Louis Westenra
Sambon
Pioneer of Tropical Medicine

GERALD HUGO RÉE
Louis Westenra Sambon; Pioneer of Tropical Medicine

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Introduction and Acknowledgements

2016 was the 150th anniversary of the birth of Louis Westenra Sambon, a pioneer of the medical speciality which became known as “tropical medicine.” On several occasions during my researches into his life, my wife asked me what was so special about the man, a question it was not always easy to answer. Modern textbooks of tropical medicine barely mention his name, there are no memorials to him at the London School of Hygiene and Tropical Medicine, where he spent much of his professional life, and, effectively, his name is largely forgotten. Yet he was clearly not unimportant. He received a small mention in Gottlieb von Olpp’s *Eminent Tropical Doctors in Words and Pictures* of 1932.¹

I have to admit, I felt an affinity for Louis. Like him I am of mixed European/English heritage, and like him, I was a lecturer at the London School, and I was, at the time, bilingual in French and English. Sambon, of course, went one better, for he also spoke Italian. Like Louis, I was also the father of three daughters, and, belatedly, a son. During a short spell in my career I was actively associated with the Seamen’s Hospital Society, which played such a large part in the establishment of the London School of Hygiene and Tropical Medicine. There, the similarities cease. I have no parasites named after me (there are several which bear the name Sambon, including a sub-order of tongue worms known as *Sambonia*) and I never offered outrageous (to some minds) hypotheses to explain the causes of disease.

There was, however, something appealing about Sambon. His ‘best’ friend in London, and his most consistent supporter when others were attacking him, was Sir Patrick Manson, the most important name in tropical medicine at the beginning of the twentieth century. It is not clear what Manson saw in the small figure of Sambon, but what he saw impressed him, and he was prepared to give Sambon a go. At the same time, there were some in the very British
world of tropical medicine at the time that disliked Sambon, and made it clear. Yet, he battled on, but was finally defeated by a conviction about the cause of pellagra, which led to his eventual downfall. This is a sad story about a man who started out with a bright future, but one which never really materialised.

It also seemed to me that, though there are numerous biographies of the great men of medicine, there are few of those who never made it to the very top of their profession, yet who made significant contributions. Sometimes, the giants have to stand on the shoulders of lesser men to reach the heights of their professions. So this little work aims to not only say something about L. W. Sambon, but to ask the question, why did he ultimately fail?

Because Sambon worked in England, and I live in Australia, I had to rely on a large number of organisations and individuals in order to complete this story. Chief among these was the Wellcome Library, which provided large numbers of photocopies of documents relating to Sambon. I would particularly like to mention Crestina Forcina, and, from the imaging department, Richard Keenan, who did his utmost to keep my expenses down to a reasonable amount. I would also like to thank Wellcome Images for access to their wonderful library of images relating to tropical medicine, most of which are made freely available.

The archives of the London School of Hygiene and Tropical Medicine were a rich source of material. Aisling O’Mally, assistant archivist, was particularly helpful, and without her assistance, I would have been lost.

The archivists at the Natural History Museum and the British Museum provided all the help I needed. The authorities of the Mauritshuis Museum in The Hague allowed me the use of Dr Tulp’s Anatomy Lesson. The Fondazione Musei Civici di Venezia gave me permission to use the painting, La Polenta, by Pietro Longhi.
The staff of the Royal Society of Tropical Medicine and Hygiene in London willingly provided me with reprints of articles of relevance. Figure 11.7 is taken from Gottlieb von Olpp’s book. The copyright of this book seems to have lapsed following political changes in pre-war Germany. If I have offended anybody, I apologize now.

Much of Sambon’s early life was spent in Naples. Unfortunately, during the Second World War Naples suffered from heavy allied bombing, which led to the loss of numerous archives, including the military records for the region. Some gaps in the biography are inevitable.

Among individuals I would particularly like to thank Margaret Shanks, who translated Italian documents for me, as well as assiduously searching the Italian language internet for items of interest or value to my story. She also did her best to chase up the copyright issues for von Olpp’s book. My brother-in-law, John Mills, a keen genealogist, gave me considerable assistance with that subject. Dr Marion Woods, Consultant Physician in Infectious Diseases at the Royal Brisbane and Women’s Hospital managed to find obscure references for me with no difficulty. Professor David Molyneux of the Liverpool School of Tropical Medicine allowed me to use images from Dr Dutton’s archives. My very patient editor, Robin Adams, checked, revised and edited the work as it was progressing, as well as making numerous excellent suggestions and amendments. Without him I would never have made any progress. Last but not least my wife only occasionally grumbled at the amount of time I was spending on the computer!
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For my grandson Luke Cassidy
Chapter 1. Beginnings

On 20 January 1910, Herbert James Read, a senior official in the Colonial Office in London, wrote to Professor Ronald Ross, the Nobel Laureate who had earlier demonstrated that mosquitoes transmitted malaria, about a possible British investigation into the disease pellagra. He wanted Louis Westenra Sambon, a lecturer at the London School of Tropical Medicine, to undertake the field work in Italy. Some years earlier, Sambon had proposed an unusual hypothesis for the epidemiology and aetiology of this disease (see chapter 8).

Read however added, “I think that someone else ought to go with him, as he is rather an erratic genius.” What prompted Read to make this rather ambiguous remark he does not say. That Sambon was a genius was, however, endorsed by Sir Patrick Manson, “the Father of Tropical Medicine,” who wrote to Ross of Sambon that “although he has many of the faults of genius, he is a genius…” He too refrained from saying what those faults were, but he did add, “He is a magnificent worker, with ideas ahead of his time, & perhaps that’s the reason, or one of the reasons, why so many affect to sneer at him.”

Another reason might have been the mixed nature of Sambon’s roots. He had French, Italian and English blood—and culture—in him. This was, after all, an age when international competition in the field of tropical medicine was intense, and the Victorian Briton had to cling ever more tightly to his sense of superiority over other peoples of the world.

Louis Westenra Sambon’s name appears rarely in the literature and historiography of early Tropical Medicine, even though he was much involved in the speciality, and made a number of significant contributions. He is perhaps best known for the things he got wrong, and there were several of
these, the most unfortunate of which was his pellagra hypothesis. Yet some of his insights were deeply prophetic. He deserves a biography, which should start with his French grandfather, also Louis Sambon, a French diplomat who left France for unstated political reasons in about 1830. Like many other Frenchmen had done between 1793 and 1860, he settled in Naples, then the capital of the Kingdom of the Two Sicilies, There were many reasons for this French exodus, some were desperately escaping the French Revolution, some arriving with the Napoleonic occupation of the Mezzogiorno between 1806 and 1815, and some moving for other economic, social or political reasons. The French Neapolitans were notably different from the mostly transient British, who came to Naples to escape the cold British winters, had no desire to integrate into the Neapolitan way of life, and formed a self-helping and supportive “community”. The French were quietly absorbed into the urban fabric of Naples.

In keeping with this cosmopolitan philosophy, Louis married an Italian girl, Anna de Cesare, a native of Bari. He developed an interest in, and collection of, the Greek and Roman coins of Italy, publishing two influential books on the subject. His son, Giulio (Jules) was born in 1837. Giulio became an eminent antiquarian, classical scholar and occasional warrior with Garibaldi in Sicily. He displayed great bravery at the Battle of Milazzo, (20 July, 1860), where victory allowed Garibaldi free access to Messina and then the Italian mainland. Giulio travelled widely. During a visit to the British Isles, he met Laura Elizabeth Day, who was said to be a relative of Charles Dickens and a descendant of the explorer Vitus Bering. Her father Robert was a wine merchant in Barnes. Giulio and Laura were married in the parish church on 24 June 1865. Two sons were born of the union: Louis (Luigi) Westenra, born in 1866 in Milan and Arthur (Arturo), born in 1867, in Portici, eight kilometres south-east of Naples at the foot of Mt Vesuvius and next to Ercolano, the Roman Herculaneum. Arthur, following in his father’s
footsteps, became an eminent numismatist and antiquarian. Despite their Italian roots, both Giulio and Arturo considered themselves French. Louis Westenra’s cultural status was more ambiguous.

Details of the boys’ childhood are scant. Their cosmopolitan family meant that both became fluent in English and French, as well as Italian. Louis spent most of his boyhood in Naples, though he travelled often:

“As an English physician and geologist, Professor Johnstone-Lavis, first taught me to read in the great stone-book. With him I studied the geology of the Campania, collected Appenninic fossils, mapped the lava streams of Mount Somma, and watched the Vesuvian crater belch forth steam and lava. The Naples Aquarium and the London Zoological Gardens, the National Museum of Naples and the British Museum were, in turn, my chief places of instruction and amusement. I owe to the late Professor Gasco the interest I take in all things zoological, to my father my love for art and archaeology, to my mother the cult of all that is good. I used to play with early Roman coins and terracotta lamps as other boys play with marbles and tops...Lizards, snakes, spiders and soldier crabs were my usual pets. I knew how the wall lizard regenerated her brittle tail, how the four-rayed snake swallowed rats and frogs...I spent several summers fishing in Mediterranean waters, others climbing Swiss mountains. In 1878, I went with my parents to the Paris world-fair, and there spent some enchanted moments of rioting boyish imagination within the great copper head of Bartholdi’s statue of Liberty, which today stands on Bedloe’s Island, in New York Harbour.”

If later his mentor Sir Patrick Manson said that Sambon was different from other men, it would be equally true to say that he was, precociously, different from other boys. His school years were spent at Hoddesdon (Hertfordshire) Grammar School, the College Gaillard in Lausanne, and the Liceo Umberto in Naples. In 1884 he went from the Liceo to the University of Naples, to study medicine.
Naples, at that time still the most populous city in Italy, was in a state of crisis. Following the unification of Italy in 1861 the fortunes of Naples had suffered as a result of the loss of its status as a royal capital, the flight of money to Rome, new free trade policies instituted by the Liberal government of Italy, and regressive tax policies.14

Naples was divided topographically and socially into two parts: on the west side lay the less populous and more affluent upper city; while the lower city, to the east and south suffered from gross overcrowding, high unemployment rates, malnutrition, a polluted water supply, inadequate sewerage, and other environmental ills. Improvement was frustrated by corruption due to organised crime; the Camorra claimed to look after the poor, but only enriched themselves.15

There had already been four major outbreaks of Asiatic cholera16 in the city (1836-7, 1854-5, 1865-6 and 1873) but the city authorities had learned little from earlier experience and were ill prepared for any new visitations of the disease. Though Robert Koch had announced cholera’s bacterial cause (Vibrio cholerae) in 1884, neither the Naples city fathers nor some in the Neapolitan medical profession agreed with him. A devastating cholera epidemic which started on 1 September, 1884 and continued through to the end of October led to the death of almost 8,000 people, the great majority from the lower city.17

The cholera outbreak caused considerable national and international concern. For political, religious, humanitarian, commercial or professional reasons, many people volunteered to help in the epidemic.18 Louis Sambon junior was one of them. It was perhaps this that stimulated his interest in exotic diseases. For his efforts, he was awarded a bronze medal, “the Public Health Award of Merit.” The Gazette described him as “assistente medico.”19 Regrettably, his written account of the cholera epidemic appears to be lost, though he became
convincing that house-flies, which he had seen feeding on patients’ excretions, might somehow be dangerous germ-carriers.

Henry Johnston-Lavis (fig. 1.1), ten years Sambon’s senior, was an English doctor, trained at University College, London. Johnston-Lavis looked after the health of the English and American populations of Naples, presumably including Sambon’s mother Laura. Though the major cholera outbreak had subsided by the end of 1884, recurrences of the disease were recorded in 1886 and 1887. The latter outbreak affected particularly the town of Pozzuoli, about eleven kilometres west of Naples.
Of Johnson-Lavis’ efforts in Pozzuoli, Sambon wrote,

“Lavis himself attended the stricken foreign employees\textsuperscript{21} and took all necessary sanitary measures for the protection of the Italian workmen. I know that he was dreadfully afraid of the Gangetic disease, insidious, swift and deadly like the very cobra, but no-one would have thought it, seeing with what apparent contempt of life he laboured day and night, often without food, always tired out, to save others. When his Italian colleague contracted the infection, he came to me. I shall never forget, he looked ghastly, and I promised to go that very night on condition that he would take some rest.”\textsuperscript{22}

Sambon was twenty-one at the time. His flamboyant style of writing and his self-confidence are already apparent. The following year, Sambon enrolled as a medical student at St Bartholomew’s Hospital in London.\textsuperscript{23} Why he did so is not clear. Apart from his enrolment, there are no other details of his time at Bart’s, but it was to be a short stay. In April 1889, he was back in Naples, helping Johnston-Lavis—who later wrote of Sambon as his friend and student—conduct a party from the Geologists’ Association through the Lipari (Aeolian) Islands.\textsuperscript{24} (Lavis says that Sambon also assisted him in April 1906, when both were “nearly overwhelmed” by the eruption of Vesuvius.)\textsuperscript{25}

Sambon was described by the petrologist Joseph Paxson Iddings (1857-1920) as “an agreeable companion, whose cheerful spirits brought out the friendly side of the Italian nature.”\textsuperscript{26}

Despite all the disturbances in his life, Sambon found time for study. In 1891 he presented his thesis, “\textit{The Parasitic Cestodes of Man}.” At about this time, he was appointed Chief Secretary of the Società Americana d’Italia. In the society’s year book, he published his \textit{Intertropical Zones of America (Comparative Climatology and Pathology.)}\textsuperscript{27} His interests were already apparent.
The Seventh International Congress of Hygiene and Demography was held in London in August 1891. Manson presented a paper on filariasis. Sambon, still only twenty-five years old and recently qualified, presented a paper on “Measures for preventing the spread of infectious diseases”, based on his cholera experience. It was here that Sambon first started to admire Manson, who would later have a great influence on his life.

On his return from London, Sambon joined the Italian Medical Staff Corps. It seems likely that he volunteered, an option open to the wealthy. By paying a fee the volunteers served for a shorter period than the three years the conscripted soldiers served.

Fig. 1.2 The Caserta Military Hospital. Courtesy of Luca Borghi,  www.himetop.wikidot.com
He served in Caserta, some forty kilometres north of Naples, probably at the Military Hospital (fig. 1.2). His linguistic skills quickly brought him to notice. He went on a number of missions for the army, including Cologne during an outbreak of cholera, and, in late 1892, a visit to Britain to inquire into the British Army medical services. He found much to admire, especially “our army’s admirable systems of hygiene”, but was apparently critical of the conditions under which the medical men had to work, and the lack of time and opportunity for study. He received two medals for his services (fig 1.3), and throughout his life, remained immensely proud of his military record.

Following his discharge, he moved to Rome in November 1893, where he established himself as a gynaecologist. The work clearly did not enthrall him. Despite this, and perhaps as a point of entry into the British medical
establishment, he successfully applied in March 1895 for a fellowship of the British Gynaecological Society. His address at the time was given as Rome.

