that these Brazilian Pasteurians were wrong, but that would only invalidate the points of passage they established. The logic behind construction and validation of microbial theories resulted in the reiteration of most of the certainties produced by physicians and hygienists. Latour’s description of the needed rectification of both direction and mentality only appeared when Oswaldo Cruz became head of Public Health. The differences can be seen in the definition of a limited number of diseases to be targeted, in the focus on the vectors of yellow fever and of the bubonic plague, and in the emphasis on vaccination – which does not escape the image of an arrow pointed at the specific flank of smallpox.

These arrows guided the action of Oswaldo Cruz’s sanitary brigades and lent clarity to their initiatives in the chaotic, tumultuous context framing the urban reform and sanitation of Rio de Janeiro. We can discern its unique trajectories amidst the offensive attack that engineers were concomitantly leading against many of the targets pinpointed by public health during the previous century (and which the sanitarians now judged irrelevant). The ties that Oswaldo Lutz’s sanitary campaigns were tearing asunder or patching together within a fraying social and urban fabric pushed into action, relation, or conflict a multitude of actors unforeseen when the new strategy was presented, with the simplicity of its experimental correlations, at the 5th Brazilian Congress of Medicine and Surgery. Turmoil swallowed the vaccine, the plague was subjugated, and yellow fever disappeared only momentarily in Rio de Janeiro.
Adolfo Lutz and microsporidia

Having intended from the very beginning to study animal haematozoons, Paul-Louis Simond, communicating with Oswaldo Cruz, had made “the most well-deserved references” to Lutz’s latest paper on ophidian haematozoa. He and Marchoux reportedly said they would inevitably go to São Paulo and, “with great pleasure” meet with “the friend [they] had already come to know well through his published papers.” This information was passed to Lutz on 20 November 1901, about two weeks after the Pasteur Institute mission’s arrival in Rio de Janeiro. Oswaldo Cruz was still a simple technician at the Manguinhos Serum Therapy Laboratory, where he worked in the preparation of serum and vaccine against the bubonic plague. It was only in December 1902 that he would take Pedro Affonso’s place as head of this establishment. The Frenchmen never did travel to São Paulo and Lutz’s opportunity to meet Simond in person came only in November 1902, when the latter came a second time to Rio, to obtain infected Stegomyia. In a letter written 9 March 1903 (two weeks before Cruz became Director General of Public Health), Simond stated:

It was with keenest interest that I read the report of the experiments you conducted with the aid of some doctors from São Paulo. They are of the utmost importance since this is the first time that the mosquito’s role in yellow fever has been confirmed outside of Havana ... I am no less interested in your other papers on pebrines and on the wild plants that host mosquito larvae. Mr. Foetterle gave me a copy of this latest publication, for which I thank you warmly. If I decide to travel to São Paulo, I will make certain to let you know in advance so that I will be sure to find you in your laboratory. It is hardly necessary to say that I would like to go strictly incognito and visit only yourself. (BR. MN., Fundo Adolpho Lutz, pasta 174, maço 1)

This letter does not add much to what we already know about its author’s investigations into the possible role of a microsporidian in the etiology of yellow fever. It is particularly enlightening with respect to the direction being taken by its recipient. Lutz had just written “Waldmosquitos und Waldmalaria” on 16 September 1902. In June, he found himself in Rio de Janeiro to collect Stegomyia for the experiments that would commence in December. That same year, in partnership with Alfonso Splendore, Adolpho Lutz published the first in a series of three papers on pebrine and microsporidia. He released these in a German periodical (CentralBlatt für
Bakteriologie, Parasitenkunde und Infektionskrankheiten), thus selecting as the audience a network of scientists that was still at a considerable remove from the network comprising Pasteur Institute investigators and the francophone majority of Brazilian doctors.

Pebrine was the name given to the infectious disease of the silkworm caused by *Nosema bombycis* Nägeli. These microsporidia had already caught Lutz’s attention during his first study of *Sporozoa*, which resulted in the 1889 publication of the article mentioned earlier, addressing a different order of *Protozoa: Myxosporidia*. In this paper, Lutz had described a myxosporidian he had found in the gall bladder of *Batrachia*. Pioneering these studies in Brazil, Lutz highlighted the growing attention focused on the pebrine microsporidian in a paper he wrote with Splendore (1902). This growing interest was grounded not only in the practical aspects of sericulture (only recently introduced in São Paulo) but also in biomedical reasons: ever more discussions were exploring how this group of *Protozoa* behaved as cellular parasites, especially with respect to malignant tumors; furthermore, as we have just seen, speculations on its role as an agent of yellow fever were a topic of the day.

The first species of microsporidian – *Nosema bombycis* – was described in 1857 by the Swiss botanist Karl Wilhelm von Nägeli (1817-91). At that time, he thought the small black spots on the *Bombyx mori* moth were a yeast that belonged to the kingdom *Fungi*. More in-depth knowledge of this organism has been attributed to Louis Pasteur. In 1865, the French government commissioned him to investigate a disease that was devastating the silkworm. In 1867, after various incorrect interpretations had been put forward, Pasteur identified certain “corpuscles” (*Nosema bombycis* spores) as the cause of the pebrine disease, although he had never understood the true nature of the parasite (Hyman, 1940, p.162). It was only in 1882 that it would be included among protozoa. After a series of studies, the French embryologist Édouard-Gérard Balbiani (1823-99) concluded that the *Nosema* described by Nägeli bore greater
affinity to sporozoans than to fungi; he then transferred it to the class *Sporozoa*, created by Rudolf Leuckart in 1879, and defined the taxon *Microsporidia* to include *Nosema bombycis* and the other species of the group.\(^{154}\)

Until a few years ago, microsporidia had been classified in the phylum *Protozoa* as an order within the subclass *Cnidosporidia* and the class *Sporozoa*, along with myxosporidia (Hyman, 1940, p.47; Store and Usinger, 1979, p.311).

Phylum Protozoa  
Class Sporozoa  
Subclass Cnidosporidia  
Order Myxosporidia  
Order Microsporidia

These microorganisms caused a great stir in 1988 when they were discovered in HIV patients. After many studies into their taxonomic classification, it was established that myxosporidia are more closely related to *Metazoa* than *Protozoa*, and microsporidia, to fungi (Cox, 2002, p.595-612).

At the time that Lutz and Splendore published their paper, few species had been described and very little was known about these sporozoans. There was a consensus that they were intra-cellular parasites of a few vertebrates (fish) and, mainly, invertebrates. It was also known that they engaged in a form of hereditary transmission, by which the female host passed the agent of infection to the next generation through its eggs. Lutz found in Splendore the ideal partner for investigating these minute parasites, whose size demanded extreme patience and a great expertise in microscopy and also in fixing and staining techniques, knowledge that Splendore had acquired in Italy during his first few years in the profession. Lutz often calls attention to the patient work of his co-author: “Since there were few specifications on fixing and dying techniques, or on use of reagents, we paid these issues meticulous attention, with Dr. Splendore undertaking numerous lengthy experiments.” In another excerpt on silkworm reproduction, Lutz praised Splendore’s skill in managing to obtain “a good number of healthy *Bombyx mori* eggs” (Lutz and Splendore, 1902, p.151).

Alfonso Splendore (1871-1953) associated himself to the director of the Bacteriological Institute of São Paulo as soon as he arrived from Italy in 1899. A graduate of Rome’s Medical and Surgery Faculty two years
earlier, he had worked as an assistant at Rome’s Hygiene Institute, alongside such masters as Angelo Celli (1857-1914), Claudio Fermi (1862-?), and Giovanni Battista Grassi (1854-1925). When he moved to Brazil, he probably brought with him recommendations addressed to Lutz, who kept close ties with Italians who were studying malaria. In addition to working with Lutz at the São Paulo institute, Splendore was responsible for founding the laboratory at Rome’s Umberto I Hospital. He also headed the laboratory at the Beneficência Portuguesa Hospital in São Paulo.\textsuperscript{155} Apart from the three papers on microsporidia, Splendore worked on a number of other papers with Lutz.\textsuperscript{156}

In the first, they established new species that parasitized another group of insects, besides moths, and a species of fish. At that time it was not known whether moths were infected by other species of \textit{Nosema}, besides \textit{Nosema bombycis}, which Lutz and Splendore also called “pebrine.” They verified this with a butterfly quite common in the region of São Paulo (\textit{Brassolis astyra} Godt), which Lutz had first examined when he began his studies on sporozoans in the 1890s. With Splendore, Lutz now demonstrated the spontaneous infectious process and its artificial transmission in a laboratory of other species of \textit{Nosema}; they further verified the hereditary transmission of the infection – an issue, as we have seen, that greatly interested Paul-Louis Simond.