Rome was the venue for the Eleventh International Medical Congress. It was to have been held in September 1893. The Italian Government invited Louis and his brother Arturo to put together a concurrent exhibition of medical antiquities for an “Esposizione d’Igiene”. The start of the Congress had to be put off to March 1894 because of scattered outbreaks of cholera in Europe. The numbers of delegates far exceeded the expectations of the organizing committees. The Tablet, a Roman Catholic paper published in England, was scathing, writing of the “ne plus ultra of chaos and confusion” that prevailed in all the arrangements (except, mirabile dictum, those made by the Irish College to entertain the Irish delegates.), and used the occasion to attack the Italian Liberal Government of Francesco Crispi, no friend of the Vatican.

Professor Rodolfo Lanciani gave a talk on “Archaeology and Medicine in Rome”, which managed to praise the Esposizione d’Igiene highly, while not crediting the efforts of the Sambon brothers by name for organizing it. Many of the antiquities from the Esposizione d’Igiene were later bought by the collector Henry Oppenheimer who showed them at the British Medical Association annual meeting held at the Savoy Hotel in London at the end of July, 1895. The exhibition was much admired. Sambon’s extensive description of the items was published in the British Medical Journal.

The Sambons claimed priority for their demonstration that many of the small terracotta votive offerings (‘donaria’) were not, as had been previously thought, offerings of fruit or such like for the shrine, but represented the body parts where the offerors hoped for relief (fig. 1.4).

Like many Italians of his era, Sambon dreamed of an Italian colonial empire in the Horn of Africa. A small step had been taken in 1890 with the
establishment of a colony in Eritrea. Eritrea bordered Ethiopia (Abyssinia), and Italians cast covetous eyes on that country. In 1888, Sambon had published a brief article on Abyssinian weapons. Two years later he published a further article, *Military Ethiopia*. In 1896, he followed this up with a small monograph on the Ethiopian Army. *The Abyssinian Army, Customs and Traditions*, seems to have been the result of considerable research, though in libraries, not in the field. His book probably underestimated the strength and materiel of the Ethiopian army. In this it reflected current Italian thinking; but it might or might not have circulated in time to contribute to the sense of superiority that, on 1 March, 1896, led the Italian army to defeat at the battle of Adwa. Adwa led to Crispi's resignation. Political chaos ensued.

Fig. 1.4 ‘Donarium’ showing a septate uterus. Courtesy of Wellcome Images.
Between 1894 and 1896, Sambon seems to have travelled on a number of occasions between Rome and London, before finally deciding, in late 1896, that London would be his home. Among the likely reasons for this decision were politics, family ties, and a feeling that the British, or at the very least the British establishment, which refrained from intervening to prevent the Italian forays in Africa,\textsuperscript{37} might receive him with open arms. Furthermore, he was working on a paper which he believed might be of interest to a British medical journal.
Chapter 2. Manson, Climate and Disease

It had been an axiom of European (and later American) thought since at least the seventeenth century that when European residents in the tropics suffered ill health, climate was to blame: and this, although the more likely causes were chronic infections (such as malaria, hookworms and intestinal disorders), excess of alcohol, and homesickness. Successive generations born in the torrid zones were assumed to be at risk of losing their racial heritage: acclimatisation led to more “native” physical attributes, as did intermarriage with native races.

Until the general acceptance of germ theory, physicians’ notions of aetiology did not extend much beyond miasmata and humoralism. Miasmata were the bad smells that caused disease. In humoralism, the body’s well-being depended on the balance of its constituent ‘humours.’ External temperature, either high or low, was a constant threat to humoral balance. The lethargy and lack of mental acuity attributed to natives of the torrid zones was seen as a direct effect of the climate, contrasting sharply with the mental agility and physical strength of those born in the cooler, bracing atmospheres of northern climes.¹

In January 1871, Senator Carl Schurz of Missouri, objecting to the treaty which would have annexed the island of Santo Domingo by the United States, stated that “here in our northern air, we stand in our strength,” and asserted that the annexation of a tropical island would have a disastrous effect on the morals and democratic institutions of America. These negative effects were entirely due to the climate and the effects of the sun’s rays.²

Sir Joseph Fayrer, FRS, a physician with more than twenty years’ experience of life and death in India told an audience of young engineers about to set off
for India in 1880 that direct exposure to the sun could induce “heat asphyxia, sunstroke, ardent fever, or other evils of a more insidious character, by injuring the nervous system, increasing irritability, depressing vital energy, and affecting the internal organs, especially the liver…” Recognizing that sometimes people were their own worst enemy, he also spoke of an Irish soldier who said of his comrades that “they eat and they drink, and they drink and they eat till they die and then they write home and say it was the climate that killed them.”

More hazardous than high temperatures was a sudden drop in temperature. This could give rise to fever, chills, dysentery, liver disorders and rheumatism. The cholera belt, a flannel strip worn around the abdomen to ward off the sudden chills that could lead to intestinal disorder, was an essential piece of clothing for residents of the tropics. If the abdomen was to be kept warm it was equally important that the head should be kept cool, by a straw hat or the more popular pith helmet, the sola topee. The sun damaged the nervous system, leading to the enervation seen so commonly in white settlers who had spent too much time in the tropics. Daniel Henry Cullimore, a retired Indian Army surgeon, stated categorically in 1890 that anaemia was a direct effect of the climate of the tropics, attributable to want of oxygen in “the rarefied, more moisture laden and diluted air of the tropics.”

Furthermore,

“Owing to lessened sanguification, the result of the reduction of blood in the chest and the accumulation of blood in the abdominal region and the portal veins, and its pollution by retained biliary and kidney excretions, and the impairment of digestion, anaemia is set up.”

Regrettably, Cullimore references only himself as the source for this piece of physiological galimatias.

The scramble for Africa by European nations, between approximately 1880 and 1914, led to an increasing interest in the effects of climate on human
health, in which the Americans joined after the American Spanish War had led them to occupy tropical areas, including Cuba and the Philippines.

Sambon studied the data on climate and health in detail. In January 1897, the *British Medical Journal* published his “Remarks on the Possibility of the Acclimatisation of Europeans in Tropical Regions.” Sambon hypothesized that it was not the heat *per se* that made the tropics uninhabitable for the white man, but rather the white man’s susceptibility to parasites. Anaemia was not due to heat, any more than liver abscess was. He concluded that the white man could live and prosper in the tropics; that the problem of the tropics was one of parasitism, not climate; and that acclimatisation was largely a question of hygiene. A leader in the same issue of the *Journal*, probably written by Manson, was broadly supportive of Sambon’s position, though the writer added a word of caution: the European was incapable of tilling the soil under the tropical sun, and was therefore incapable of feeding himself. Until he could do so, he could not be said to be properly acclimatised. The leader writer hinted at the possibility that eugenics might serve to hurry these matters along.6

Sherman Bigg, a physician with Indian experience, arguing against Sambon, said that the effect of heat on the European was first to exhilarate, then to over-stimulate, and finally to depress and impair the functions of the various organs of the body. He considered that Sambon’s views were unsound, his conclusions fallacious: accepting his thesis would lead to ever greater loss of life in the tropics.7

Shortly after the publication of Sambon’s paper, Manson and Sambon met in the library of the British Medical Association. They must have made an interesting pair, Manson (fig. 2.1) middle-aged, tall and placid, and speaking with a soft Scottish burr, Sambon (fig.2.2) stocky in build, full of youthful energy, sporting a luxuriant moustache and talking with a decidedly Italian
accent. Despite the differences, they became firm friends. Manson could see great advantage in having a colleague at his side who was trilingual, since important work in the study of malaria was also being carried out by French and Italian researchers. Manson was sufficiently impressed with Sambon to ask him to read, and revise if necessary, his forthcoming text book on tropical diseases.⁸

On 27 April 1898, with Manson’s support, Sambon gave his paper on acclimatisation to a meeting of the Royal Geographical Society.⁹ Perhaps bending the truth somewhat, the chairman, Sir John Kirk, in introducing Sambon, said that “he has had great opportunities of studying the question of acclimatization of white men in tropical Africa,” a rather ambiguous remark which has been taken by some to mean, erroneously, that Sambon had worked in the tropics.
Sambon’s paper largely repeated and reinforced the arguments of his January 1897 paper. His influential audience included Manson; the well-known traveller Mary Henrietta Kingsley (fig. 2.3); Sir Henry Wylie Norman, GCB, GCMG, (fig. 2.4) a past governor of Jamaica and Queensland; Sir Henry (“Harry”) Johnston, a botanist, explorer and colonial administrator; and the medical missionary Robert Felkin. Once again Manson supported his protégé’s thesis but Sambon failed to persuade the majority of his audience. Felkin called Sambon’s thesis “rather dangerous,” but thought that “in three or four hundred years” the tropics might be occupied by Europeans. Johnston, who had spent a few months in India, agreed broadly with Sambon, but considered that Britons born in India failed to “reach the standard of stamina seen in the home-bred Briton.”

A *British Medical Journal* leader in September 1898 noted:

“If climate pure and simple be the cause of the unhealthiness of the tropics, the position is hopeless; we cannot materially modify climate. But, after all, it is mainly
the parasites of malaria, of dysentery and of typhoid that make the tropics so unhealthy for the European. Take away the malaria microbe, and the dysentery microbe from West Africa, and this country would become as healthy as Europe.”

There were, however, other powerful voices arguing against Sambon’s hypothesis. Robert De Courcy Ward, a climatologist and the first professor of climatology at Harvard, wrote shortly after the occupation of the Philippines by American troops:

“There is much confusion in the public mind just now as to the question of the health of North American troops during a temporary sojourn in the Philippines, and also as to the larger question of possible acclimatization of our people in those islands in case of permanent occupation. No definite answers can be given to these two questions, but…three things may well be borne in mind. First: By means of a strict observance of hygienic principles, the death rate among foreigners in a tropical country can be very much reduced. Second: The great majority of the best authorities are agreed that complete acclimatisation of Europeans (and hence, we may add, of North Americans) in the tropics is impossible… Third: The Anglo-Saxons are universally acknowledged to be the least fitted, the Mediterranean nations the best fitted, to colonize the tropics.

The self-educated sociologist Benjamin Kidd, wrote three articles, “The Control of the Tropics”, for the London Times. Though much of the material was socio-economic and political, he agreed with Ward:

“In France in connexion with this subject, a literature in itself has gradually arisen dealing with the acclimatisation of the white man in the tropics. Yet, anyone who endeavours to follow it for himself, and who has been able to approach it with an open mind, will probably find himself possessed, sooner or later, of an overmastering conviction of the innate unnaturalness of the whole idea, and of every attempt arising out of it to reverse by any effort within human range the long slow process of evolution which has produced such a profound dividing line between the inhabitants of the tropics and those of temperate regions.”
Not all agreed with Kidd. A pseudonymous correspondent to the newspaper pointed out that north Queensland lay within the tropics, yet a major railway building programme, and extensive mining operations had been successfully carried out by white settlers, who were also beginning to undertake field labour. Any racial degeneracy was not physical due to climate, but was moral, due to isolation. The leader writer of the British Medical Journal agreed, suggesting that Kidd was suffering from preconceived ideas and had failed to read the relevant English and German literature on the subject, including the works of Dr Sambon.14

Old ideas die hard, despite compelling evidence. In July 1910, at the British Medical Association meeting in London Sir Richard Havelock Charles, an eminent physician with long experience in India, said that, independently of disease, the heat and moisture of tropical lands caused deterioration, marked chiefly by anaemia.15 As late as 1915, some experts were still suggesting that “in hot countries [a chill] is very liable to be followed by a severe attack of either diarrhoea or dysentery.”16 In January 1915, at a meeting of the Tropical Medicine Society in London, Sir Andrew Balfour, who was having serious disagreements with Sambon at the time (see chapter 10) managed to mention the problem of climate and health in the tropics without any mention of Sambon’s hypothesis. Referring to the work of Major Weston Chamberlain, President of the United States Army Board for the Study of Tropical Diseases in the Philippines, Balfour said it seemed likely that climate per se played little part in the health of the Americans in the archipelago, and most of the morbidity and mortality were due to “nostalgia, isolation, tedium, venereal disease, alcoholic excess, and especially to infection with various parasites.”17 It was an interesting juxtaposition of words.

However, Sambon received recognition that he was on the right track when, visiting the Americas in 1913, he was congratulated by General William
Gorgas (1854-1920) (fig. 2.5), the military surgeon who made the construction of the Panama Canal by the Americans possible through extensive sanitary programs, for his opinions. Gorgas himself came to London in March 1914, and told a meeting of the Royal Society of Medicine,

“All that has been done at Panama is to protect from the infectious diseases, principally malaria and yellow fever. This is evidence that it is the infections, not the climatic conditions, that have heretofore caused the wilting of the white man in the tropics. Protect him from the infection, and he will thrive, no matter what exposure he has to climatic conditions.”\(^{18}\)

Fig. 2.5 Sambon (back row) with General William Gorgas, 1913. Courtesy of the Wellcome Library
Sambon, looking back at Sherman Bigg’s criticisms, was gleefully able to say to the meeting, “My views were not unsound, conclusions not fallacious, nor my doctrines dangerous.”

Shortly before the meeting of April 1898, the *British Medical Journal* published another controversial paper by Sambon, in which he attempted to show that what was then called sunstroke (or heat stroke) consisted of two different conditions: heat exhaustion, or syncope, and “thermic fever”, which he called siriasis.\textsuperscript{19} As a medical term ‘siriasis’ had a long history. It is derived from Sirius, the dog star, which (in the northern hemisphere) rises and sets with the sun during the hottest months of the year. The word was known to the elder Pliny\textsuperscript{20} and was used at Alexandria by the late 7\textsuperscript{th} century CE surgeon, Paul of Aegina, who described siriasis as “an inflammation of the parts about the brain and its membranes.”\textsuperscript{21}

Sambon described siriasis as an acute disease, characterised by very high fever, coma and intense pulmonary congestion. The mortality rate was high. He posited an infectious cause, basing this idea on the geographical distribution, which, though limited, was always in regions where the ambient temperature was high. The disease was not directly due to heat, since people could live and work under very hot conditions without developing it. Immunity increased as people acclimatised. He suggested that the disease was caused by a bacterium that lived in soil, like tetanus.