They carried out a series of experiments using healthy eggs from silkworm caterpillars obtained locally. With the help of Alfonse’s brother, Dr. Achille Splendore,\textsuperscript{157} they managed to bring in from Italy animals infected with pebrine, thus making possible comparisons and transmission experiments. They faced many difficulties, especially concerning classification of the species of host butterflies. Since there was no systematic classification of the Brazilian species, Lutz and Splendore were forced to rely on Adolfo P. Mabilde’s work on butterflies of the state of Rio Grande do Sul and on the general treatise published by W. J. Holland in 1898, \textit{The Butterfly Book}. They based themselves solely on the form of the spores in identifying the species of microsporidia. In his studies on myxosporidia, the Belgian scientist P. Thélohan (1892, 1895) had identified a polar capsule with a spiral filament in this group of microorganisms, but Lutz and Splendore did not consider it because they could not discern such a minute morphologic characteristic. They relied on another criterion to distinguish the species: their association with hosts. This was not an acceptable criterion
according to the day’s classification rules, and the authors themselves admit it: “It seems impossible to name species based solely on spore characteristics that appear in varying quantities; that is why we have adopted a method that is largely disapproved of today, which is to designate the species by the name of the first host in which it was discovered” (Lutz & Splendore, 1902, p.153).

In this article they identified nine new species of *Nosema*, most of which were *Lepidoptera* parasites. They verified that naturally occurring infections only had lethal consequences in silkworm pebrine. In the case of *Brassolis astyra*, São Paulo’s common butterfly, an uncommon atrophy of the caterpillar was a sign of a very serious infection, but no visible symptoms were observed in other species. The authors observed that another butterfly, *Dione juno*, would go through normal metamorphosis despite extensive infection. It became clear that hosts who spent lengthier periods in the larvae stage on the one hand favored rapid propagation of the species corresponding to microsporidia and, on the other, favored the harmful aspects of this generally benign infectious process.

The work of these two scientists from the Bacteriological Institute of São Paulo attracted the attention of other researchers interested in the group. F. Mesnil wrote a review on it for the *Bulletin do Institut Pasteur* (1903, v.I, p.62). That same year, Paul-Louis Simond published a note in the *Comptes Rendus de la Société de Biologie de Paris* on *Myxococcidium stegomyia*, the microsporidium of the genus *Nosema* discovered in *Stegomyia fasciata*, initially in specimens that had drawn blood from a yellow-fever patient, leading the French bacteriologist to the supposition that it was the causative agent of the disease.158

Lutz and Splendore also focused on the microsporidia that parasitized blood-eating hematophagous diptera and, in the paper published in 1904, they referred to “some forms” that had been recently observed, especially “parasites that one of us [Lutz] found in *Simulium* larvae.” They found different species of *Nosema*, which they described without, however, defining which genus they belonged to, for lack of specialized literature. This article was published in *CentralBlatt für Bakteriologie, Parasitenkunde und Infektionskrankheiten* as an addendum to the earlier one: in addition to describing new species, they included life-size drawings of the various hosts of microsporidia. The 1904 article benefited from an intense exchange of information between Adolpho Lutz and the Austrian
lpidopterologist Joseph Foetterle, who lived in Petrópolis, then a spot favored by German and Austrian immigrants. Although he worked as a violin teacher at Colégio Sion in Rio, his passion was collecting butterflies. He had a good knowledge of insects and maintained close ties with physicians that were investigating transmitters, especially those who lived in or regularly paid visits to that pleasant mountain city. In the collection of Paul-Louis Simond, who became Foetterle’s good friend, we find photographs taken by the Frenchman, showing the Austrian proudly displaying his collection. Foetterle also corresponded with Austrians and other Europeans and mediated relations between researchers who came to Brazil and the “natives.” In his letters to Adolpho Lutz, who was a close friend, he always kept him abreast of news on members of the French mission.

Foetterle was also involved in the search for new microsporidia. His correspondence with Lutz contains much important information on pebrine, dyes and butterfly classification. The director of the Bacteriological Institute would send Foetterle material to classify, while the Austrian would send him insects contaminated with pebrine. “I received your friendly card on Saturday and, on Sunday, the moth you sent, which belongs to the genus Caeculia (Fam. Lasiocampidae),” the Austrian naturalist wrote, for example, on 1 March 1903.

I am afraid that I can not tell what species it is. I have not yet raised the species you sent me, but I am familiar with the caterpillars of species that are very close to it ... Regrettably, I have but few duplicate samples of these rascals, which, however, I could cede to you. The other day I finally found pebrine in a species close to yours ... It took me a while to find these subjects that made such mockery of me, and it was a fair amount of time and work that I spent on it. I do not have the time to set them into permanent preparations today, but I will certainly do so tomorrow and send them to you, along with the moths. I have also experimented with pyoctanin as a dye, and had quite good results. Have you also used this dye?159

In another letter, dated 19 October, which was sent along with more material, he states: “The species in which I found pebrine is in the paper tube ... and I hope that this time you can confirm my findings and can also discover pebrine in the three preparations I am sending with it. If, once more, there are not any, then it is better that I stop meddling in this.”160

Foetterle’s contributions were incorporated into the work of Lutz and Splendore, who did not fail to mention his valuable help.
FFoetterle in “colonial” clothing, gathering samples. Photograph autographed by the subject on 25 April 1905.
State of Rio de Janeiro (Musée de l’Institut Pasteur, MP31328-2.tif).
Four years elapsed before the last paper in the series on microsporidia came to light in 1908, in the same German journal that had published the other two. Its great merit lies in identifying worms and even other protozoa that served as hosts to microsporidia. As the authors themselves pointed out, it was already well known that these did not parasitize only arthropods and fish, but information was scant and incomplete (Lutz and Splendore, 1908). Lutz and Splendore described new species and identified others already described in hematophagous insects, such as *Nosema simulii* in *Simulium* larvae, *Nosema chironomi* in *Chironomidae* larvae, *Nosema ephemerae* in the intestines of ephemerid larvae, and *Nosema stegomyia* in the imago of *Stegomyia fasciata*. They found *Nosema mystacis* in two female samples of *Ascaris mystax* taken from the intestines of a cat, and *Nosema distomi* in a small *Distomum* that inhabited the intestines of *Bufo marinus*. In the *Balantidium* present in the terminal intestine of *Bufo marinus*, they found another microsporidium, *Nosema balantidi*.

Lutz and Splendore came to the conclusion that these worms and infusoria were contaminated by direct infection, discarding the hypothesis of hereditary transmission. They still had problems finding the polar filament and for this reason did not accept it as a valid character in identifying the species. Having found this morphological characteristic in only one species, they even raised doubts about its occurrence, reaching the conclusion that, in the future, its absence might prove important for the group’s systematics. It was in fact later proven that all microsporidia spores have a long rolled-up filament that represents the polar capsule (Hyman, 1940, p.162).

This was the last work that the two authors wrote on protozoa and also Lutz’s last publication as head of the Bacteriological Institute of São Paulo. From 1919 to 1920, Splendore would teach bacteriology at the universities of Parma and Rome. In 1908, invited by Oswaldo Cruz, Lutz transferred to the Manguinhos Institute in Rio de Janeiro, where he would devote most of his time to an area of zoology that had won over his heart and his mind: entomology – which is, as he liked to say, “precisely” the topic of the next two books in this volume of the scientist’s *Collected Works*. 
Notes

1 Although evidence suggests that yellow fever had been present in Brazil since 1694 (Ministério da Saúde, 1971), it was only as of the mid-19th century that it became the country’s major public health issue.

2 On magnetic tape, Bertha Lutz, daughter of Adolpho Lutz, recorded interesting facts about the family history and the life of her father. Entitled Lutziana, the tape registers in her own voice the outline of a biography she never came to write (BR. MN. [Brasil, Museu Nacional J] – Fundo Adolpho Lutz).


5 The bibliography of Adolpho Lutz compiled by Herman Lent (Neiva, 1941) was reprinted, with corrections and additions, in História, Ciências, Saúde – Manguinhos, v.10, no. 1, p.362-409.

6 Deane (1955, p.77-80). As this author shows, Lutz advocated the use of feces examinations to diagnose these helminthoses; he also pointed out that this practice was not given due credit even in the three German-speaking universities where he had studied. Lutz’s biographers underscore his pioneer role in veterinary research. His first work in the area was the description of a species of Dipylidium caninum; about stephanurosis, cysticercosis, and other helminthiasis common in animals; about Fasciola hepatica; and also about the wild hosts of Dioctophime renale, a parasite of the kidneys of several domestic animals.