An anonymous editorial in the *British Medical Journal*, again probably by Manson, cautioned against confusing an association with a cause-and-effect relationship. He gave as an example, the alleged relationship between ‘siriasis’ and temperature, before suggesting that Sambon’s hypothesis, clearly and logically stated, could not fail to attract attention and lead to a reconsideration of the whole question of heat-stroke.\textsuperscript{22} In the chapter on Heat Stroke (chapter 12) in his forthcoming textbook on tropical diseases, Manson
used Sambon’s descriptions and terminology, including siriasis, and continued
to do so until at least 1914. Manson’s attachment to siriasis (or its author)
probably deserves Ross’s stricture, in his 1909 inaugural presidential speech
to the Society of Tropical Medicine and Hygiene;,

“The general text-book on tropical medicine, written by a single author, is now
becoming impossible. I will not depreciate any writer by remarking that, whatever
may have been the case in the past, the gigantic infant is rapidly growing beyond the
control of any sole nurse, wet or dry. A man is fortunate if he knows thoroughly
even one item of our subject. Moreover, advance is so quick that many chapters of
such text-books become obsolete a few months after publication. Even the
monograph, written by the most capable expert of the time, is apt to become wrinkled
and old immediately after birth and is superseded by another a few weeks
younger.”23

Manson’s reputation did not mean that everyone automatically agreed with
him about siriasis. At the British Medical Association meeting in Portsmouth
in August 1899, Kenneth Macleod, Professor of Clinical Military Medicine at
the Army Medical School, Netley, had poked fun at the name siriasis (and, by
implication, disagreed with the rest of Sambon’s ideas). The name siriasis
was more mythological than descriptive: the disease was not limited to the
dog days. A pathological name, once the pathology was worked out, was
preferable to a fanciful one; and the term ‘siriasis’ had become wedded to
what was merely a hypothesis. Furthermore, it mattered little whether the
disease was due to heat or to infection, the important thing was to reduce the
temperature. Until someone found the alleged microbes, Sambon’s idea
would remain merely a hypothesis.24 Other military speakers broadly
supported Macleod’s opinion. Manson was having none of this, saying that
though Sambon had not completely proved his point, his arguments were
more potent than Macleod’s.
Though there still remain problems about the pathogenesis of heat stroke (especially why some people will suffer in a particular climate, while others, exposed to exactly the same conditions, do not) it is today generally accepted that heat stroke has nothing to do with infection, but is merely the most dangerous response to heat on a spectrum which goes from heat syncope (the bearskin-wearing guardsman who falls to the ground after standing to attention for long periods in the sun), through heat exhaustion, to heat stroke.

Sambon was earning something of a reputation. Probably through Manson, Sambon was offered the post of ship’s doctor on the tug, *Oceana*, temporarily fitted out as a research vessel. The voyage, to an area of the Atlantic due west of the Tearaght Light (Blanket Islands), lasted through a stormy November 1898. Its purpose was to investigate marine life in the intermediate depths of the ocean, thus contributing to the contemporary debate whether those depths supported any life at all. Working days were long, and conditions often vile. Sambon no doubt hauled and heaved with the best of them. The trip provided a useful interlude before Sambon ventured further into the world of tropical medicine.
Chapter 3. A School of Tropical Medicine

Since Manson’s return to Britain in 1889 after 23 years in the Far East, he had been thinking about the education of doctors interested in an overseas career. His appointment to the Seamen’s Hospital Society hospital at the Royal Albert Dock (fig. 3.1) in 1892, followed, in 1897 by his appointment as Medical Adviser to the Colonial Office, provided singular opportunities. Ernest Hart, the editor of the British Medical Journal, had recently, after a visit to India, bemoaned the British medical schools’ failure to teach tropical diseases.¹

Manson took up the challenge. His first lectures were delivered in 1894 at the Livingstone College in Leytonstone, to missionaries aiming for service in the

Fig. 3.1 The Royal Albert Dock Hospital. Courtesy of Wellcome Images
tropics. The following year, he started a course of twenty lectures on tropical diseases, first to students at St George’s Hospital, then at Charing Cross Hospital. Joseph Chamberlain, Secretary of State for the Colonies, (fig. 3.2) an enthusiastic imperialist, was impressed by Manson.

In Liverpool, Sir Alfred Jones, (fig. 3.3) an important ship-owner with extensive commercial interests in West Africa and the Congo, saw the need for expertise in tropical medicine. He contributed large sums towards the establishment of a school in that city, and persuaded other commercial interests to join him.² Lord Lister, President of the Royal Society, formally opened the Liverpool School of Tropical Medicine on 22 April, 1899. It achieved instant fame by appointing Ross as its principal lecturer.

Fig. 3.2 Sir Joseph Chamberlain. Courtesy of Wellcome Images
Fig.3.3 Sir Alfred Jones From the Journal of Tropical Medicine, 1903:6;370

After considerable difficulties, and despite opposition from the military medical schools (at Netley, on Southampton Water, and Haslar, near
Portsmouth) and some senior figures in the London medical establishment, the London School of Tropical Medicine was established at the Royal Albert Dock Hospital, taking its first students in October 1899. The endeavour was only made possible with the support of Pietro James (later Sir James) Michelli, (1853-1935) secretary of the Seamen’s Hospital Society, who warmly supported Manson’s ideas. Michelli’s obituary said:

“For his services to tropical medicine in general, and for the teaching of this branch of medicine in London in particular, he will always be held in high esteem, for without Michelli it is probable that the London School of Tropical Medicine, which was instituted under his guidance in 1899, and its subsequent unexpected expansion, would not have existed.”

Sambon was appointed a lecturer at the school, although he was not British, and, unlike many other pioneers of tropical medicine at the time, had no experience of life or medicine in any tropical country. Whether Michelli, who, despite being born in Ireland, had Italian roots, played any part in the appointment of Sambon is not known. The first report of the school made it clear that Sambon was on the teaching staff, not the hospital staff, but, perhaps in recognition of his family connections, he was appointed curator of the museum and librarian.

Sir Philip Manson-Bahr, Manson’s son-in-law, said of Sambon, “by reason of his ancestry and training [he] did not fit in easily with the English scene.” His responsibilities were parasitology, the geography of disease, snakes and venoms (including poisoned arrows) and, of course, the effects of heat, including siriasis.

He was at the School on Mondays and Thursdays. Manson-Bahr added that “As a lecturer Sambon was brilliant and entertaining and his histrionic performances were always a great draw. He strutted and gesticulated on the stage, using the arts and actions of a great actor.” He liked to pepper his
lectures with antiquarian anecdotes of doubtful scientific validity. A favourite was his story that the Egyptians revered the scarab beetle because, as a dung beetle, it rapidly disposed of human faeces, and thus contributed to the control of hookworms.

Sambon also claimed that attempting to control plague through anti rat measures was a waste of time, since rats bred so rapidly. The ancient Romans, he said, understood about rats and plague. When plague broke out in 293 BC the Tribune Quintus Ogulgius went to Epidaurus to ask the priests at the temple of Aesklepius—the most famous in the classical world—for advice. Sambon suggested that Ogulgius was given a number of “rat-eating snakes” and that soon put an end to the disease. (The conventional story is that when plague broke out in 293 BC, the worship of Aesklepius was introduced into Rome by order of the Sibyline Books, the god being brought to Rome in the form of a snake.¹⁰) Sambon’s students, presumably brought up in the lofty tradition that classical myths were nothing more than myth, might well have found Sambon’s approach memorable.

His knowledge of medical history tempted him to relate anecdotes showing how germ theory developed. A favourite was the story of the Corsican Francesco Renucci and the itchy skin disease scabies.

According to Sambon, the Arab physicians of the twelfth century knew that scabies was caused by a tiny mite in the skin, but European medical men continued to believe it was a miasmatic disease until 1834. In that year, Renucci, hearing the existence of the mite denied during a lecture at the Hôpital St Louis in Paris, extracted the mite from the skin of a scabby patient with the point of a needle, as he had seen peasant women do in Corsica.¹¹ The hospital physicians wisely demanded a repeat performance. It took place a week later, and they were thereupon convinced. Parasitism was here to stay. Sambon, however, had shown that he was not averse to bending the truth if it
suited his purpose. The mite of scabies (*Sarcoptes scabei*) was known to early Renaissance physicians, and was described by Thomas Moffet in his *Insectorum, sive minimorum animalium*, published posthumously in 1634.\(^{12}\) Two Italians, Giovanni Cosimo Bonono and Diacinto Cestoni, suggested in 1687 that the mites were the cause of the intense itching associated with the infection. That the mite caused the itch was proved by Jean-Chrysante Galès in 1812, though this work provoked considerable controversy until 1834 and the arrival of the needle-bearing Renucci.\(^{13}\)

Sambon’s regular references to classical mythology and medical history no doubt helped to romanticise parasitologists, the creators and defenders of an empire under attack by a monstrous host of animalcula.\(^{14}\)

Before long Sambon was known to colleagues as ‘Sammy-Boy.’ How he spent the rest of his time at this early stage of his career is not clear. The first notice of his registration as a medical practitioner in Great Britain was in October, 1902. Whether he practiced among the large Italian community of London he never said. Further financial support however was not far off.

The two schools of tropical medicine went down different paths. In London, Manson focused on clinical medicine, coupled with detailed laboratory work. In Liverpool, the laboratory work was subordinated to the school’s specialism, sanitation. The Liverpool School put its theoretical teaching to practical use by sending out numerous expeditions to various parts of the world to study and, if possible, manage local endemic or epidemic disease. The London School lent its students or staff for expeditions organised by others, the Royal Society for example, but organised few of its own. Its first, a mosquito-malaria study, achieved world-wide recognition.

The eminent French physician and parasitologist Raphael Blanchard visited the London School in 1899. He saw the need for similar institutions in France. It
seems likely that Blanchard and Sambon struck up a friendship, perhaps based on Sambon’s ability to speak French and a shared interest in the history of medicine. After three difficult years,\(^{15}\) Blanchard eventually opened the *Institut de Médecine Coloniale*. The official photograph taken at the first session of the Institute in October 1902 shows Sambon sitting in the front row with the sanitarian Adrien - Achille Proust\(^ {16}\) and Blanchard (fig. 3.4). Sambon also became a corresponding member of the French *Société de Pathologie Exotique*, and remained so for the rest of his life.\(^ {17}\)

Blanchard’s initiative was part of a larger attempt to develop the speciality of Tropical Medicine within Europe. Schools of Tropical Medicine opened in
Germany, Portugal, Belgium, The Netherlands, all countries with significant colonial empires. While a spirit of competition motivated some of these new institutions, there also developed transnational networks, which in turn led to

Fig. 3.5 Cartoon by Amedeo Terzi commemorating a visit by members of the French Institute for Colonial Medicine to the London School, December 1903. The cartoon shows, from the right, Manson, dressed in Chinese costume and carrying a Chinese fiddle; James Cantlie, also a Scotsman, in a kilt, with bagpipes; Ross, and Sambon dressed as a bandit, with a bass drum. Courtesy of Wellcome Library.

the establishment of new journals, and of congresses and conferences devoted to the new discipline.\textsuperscript{18} Visits by individuals or groups to each other’s institutions were common. In December 1903, a group of French doctors from the Institute for Colonial Medicine, headed by Blanchard and Professor Robert Wurtz, a hygienist and pathologist, visited London, giving the artist Amedeo Terzi an opportunity to exercise skills (fig. 3.5) that were more usually reserved for minutely accurate illustrations of blood sucking diptera.
A dinner was held for the guests at the Trocadero Restaurant, at which Sambon proposed the health of the visitors. But before this memorable international visit took place, Sambon had participated in an important investigation.
Chapter 4. Malaria and Mosquitoes

For many centuries malaria (from the Italian, “bad air”) was considered to be the paradigm of miasmatic disease: its association with swamps reflected in the French name for the disease, *paludisme*, derived from the Latin *palus*, (genitive case, *paludis*), a swamp. Though not the first to hold the opinion, Manson, from 1894 onwards, was promoting the idea that malaria was transmitted by mosquitoes, even though it was uncertain how the malaria ‘germ’ got into humans. Manson believed that the mosquito, after only one bite, laid its eggs in water, and then died. The ‘germ’ of malaria was possibly imbibed in this water, though he thought that this was not the only way transmission might occur. By 1896, Ross working in India, was suggesting that it was the bite of the mosquito that was important: “She [the female mosquito] always injects a small quantity of fluid with her bite, I believe – what if the parasites get into the system in this manner?”

The following year, 1897, Ross (fig. 4.1) showed that malaria parasites of birds developed a sexual stage in mosquitoes, but still had to prove that the transmission of the disease was through these insects. Twenty years later Sambon wrote,

“The last details of Ross’s work reached London in July 1898, just before the meeting of the British Medical Association. Manson was suffering from a severe attack of gout—[he was then fifty-three, but had suffered from the condition for the past ten years]—, but, this notwithstanding, he travelled to Edinburgh with Dr (now Sir James) Cantlie and myself, to announce that, partly by direct observation, partly by analogy, Ross had proved that the malaria parasites of man undergo a necessary stage of development within the bodies of mosquitoes, and that these insects do indeed transfer the malaria infection eventually from man to man. On arrival, Manson was so ill that he had to lie in bed unable to move, so I took down the
address he had been asked to give on the mosquito-malarial theory…It was a great
day for the absent Ross, and we cheered him until we were hoarse. It was a day of
thankfulness and rejoicing for the whole of mankind, because…one of the greatest
disease scourges had been made preventable.\textsuperscript{34}

True, but not entirely true.

![Fig. 4.1 Sir Ronald Ross. Courtesy of Wellcome Images](image)

Traditionally, the annual meetings of the British Medical Association were
divided into a number of sections, dealing with, for example, medicine,
surgery, public health, obstetrics etc. Thanks to Manson, the Edinburgh
meeting was the first to include a section of Tropical Medicine, with,
unsurprisingly, Manson as its president.

Ross was neither a zoologist nor an entomologist. Throughout his
experimental period, he described the mosquitoes he was using by their
colour, brown or grey, or their external characteristics, striped or brindled or banded. He sometimes called the mosquito a gnat, a term traditionally applied to smaller biting insects, but which sometimes included mosquitoes. By late 1897, he had found the mosquitoes that transmitted malaria, which he described as “grey” or “dappled winged,” and on 4 July, 1898, found the organism in the mosquito salivary gland.⁵

Work in the Roman Campagna by a group of Italian researchers, Amico Bignami, Giuseppe Bastianelli and Angelo Celli and organised by Giovanni Battista Grassi, a zoologist, showed in November 1898 that Anopheles mosquitoes transmitted malaria to humans.⁶ Grassi claimed that he had arrived at the mosquito-malaria connection independently of Ross’s work. The claim would lead to personal animosity between Grassi and Ross, a not unusual situation in the heated climate of tropical medicine researches.⁷ Fifteen years later, Ross wrote to Sambon, who had helped collect material for the Wellcome Historical Museum (see chapter 6), asking that his portrait, which he did not like anyway, be removed, as it was too close to Grassi’s.⁸ Sambon obviously had no authority to do this. Ross later wrote to Wellcome to complain that he had asked Sambon three or four times to have the portrait removed. He was certain that its remaining in place was a deliberate act of Sambon’s.⁹ Wellcome replied that he was unaware of the request and Sambon anyway was in America; he complied with it.

The general British ignorance about the biology of mosquitoes led Chamberlain to issue a circular in late 1898 to the Governors of all the Crown colonies, requesting them to make collections of as many mosquitoes as possible. By September 1900 over 3,000 specimens of the insects had been sent to the British Museum (Natural History).¹⁰
In his memoirs, written long after the Grassi affair should have been laid to rest, Ross remained bitter, accusing the Italians of piracy, theft and falsification.\textsuperscript{11} His dislike of Italians soon extended to Sambon. He wrote in 1903 about a paper of Sambon’s on sleeping sickness, “This paper is so full of carefully construed falsifications in the true Italian manner, I have thrown most of it away as rubbish.”\textsuperscript{12}

At the Edinburgh meeting, Sambon, now confident about his perceptions and understandings of malaria, presented a paper on blackwater fever. In this disease, the red blood cells are rapidly haemolysed, leading to severe anaemia, haemoglobin in the urine (hence the ‘black water’), jaundice, and kidney failure. The mortality at the time was high, varying, in different series, between 30\% and 90\%. Most observers, but not all, thought that blackwater fever was somehow connected with malaria (the disease was only seen in malarious zones, or in people who had lived in malarious areas). Opponents of the malaria hypothesis argued that blackwater fever did not occur in all malarious areas, malaria parasites could not be demonstrated in all cases of blackwater fever, and furthermore quinine did not cure the disease. There had to be some other explanation. Quinine, the only drug known to be effective against malaria, itself came under suspicion in 1874, when the Italian Salvatore Tomaselli suggested it might be the cause of the haemolysis. Only after Robert Koch became interested in the problem of malaria and blackwater fever were interest and controversy fully aroused.

Koch visited East Africa in 1897 in order to study malaria. He came to believe that blackwater fever was not related to malaria, saying that if malaria parasites were found in the blood of these patients, it was a co-infection, rather than the cause; and that blackwater fever was most probably due to quinine.\textsuperscript{13} Koch’s theory led to bitter controversy, particularly among British doctors, about the value of quinine. The arguments were aired in the British
lay press.\textsuperscript{14} The Scotsman of 26 July 1898 noted that the mortality from blackwater fever had become alarming among the Europeans (among whom were a number of influential Scottish missionaries) in East Africa. It suggested that Koch’s notion of “overdosing” with quinine was not only unsupported, but was also contradicted by “many patent facts”, though without mentioning what these were. The leader also suggested that since the malaria parasite was not a bacterium in the accepted sense, a bacteriologist alone might not be the right person to resolve the problems. The author of the leader thought that the problem of blackwater fever should be thoroughly discussed at the Edinburgh meeting. It was.

Sambon said that blackwater fever had nothing to do with malaria, and nothing to do with quinine. He suggested that paroxysmal haemoglobinuria [occurring in temperate climates, it is a rare condition in which sudden haemolysis of red cells leads to the presence of haemoglobin in the urine], might be analogous\textsuperscript{15} but admitted that as nothing was known about its cause, the analogy was not very helpful. He noted that paroxysmal haemoglobinuria was rarely fatal, but suggested that blackwater fever had a high mortality because it occurred in individuals ‘wrecked’ by previous tropical malaria. He pondered on the possibility of a relationship between blackwater fever and the haemoglobinuria of cattle, which had been shown by the Romanian bacteriologist Victor Babes to be caused by protozoa,\textsuperscript{16} later called Babesia.

After the Edinburgh meeting a leader in the British Medical Journal noted the increasing importance of blackwater fever, and pointed out that, until the question of aetiology was settled, the African practitioner faced a grave dilemma. If the disease was of malarial origin, to withhold quinine was dangerous; if it was not, to give quinine was useless, while if it were due to quinine, administering the drug was ‘homicidal.’\textsuperscript{17} The dilemma grumbled on for a number of years, though by the end of the Great War, the opinion that
blackwater fever was a complication of *falciparum* malaria was increasingly though not universally, accepted. Sambon, as late as 1918, continued to promote his hypothesis that it was a form of babesiosis, probably spread by ticks.\textsuperscript{18} The exact genetic, pharmacological and immunological mechanisms by which the red cells are destroyed still remain unclear.

Sambon meanwhile became engaged to be married. Hilda Lucia Nicoletta Balestra, born in Lower Norwood in 1872, was the second child of Italian parents. Her father, Federico Stefano, was Neapolitan, her mother, Antonetta, was born in Bari. When or why they migrated to England is not known, although since their names do not appear in the 1861 UK census they presumably arrived sometime between 1861 and 1869. Following the unification of 1861, as explained in chapter 1, the fortunes of Naples had slumped, and emigration had increased. Hilda was a friend of Edith, Manson’s eldest daughter, though how they met is not known. Manson-Bahr describes an episode [he puts it in 1903, which is clearly incorrect, the Italian visit took place in December, 1899] in which a number of British scientists had been invited to Palermo by a local millionaire industrialist, Commendatore Ignazio Florio, to inspect the sanatorium he had founded, the *Villa Igiea*.\textsuperscript{19} The British used the opportunity to have a meeting in Rome with the Italian malaria experts, including Grassi, Celli, Bastianelli and Bignami. Grassi had just completed the first edition of his book on malaria parasites, (though nothing was reportedly said of Ross and his work). Sambon acted as the intermediary between the British and Italian workers, and, according to the anonymous report, “the success of the meeting was due to his tact, courtesy and infinite capacity for taking trouble.”\textsuperscript{20}

Sambon was fond of swimming, and equally fond of showing off his prowess. Among the guests were Edith Manson, and “a friend.” Edith said to her friend, “I would not, if I were you, have anything to do with Sammy Boy. He
eats garlic.” “I know that only too well,” the other replied, “for, you see, we are engaged.” In April 1902, Sambon married Hilda in Richmond. They were to have almost thirty years of happy life together.

Angelo Celli (fig. 4.2) was increasingly managing to protect the employees of the Rome-Tivoli Railway and their families from malaria by screening the houses in which they lived. After Manson had seen Celli’s work, he proposed an experiment that he hoped would complete the mosquito theory. It could

Fig 4.2 Angelo Celli. Courtesy of Wellcome Images.

also show how malaria might be prevented. The Colonial Office agreed to pay. Manson proposed that, at the height of the malarial season, volunteers would live for three months in a mosquito-proof house in the Roman Campagna. The volunteers would not be allowed out of the house from an
hour before dusk to an hour after sunrise, and would not have access to quinine. Manson wished to add further power to the experiment. He arranged, with Bastianelli’s cooperation, for mosquitoes from the Campagna to be fed on malaria patients, and then sent to London in the embassy bag. These mosquitoes would then be allowed to feed on human volunteers.

The box in which the mosquitoes were to be transported was designed by Sambon. His innovation was to protect the mosquitoes from shaking by putting them in tubes of mosquito net, which were then placed in a ventilated wooden box. A piece of watermelon was placed in each tube for feeding. Of the two volunteers for the transmission experiment, one was Patrick Thurburn Manson, Sir Patrick’s eldest son (fig. 4.3). The second volunteer, Mr George Warren, was a laboratory assistant at the London School of Tropical Medicine. Both volunteers duly exposed their arms to the bites of the mosquitoes and both developed tertian malaria.

The presence of parasites in the volunteers was confirmed by a number of experienced medical men, including Manson Senior and James Cantlie. The eminent surgeon, William Watson Cheyne, FRS, confirmed that the spleen was enlarged, a normal finding in acute malaria. The volunteers were then given a course of quinine, and recovered. Both relapsed, (Manson twice) which could not have been due to re-infection.

Sammy-Boy was an obvious candidate to act as a guinea pig for the second part of the experiment, but Manson thought he needed someone with him for what might be a lonely three months. George Carmichael Low seemed an appropriate candidate. Though six years Sambon’s junior, he was a well-qualified doctor who, at Heidelberg and Vienna, had learned dissection techniques that allowed for more detailed studies of parasites in mosquitoes. Shortly before setting off for Italy, Low had been able to demonstrate that the
filarial larvae emerged from the proboscis of the mosquito, and were not, as Manson had imagined, imbibed in water in which mosquitoes had died.

Fig. 4.3 Patrick Thurburn Manson. Courtesy of Wellcome Images. Manson died following an accidental shooting while investigating beriberi on Christmas Island in 1902.

“Two Englishmen, Dr Sambon and Dr Lowe [sic], of London, are to make an interesting experiment and take their lives in their hands for the sake of humanity. These two men are to go to the Roman campagna, the deadliest place in the world, with instructions to stay there all summer and see if it kills them. If they return home unharmed, then the world’s most fatal disease next to consumption—malaria—will be at the mercy of science. If they die, the government will pay the funeral expenses and the theory about malaria will be upset,” one American newspaper reported lugubriously, and many others published the story.

The third member of the group was Amedeo John Engel Terzi, who was commissioned to make drawings of whatever insects—or other things—Low
and Sambon thought necessary. The Terzi brothers were known to Sambon. Aleardo, the older brother, had been one of two artists who had illustrated Sambon’s book on the Abyssinian Army. Amedeo acquired a reputation for cantankerousness, and made whimsical cartoons of the expedition (fig. 4.4) A male servant, Silvestri, who had been a working naturalist, also joined the group.

In June, 1900, Sambon and Low went to Italy to supervise the siting and erection of the mosquito-proof hut. The first edition of Grassi’s *Studii di uno Zoologo sulla Malaria*, had appeared at the beginning of the month. Sambon reported to Manson that the book was largely a polemic against Koch, who had also become interested in the problem of malaria, and Ross. Ross later wrote of this episode, “Yet Manson accepted the dedication of this very book of Grassi’s and afterwards pretended he did not know that it attacked me infamously.”

A wooden house, constructed by Messrs. Humphreys of Knightsbridge, (fig. 4.5) was erected at Castel Fusano, near the coast and about two miles south-by-south east from Old Ostia. Celli reckoned the site to be highly malarious. The windows and doors of the hut were screened with wire gauze. The beds had mosquito nets.

While digging the foundations of the hut, Sambon’s labourers came across the terracotta tomb of a young woman. Close to the skull was Charon’s obol, in the form of a coin of the Roman emperor Commodus, who issued coins between 175 and 192CE, Sambon reported. The only drawback to the site was the absence of a large at risk population. Many of the cultivators left the area when the malaria season approached. The controls problem was resolved by an unhappy event, the anarchist assassination King Umberto I in Monza on 29 July. A party of fifteen (or so) policemen, trawling the Ostian marshlands
Fig. 4.4 (a) Cartoon by Terzi. Courtesy of Wellcome Images. Sambon (carrying a rifle) is in the lead, followed by Terzi, carrying his painting materials and Low with an Aesklipian snake in his helmet.

Fig. 4.4(b) Sambon has exchanged his rifle for a knife, Terzi is carrying a revolver and a bottle of Ferro-China Bilsleri while Low, still smoking, carries nothing.
for anarchists who might assist them with their inquiries, spent a part of one night in the area. Celli, on the authority of the police medical officer, told Sambon that within the next couple of weeks, the entire posse developed malarial fevers.

The hut experiment lasted from 19 July to 19 October. Adhering strictly to the rules, (though Andrew Balfour claimed years later that they hadn’t: see below) none of the volunteers developed malaria. During the day, they undertook numerous entomological investigations, some of which were later published in the *British Medical Journal*, accompanied by a colour plate of drawings by Terzi.

The human guinea-pigs were visited by a number of observers, including Grassi, (fig.4.5) who sent a telegram to Manson: “Assembled in British experimental hut, having witnessed perfect health experimenters amidst malaria-stricken inhabitants. Italian physicians congratulate Manson who first formulated mosquito-malaria theory.” To the amazement of the few local inhabitants who remained in the area, the men slept with the bedroom windows open, turned over the soil, bathed in the sea at Ostia, and despite
getting an occasional soaking while looking for insects during periods of rain, never developed any fevers. (Sambon did, however, have a bout of “gastro-enteric catarrh”—perhaps travellers’ diarrhoea—but recovered quickly. Blood tests by Low failed to find any evidence of malaria parasites.) The two experiments showed conclusively that malaria was transmitted by mosquitoes, and could be prevented by anti-mosquito measures.

Ross doubted the validity of the experiment. In a letter to the *Lancet*, he said,
their way, but they will never be used by the bulk of those who live in malarious places…”

Ross and the entomologist Ernest Austen had claimed that Anopheline mosquitoes could be readily identified by their habit of assuming a resting position at an angle of about $45^0$ to the vertical. Sambon and Low challenged this. One mosquito they had examined in Italy, *A. claviger*, did not always assume this position. Rather rashly, they quoted Grassi in support of their argument. Ross replied that he and Austen were only referring to mosquitoes that they had observed themselves; they could not be held responsible for other people’s mosquitoes.

Grassi’s criticism got under Ross’s skin:

“As a large part of this memoir [Grassi’s] (which has been dedicated to Dr Manson, and is being translated, I understand, by my friend Dr Sambon), consists of an attack upon me, this fact is not to be wondered at. The question is whether Grassi, who studies in Italy, is in a position to reproach me with justice for observations made in India and West Africa.”

At the end of the experiment, the hut was presented to the Rome Municipality, which had helped in its erection, and had given the volunteers other services. The entire experiment cost the Colonial Office £500, but the potential savings were incalculable. On their return to England, Sambon and Low achieved some celebrity. Fifteen years later, Andrew Balfour, then the Director of the Wellcome Bureau of Scientific Research (where Sambon was also working), said of Sambon, “I know now that his selfishness and carelessness nearly wrecked the historic malaria research in Italy,” but did not say what Sambon had done to earn such criticism. In the official report Sambon mentions that they examined the blood of a number of local bats, but does not say by whom or when the animals were trapped. Bats being crepuscular, Sambon perhaps crept out of the hut of an evening. He later told Alphonse Laveran that he had
found trypanosomes in some of the bats,\textsuperscript{40} though this is not stated in the report.

Despite the Grassi telegram, a rumour was soon being ‘industriously circulated’ in Italy that the experiment was a failure.\textsuperscript{41} Manson rushed into print, alleging that the purpose of the experiment was an exercise in public education:

‘[B]eing anxious to see some fruit from a theory which I knew to be true and for which I was in a measure responsible, I cast about for means by which the conversion and co-operation of the public might be secured. I felt that unless the public believed in the efficiency of the sanitary measures so definitely indicated by the mosquito-malaria theory, and understood the principles on which these measures should be founded, they would not adopt them, nor, what is so necessary to the success of all such measures, co-operate heartily in carrying them out.’\textsuperscript{42}

The public was convinced. By 1905, the Italian government had enacted laws to make quinine available to all at risk and to require employers of labour in malarious regions to provide lodgings that had been appropriately protected against mosquitoes. Celli worried that the expense of “proper window-nets and screen doors” would prevent this method of protection from ever becoming general among the peasantry.\textsuperscript{43} He thought prophylactic quinine a better option.