7 See also Councilman and Lafleur (1891, p.396) and www.whonamedit.com/doctor.cfm/2929.html (consulted on 13 Apr. 2005).

8 “Lösch was the first author to provide a more accurate description of the amoeba species found in the stool of a patient with dysentery, accompanied by a thorough clinical record and an autopsy report. This was truly a pioneer study, containing excellent observations and descriptions of the organism called Amoeba coli, to which very little was added subsequently” (Councilman and Lafleur, 1891, p.397).

9 “All his reports merely make mention of the existence of these microorganisms, without attributing any great import to them” (Councilman and Lafleur, 1891, p.400).

10 According to Councilman and Lafleur (1891, p.400-1), after Lösch’s, the most important study is the one that prompted Kartulis to publish a series of articles in Virchow’s Archiv.

11 “His results were published in Czech, a language unfamiliar to most researchers ... Kartulis mistook ‘O. uplavici’, which in Czech means ‘About dysentery’, as the name of the paper’s author ... And thus the phantasmagoric professor ‘Uplavici, O.’ made his way into a number of specialized bibliographies, until Dobell untangled the confusion in 1938” (Martinez-Palomo, 1996).

12 Born in Canada, Osler had begun his clinical practice in Dundas, Ontario, but was soon appointed lecturer and then professor of physiology, pathology, and medicine at McGill University. In 1884, he took the chair in clinical medicine at the University of Pennsylvania, in Philadelphia. There he became one of the founding members of the Association of American Physicians. In 1888, Osler accepted an invitation to serve as the first professor of medicine at Johns Hopkins University Medical School, in Baltimore. Together with William Henry Welch (1850-1934), head of pathology; Howard Atwood Kelly (1858-1943), head of gynecology and obstetrics; and William Stewart Halsted (1852-1922), responsible for surgery, they were to turn Johns Hopkins into one of the world’s most prestigious medical schools (Risse, p.407; see also www.whonamedit.com/doctor.cfm/ 1627.html; consulted on 13 Apr. 2005).

13 A graduate of the University of Maryland, Councilman had traveled to Europe in 1880 to specialize in pathology. He was in the same cities as Lutz – Vienna, Strasbourg, Leipzig, and Prague – and there spent time with people who were also connected with the Brazilian researcher’s education. In Vienna, he trained with Karl Freiherr von Rokitansky (1804-78); in Strasbourg, he worked with Friedrich Daniel von Recklinghausen (1833-1910); in Leipzig, with Julius Friedrich Cohnheim (1839-84) and Carl Weigert (1845-1904); and, lastly, in Prague, with Hans Chiari (1851-1916) (www.whonamedit.com/doctor.cfm/2860.html). In 1886, he joined the team of William Henry Welch (1850-1934) as well as the group of researchers at the newly inaugurated pathology laboratory where Osler also worked.

14 His paper was published in the Bulletin of the Johns Hopkins Hospital in 1890. The article on the patient’s death, on 5 April 1890, appeared the following month in the same periodical (Martinez-Palomo, 1996).

15 Among others, he cited Stengel (Medical News, 15 Nov. 1890); Musser (University Medical Magazine, Dec. 1890); and Dock (Texas Medical Journal, Apr. 1891).
He cites Osler, Nasse, Harold, Stengel, Eichberg, Kowacz, Fenaglio, Boas, Vivaldi, Babés and Zigura, Sorgo, Manner, Júrgens, and Roemer, in North and South America, Italy, Germany, and other countries.

For these authors, there was such a wide diversity of opinions on the protozoans’ role because all amoebae had been confused. Only *A. dysenteriae* caused dysentery. They conducted experiments on cats, and observed that they were only infected by amoebae that had ingested red blood cells, whereas amoebae that had not ingested such cells did not cause the disease (Cox, F.E.G., 2002). Martinez-Palomo (ibid.) points out that these authors discovered the resistant form of the amoeba, the cyst (we now know that two forms of *E. histolytica* exist: trophozoites and cysts).

Dopter (ibid.) also makes reference to Gregorieff’s work incriminating a bacillus identical to the one described earlier by Chantemesse and Widal. Ogata likewise attributed cases observed in Japan to a bacillus. For Zancarol, it was a streptococcus that he had found in stools and liver abscesses. Casagrandi and Barbagallo actually affirmed that these protozoans were saprophytes that were useful to the intestine because they contributed to the destruction of virulent bacteria. Schuberg (1895) gave a purgative to a number of healthy individuals and verified that half of these were hosting inoffensive amoebae in their intestines.

Maciel, *Discriminações raciais*, p.57; and Lisboa, *Almanaque de Campinas para 1871*, both cited by Santos Filho and Novaes (p.35).

Francisco Rangel Pestana (1839-1903) and Américo Brasiliense de Almeida Melo (1833-96). Two republican leaders lived in the city: Manual Ferraz de Campos Sales (1841-1913) and Francisco Glicério de Cerqueira Leite (1846-1916), in addition to Américo Brasílio de Campos (1838-1900) and Francisco Quirino dos Santos (1841-86) (Santos Filho and Novaes, p.14).

In 1889, he was to present another treatise: “Yellow fever in Campinas: two words about this epidemic, read before the Imperial Academy of Medicine of Rio de Janeiro in the session of 27 April 1889” [in Port.] (*Anais da Acad. de Med. do Rio de Janeiro*, 1889, p.331-49). According to Santos Filho and Novaes (p.29), these cases of yellow fever had not set off an epidemic, as in 1889, because its transmitter, *Aedes aegypti*, was not yet present in the city.

Santos Filho and Novaes (p.175-6). Doctors believed the epidemic resumed in May because of the return of infected inhabitants who had fled. They then appealed to all those who had left to delay their return until the crisis had passed.

The last Chamber of the monarchy was elected for the 1887-90 legislature and was dissolved by republican decree on 21 January 1890. It was first chaired by the pharmacist Otto Langgaard, who lost his children to the epidemic. At the height of the crisis, the nine aldermen did not meet but in May a session took place at one of their homes (Santos Filho and Novaes, p.47).

Lapa (1996, p.260-1) also mentions the elimination of ‘black ditches’, the filling in of swampy areas, and the channeling of streams. He notes that the measures adopted to eliminate the bad odors and miasmas ‘managed to have some effect, ignored, however, by physicians and authorities, that is, they scared off the mosquito *Aedes aegypti*.”

On 11 April, he was replaced by the Baron de Jaguara (1837-95).

According to Ângelo Simões (1897, p.23-4), “of the twenty-three general practitioners that we were, only three stayed, taking on an insane job.” The three were João Guilherme da Costa Aguiar (1856-89), Germano Melchert (1844-1921), and Simões himself, “the second having fallen ill and the first having paid with his own life for the heroism that kept him in the struggle against such a dreadful enemy.”

*Diario de Campinas*, 29 May 1889, cited in Santos Filho and Novaes (p.49).

Author of *Estudos sobre a febre amarela* (1880), upon his return to Portugal he published *A epidemia de Campinas em 1889* (Rio de Janeiro, 1889).

A compound of ferrous sulphate (400 grs.), crystallized ferric acid (100 grs.), and boiling water (2 liters).

He is said to have signed eleven certificates in April and one on 5 May, in Santa Cruz, and another three in April, in Conceição (Santos Filho and Novaes, p.53).

Santos Filho and Novaes (p.11). Drs. Francisco Marques de Araújo Góis, Claro Marcondes Homem de Melo, and Bráulio Gomes were awarded medals commissioned by the Chamber. “Adolfo Lutz was not paid this honor, which is impossible to understand,” write Santos Filho and Novaes (p.53). They explain this “because the three who received the medals could easily travel to São Paulo – something they had always done – thereby not failing to provide steady, more effective assistance.” Lutz’s work there notwithstanding (vouched by the number of certificates he signed), the authors state that as a “wise researcher already, [he] was more concerned about observing the disease and its contagion than in actually practicing medicine” (ibid., p.50, 53). It is possible that Lutz left earlier owing to his upcoming travels to Hamburg and Hawaii. On this point, see Benchimol and Sá (2004).
32 According to Freire (1890a), fifteen lives were claimed in Vassouras (three immigrants), eleven in Resende (three immigrants), twenty-two in Desengano (seven immigrants), twenty-one in Serraria, and twenty in Cataguases, bringing deaths to a total of 4,137 (2,802 immigrants), with Rio de Janeiro, Campinas, and Santos included. Santos Filho and Novaes (p.38-13) point to the contrast between the assistance given these cities and that given Campinas in 1889. The central, provincial, and municipal governments spared no efforts in aiding the victims of the first epidemic. On the other hand, the imperial government appointed only one physician (Augusto Daniel de Araújo Lima) to attend to the indigent ill in Cataguases, while three doctors residing in the town volunteered to attend the poor free of charge. For the authors, this contrast is explained by the fact that Campinas was then "a very prestigious socioeconomic and political center ... one of the most thriving cities, with a solid economic situation and among the most culturally advanced, if not in all Brazil at least in the entire province of São Paulo and in Southern Brazil."