Before returning to London, Sambon paid a visit to northern Italy, where he made a brief study of the disease pellagra in the \textit{pellagrosarium} (chapter 8) of the town of Inzago, some thirty kilometres east-north-east of Milan. He said nothing about pellagra in 1900, but returned to the area in 1904. He was formulating an idea about pellagra that would provoke considerable interest and discussion. He was also proposing to write a book on malaria, even asking Ross for a photograph to include in the book, but neither the book nor the photograph materialised.
Terzi (fig. 4.6) also returned to England in October, 1900. Three months later he was appointed illustrator for the London School at Manson’s request, but left a year later “for unexplained reasons.” He then joined the staff of the Natural History Museum and stayed there for most of the rest of his life.

Fig. 4.6 Amedeo J. E. Terzi, taken from a group photograph at the London School of Tropical Medicine, 1901. Courtesy of Wellcome Images.

Terzi was devoted to Sambon, seeing him as the man who had started him on his journey to success as a scientific illustrator. Sambon responded by naming a parasite of South American boa constrictors, \textit{Haemogregarina terzii}. Sambon not one to rest on his laurels, was about to get embroiled in another controversy, again with a British Army officer, about the disease known in earlier times as “Negro Lethargy.”
Chapter 5. Confrontations with a military man.

The military man with whom Sambon was to cross swords was Lieutenant-Colonel David Bruce of the Royal Army Medical Corps. Bruce (fig. 5.1) had achieved considerable fame in 1889 when he identified the micrococcus responsible for Malta Fever,² (undulant fever or brucellosis), a disease that caused considerable sickness among the British troops stationed on the island, and in other Mediterranean garrisons.

Fig 5.1 Sir David Bruce and his wife Mary. Courtesy of Wellcome Images.
Bruce’s personality was readily accommodated in Victoria’s army. He was no fool, but was reserved and distant. He could be outspoken, caustic and thuggish. In 1901 in the course of a haircut in Liverpool, the barber cut off a small piece of Bruce’s ear. In a rage, Bruce, who had been a good boxer, knocked out the unfortunate barber. He came before the magistrate, and was fined.\(^3\)

Bruce was posted to South Africa in 1894, with the rank of Surgeon-Captain. Following promotion to Surgeon-Major in August 1895, he was promoted to Lieutenant-Colonel in November 1900, for his services during the siege of Ladysmith. In South Africa, he became involved in research that would lead eventually to the unravelling of the cause of African sleeping sickness.

Two tributaries meet to form the history of African sleeping sickness, a disease sometimes known in the nineteenth century as “Negro Lethargy,” or “African Lethargy.” The first rises from the clinical symptoms, the other from the alleged causes of the disease. The confluence occurred in Uganda, in November 1902, when the aetiology of, and potentially the means for preventing, sleeping sickness became clear.

African sleeping sickness has a long history, but it was only by the middle of the eighteenth century that European observers became aware of this debilitating illness. Dr John Atkins, a naval surgeon, writing about his observations of the “Coast of Guiney” in 1742, mentioned “the sleeping distemper.” Atkins noted that no amount of “pulling, drubbing or whipping will scarce stir up sense and power enough to move, and the moment you cease beating the smart is forgot, and down they fall again into a state of insensibility…”\(^4\) The distemper was characterised by swollen glands in the neck—later called Winterbottom’s sign, after its description in 1803 by Dr Thomas Winterbottom, a surgeon who had worked in Sierra Leone—and increasing signs of lethargy, leading ultimately to death. Atkins believed that
this disease affected only Africans, and was due to a natural weakness of the African brain. Winterbottom said that Arab slavers rejected slaves with enlarged cervical glands. Despite this, a number of instances of “sleeping sickness” were reported from the West Indies, particularly by French doctors. These doctors made the important observation that the disease was not found in the descendants of the slaves, though why this was so was not known. Though the disease was thought to affect only Africans, Manson could see no reason why Europeans should not be susceptible.

Manson was present at a meeting of the Clinical Society of London on 14 November 1890 when Dr (later Sir) Stephen Mackenzie, a physician at the London Hospital, and brother of the famous laryngologist Sir Morell Mackenzie, presented a case of “Negro Lethargy.” A filarial worm had been found in the young man (a native of the Middle Congo named Madombe) but Manson thought this was purely fortuitous. He agreed with a suggestion from the explorer Henry Morton Stanley that the disease might be due to some poisonous influence in food.

In 1898, however, two young Congolese patients, Eli Mboko, aged about 20, and Tenda Mkuloo, aged 11, were brought to England (“through the… enlightened benevolence of Dr Henry Grattan Guinness,” of the Congo Balolo Baptist Mission) for further investigation. In these two patients Manson found the same filarial worm, which he called *Filaria perstans*. He now started to think that this filaria was possibly the cause of their symptoms, but cautioned against assuming cause and effect. The two young men eventually died. At autopsy, Dr Frederick Mott (fig. 5.2), the foremost neuropathologist of his day, found a perivascular infiltration of both large and small vessels of the brain with lymphocytes and other cells, but no parasites.

Numerous candidate causes for the disease were current at the time. They
included: malaria, smoking hemp, the effects of the sun (‘insolation’), and an excess of palm wine. French doctors in the West Indies thought homesickness (*nostalgie*) might either be the cause, or predispose to the illness, while others thought that sleeping sickness might be a manifestation of beriberi. 13

![Frederick Walter Mott](image)

**Fig. 5.2** Frederick Walter Mott. Courtesy of Wellcome Images

In 1841, Gabriel Gustav Valentin (1810-1883), a German physician working in Berne, had noted the presence of a very motile organism in the blood of a salmon, though the nature of this organism was uncertain. Two years later, David Gruby (1810-1898), a Hungarian physician working in France, found a previously unknown organism in the blood of a frog. He named it *trypanosome*, (from the Greek *trypanon*, an auger, and *soma*, a cell) because its motion suggested the movement of a corkscrew or auger. 14 These parasites are long, undulating protozoa which possess a ‘flagellum’, distinguished by a crimson tint in fig. 5.3. The flagellum extends along and around the length of the organism and emerges from the posterior end.
Beating of the flagellum makes the entire cell rotate, like an auger, usually, but not always, with the sharp end in front.

Fig. 5.3 *Trypanosoma gambiense* drawn by Dr Everett Dutton. Courtesy of Professor David Molyneux, Liverpool School of Tropical Medicine.

That these creatures parasitised higher animals was shown in 1879 by Dr Timothy Richards Lewis (1841-1886)\(^\text{15}\) (fig. 5.4) who found the parasite (*Trypanosoma lewisi*) in the blood of apparently healthy rats in Calcutta. The following year, also in India, Inspecting Veterinary Surgeon Griffith Evans (1835-1935) (fig. 5.4) found trypanosomal parasites in the blood of horses suffering from a chronic wasting disease known locally as *surra*. Though Evans was initially unaware of the nature of the parasite, (and his superiors largely ignored his findings) he did suggest that it might be transmitted by flies, a suggestion proved to be correct by Leonard Rogers in 1899. The parasite was shown to be a trypanosome, which led Lewis to say that this organism could not be the cause of *surra*, as trypanosomes were non-pathogenic. He was wrong. In Evans’ honour, the parasite was later named *Trypanosoma evansi*.\(^\text{16}\)

Shortly after arriving in South Africa in 1894, Bruce was sent to Zululand by the Governor of Natal, Sir Walter Hely-Hutchinson, to investigate an outbreak of *N’Gana* (or *Nagana*) a feverish, wasting, and ultimately fatal disease of
native cattle. Hely-Hutchinson had been Lieutenant-Governor of Malta at the time of Bruce’s discovery of the cause of Malta fever, and was presumably familiar with Bruce’s work.

Nagana was thought by the local people to be spread by the tsetse fly, an aggressive fly about the size of a domestic fly. Tsetse flies live in dense woodlands close to waterways or swamps. The missionary David Livingstone had reported in 1858 that horses and cattle in parts of what is now Botswana died from the bites of these insects: he had tried, unsuccessfully, to treat the disease with the Victorian panacea, arsenic, a derivative of which (Atoxyl) was later found to be an effective, if toxic, remedy for diseases due to trypanosomes. Manson’s patient was treated with “large doses of arsenic”: his general health “quickly underwent a marked improvement,” though unfortunately this was not maintained.
In Zululand Bruce discovered a parasite in sick cattle that proved to be a trypanosome; it was similar to the organism discovered by Evans and was in due course named *Trypanosoma brucii*, later amended to *T. brucei*. Bruce’s investigations also showed that the tsetse fly was somehow involved, and that other large mammals, including horses and many wild animals, could be infected with trypanosomes. But what about humans?

In 1901, Dr Robert Michael Forde, an early student of the London School of Tropical Medicine, but now a medical officer in The Gambia, found a parasite, which he thought was a worm, in the blood of a European, a Mr Kelly, the master of a government steamer on the River Gambia, who suffered an irregular fever and other symptoms. Coincidentally, Joseph Everett Dutton, an alumnus of the Liverpool School, was in The Gambia investigating endemic malaria. Dutton was shown Forde’s slides, and identified the organism as a trypanosome (fig. 5.3). He proposed the name *Trypanosoma gambiense*, and *trypanosoma fever* for the disease. Mr Kelly (fig.5.5) returned to England and was admitted to hospital in Liverpool.

Manson, Charles Daniels (at the time the superintendent of the London School, a medical man with considerable colonial experience), and Sambon, attending the British Medical Association meeting in Manchester in July 1902, used the opportunity to travel to Liverpool. They examined Mr Kelly and saw slides showing the trypanosomes in the blood. Shortly after, Manson was consulted about a patient, “Mrs S.,” the wife of a missionary from the Congo, who suffered from symptoms similar to Mr Kelly’s. At Manson’s suggestion, Drs Duncan Whyte and George Carmichael Low started to look for trypanosomes in the blood of Mrs S. They found them, eventually.
Manson spoke about Mrs S’s illness at a meeting of the Pathological Society of London, and a mention of this appeared in the _Lancet_ of 22 November, 1902. Fuller details were published the following year in the _British Medical Journal_.\textsuperscript{19} Besides these reports, an anonymous account appeared in the _Journal of Tropical Medicine_, a journal produced by the London School. After describing the illnesses of Mr Kelly and Mrs S, the author said that the presence of trypanosomes in two patients with similar and very grave symptoms must indicate that the parasite was an important pathogen, a new disease-germ for which pathologists had to be on the look-out. And if Africa, why not Tropical Asia and America? Perhaps the parasite explained some “anomalous chronic fevers”, such as kala-azar.\textsuperscript{20} This article was apparently picked up by a number of other publications, and re-published with errors.
The report of Manson’s lecture and the anonymous reports aroused the fury of the Liverpool School professorial establishment. Ross had just been awarded his Nobel Prize, Rubert (later Sir Rubert) Boyce, the head of the School, had recently been elected a Fellow of the Royal Society. The neurophysiologist Charles Sherrington would likewise later receive a knighthood (fig. 5.6.) These three wrote a strongly worded letter to the Lancer and the British Medical Journal complaining that both publications had failed to give any credit to Dutton. Sambon then admitted that he was the author of the anonymous account in the Journal of Tropical Medicine. All he was doing, he said, was bringing to notice another case of human trypanosomiasis; he did not mention Dutton’s name because Dutton had nothing to do with Manson’s case. He had published the account anonymously because “its object was not that of advertising my name.” Sambon should have stopped there, but could not help himself: “I am sorry that Major Ross, who has so strongly resented
Professor Grassi’s encroachments on his work, should now so easily set aside information of which he is evidently aware for the purpose of claiming for his own school the discovery of trypanosoma in man.” The information to which Sambon referred was the alleged discovery by Gustave Nepveu, a Frenchman, of trypanosomes in human blood. The findings, such as they were, had been published in a French journal in 1891 and 1898.24

Forde, who was at the time on leave in Worthing, thought that, while Boyce, Ross and Sherrington might think that Manson would not have considered trypanosomes had it not been for Dutton, he, Forde, could say that the case would have escaped Dutton’s notice had it not been for his, Forde’s, observations.25

The students of the London School also wrote to the British Medical Journal:

“We feel indignant at the tenour [sic] of the letter, which, by implication, asperses Drs Manson and Daniels with filching the credit due to other men for their discoveries… On the day on which the parasite was first found, it fell due that Dr Manson should lecture in due course. He did so, and began by referring to the case first clearing the ground by disclaiming all originality on his part. He told us that it was a case, the clinical features of which had been demonstrated to him at Liverpool which had put him on the track of the cause of the one under dispute. He recounted to us clearly the work which had been done at Liverpool in connexion with the parasite…and above all, did full justice to the Liverpool School.”

Manson chose not to respond to the Liverpool group’s accusations in print, but wrote privately to Ross, saying,

“I very much wish you had not been a cosignatory of that letter in the B.M.J. and Lancet of Saturday. It is absolutely wrong & unjust. I have not written a word about the trypanosome business. I have spoken of it in public three times – once at Liverpool, once at a laboratory meeting of the Path. Socy reported in last Lancet very
briefly, once at the School. Each time I fully acknowledged Dutton’s work & never once claimed any discovery for myself.”  

The Liverpool group responded to Sambon’s accusations by suggesting that Nepveu’s descriptions and drawings were so vague as to be useless, a statement with which even the French had to agree:

“Previously, Nepveu had called attention to the presence of trypanosomes in the blood of several patients from Algeria, but Nepveu’s descriptions and the drawings attached to one of his notes are so inexact as to cast doubt on his diagnosis. Furthermore, in the past years numerous blood tests have been performed, in Algeria, on a large number of patients, but trypanosomes have never been found.”[Author’s translation]。

Manson, however, as late as 1914, was still promoting Nepveu’s claim, although with some equivocation.

At about this time relations between Manson and Ross started to deteriorate, indirectly affecting Sambon. Mr Kelly died in January 1903, having developed cerebral symptoms.

By 1900 epidemic sleeping sickness had begun to endanger commercial interests in Uganda and the Congo, as thousands, and then tens of thousands, of men, women and children (fig. 5.7) fell ill with the fatal disease. Belatedly, the British Foreign Office asked the Royal Society in 1902 to organise a commission to study the disease in Uganda. Manson, a Fellow of the Society since 1900 was asked to nominate the members.

There are two stories about the formation of the group. Daniels said that there was no one at the London School he could recommend, except for Sambon, but he thought him “too theoretical for practical work.” Gerald Holroyde says that Sambon was invited to go, but that, because of his impending
marriage, he turned it down, suggesting instead his student and compatriot, Aldo Castellani (fig. 5.8). Sambon himself later said the same.  