33 On Freire and other hunters of the yellow fever microbe, see Benchimol (1999).

34 The provincial president dismissed the vaccination agents at work in Bahia’s municipalities after the local legislature cut off funding, alleging that this service should fall to the central government. Francisco Antunes Maciel, minister of the Empire, did not concur with protests lodged by the Central Board of Public Health. On 4 October 1883, the members of the Board resigned. On 8 October, Domingos José Freire took over as president of a new Board.

35 The letters reproduced by Vallery-Radot (1930, p.397-411) and other letters can be found at the Museu Imperial, Setor de Documentação e Referência, Arquivos da Casa Imperial (Petrópolis). On the history of the smallpox vaccine in Brazil, see Fernandes (1999).

36 The special communication read on 7 September was entitled "Vaccination avec la culture atténuée du microbe de la fièvre jaune." It was summarized in Medical News (17 Sep. 1887, v.51, p.330-4), in the newspapers Jornal do Commercio, O Paiz, and Gazeta de Notícias (22-23 Aug. 1899) and in Brazil-Medico (no. 33, 1 Sep. 1899, p.319). Freire wrote over one hundred works on chemistry, medicine, and public health, in the form of reports, compendia, books, monographs, and communications.

37 The majority of those vaccinated were Brazilian. This shows that Freire’s prestige itself played a greater role than foreign immigration in spreading use of the vaccine. In Rio de Janeiro, his data were authenticated by Counselor Caminhoá and two attorneys, Julio Ottoni and Oliveira Coelho; Joaquim Caminhoá, the counselor’s son, and Virgílio Ottoni – both of whom were assistants at the School of Medicine’s organic chemistry lab – took part in the vaccinations; others included the physicians Silva Santos, hygiene delegate; Guilherme Affonso, Alfredo Barcellos; and Campos da Paz. In a letter dated 19 October 1889, Dr. Pinto Netto, director of the Santa Izabel Maritime Hospital, in Jurujuba, informed Freire that a commission from the Society of Medicine and Surgery was investigating his preventive method. Many of these vaccinations were observed by members of the commission, which included two of Freire’s ex-assistants and Drs. Chapot Prévost and Benicio de Abreu, then professors at the School of Medicine. We read in one newspaper, for instance, that “on Sunday, from eight until nine in the morning, Dr. Virgílio will be offering vaccinations in Botafogo, at 66 Voluntários da Pátria Street, assisted by Dr. Carlos Costa, member of the commission of the Society of Medicine and Surgery” (Gazeta de Notícias, 7 Feb. 1889).

38 Fifteen of those not vaccinated died (three immigrants and twelve Brazilians, three of whom black). A large number of people fell ill, although records only specify that 115 of them were black. Freire’s published results were endorsed by Dr. Joaquim Francisco Moreira, president of the Chamber, and Luis E. de Lemos, secretary; by the jurist Sebastião Eurico Gonçalves de Lacerda; and by the hygiene delegate Dr. Augusto de Paiva Magalhães Calvet (Freire, op. cit., p.23, 25-6).

39 None of those vaccinated died. Among the non-vaccinated, yellow fever claimed 650 lives. Present were Drs. Julio Alves de Moraes, hygiene delegate; municipal physician Joaquim da Motta Silva; Giovanni Ebboli, of Santa Casa de Misericórdia Hospital; Carl Hertschel, local practitioner; the public prosecutor, Dr. João Nepomuceno Freire; the Baron de São Domingos, a state judge; the president of the Chamber, Julio Corrêa; Alfred Esquivel; Lucas Fortunato; and Americo Martins, among others.

40 Only three failures were reported, i.e., 0.46% of all those vaccinated. According to Simões’ report, mortality among the unvaccinated was estimated at 810 inhabitants, but for another 300 of those buried, the death certificates indicated such questionable causes of death as “icterohemorrhagic fever” and “reigning fever”, among others. Simões published the evaluation that he had sent Freire in the Correio de Campinas (10 Jun. 1889). On 11 May 1889, he transcribed a report and forwarded it to the president of the province of São Paulo, the Baron de Jaguar: he planned to continue vaccinating, especially people who were returning to Campinas despite instructions published in newspapers advising that they wait until the city had been wholly disinfected. Freire published this letter in the Gazeta de Notícias (12 May 1889). In Tratamento da febre amarela pela agua chlorada, published in 1897, Simões still wrote: “Save later proof to the contrary, the discovery of the yellow fever microbe inarguably falls to Freire” (p.40 ff.)

41 The newspaper’s representatives were: in Araras, professor J. Voss; in Botucatu, professor Carlos Knüppel; in Piracicaba, J.J. Huflienbaecher; in Pirassununga, Gustav Beck; in Ribeirão Preto, Carl C. Petersen; in Rio Claro, Luiz F. Barthmann; in Santos, Paul Wilkens; in São Carlos do Pinhal, C. Priester; and in Sorocaba, Th.
Kaysel, hat manufacturer. The masthead also listed Gottlieb Müller and S. Lauer, from Curitiba and Joinville, respectively. W. Diebener’s bookstore represented the paper in Leipzig. Carlos Constantin Knüppel, first clerk of court in Joinville, was founder and editor of Beobachter am Mathiastrom, the first newspaper (handwritten) launched in that town. He moved to São Paulo where he was a teacher at the first Escola Alemã (German School). He later moved to Botucatu, where he also taught.

Freire won the support of municipal authorities in Rio and other cities and also of many doctors residing on the coast and inland, including delegates from the central hygiene department. He likewise had the backing of various institutes from civil society. But the vaccinations were met with hostility by the Ministry of the Empire, by the leadership of the General Inspectorship of Public Hygiene, and by the Imperial Academy of Medicine (the latter theoretically the realm of Brazil’s medical elite, where the interests of the profession and of the government intertwined).

On this topic, see Benchimol (1999).

On this topic, see Soriano (2002) and Benchimol (1999).

Finlay (1965, p.317-48). The Cuban physician located the disease’s source of morbidity in the trajectory of the bite, through the inner walls of blood vessels, assuming that the insect inoculated the disease into the corresponding tissues of healthy people. According to Delaporte (1989, p.69, 73), this theory identified yellow fever with smallpox: “If one needed to summarize Finlay’s outlook, one could easily say that he saw the mosquito through Jenner’s eyes.”

Lacerda recounts part of his history at the institute in Fastos do Museu Nacional (1905). The bio-bibliography published by the Museu Nacional in 1951 entirely omits works on yellow fever and bacteriology, which are analyzed by Benchimol (1999).

On this topic, see Benchimol (2004).

Yellow fever is caused by an arbovirus of the genus Flavivirus (Veronesi, 1991, p.174-82). It presents a number of clinical manifestations in human beings: high fever, chronic headache, muscle aches – in short, all the signs of an acute infection, which soon affects the digestive tract. After two or three days, if the disease does not recede, the patient’s vomit and feces will turn bloody and jaundice will worsen, as will abdominal pain; the patient hemorrhages from the fossa canina, gums, or skin. Decreased urination triggers the toxemic stage, which develops into coma and leads to death. These symptoms are caused by virus replication inside the organism after inoculation by a mosquito of the genus Aedes or Haemagogus. The disease’s viral etiology was only established in 1927; in March 1937, on the premises of the Oswaldo Cruz Institute, the Rockefeller Foundation began to produce a vaccine made from virus cultured in chicken embryo, a vaccine which is still used today (Benchimol, 2001).

See also Stepan, 1976; Benchimol, 1990.


Havelburg’s paper, published in Annales de L’Institut Pasteur (Jun. 1897), was a condensed version of the article that had come out in Berliner Klinische Wochenschrift (1897). It was released together with Sanarelli’s first special communication. Both sent cultures of their bacilli. Sanarelli’s supplementary evidence was published in September and October. Havelburg’s conference was published by Brazil-Medico and, in book form, by the Jornal do Commercio. He had just been appointed head of the hospital’s anatomical-pathological laboratory (Brazil-Medico, 8 Apr. 1897, p.119). Lutz commented on Sanarelli’s sealed letter in Brazil-Medico (15 Dec. 1898, p.416-7).