Cook wrote that it was Sambon who suggested that Castellani apply for admission to the London School, though Castellani himself says otherwise: a family friend, Prospero Sonsino, a noted Italian parasitologist, had given him an introduction to Manson, who knew Sonsino well. Manson offered Castellani a place at the London School. Castellani, born in Florence in 1877, qualified in medicine in 1899. He studied bacteriology under Professor Walter Kruse in Bonn before coming to London to study tropical medicine. By a curious coincidence, Kruse had worked in Naples between 1889 and 1892, though whether he met Sambon is not known. Curiously,
despite their shared Italian background, Castellani makes no mention of Sambon by name in his autobiography, though he wrote disparagingly, “one of our lecturers [at the London School of Tropical Medicine] and a very clever one, taught us in all seriousness that sunstroke was a bacterial infection, which he called siriasis.”

Fig 5.8. Prospero Sonsino (1835-1901) and Aldo Castellani (1877-1971). Courtesy of Wellcome Images.

Castellani says that when Manson asked his class at the London School whether any wished to go to Uganda, the entire class volunteered. Manson then held an exam, and promised that the student with the best results would be chosen. The winner was Castellani. A Council meeting of the Royal Society on 20 May, 1902, therefore charged three men, Dr George Low, Dr Cuthbert Christy and Dr Castellani with the commission. Low had the entomological experience, Christy was a medical man and a gifted naturalist and Castellani was the microbiologist of the expedition.
The commission was not a success. Christy, abusive, truculent and prone to violence, resented the fact that he was not seen to be the leader of the expedition. On arrival at Mombasa railway station, he was so incensed at finding the door to their compartment labelled “Dr Low and Party” that he punched Low. Once the party arrived in Entebbe, he spent a great deal of time big-game hunting. Castellani thought that sleeping sickness was due to a streptococcus he found in the cerebro-spinal fluid of some patients. Low agreed, and wrote to the Royal Society asking for permission to publish the results. The Royal Society recommended that the results should not be published, yet. Later in the year, on 12 November, 1902 Castellani found a trypanosome in spinal fluid, but seems to have ignored his finding. As he had found trypanosomes in the blood of some patients, he worried that others might think the trypanosome he found in the spinal fluid was a contaminant from blood, but he continued to look.

The Royal Society, aware of increasing discord, sent Bruce and Dr David Nabarro, a bacteriologist, to sort things out. By the time they arrived in Uganda, on 16 March 1903, only Castellani was there to greet them; and even he was displeased with the Royal Society’s decision to delay publication of his findings. Low had returned early, having concluded, as had others by then, that *Filaria perstans* had nothing to do with sleeping sickness. Christy was making his way home. He travelled up the Nile, in accordance with the Royal Society’s instructions. Castellani’s paper on the streptococcal origins of sleeping sickness was read at a meeting of the Royal Society and was published in the *Lancet* on 14 March, 1903.

Castellani now told Bruce, on condition that he said nothing to Nabarro, that he had found trypanosomes in the spinal fluid of five out of fifteen patients. Bruce, by his own account, immediately recognised the significance of the finding and urged Castellani to look further. He then went on to show that the
parasites could be found in the spinal fluid of twenty out of thirty four patients.

Castellani could not quite let go of the streptococcus, and in his report suggested that, late in the disease, the streptococcus might play a part. Bruce believed that the trypanosome found in Uganda was the same as the one described by Dutton in Mr Kelly, though Kruse suggested otherwise, and recommended the name *Trypanosoma castellani*. Cantlie, as editor of the *Journal of Tropical Diseases* praised Castellani’s work, and argued for a close study of possible different ailments “induced by the possible varieties of the parasite.”

In May 1903, Bruce and Nabarro were joined by Captain E. D. W. Greig, of the Indian Medical Service, a bacteriologist, who had been sent to Uganda by the Indian Government to study sleeping sickness.

Bruce and Nabarro, confirming Castellani’s findings, reported the presence of trypanosomes not only in the spinal fluid, but also in the blood of sleeping sickness patients. This immediately raised the question of the relationship between Dutton’s “trypanosoma fever” and sleeping sickness.

At the Swansea meeting of the British Medical Association (a tropical medicine section had been included only after some vigorous lobbying by the faculty) in July 1903, the subject of trypanosomiasis was thoroughly discussed. Castellani’s findings were mentioned but received scant attention, only Sambon supporting them. He based his opinion on the many analogies that existed between sleeping sickness and the trypanosomal diseases of other mammals.

However, in November 1903, Bruce and Nabarro reported two patients suffering from trypanosoma fever who, after a short while, developed cerebral
symptoms, whose onset coincided with the first detection of the parasite in the spinal fluid.

Bruce would soon cross swords with the two Italians, Castellani and Sambon.

Manson had also kept a close eye on his patient, Mrs S., with trypanosoma fever. In December, 1903, he reported that she had died, after two months of neurological deterioration. He suggested that this proved that the trypanosome was the cause of sleeping sickness.43

Sambon reported that the trypanosomes in her blood were identical with those that Castellani had found in the spinal fluid (fig.5.9) of his African patients. The pieces of the puzzle were falling into place.

Fig.5.9 Trypanosoma castellani (Kruse) From the Journal of Tropical Medicine, 1903, page 249.

Which scientist, or scientists, should have the credit for a particular discovery was a question that vexed many members of the tropical medicine community at a time when new discoveries were constantly being made. International
rivalries, such as the Ross-Grassi controversy, were particularly bitter. Now Bruce and Castellani would bicker about who discovered the role of the trypanosome in the aetiology of sleeping sickness. The Bruce-Castellani debate was kept alive by the large figure and personality of E. Ray Lankester, (fig. 5.10) a prominent zoologist who had been a member of the Royal Society committees responsible for the sleeping sickness commissions. At a meeting of the Epidemiological Society in December 1904 (see below), he stated for the first time that Castellani could not be considered the discoverer of
the role of the trypanosome in sleeping sickness. He repeated the claim in his *The Kingdom of Man*, published in 1907, attributing the discovery of the role of the trypanosome to Bruce. In a letter published in *The Times* of 14 August, 1913 Lankester said that Castellani, when telling Bruce in Entebbe that he had seen trypanosomes “did not consider them to be of any importance as regards sleeping sickness.” This statement was borne out by Dr Robert Unwin Moffat, Principal Medical Officer in Uganda when Castellani was conducting his investigation. Castellani allegedly told Moffat that he considered the trypanosomes he had found in sleeping sickness patients examples of “accidental parasitism”. Lankester repeated his claim in a letter to the *British Medical Journal* in 1917. Relations between Bruce and Nabarro, who accused Bruce of ignoring the share of the work done by him, Nabarro, in Uganda had deteriorated. Not surprisingly, Nabarro championed Castellani’s claim, and categorised Lankester’s claim as “grotesque.” Eventually, Castellani was acknowledged as the discoverer of the cause of sleeping sickness, but the episode was unedifying.

Sambon, never averse to a dispute, read the literature on sleeping sickness with great interest. Despite having no particular expertise in the area, he wrote a long article about the disease. It appeared, in July 1903, in the *Journal of Tropical Medicine*, where he had recently been made assistant editor. This appointment had irritated George Nuttall, an Anglo-American microbiologist and the founder and editor of the *Journal of Hygiene* who wrote to Ross that “Sambon practically runs the whole show now, we might by uniting change one aspect of things.”

In his article, Sambon described the symptoms and the morbid anatomy of sleeping sickness before turning to its epidemiology. He noted the association with streams and water and believed that the cause was the trypanosome discovered by Castellani. He knew that Bruce (and many others) considered
that nagana was spread by tsetse flies. (fig.5.11). Bruce however believed that the transmission was purely mechanical. Not so, wrote Sambon. If transmission was purely mechanical, there was no reason why other blood sucking insects could not transmit the disease. Yet Bruce agreed that nagana was limited to the “fly-belt.” Sambon believed further investigation would show that trypanosomes had a double-cycle life history, one in mammals and

![Image of Glossina morsitans, a tsetse fly, drawn by Terzi. Courtesy of Wellcome Images.](image)

the other in the fly. He wondered whether the infected adult fly might pass on the infection to its progeny, as was known to be the case with certain tick borne diseases, but could offer no definite conclusions. He also speculated that the association of the fly with water was because the tsetse flies fed on fish. To rub further salt into the wound, Sambon read a paper on sleeping sickness at a meeting of the Epidemiological Society of London on 12 December, 1903. It consisted largely of a repeat of his *Journal of Tropical Medicine* article. Most discussion was held over till January, 1904.

Meanwhile, Ross wrote a scathing letter to Dr Herbert Timbrell Bulstrode, Honorary Secretary of the Epidemiological Society, about Sambon’s
qualifications to criticise. In the January discussions, Bruce told Bulstrode that Sambon’s paper ‘teemed’ with inaccuracies,’ and refused to participate in the discussions.

Sambon accepted that the disease distribution coincided with that of the flies, but again argued that, if transmission was merely mechanical, other biting flies could easily transmit the disease, but this did not appear to be the case. Furthermore, his careful study of the morphology of human trypanosomes led him to conjecture that a sexual stage existed, and that this probably occurred within the fly, 55 a hypothesis with which Manson later agreed. Bruce was not amused, and after the meeting wrote to Ross that Sambon had been “quite impenitent.” 56

At the British Medical Association meeting held in Oxford at the end of July 1904, Bruce stated firmly that transmission was purely mechanical. There was, he insisted, no proof that the trypanosomes metamorphosed in any way in the tsetse fly. Sambon, who was at the meeting, said that he disagreed, and insisted on his argument about other biting insects. Bruce’s reply—if there was one—was not printed.

Meanwhile, Greig continued the work of the Royal Society’s Sleeping Sickness commission, with the assistance of Lieutenant Gray, RAMC. In 1905, they were joined by Lieutenant Forbes Tulloch, RAMC. At the same time, the Liverpool School, at the invitation of King Leopold II, sent out a team to investigate trypanosomiasis in the Congo. This consisted of Dutton, Christy, recently returned from Uganda, and John Lancelot Todd, (1876-1949) a Canadian physician and parasitologist. Christy returned to England in June 1904. Dutton and Todd continued into the interior of the Congo, and in the course of their investigations, found the spirochaete that causes Relapsing Fever, and showed how it was transmitted by species of soft ticks. Unfortunately, both men developed the disease. Todd recovered, but Dutton,
after a severe relapse, died in February 1905. Ross in his obituary wrote of the Knights of Science, and labelled Dutton the Galahad of the group of young men who were advancing the cause of tropical medical science.\textsuperscript{57}

The Knights of Science would soon lose another of their members. Twenty-seven year old Tulloch accidentally cut his hand while dissecting an infected rat in Entebbe, Uganda. He developed a rapidly progressive trypanosomal infection, and died at the Millbank Military Hospital on 20 June, 1906. These young men were neither the first, nor would they be the last, to die while investigating the diseases of the tropical world.

An International Conference on Sleeping Sickness was held in London in June 1907. Sambon attended as an official delegate of the Italian government. The British government introduced three proposals, and offered to meet the costs of the second:

1. An annual or biennial conference of delegates from countries interested in the disease.
2. The establishment of a central bureau (in London) to extract and circulate all new literature on the subject.
3. To assign particular topics for investigation and reporting by particular countries or individuals.

The first and second proposals were accepted, but the chief German delegate, Herr von Jacobs, objected to the third, insisting that the delegates had no diplomatic authority for such actions. At a further conference the following March, the idea of a central bureau in London now also came under attack, following the international agreement of December 1907 to establish an \textit{Office International d'Hygiène Publique} (International Office of Public Health) in Paris. The French and Italian delegates, no longer including Sambon, wanted the proposed sleeping sickness bureau to be part of the Paris office. The conference ended without any recommendations. In one of his last acts as
Secretary of State for the Colonies, Lord Elgin decided to establish a sleeping sickness bureau in London. This was done in 1908, with the assistance and cooperation of the Royal Society and funding from the Imperial and Sudanese Governments.\textsuperscript{58}

Sambon made it a point to attend as many of the annual meetings of the British Medical Association as he could. He spoke frequently. For the 1908 Sheffield meeting, much to his pleasure, he was appointed a Vice-President of the Tropical Medicine section. Havelock Charles was the president. If Sambon hoped that the following year he would be president, his hopes were dashed when Charles Daniels was appointed.

Sambon’s hypothesis that transmission of the trypanosome was not merely mechanical was proved correct in 1909 by the German military doctor, Friedrich Karl Kleine,\textsuperscript{59} a finding confirmed a year later by the protozoologist Muriel Robertson (1883-1973). The remarkable Miss Robertson\textsuperscript{60} also showed that the trypanosomes migrated from the gut of the tsetse flies to the salivary glands. Whether Bruce ever admitted that Sambon was correct is not recorded, but seems unlikely. Bruce, however, continued to maintain that sleeping sickness trypanosomes did not undergo any metamorphosis in the tsetse fly, a point vigorously contested by Sambon at the British Medical Association meeting held in London in July 1910.\textsuperscript{61}

The noise that Sambon was making in 1903 brought him to the attention of another giant who was taking a particular interest in the problems of tropical health. Henry Wellcome, an aggressive collector of many things, had since 1895 been the sole owner of the pharmaceutical company, Burroughs Wellcome & Co.
Chapter 6. Wellcome adventures

The modern pharmaceutical industry owes much of its success to nineteenth century American technical ingenuity. Mechanisation and standardisation were combined in the improved rotary tablet press developed in 1872 by John Wyeth’s employee, Henry Bower, that transformed the production of pills and tablets.¹ Wyeth sent one of his best, but most unmanageable, salesmen, Silas Burroughs, to England in 1878 to set up as an independent agent for Wyeth’s goods. Three tablet presses followed in 1882;² but Burroughs had already begun a side-line, trade-marking and selling his own products, many derived from British sources.

Henry Wellcome, (fig. 6.1) like Burroughs a graduate of the Philadelphia College of Pharmacy, probably met Burroughs in 1877. In 1880, Burroughs

Fig. 6.1 Henry Wellcome. Courtesy of Wellcome Images
persuaded Wellcome to come to London to establish a joint business. Despite increasing discord between the senior partners, Burroughs Wellcome & Co Ltd flourished through the use of aggressive American marketing strategies. Silas Burrough died from pneumonia in February 1895. Henry Wellcome should quickly have become the sole, and extremely wealthy, proprietor of the Burroughs Wellcome company. However, Olive Burrough, Silas’ widow, who disliked Wellcome intensely, put so many legal obstacles in his way that it was not until July 1898 that a court order made him the sole proprietor of Burroughs Wellcome. He could now use his considerable wealth to indulge his private pastime to its full obsessional extent.³

Wellcome was a collector of medical (and other) curiosities. His principal curator was Charles John Samuel Thompson, who had studied pharmacy in Liverpool, and whose Practical Dispensing for Students, Pharmaceutical and Medical, was already into its second edition (“enlarged” of course) in 1891. In 1903 Wellcome started planning a major exhibition of medical history,⁴ and for this he needed more artefacts. He had seen, and was very interested in the Oppenheimer Collection of Italian antiquities, formed by the Sambons, and exhibited at the International Congress of Medicine held in Rome in 1894 and at the British Medical Association meeting in 1895. In October 1903, Wellcome invited Sambon to a meeting “for a little chat regarding his proposed historical medical exhibition.” Sambon in reply suggested that Wellcome come to lunch at his house in Barnes. Wellcome, allegedly under pressure of urgent and important work, declined.