In July 1897, he was replaced by Vital Brazil Mineira da Campanha (caixa 25, pasta 16, maço 3: “Auxiliares do Dr. Lutz”; BR. MN. Fundo Adolpho Lutz).

Letter from Francisco Fajardo to Adolpho Lutz, dated 18 March 1897 (BR. MN. Fundo Adolpho Lutz).


Born in Capivari (São Paulo) on 6 December 1871, he graduated from the Brussels School of Medicine in 1895, and the following year joined the Bacteriological Institute of São Paulo as an “adjunct physician.” Between May and November 1901, he traveled to Europe to study wine yeast cultures using Pasteur’s system. In 1901, he took the chair in bacteriology at the Escola Livre de Farmácia de São Paulo (São Paulo Free School of Pharmacy), now the University of São Paulo’s School of Dentistry. He died on 24 April 1903, a victim of yellow fever.
59 In an official letter to Dr. J.J. da Silva Pinto, director general of the Sanitation Service (dated 25 Mar. 1897), Lutz advised that he had received reliable information that Sanarelli was planning to hold a conference in May where he would reveal his discovery: “I would like to suggest that it would be appropriate for our institute to be represented upon this occasion, so that we may be capable, as early as possible, of assessing the value of the practical usefulness of the aforementioned discovery.” According to an official letter to Silva Pinto from the State Department of Internal Affairs, signed by A. Dino Bueno and dated 4 June 1897, Lutz and Mendonça had traveled to Montevideo the previous day on assignment to the Government of the State. Source: excerpts from Prof. Lutz’s reports on yellow fever as director of the Bacteriological Institute, 1893-94 (pasta 212 f maço 4, BR. MN. Fundo Adolpho Lutz).

60 On this topic, see Benchimol (2004).


62 Lutz said he would postpone discussion of the conference until Sanarelli’s treatise had been published in the Annales de l’Institut Pasteur (it would appear in nos. 6, 8, and 9, in Jun., Sep., and Oct. 1897). In this first report, Lutz voiced a preliminary opinion of Sanarelli’s ideas and promised to return to the topic in the next Bacteriological Institute report.

63 In a letter dated 28 July, Fajardo asked Lutz for a “copy of the notes [he] had taken on the yellow fever experiments conducted by Sanarelli in our presence” (pasta 212 f maço 4; BR. MN. Fundo Adolpho Lutz). The Military Bacteriological Laboratory had been inaugurated on 2 July 1896, at a private residence located at 24-A Senador Furtado Street. In late 1897, the director, Ismael da Rocha, and Drs. Antonio Ferreira do Amaral, Raymundo Firmino de Assis, and Manuel Saraiva de Campos worked there (Brazil-Medico, 22 Oct. 1897, p.355). The lab was later transferred to one of the pavilions of the Central Army Hospital, located at 40 Duque de Saxe Street (Brazil-Medico, 1 Jan. 1899).

64 He released his first results in the annals of the University of Montevideo, in the Jornal do Comercio (22 Oct. 1897), and in Brazil-Medico (Nov. 1897, p.379-81). Sanarelli guaranteed that the serum was not dangerous. He suggested an initial dose of 20cc; were no improvement observed, a second dose should be injected, then a third, and so on, taking into consideration the patient’s resistance and condition, the stage of the disease, etc. The injections should be made subcutaneously, in the flank or buttocks, and, in urgent cases, directly into the veins (Rêvue Physicien-Chirurgicale du Brésil, Feb. 1898, p.39-42; see also The American Medical Association Journal, 26 Mar. 1898, p.745).

65 Nevertheless, in July he asserted the priority of his invention (Brazil-Medico, 15 Jul. 1898, p.237). The reaction in the United States was favorable (The American Medical Association Journal, 28 May 1898, p.1304-5).

66 Starting in 1900, correspondence was sent directly to the Secretary of Agriculture, Commerce, and Public Works; Laufer’s aid, Otto Herbst, would later take over the responsibility. Information on this settlement was sourced from Livro de Correspondências 1899 – 1903 (E 01834), and Arquivo Livro Relatório 1900 (E 01838), Arquivo Permanente / Arquivo do Estado de São Paulo (APIAESP).

67 The proportions were as follows: Germans, 45.51%; Swiss, 9.93%; Austrians, 9.13%; and German-Brazilians, 3.84%. Other nationalities were: Hungarian, 8.65%; Swedes, 9.29%; Brazilian, 7.21%; Italians, 5.26%; and Portuguese, 1.12%.

68 Lacerta (1900, p.44-5). First published in Annaes da Academia de Medicina do Rio de Janeiro (1899) and Brazil-Medico (1899).

69 The municipality was responsible for sanitation and hygiene services until promulgation of a law dated 3 August 1896 and of a regulation dated 7 October 1896, whereby these duties were transferred to the state. The Municipal Intendancy retained responsibility for garbage collection and street cleaning (Lapa, 1996, p.261).

70 The Commission set up offices in a building that the Municipal Chamber had originally constructed to house a market inaugurated in 1861, located on what is now known as Anchiesta Avenue. It was adapted to accommodate “carriage barns, sterilizers, clothes closets, bathrooms, sulphurous gas chambers, incinerators, etc.” Like the São Paulo Central Disinfecting Station, it was called the Campinas Central Disinfecting Station. It served this purpose from 1896 to 1918, when it was torn down and a Normal School built in its place, now called the Instituto de Educação Carlos Gomes. Around the same time, a disinfecting station was set up at the terminal where passengers arrived in town (Lapa, 1996, p.263).

71 Santos Filho and Novaes, p.258-60. In 1896, Brito laid out a design for “sanitation works that included drainage, better water impounding, creation of new reservoirs, installation of a sedimentation box, and a whole set of improvements, the most important of which would be drainage ditches, abutted by tree-lined avenues, which would actually come into being when Anchiesta and Orozimbo Maia avenues were later laid” (Lapa, 1996, p.262).

The Academy of Medicine formed a committee to analyze the matter. Gurgel do Amaral (future mayor of the city) endorsed the position defended by the Public Health demographer, Aureliano Portugal, who believed the soil should be drained (Gazeta de Noticias, 8 Dec. 1892, p.1). Arguing with Rocha Faria, Aureliano Portugal reiterated his faith in Pettenkofer: “Unless the soil is sanitized through drainage, this dreadful malady will not abandon these parts.” José Lourenço and Clemente Ferreira presented their own opinion, contending that yellow fever was imported and contagious. After Lourenço’s treatise was read before the Academy (cited earlier), Portugal replied in the Gazeta de Noticias (28 Dec. 1892, p.2).

This topic is addressed in Benchimol (2003).


In an article in which he disagreed with Freire, Fajardo (15 Jun. 1894, p.180) transcribed statements by Laveran himself and by the Italian pathologist Camilo Golgi. Both had very favorable things to say about the preparations that the histologist from Rio had made in his effort to detect the forms of malaria plasmodium found in the blood of Rio de Janeiro residents. Laveran wrote from Paris (Mar. 1893): “At the next session of the Biology Society, I shall speak of your preparations and I shall say that you have managed in Brazil to find the haematozoon of paludism; if you would send me a paper on this, it would be my pleasure to submit it to one of our scientific societies. You are on the right path and Mr. Domingos Freire, on the wrong one. I would like to thank you for having sent me Dr. Freire’s latest paper.” In a letter dated 18 August 1893, Golgi expressed his thanks for two “very finely obtained” preparations of malarial blood, which he had received from Fajardo.

The discovery took place almost by chance, when he was in Algeria. Busvine (1993, p.18, 20) explains that malaria parasites contain a black pigment, which is the partially digested hemoglobin of blood cells. The black clusters had already been described by other physicians as degenerated blood corpuscles. When he examined a soldier’s fresh blood, Laveran witnessed a phenomenon that his predecessors hadn’t, according to Busvine, because they had only observed dead cells from the blood: some of the pigmented bodies became rounded and then extruded many thin, rapidly waving filaments. This process would become known as exflagellation; the filaments are spermatozoids that are released to fertilize gametes in the mosquito’s stomach. In 1880, Laveran published the discovery of Oscillaria malariae in the Annales de Dermatologie (1:173) and in the Bulletin de l’Academie de Medicine de Paris (44, 2nd ser., v.9, p.1346).