The two men finally met in December 1903. Sambon readily accepted Wellcome’s invitation to work for him, initially for a period of six months. The offer came with a generous honorarium of £35 a month for the time spent working for Wellcome, a useful addition to the salary he received at the London School.⁵
Among the first tasks given to Sambon was the acquisition of the Oppenheimer collection. Wellcome, always passionate about secrecy, insisted that “it would be undesirable to let Mr O [sic] gain any idea that your negotiations have anything to do with me, or the exhibition.” Unsurprisingly, nothing came of Sambon’s earliest overtures. Wellcome would have to wait until 1910, when he was approached to see if he was still interested in the collection. Cambridge University had allegedly offered £2,000 some years earlier, but the price had now dropped to £1,000. Sambon advised Wellcome about the contents of the collection and about its value. In November, 1910, Wellcome bought the lot for £375, after hard bargaining, and deception, by Thompson.

After the failure of the early Oppenheimer negotiations, Wellcome asked Sambon to travel to France and Italy to see what he could find for the exhibition. Aleardo Terzi, Amedeo’s older brother, was taken on as artist, at a fee of £6 per week, plus a daily rate of ten francs (about eight shillings at the time) for expenses. Wellcome stipulated that all Sambon’s and Terzi’s notes, drawings, photographs, sketches and any other material gathered for the exhibition were to be solely the property of Wellcome. Neither man was to make any notes, drawings, photographs, or sketches for his own use or for communication to others.

Italian bureaucracy required a photograph of Sambon, but he delayed. The trait would cause endless problems in later years. Sambon told Wellcome that he would be ready to leave by the first of March, but again, he was not ready.

The trip started on 1 April, 1904. At the Hôtel Dieu Hospital in Lyon, Sambon was startled to discover that the nuns who acted as the hospital pharmacists were still making that distinctly pre-Christian elixir, theriac (“theriaque.”) Theriac, according to Galen’s recipe, contained at least fifty-four different ingredients, including powdered viper! Over the years the recipe was changed
to suit local conditions, though the main active ingredient, opium, remained. William Bullein, (circa 1515-1576)\(^9\) an English physician and cleric, recommended the use of mummy parts in his recipe for theriac, since he believed that mummies were embalmed with precious ointments and spices, including myrrh, saffron and aloe. In 1745, William Heberden wrote an article denouncing the use of theriac, though it was not until 1788 that it was removed from English pharmacopoeias. In France its use continued. The 1866 Codex still included powdered viper, though this had disappeared by the 1884 edition, by which time it still consisted of fifty-six substances.\(^{10}\) By 1908, theriac had disappeared from the French pharmacopoeia.

The hospital itself Sambon described as a terrible place. As the façade was being restored, he shocked the concierge by suggesting that Dante’s words for a similar place should be inscribed above it.\(^{11}\)

In the Palais St Pierre (the Museum of Antiquities) in Lyon, he came across a “most interesting and magnificent high relief in painted wood representing an invocation from the clergy and the people for delivery from the plague.” Terzi’s watercolour of this relief (fig.6.2) shows his skill as an illustrator. Yet Wellcome was so persistently dissatisfied with Aleardo’s performance in copying an illustration of *Fabbricazione della Teriaca* (Making Theriac) that Aleardo dismissed himself in May 1905.\(^{12}\)

Sambon wrote to Thompson from Milan, describing some of his experiences during this trip:

“We left Marseilles on the 16\(^{th}\) [April, 1904] after midnight and travelled straight through to Milan. It was somewhat tantalising to have to pass Nice where we had relations, Cannes where we had friends, and Monte Carlo where we might have made a fortune, but our goal was Italy, our object the history of medicine- we partook of an abominable and most expensive luncheon at the station of Geneva [the Porta Genova station in Milan] and were
literally poisoned by some mushrooms which were served with the meat. I was obliged to
spend the 18th in bed and the night preceding mostly out of bed.” 13

Fig.6.2 Aleardo Terzi. Watercolour The people of Lyon receiving deliverance from the
plague. Courtesy of Wellcome Images.

Sambon also wrote disparagingly of French officials, who had little interest in
helping strangers. There were endless delays, in both France and Italy, which
meant that he would be unable to be in London in May for the start of the
second session of the London School. He had written to Manson, who was
away. Receiving no answer, he wrote to Low asking him if he would do his
lectures, and offering to pay Low a guinea for each lecture. Again receiving
no answer, at the end of May, he asked Wellcome to intervene on his behalf,
but Wellcome replied that he had no wish to interfere in Sambon’s relations
with the school.14

In Milan, he visited his father, who had an impressive collection of
antiquarian medical memorabilia. Sambon told Wellcome that he would try to
persuade his father (successfully, in the event) to lend some of the material for
Wellcome’s great exhibition.
Sambon returned to England at the beginning of July, but his work with Wellcome continued until 22 October, 1904. Hilda, now far advanced in her pregnancy, was unwell, and for a fortnight in August, he had to give up work. On 15 August, 1904, his daughter Laura Antoinette was born (“after a rather bad confinement.”) Three years later Juliet Edith was born followed in 1911 by Mabel Emily and, in December 1914, by his son Julius Arthur.15 Sambon returned to Europe at the end of November 1904, but had to travel to Paris when Jeanne, the wife of his brother Arthur, was diagnosed there with a malignant uterine tumour. He lightened the burden by investigating what the Paris curio dealers had to offer.

Wellcome himself was always on the look-out for artefacts during his many tours. He particularly enjoyed ferreting around in small shops in the less salubrious quarters, where bargains might, he hoped, be obtained.16 In May, 1905, Thompson wrote to Sambon, describing “a curio shop at the commencement of the Rue de Lorette” in which on an earlier occasion Wellcome had seen some surgical instruments. The shop, Thompson said, was “a few doors up on the right hand side from the Rue de Chateaudun and opposite the Church de Lorette.” Wrong directions, Sambon replied, and despite a diligent search he was unable to find any instruments.

Sambon seemed happy enough at the way things were going with Wellcome, though he found Wellcome’s insistence on regular reports irksome. In later years, a postcard, giving few details, instead of the typed letters Wellcome expected, was often all he would manage. When he did write, it was to emphasise how hard he was working, or how difficult things were. He wrote from Paris in April 1908,

“All libraries and museums were closed, all those who could have given me useful information were away from town…So I walked and walked until I had the feu de St
Antoine in my feet I looked at thousands and thousands of prints until I had the feu de St Main in my fingers, but found nothing.”

Thompson would later point out a flaw in Sambon’s character:

“From a long observation of Dr Sambon’s manner of work, I may point out that his chief drawback is a lack of staying power, and no method whatsoever, and there is the greatest difficulty in keeping him concentrated on any one branch of work, even for a few days. It is in these points that failure is to be expected in carrying the investigation to a successful conclusion.”

Sambon generally promised to try to do better in the future, but his promises wore thin.

However, a dark cloud was gathering. Wellcome had offered Thompson the opportunity of travelling to the continent, an offer that was much appreciated by Thompson. Thompson, Wellcome said, would accompany Sambon, much to the latter’s irritation.

“I have great respect for Mr Thompson, and I do not think you could place your confidence in better hands, but I must say it is rather difficult for a man of my qualifications to accept the conditions you wish to impose, especially as the work we are desired to carry out is essentially medical. Then again, you must remember it is not the first time I undertake such work, because in 1894 I was appointed by the Italian Government to organise the first exhibition of the kind which was ever held. Since then, I have had the honour of carrying out two important medical missions for the Italian War Office… Under the altered circumstances I am very reluctantly obliged to give up a line of work which interests me exceedingly…”

He was persuaded, probably with little difficulty, to carry on. The London School, however, was starting to be unhappy at the amount of time he was spending away from the school.
As a medical student in Italy, Sambon had learned a great deal about the
effects of pellagra, a disease thought by the Italians to be associated in some
way with the consumption of maize, or Indian corn. In mid-1905 he wrote an
article reviewing previous work and speculation. In its closing paragraph,
almost as an afterthought, he wrote, “If I were asked to suggest a new theory
of pellagra, merely as a working hypothesis, I should feel inclined to draw
attention to the many analogies between pellagra and some of the protozoal
diseases which have been recently worked out.” The article was published in
the British Medical Journal on 11 November, 1905.

Sambon now told Wellcome that the School management committee would
have no objections to his visiting continental Europe if he was also permitted
“to carry out efficiently an investigation into the etiology of pellagra…”
Wellcome concurred.

Wellcome was never an easy task-master. He saw Sambon and, later other
associates merely as collectors, while the intellectual content associated with
the collections remained his property. He did his best to restrict attributions in
any publications associated with either the company or the collection. Even
Thompson, who fancied himself as a novelist, wrote under a nom de plume.
He was forced to resign in 1927 after breaking his contract with Wellcome by
accepting royalties (which, however, had never been paid) from one of his
books.

The historical collection grew larger and larger. In 1911, Wellcome bought a
London property to house it. This property, 54a, Wigmore Street, was next
door to the main Burroughs Wellcome & Co showroom. The Historical
Medical Museum opened on 29 June, 1913 as an adjunct to the 17th
International Congress of Medicine, held in London in August that year. At
the opening address at the museum, Dr (later Sir) Norman Moore commented
specifically that the arrangements for the museum had been admirably carried
out by Thompson and Sambon. Wellcome in reply “made graceful reference…to the invaluable assistance given by Mr C J S Thompson, Dr L Westenra Sambon and other members of his staff.”22 In the account published later by the Wellcome Historical Museum, the names of “his staff” are omitted altogether.

Relations with Wellcome became increasingly difficult. The Great War put a temporary halt to Sambon’s role, which continued only sporadically after the Armistice, then petered out altogether. Sambon was, however, involved in one final coup for Henry Wellcome.

The Italian lyric tenor Evangelista Gorga (1865-1957) had given up professional singing in 1899 to concentrate on his obsession with collecting ancient artefacts. By the time Sambon came to hear of the collection in February 1912, Gorga had accumulated an immense quantity of artefacts, which included numerous Greco-Roman medical items. Wellcome was desperate to get his hands on the collection. Early negotiations broke down, the war intervened, and it was only in 1924 that Wellcome, with some help from Sambon, managed to acquire some of the collection.23

Sambon also took the opportunities offered by Henry Wellcome to enlarge his own collections of ancient books, manuscripts, and items of Roman culinary and medical interest. In 1923, a visitor described Sambon’s house in Fordwyck Road, Hampstead as having something of the nature of a museum:

“Grotesque wooden idols from wildest Africa winked and goggled at porcelain damsels of rare beauty, some in Dresden flowery gowns, others in Capo-di-Monte deshabille. There were finely cracked Satzuma vases decorated with birds and chrysanthemums of exquisite colouring, and Campanian fish-plates of the third century BC, marble and terracotta votive offerings of medical interest from the Capuan Temple of Maternity and the Tiberine Aesclepieion, and Phoenician glasses and Brazilian Morpho butterflies vieing in the splendour of their colouring…Among
bronze surgical instruments, found near Lake Trasimenus, there was a —lady’s hairpin! The lancets and probes and forceps were handmade, beautifully shaped and well-adapted to their use.²⁴

After Sambon’s death in 1931, his books were sold at auction for £1,695 (equivalent today to just over £100,000.)²⁵

Despite the amount of time Sambon was devoting to the Wellcome collections, his prime loyalty remained to Patrick Manson, and he never forgot this. He worked hard at his parasitology, describing and naming new species. Some of this work caused controversy. One spat arose over the nature of the lateral spined eggs of the trematode that is today called *Schistosoma mansoni.*
Chapter 7. Taxonomic battles

Until 1903, it was generally believed, despite evidence to the contrary, that there was only one species of human schistosome, the *Schistosoma haematobium*. In that year Sir Patrick Manson, having previously seen schistosome eggs in the faeces of a patient from the West Indies,\(^1\) proposed, on epidemiologic and clinical grounds, that there might be two species.\(^2\) Sambon, the faithful retainer, determined to prove Manson right, and in the process raised further perturbations in the field of tropical medicine.

The trematodes, or flukes, are flat worms which have complicated lifecycles involving aquatic snails. Most of the human flukes are hermaphroditic and live in the intestinal tract. The schistosomes are different, being dioecious\(^3\) and living in the portal vein and its tributaries.

In 1850, Theodor Bilharz (fig. 7.1) a young German physician about to set

![Fig. 6.1. Theodor Maximilian Bilharz. Courtesy of Wellcome Images.](image)
off for Egypt, asked the zoologist Carl von Siebold to recommend him a speciality into which he should direct his energies. Human worms, von Siebold suggested. In Egypt, Bilharz conducted numerous autopsies and duly came across many varieties of intestinal worm. Since worms had never been found within blood vessels, Bilharz was astonished when, in 1851, he found a worm in a branch of the portal vein. He called this worm *Distomum*, because it seemed to have two mouths, and classified it among the *trematodes*, or flukes. Even more astonishing, he also discovered that these worms were not, as previously discovered trematodes were known to be, hermaphroditic, but were dioecious (fig. 7.2). In August 1852, Bilharz told von Siebold that he

![Fig. 7.2](image.png)
had seen a young man who had haematuria (blood in the urine); that the urine contained eggs; that these eggs had a ‘spine’ at one end; and that they came from his *distoma* worms.

Haematuria is described in the Ebers Papyrus. In the early years of the twentieth century, calcified eggs of urinary schistosomes were found in mummies dating from the Egyptian Twentieth Dynasty, (about 19190 BCE to 1075 BCE), or about 3,000 years ago.⁵

In 1864, Dr John Harley, assistant-physician to King’s College Hospital and the London Fever Hospital, demonstrated the presence of terminal-spined eggs in the urine of a young man from South Africa who suffered from haematuria.⁶ In a further communication, Harley quoted a Dr Rubidge, of Port Elizabeth, who said “Pretty extensive enquiries lead me to believe that bathing in rivers has something to do with the production of the disease.”