Tertian malaria, the most common form, is caused by Plasmodium vivax; fever occurs every other day. Although the tertian form is milder, it can lead to a state of chronic illness. People afflicted with tertian malaria suffer relapses. In the case of quartan malaria, caused by Plasmodium malariae, fever recurs every 72 hours, while in the case of quotidian (or irregular) malaria, caused by Plasmodium falciparum, fever and chills appear at irregular intervals. The latter is the most pernicious form of the disease. (www.noolhar.com/opovo/ceara/).
Pereira Barreto, one of the political bosses of the Republican Party, held the same view in regard to yellow fever. Although he was a minority opinion within São Paulo’s Society of Medicine and Surgery, his ideas influenced the sanitation works executed in 1896, under Manuel Ferraz de Campos Sales’ administration, in the capital of São Paulo, Santos, Campinas, and other places hit by the ‘black vomit’ (Teixeira, 2001, p.121-3; Tellaroli Jr., 1996).

85 “Relatório de 1895” (BR. MN. Fundo Adolpho Lutz).
86 “Relatório de 1896” (BR. MN. Fundo Adolpho Lutz).
87 In the species Python molurus, Tropidonotus stolatus, and Bungarus fasciatus. This knowledge came from Lutz’s reading of Wasielewsky’s “Sporozoenkunde” (Report on Wasielewsky’s sporozoans) and from “Baumgarten’s Jahresbericht” (Jahresbericht über die Fortschritte in der Lehre von den pathogenen Mikroorganismen [1885-1911]), an annual publication created by Dr. Paul Clemens von Baumgarten [1848-1928], professor of pathological anatomy at Königsberg). An addendum to the 1901 article presents the following observation: “Only after this work was finished did I obtain access to a copy of Labbé’s monograph, sold out in the bookshops ... My results harmonize with his data. Our divergences only appear when it comes to interpreting what was observed. Nevertheless, I see no reason to alter my conceptions ... A revised and expanded edition of this fine monograph would certainly be opportune and welcome.”
88 “Relatório de 1896” (BR. MN. Fundo Adolpho Lutz).
89 “Relatório de 1897” (BR. MN. Fundo Adolpho Lutz).
90 “Relatório de 1898” (BR. MN. Fundo Adolpho Lutz).
91 Lutz thought absurd the theory that all quotidian fevers were the result of “two attacks of intermittent tertian fever, or three attacks of quartan fever.”
92 On 13 September 1946, a few years before the end of the English license period, the São Paulo Railway was taken back by the Brazilian government. Two years later, it was renamed Estrada de Ferro Santos-Jundiaí and was later joined into the national railroad network. A cog railway — a new traction system for crossing the mountains — was opened in 1974, making the funicular system built in the 1860s obsolete. Its replacement was in use until 1982, when it also stopped operating commercially. See “Endless rope” and “São Paulo Railway” in Wikipedia, the free encyclopedia s.l.: 27 Jan. 2005. Available at: pt.wikipedia.org/wiki/Endless_rope (accessed on 5 May 2005); MINAMI (n.d.); “Empreendimentos que honram o Estado de São Paulo,” O Diário, 26 Jan. 1939; Santos and Lichi, 1996.
93 “As a matter of fact,” comments Lutz, “this staining method is better than any other for the clarity with which it shows both the chromatin and the protoplasm of the plasmodia and their respective evolution phenomena. However, it is very delicate to use ... often causing great difficulty. Thanks to our extensive practice we eventually managed to successfully avoid the frequent failures that occurred at the beginning of these studies” (“Relatório de 1901: Malaria,” BR. MN. Fundo Adolpho Lutz).
94 “Relatório de 1902” (BR. MN. Fundo Adolpho Lutz).
95 “Relatório de 1901” (BR. MN. Fundo Adolpho Lutz).
97 Some of the species in the plants fed on humus enriched by the presence of microscopic organisms (rotifers, infusórios, diatoms, etc.); others, like Coleoptera larvae, hunted tiny creatures. Megarhinus larvae did a “good cleanup” among the small culicids (Lutz, 1903).
98 In an article published in 1903, he thanked the collaboration of a number of friends, citing Mr. Ahrens, but Professor von Wettstein, who had provided him with water from bromeliads containing mosquito larvae from a variety of locations; Mr. Schmalz of Joinville, who had, at his request, bred a large number of these mosquitoes; and Messrs. Loefgren and Edwall of the São Paulo Geography Commission, who had supplied invaluable bibliographic information about botany. An amateur collector, João Paulo Schmalz started corresponding with Lutz in 1899, when he was searching for Diptera. In the first letter, dated 30 June 1899, he said that his collection basically comprised Coleoptera and Lepidoptera, but few Diptera, but that he would collect specimens of this group for Lutz. From then on, he sent him a number of packages (BR. MN. Fundo Adolpho Lutz, Pasta 216, maço 12).
99 It was republished in Portuguese on 30 April 1950 by Revista Brasileira de Malariologia, v.II, no. 2, (“Mosquitos da floresta e malária silvestre,” p.91-100) and in English (“Forest Mosquitoes and Forest Malaria,” p.101-10). In the present edition, apart from updating the spelling, we have added footnotes with the corrections made by Adolpho Lutz’s children and ourselves, based on a comparison with “Waldmoskitos und Waldmalaria.”
habitat of species of know that the malaria relationship is highly specialized. Each type of malaria is generally carried by one or two as opposed to other mosquitoes. Lutz’s conclusion at the time was therefore natural and plausible. But now we relationship with malaria had reached. It was thought that human malaria was simply transmitted by articles about bromeliads.

peryassui Freitas lagoon. In 1907 Oswaldo Cruz also identified Manguinosia lutzi, which was renamed Anopheles peryarassui the following year, when it was discovered that it was actually an anopheline. The Anopheles lutzi was captured in 1901 in Rio de Janeiro by Oswaldo Cruz on the banks of Rodrigo de Freitas lagoon. In 1907 Oswaldo Cruz also identified Manguinosia lutzi, which was renamed Anopheles peryarassui the following year, when it was discovered that it was actually an anopheline. The Anopheles lutzi was captured in 1901 in Rio de Janeiro by Oswaldo Cruz on the banks of Rodrigo de Freitas lagoon. In 1907 Oswaldo Cruz also identified Manguinosia lutzi, which was renamed Anopheles peryarassui the following year, when it was discovered that it was actually an anopheline. The Anopheles lutzi was captured in 1901 in Rio de Janeiro by Oswaldo Cruz on the banks of Rodrigo de Freitas lagoon. In 1907 Oswaldo Cruz also identified Manguinosia lutzi, which was renamed Anopheles peryarassui the following year, when it was discovered that it was actually an anopheline.

In the view of Gadelha (1994, p.175-95), author of the best study on the subject, the expression ‘bromeliad malaria’ was first used by Downs and Pittendrigh (1946). The issue of Parasitologia in which Paulo Gadelha’s article was published was edited by W.F. Bynun and B. Fantini, and is entirely dedicated to “Malaria and Ecosystems: Historical Aspects.”

Picado states that nobody had yet studied the set of bromeliad fauna and the variety of environmental conditions these plants provided. It was these aims that he intended to reach in the work that won him his degree in botany from the Sorbonne (1912) and his doctorate from the University of Paris (18 Nov. 1913). The library at the Museu Nacional has a copy of his thesis, dedicated to Adolpho Lutz. Born in San Marcos de Nicaragua on 17 April 1887, Clodomiro Picado Twilight graduated from Liceo de Costa Rica in 1906. From 1907 to 1908, he taught natural sciences at Colégio San Luis Gonzaga and published a number of articles about the wildlife of Costa Rica. After winning a scholarship from Congress at the request of his teachers, he traveled to Europe in October 1908. He returned to his country in 1910, when an earthquake hit Cartago. During his stay in Costa Rica, he collected material for his doctoral thesis. In March 1911, he returned to France with drawings, photographs, and biological material. In 1912, the Comptes Rendus de l’Academia des Sciences published three articles about bromeliads.

As well as giving Bouvier, Calvert, and Champion special mention, Peyerimhoff and Scott also mention Knab for the information given to Picado about bromeliad fauna. They list seven articles written by Knab (1904, 1905, 1912a, 1912b, 1913a, 1913b, 1913c).

The previous year, Cândido Gafré had asked Oswaldo Cruz, Director General of public health and director of the Manguiñhos Serum Therapy Institute, to recommend a doctor capable of preventing malaria in Itatinga, where the Companhia Docas de Santos was building a hydroelectric dam whose construction was almost at a standstill because of malaria. Oswaldo Cruz appointed Chagas, who established procedures that came to be adopted in other campaigns against malaria. Most importantly, he found out that the only way to stop the disease from spreading in regions where sanitation actions were unfeasible was to focus preventive measures in the housing where the infected people and mosquitoes lived. For more on this, see Chagas Filho, 1993; Albuquerque et. al., 1991; Benchimol, 1990.

This rule states that the width of the head capsule of Lepidoptera larvae follows a geometrically regular progression in its successive instars.

Lutz was not mentioned in this communiqué.

Knab had already published an article about this (Proceedings of the Entomological Society of Washington, v.XIII, p.71, 1911), showing that triatoma was notable among American members of the genus, due to its close adaptation to man.

In his following article (Knab, 1913a), he states what very similar line of thought it was that had led Grassi to his great discovery, “which was not, as is generally thought, that Anopheles transmits malaria, but that specific species do so.” The Italian parasitologist had started from the theory that the blood-sucking insect responsible for the transmission must be distributed along the same lines as the disease. Gadelha (1994) states that Grassi’s ideas about the specificity of the relationship between anophelae and human plasmodium were taken by Knab, who then extrapolated the same relationship for the different anophelae and human hosts of the parasites.

Dyar’s words: “At the time of this investigation, it was not known to what degree of specialization the relationship with malaria had reached. It was thought that human malaria was simply transmitted by Anopheles as opposed to other mosquitoes. Lutz’s conclusion at the time was therefore natural and plausible. But now we know that the malaria relationship is highly specialized. Each type of malaria is generally carried by one or two species of Anopheles in a location. We often find different species of Anopheles in a given place, but only one is capable of transmitting the form of malaria prevalent there. Mr. Knab has shown that for such a delicate relationship to have been established, one would need for it to have been preceded by a habitual association to the vertebrate host and the mosquito – in other words, only a domestic or semi-domestic Anopheles is capable of operating as a malaria transmitter (Knab, 1913a).” In Knab, “The contentions regarding ‘forest malaria’,” Proceedings of the Entomological Society of Washington (v.XV, part 2, p.110-8, 1913).
American historians. Stepan (op. cit., p.421) contrasts these errors with judgments of Finlay's incompetence made by North (p.106-9) shows that it was written based on combining Durham and Myers' article with Lazear's field notes. Respectively.

Repeated twice, the first time with three volunteers and then with two, for twenty-one and eighteen nights, respectively.

70). He wanted to know the number of patients treated at São Sebastião to "prove that the experiments with the spirit" of Lutz's counter-rebuttal. "As it was, the letter very appropriately put an end to a controversy in which the arguments had been exhausted on both sides; now we only have a weak statement. To sum up, Dr. Lutz's comments were emasculated."

Genus created by Theobald in 1905 and considered by Dyar to be a subgenus of Anopheles in the 1920s. This entomologist classified all the bromeliad species identified as a separate group by Knab in 1913 as Kerteszio (Zavortink, 1973, p.4; Knab, 1913b).

"Relatório de 1900" (BR. MN. Fundo Adolpho Lutz).

The preliminary note was published in the Philadelphia Medical Journal (27 Oct. 1900). Delaporte (op. cit., p.106-9) shows that it was written based on combining Durham and Myers' article with Lazear's field notes. Stepan (op. cit., p.421) contrasts these errors with judgments of Finlay's incompetence made by North American historians.

According to Peller (op. cit., p.200), between 5 Dec. 1900 and 7 Feb. 1901, two series of experiments were conducted. In the first, ten of the twelve inoculated volunteers fell ill; in the second, four out of five. In both, Finlay made the clinical diagnosis. When the dispute over priority emerged, the main argument challenging his merit came from these experiments. His error consisted of adjusting the experimental inoculations to the time it took the mosquito to have a new meal of blood, and not to the period of maturation of the 'virus'. He had used mosquitoes that had only been contaminated two to six days earlier, relying on the observation that it took two to three days to digest the blood and prompt the mosquito to search for more food. Eight volunteers bitten by mosquitoes that had been contaminated less than twelve days earlier stayed healthy. The twelfth day (i.e., the same period Ross had established for malaria) would be the dividing line, distinguishing successful experiments from failures.

According to Peller, this experiment, carried out between 30 November and 19 December 1900, was repeated twice, the first time with three volunteers and then with two, for twenty-one and eighteen nights, respectively.

This is, in fact, the central topic of Peller's article: to redress the injustice committed by U.S. historians and by Reed himself, who omitted the work of S. Firth, to whom full credit should be given for discovering that yellow fever was not transmitted by fomites; the commission had employed the same methodology as its precursor (see also Harvay, 1981).

Although a healthy volunteer had been contaminated with filtered serum from a sick man, according to criteria established by Löffler and Frosch, only serial transmission would prove that the etiological agent was an ultramicroscopic virus. An isolated case of transmission did not exclude the action of toxins secreted by a bacterium (Löwy, 1991).

On 14 April 1898, João Batista de Lacerda criticized the communication that Caldas had submitted to the National Academy of Medicine in May of the previous year and to the Pasteur Institute. Carlos Seidl, who had conducted clinical testing, had to justify the serum's hasty adoption at São Sebastião Hospital. Rocha Faria, president of the commission formed to verify it, proposed that the Academy issue a clear verdict in order to protect its scientific reputation, shaken by the report of the bacteriologists from São Paulo. In an article published in O Brazil-Medico (1 May 1898, p.145), Seidl once more stated that empiricism was not an obstacle for clinical experimentation involving diseases of dubious or obscure etiology. "Did not Richet and Hericourt, the founders of serum therapy, dedicate themselves to exploring this new system of treatment on diseases such as cancer and syphilis? So too did Emmerich, Stoll, Roger, Blecere, Thomaselli, Istamanoff, Fournier, Gilbert, Auché, Laudmann, Pellizar, Carraquilla, etc., etc., with cancer, smallpox, scarlet fever, syphilis, acute joint rheumatism, whooping cough, leprosy, etc. Even without being specific, Caldas' serum could have ‘the virtues inherent to the simple horse serum.'" Seidl was criticized by Abel Parente (Brazil-Medico, 15 May 1898, p.169-70). He wanted to know the number of patients treated at São Sebastião to "prove that the experiments with the Caldas serum ... were fatal to the patient." Seidl's irate response can be found in Brazil-Medico (1 Jun. 1898, p.187-8).


Brazil-Medico (6 Dec. 1901, p.456-7). Transcript of a letter sent from Cuba, on 2 September, by V. Harvard; also published in Medical News (14 Sep. 1901). The commission’s report was originally published in Havana, in Revista de Medicina Tropical (no. 9, Sep. 1901).

They also asked him if he had proof of the vaccine's efficacy. Caldas said that in the last two years he had vaccinated 23 students in Rio de Janeiro; as far as the serum was concerned, he claimed to have cured a ratio of 85% of his subjects in Brazil and Mexico. A little earlier, Matienzo had presented results to the Mexican Health Council regarding experiments carried out in Veracruz with a healing and preventive serum prepared at the New York City Department of Health, in collaboration with Dr. Baker, of whom we will speak later (Revista Médica de São Paulo, 1900, p.33).
The Caldas affair continued until 1902. Such was his confidence in his vaccine that he inoculated two of his own children, one of whom died. For more on this topic, see Brazil-Médico (15 Oct. 1901, p.387; 8 Jul. 1901, p.256-7; 8 Dec. 1901, p.457; 15 Feb. 1902, p.86-7) and Revista Médica de S. Paulo (1902, p.74).

This expedition resulted in Durham and Myers, 1902.

Another researcher from Liverpool ended his career in the Amazon. Dr. Harold Howard Shearman Wolfster Thomas passed away in Manaus, on 8 May 1931, after spending twenty years with the Yellow Fever Research Laboratory. Before starting the school’s fifteenth overseas expedition in April 1905, along with Anton Breini, he studied trypanosomiasis in Africa and in 1904 verified the therapeutic value of atoxyl, the first substance found capable of blocking the action of this species of protozoa in animals. In the Amazon, the objective was also to investigate yellow fever, which both of them caught. Breini returned to England, while Thomas remained until 1909, publishing in the annals of the institution “Yellow Fever” and “The sanitary conditions and diseases prevailing in Manaus, North Brazil, 1905-1909” (1910). He returned to the Amazon in 1910 and was only to leave the region one last time, in order to obtain research funds and hire three assistants (Miller, 1998, p.34-40; Smith, 1993).

BR. MN. Fundo Adolpho Lutz, pasta 267, maço 2.

BR. MN. Fundo Adolpho Lutz, pasta 267, maço 2.

In the description of Culex taeniatus (name given by Wildeman in 1828), the following were indicated as synonymous species: Culex taeniatus Meigen; C. mosquito Robineau-Desvoidy and Lynch-Arribálzaga (Cuba and Buenos Aires); C. frater Robineau-Desvoidy; C. calopus Hoffmannsegg (Portugal); C. elegans Ficalbi (southern Italy); C. vittatum Bigot (Corsica); and C. Rossi Gies (India).


Lemos (1954, p.65-7) states it was written on 20 February 1903, but its authors report on events that took place on the 24th.

They were Paschoal Ceraballo and Francisco Ceraballo, from São José do Rio Pardo; having taken ill in the capital city, they were transferred to the Isolation Hospital, where the former passed away on 18 February and the latter on 23 February of the same year.

The Public Health and Marine Hospital Service had many duties: superintendence of navy hospitals and disinfection stations, of inland, island, and overseas quarantines, and of immigrant inspection, plus compilation of statistics. It was also in charge of organizing conferences where state sanitary authorities would settle public health issues together, at least once a year. The division had a subsection for scientific research and a Yellow Fever Institute (Oswaldo Cruz, “Prophylaxia da febre amarela,” Arch. f. Schiffs- u. Tropenhyg. (1899), vol. 5, fasc. 3, p.356-506) under the title “Studien über Gelbfieber in Brasilien” (Studien über Gelbstudien; (name given by Wildeman in 1828), the following were indicated as synonymous species: Culex taeniatus Meigen; C. mosquito Robineau-Desvoidy and Lynch-Arribálzaga (Cuba and Buenos Aires); C. frater Robineau-Desvoidy; C. calopus Hoffmannsegg (Portugal); C. elegans Ficalbi (southern Italy); C. vittatum Bigot (Corsica); and C. Rossi Gies (India).


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139 Entitled “Retour du Brésil. La fièvre jaune vaincue,” with the subtitle “Comment les docteurs Marchoux et Simond ont vaincus un des plus terribles fléaux qui dévastent nos colonies” (Archives de l’Institut Pasteur, Fund Simon: SIM 22, Pl. Simond Presse). According to Trans (1959, p. 67), this discovery remained in limbo until it was confirmed in the 1970s, thanks to modern immunofluorescence: a U.S. team found the La Crosse virus in larvae of Aedes triseriatus (1973); three years later a French team in Senegal demonstrated the transovarian transmission of a flavivirus, the Koutango, in Aedes aegypti. In 1980, it was verified that the yellow fever virus could also be transmitted in the same manner, making it possible to detect the presence of viral antigens in the bodies of mosquitoes.

140 In 1903, Oswaldo Cruz commented: “If we set aside the fact that they found a protozoan in the body of the mosquitoes … for it is still too early to reach a conclusion on this microorganism’s etiological role, this conclusion brought us two important results: The first is that the mosquito Stegomyia fasciata that had, twenty-two days earlier, bitten a victim of yellow fever on the second day of the disease produced “a serious accumulation of the disease” when it was made to bite a non-immune subject; the second is that protecting yellow fever sufferers from mosquitoes is certain to stop the disease’s dissemination.” Dr. John W. Ross, U.S.N., Chairman, Washington, D.C., comments on the work of these doctors as well as on Finlay’s in his “Report of the Committee on the Etiology of Yellow Fever,” source: etext.lib.virginia.edu/etcbin/fever-browse?iid=02708001

141 According to Tran (1998, p. 56-7), even at a distance Roux exerted some influence over the operations. He was considered by all three to be their scientific mentor, and his relationship with this commission was similar to that of Sternberg with the Reed Commission in Cuba.

142 Born in 1871, Schaudinn died prematurely in 1905, at the age of 35. His short scientific career was productive: in addition to amoebae, he studied the evolution of plasmodia in the digestive tract of anophelines and in human blood; he formulated the long-lasting hypothesis of regressive schizogony (division of female gametes into a certain number of merozoites) to explain the frequent relapses of victims of paludism; and he created the genus Treponema (Kruif, 1945).

143 Blood and viscera analyses led nowhere and attempts at transmitting the disease to monkeys were a fiasco. The parasite discovered in the mosquitoes was very common and wholly unrelated to yellow fever.

144 In the Pasteur Institute archives, Fund Simon, there are six “cahiers d’expérience” (SIM 10), one of which bears the title “individus payés” (37 p.).

145 Archive de l’Institut Pasteur, Fond Simon, SIM 9 (1901-05).

146 Roux had already suggested this in March 1902. At the time, the Pasteur Institute was on the cutting edge of vaccine and serum research. One of Simond’s notebooks tells of these experiments. Better than the official version that appears in the Annals of the Pasteur Institute (Nov. 1903), this document shows the difficulties they encountered in carrying out and interpreting these experiments.


148 Mendonça was also clinical head at Santa Casa de Misericórdia Hospital and president of the Associação Médica Beneficente, founded in 1902. He died in 1915 (Almeida, 2003, p. 218-20). Antunes (1992, p. 54) refers to another, non-documented motive for differences with Lutz: Mendonça was against appointing Vital Brasil to head the recently created Butantan Serum Therapy Institute.

149 Hired on 1 January 1902 as an assistant at the Bacteriological Institute, he had taken part in commissions related to the plague and paludism, and had helped Lutz to collect mosquitoes in Rio de Janeiro in 1902. On 31 January 1903, Arthur Palmeira Ripper replaced him.

150 Bandi, in V Congresso (v. 2, p. 9-18). Pereira (Brazil-Medico, 22 Nov. 1903, 1-8-15.12.1903); V Congresso (v. 2, p. 113-43). For the doctor from the state of Bahia, for example, the history of yellow fever epidemics was proof that the disease was imported. The mosquito was its most active agent of dissemination but most likely not the only one. Perhaps other insects could pick up the disease’s germs off contaminated clothes and objects, inoculating them through bites. Prophylaxis should be a combination of mosquito fighting and the disinfection of contaminated homes and objects.

151 Ribas (Brazil-Medico, 22 Sep. 1903, 1-8-15.10.1903). See also, V Congresso (v. 2, p. 57-110).

152 Nägeli was born in Kilchberg, Switzerland, and graduated in medicine in Zurich in 1836. He obtained his certification in 1843 after having worked with the botanist August Pyramus de Candolle in Genf and at Manfred Schleiden’s laboratory. In 1849, he became adjunct professor at the University of Zurich and moved in 1852 to the University of Freiburg im Breisgau, teaching botany at both universities. In 1858 he was hired by the University of Munich, where he researched the process of division in pollen grains and in unicellular algae. He was noted for his studies on cytology and plant development, having discriminated the fern antheridium and spermatozoids. He died in Munich in 1891. www.answers.com/topic/karl-wilhelm-von-nageli and home.tilscalinet.ch/biografien/biografien/nageli.htm.
Although attributed to Pasteur, priority in discovery of the cause of the silkworm disease was controversial. Pierre Jacques Antoine Bechamp, professor of medical chemistry and pharmacy at the University of Montpelier’s School of Science, was already searching for the cause before Pasteur was commissioned by the government. He stated in the first months of 1865 that it was caused by a “small parasite” and presented his research results at the Agriculture Society of Hérault. He also suggested the use of creosote vapor to fight the microorganism. His studies did not have great repercussion because that same year the government commissioned Louis Pasteur to investigate the disease, and all attention was then centered on his work. In September 1865, he stated that the disease was not caused by a microorganism. A dissenting Bechamp rose to the defense of his own research and sent a paper to the Academy of Sciences, where he restated the parasite etiology. The discussion continued until 1867, when Pasteur finally confirmed Bechamp’s theory, with the aid of his assistants Désiré Gernez (1834-1910) and Eugène Maillot (1841-89). The discovery of the cause of pebrine thus became linked with Pasteur, and Bechamp’s earlier work was forgotten, his continued efforts to claim priority notwithstanding.

Nevertheless, many authors still consider microsporidia a group within Protozoa. See D. Lipscomb, Protozoa, in biology.usgs.gov/s+t/frame/m2083.htm

Alphonse Splendore was born in the province of Cosenza in southern Italy in 1871. He studied in Cosenza, Naples, and Rome, obtaining his medical diploma in Rome in 1897. He was licensed to practice medicine in Brazil by the School of Medicine of Rio de Janeiro, which took into consideration only the weight of his previous work. In São Paulo his work included many papers on protozoology and mycology, many in collaboration with Adolpho Lutz. He returned to Italy in 1912 but in 1920 was back in São Paulo, where he remained until his death in April of 1953. (www.whonamedit.com/doctor.cfm;1534.html and www.area.cs.cnr.it/imseb/malaria/grassi/splend/).


Brother of Alfonso Splendore, Achille Splendore was noted for his work on tobacco raising. www.area.cs.cnr.it/ imseb/malaria/grassi/splen.

This fact is mentioned in Medical Dictionary –www.dictionarybarn.com/Myxococcidiumstegomyia.

BR. MN. Fundo Adolpho Lutz, pasta 157, maço 44.

BR. MN. Fundo Adolpho Lutz, pasta 157, maço 44.


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