After commenting that boys who bathed only in the sea did not suffer from haematuria, while those who bathed in the rivers did, Rubidge added, “My impression is that the parasite gains entrance into the skin while the individual is bathing in the river, and I may mention that the lad Jones…described a sort of urticarious eruption attended with great irritation, as a frequent result of bathing in Booker’s River.”⁷

Not everyone agreed: as late as 1882, Thomas Spencer Cobbold (1828-1885), the foremost parasitologist in England, believed that sheep became infected with liver flukes (*Fasciola hepatica*) by drinking the cercariae which emerged from infected snails and thought that the same applied to the schistosomes. Prevention therefore lay in using uncontaminated water, from deep wells, for example, or water that had been filtered or boiled for consumption. He thought that bathing might be a source of infection, but was a rare one.⁸
Later discoveries confirmed that the sexual phase of the fluke’s life cycle takes place in the human, while intermediate asexual stages occur in target species of aquatic snail. The *cercariae*, latter-phase tadpole like-larvae that emerge from the snail, swim freely until they find and penetrate the skin of the human host. This penetration is now known to be associated with itching, a condition that, although sometimes called “swimmer’s itch,” might in all fairness be called “Rubidge’s itch.”

Bilharz had also found eggs that had a spine on the side, rather than at the end. Prospero Sonsino, whom we have met as Castellani’s sponsor, thought initially that these lateral-spined must represent a different species, but later changed his mind, concluding that one form of egg developed into male flukes, the other into female flukes. Later clinical studies showed that the two types of egg were associated with different sites and different clinical symptoms. The terminal spined egg, found only in the urine, was associated with haematuria and, eventually, renal failure; the lateral spined egg, found only in faeces, was associated with liver disease.

Bilharz did not survive to further his investigations into the *Distoma* fluke. He died in Egypt, from typhus, at the age of thirty-seven. The name *Distoma* for the fluke was later changed, in his honour, to *Bilharzia*, but subsequent preference was given to the name by which it is known today, *Schistosoma* which alludes to the apparent cleft in the male’s body.

Manson’s 1902 patient was a 38 year old Englishman who had spent 15 years in a number of West Indian Islands. Examination of his stools (Manson thought he might be suffering from hookworm disease) showed the lateral-spined bilharzia ova. No abnormalities were found in the urine. In the third Edition of his *Tropical Diseases A Manual of Diseases of Warm Climates*, Manson suggested that there might be two species of schistosomes, “one, with lateral-spined ova, depositing its eggs in the rectum only; the other haunting
Subsequently a number of reports from Puerto Rico and the West Indies showed that lateral-spined eggs could be found in faeces, but no terminal-spined eggs could be found in urine or elsewhere.

Sambon saw immediate possibilities in the challenge. By careful study of the eggs, he concluded in 1907 that there were in fact two distinct species. At a meeting of the Zoological Society of London on 19 March, 1907, he reported,

“In the Congo Free State, in other parts of Africa, and in the West Indies there is a form of Bilharziasis clinically and pathologically similar to the Asiatic form caused by Schistosomum japonicum, and unlike the classic East African form due to S. haematobium. The eggs of the species which causes this peculiar form are never found in the urine, but seem to be eliminated through the intestine only. They differ from those of S. haematobium in having a broad lateral spine totally different in size, shape and position from the small, straight, terminal spine which characterises the ova of S. haematobium. Hitherto, the laterally spined ova, usually observed in Egypt in cases of mixed infection, have been looked upon as having been distorted while passing through the rectal mucosa. Sir Patrick Manson suggested several years ago that the laterally spined ova found in the faeces of patients, and never in the urine, might represent a new species. In appreciation of this, one of his many genial intuitions, the new species is dedicated to him.”

He later added further detail of geographical and pathological differences, to justify his adoption of a new species. His suggestion received support from Passed Assistant Surgeon R. C. Holscomb, who wrote a detailed report in July 1907, which included the statement:

“The fact that the West Indian Islands— Martinique, Antiqua, Vieques, Culebra, Porto Rico, and Cuba — and the isthmus of Panama, Venezuela and Brazil have so far shown the intestinal type almost exclusively would appear to be good evidence to show that the Schistosomum mansoni is a species distinct from the Schistosomum
hematobium, and that the former has found in these localities the conditions favourable for its dissemination.”

Arthur Looss, a well-known and much respected German parasitologist then working in Egypt, disagreed with Sambon’s opinion. Many years later, his obituary in the *British Medical Journal* would say that, despite being in private life a man of a simple and lovable character, Looss had a peculiarly dogmatic manner and an acrid controversial style. Sambon, who in 1906 had been appointed a part-time parasitologist at the Royal Institute of Public Health,—a position that from lack of financial support lasted for only four years—was about to be on the receiving end of just such acridity.

Loos visited both London and Liverpool in 1906. He met Manson and Sambon in London, where, he said later, “I dropped some well-meant hints of warning to be cautious, whether to Sir Patrick Manson or to Dr Sambon I do not remember.” In Liverpool he met Ross, who, in addition to his other duties, was about to become editor of the Liverpool School’s new journal, the *Annals of Tropical Medicine and Parasitology*. Whether they discussed Sambon or his ideas is not recorded, but Looss used the new journal to launch his counterattack on Sambon’s hypothesis, even though the Zoological Society’s and the London School’s journals had both carried Sambon’s hypothesis. Looss called his lengthy article “What is “Schistosoma Mansoni,” Sambon, 1907”, and, like Ross started his assault against Sambon’s position by attacking Sambon’s credentials and way of proceeding:

“Among scientific workers, it is a good custom that anyone who believes he has made a new discovery also takes the trouble to prove it; it is not customary among scientists to assert something then call for the help of others to establish it.”

Looss did not believe that it was possible to identify a new species merely from the eggs, the adults also had to be examined. Furthermore, he insisted that he and Bilharz had seen both types of eggs, terminal-spined and lateral-
spined, in the same female worm. He believed that the lateral spined eggs were abnormal variants, possibly produced by mature but unimpregnated female worms.\textsuperscript{16} He thought he had clinched the matter by his confident assertion that for many years it was known that both terminal and lateral-spined eggs could be found in the same female worm. He even questioned whether an intermediate host was needed. And to explain the clinical differences between the West Indies and Egypt, he developed the curious hypothesis that these differences were based on the number of infecting larvae that entered the human host.

Looss was supported by a number of researchers, some of whom had worked with him in Egypt. Andrew Balfour, the director of the Wellcome Research Laboratories in Khartoum saw Looss’s vast experience and reputation as a reason for tending to agree with him.

Both Carmichael Low and Fleming Mant Sandwith, a physician with long experience of Egypt, and, since 1904, a lecturer at the London School of Tropical Medicine, also supported Looss.

Robert Leiper had been appointed lecturer in helminthology to the London School in 1905. In 1906 and 1907, he had spent a considerable amount of time in Egypt, studying the schistosomes with Looss. He found himself in a difficult position. Writing to Manuel Pirajá da Silva—a Brazilian researcher who, in 1908, reported subtle distinctions between adult mansoni and haematobium worms—that “There is a good deal too much theory alike in Looss’ as in Sambon’s position. I mean they require more facts,” he added that, having worked with Looss, he felt he could rely on his observations slightly more than on Sambons.\textsuperscript{17} The clinicians were more certain. Both Manson (1907) and Castellani (1910) placed Schistosoma mansoni as a separate species in their textbooks, though the Germans remained ambivalent. In their 1912 edition of \textit{Tropical Diseases and Tropical Hygiene}, Ruge and
zur Verth say that “it has not yet been determined if the eggs with its lateral spine belong to a special variety.”

Sambon replied to Looss’s arguments in January 1909. He rather impudently gave his paper the same title as Looss had used six months earlier:

“Under the above title…Professor A. Looss, the distinguished helminthologist of the Cairo School of Medicine, published a long critical article. The article was violent and perhaps ill-considered, notwithstanding that many months had elapsed since the reading of the paper it attacks. I ought to have refuted it at once, but being very certain of the position I had taken, I thought it wiser to leave my justification to time rather than to launch out in angry reprisals. I felt sure that either Professor Looss himself, or some other investigator, with material at hand, would soon be able to describe those structural details of the adult *Schistosoma mansoni* which, according to my learned critic, are indispensable to establish its specificness. However, such descriptions not having appeared, and having myself been unable to obtain the necessary material, and fearing lest my silence should be wrongly interpreted, I have now decided to refute Professor Looss’ objections, and show that I had very good reasons to establish a new species for the laterally-spined-ovum-producing agent of intestinal bilharziosis…My regard for the eminent scientists who have studied the parasitology of Egypt for the past half century is very great indeed, but this regard need not interfere with criticism, whether of observation or inference. Respect to authority is one thing, slavish submission to authority is another thing.”

Sambon pointed out that different schistosomes were known to produce different eggs, presupposing differences in the structure and positions of the egg-forming organs (fig. 7. 3). Furthermore, the fluke that produced the lateral-spined egg never affected the bladder, and the eggs were only found in the intestine and liver. That fluke was found in the West Indies and South America, where *S. haematobium* was known not to exist. He said that he would only accept Looss’s assertion that both types of eggs could be found in
the same female worm when he had himself examined a specimen showing this anomaly. He pointed out that Looss himself had established two different

![Fig. 7.3 Showing the different shapes of eggs from different species of Schistosome. From Sambon, (reference 13).](image)

species of both Opisthorchis and Clonorchis (species of liver fluke) on the basis of minor zoological or geographical differences. He challenged Looss’s hypothesis that an intermediate host was not required, pointing out that the peculiar topographical distribution of the various schistosomes, and their proximity to rivers and pools, suggested the need for an aquatic intermediate host. It was a well-argued case, presented in an authoritative manner. He concluded:

“Before concluding I would like to say a word in deprecation of the style Professor Looss has chosen to adopt in criticizing my paper, and my standing as a
helminthologist. I never for a moment placed myself on the same level in the latter respect with the celebrated Professor of Cairo, but at the same time I would say that I have paid some attention to the subject, and that I cannot abandon my independence of judgment or my right to give expression to my views.”

By now Leiper was just beginning to move towards Sambon’s position, agreeing that the lateral-spined egg was only found in the rectum. Furthermore, he noted that male schistosomes could be divided into two groups: those with four, rather large and angular testes, and those with seven to nine small, round testes. In one conjoined specimen, he found evidence of the association of a male with seven testes and a female with lateral-spined eggs.20 Despite the substantial material Sambon produced, Looss returned to the fray in 1911, repeating much of what he had said earlier, but now convinced that the lateral spined eggs came only from unfertilized females. Students of the disease in the Americas were never convinced by Looss’s arguments, but politics was about to change the situation. The general outbreak of war in August 1914 meant that Looss had to leave Egypt. Leiper, investigating Schistosoma japonicum in China, returned hurriedly from Shanghai to London. He received a temporary commission of Lieutenant-Colonel in the Royal Army Medical Corps. In February 1915, following orders from Lord Kitchener’s office, he arrived in Egypt with Drs R. P. Cockin and J. P. Thomson to work out how schistosomiasis was transmitted, and to develop means of preventing the disease in the troops stationed there.

Working under extreme pressure, Leiper and his colleagues showed conclusively that the larval form (the ‘miracidium’) that emerged from the egg required an aquatic snail as an intermediate host—which Looss had denied. Leiper also distinguished the terminal-spined egg, (whose miracidia developed in aquatic snails of the species Bulinus,) from the lateral spined egg, whose miracidia developed in snails of the species Biomphalaria. The parasite found in the West Indies, Schistosoma mansoni, always produced
lateral spined eggs. These eggs were only found in the liver and rectum, and never in the bladder mucosa, and the course of the disease was quite different. There could therefore be no doubt that *S. haematobium* and *S. mansoni* were two distinct species. Leiper now admitted that Sambon’s position was the correct one, and added that Sambon had “maintained with great skill a vigorous polemic against Looss’ position.” It was as close to an apology as Sambon might have wished. Whether Looss (who died in 1923) ever acknowledged his error is not certain.

1907 was a busy year for Sambon. In addition to his work for Manson, he was working on an idea which, he hoped, might make him some money.

A major concern of those contemplating life in the tropics was the choice of clothing. Many doctors—and manufacturers of clothing—had their own ideas, some favouring wool, others flannel, linen or cotton. It was well known that dark coloured materials absorbed more heat than white materials. It followed that, whatever material was used, light coloured clothing made the most sense. Yet most observers were aware of a conundrum: the black skin of Africans must be a protective adaptation, since Africans did not die of heat stroke. With his interest in climate, it was only natural that Sambon should turn to a study of tropical clothing. He believed that white people who had lived in the tropics for several generations became darker skinned, while conversely, dark skinned people who lived in temperate climates for many generations became lighter skinned. This curious belief led him to agree that pigmentation must be a protective mechanism. He realised that it was the short, or actinic rays of the sun that caused tanning, but whether these rays had any other harmful effects he was not yet ready to say. (The American military surgeon, Charles Edward Woodruff’s *The Effect of Tropical Light on White Men*, published in 1905, purported to show that ultra-violet radiation
from the sun damaged the nerve protoplasm of light skinned individuals, leading inevitably to neurasthenia and other nervous disorders.\textsuperscript{22})

In collaboration with Edward C. C. Baly of University College, London, Sambon showed that pigment from dark skins did indeed block the actinic, or ultra-violet, rays. White cloth reflected the long rays (the infra-red rays) and therefore limited heating. In the matter of clothing, a combination of white and dark colours was to be preferred. After considerable experimentation, Sambon developed a woollen fabric in which the weft, or under surface, was dark red, and the warp a light khaki. He promoted this under the name of ‘Solaro’\textsuperscript{23} (fig. 7.4). Manson endorsed its use:

Fig. 7.4 Advertisement for “Solaro”. \textit{The Times} (London) 18 May, 1907, page 9.
“The natives of warm climates invariably have dark skins; a natural provision of protection against the actinic rays of the solar spectrum. Exposure to the sun tans the European; a natural protective reaction. Therefore, the European in the Tropics, conformably to this hint from Nature, should invariably wear non-actinic colours—red or yellow shirt, or a fabric (solaro) into which these colours enter, and such as is now manufactured.”

Castellani and Chalmers in the first (1910) edition of their *Manual of Tropical Medicine* thought Solaro was “the best cloth” for clothing, though their enthusiasm waned a little by the time of the second (1913) edition. Now they recommended that the fabric should only be used as a coat, made as light as possible, and without any lining. If Sambon had any profit from Solaro, it was slight.

Up to the end of 1907 Sambon’s professional life had been one of quiet, but hardly spectacular, achievement. His theory that temperature itself did not cause fevers was proving correct. He had theorised correctly that trypanosomes must have a specific life cycle in tsetse flies. He had taken part in the widely recognised and applauded mosquito-malaria transmission experiment. He had made important discoveries in the parasites of reptiles, and of snakes in particular. He collaborated with Charles Gabriel Seligman (1873-1940) in some of this parasitological work. Seligman, a noted physician, pathologist and anthropologist, had worked in the Sudan, and was well known to Henry Wellcome, through whose agency the two met.) Sambon had suggested in 1907 that Rocky Mountain Spotted Fever, a disease he had never seen, was a form of typhus, caused by a protozoon, and transmitted by ticks. The first and third suggestions were later proved to be correct. At the seventy-fifth annual meeting of the British Medical Association in 1907 he had suggested that diabetes would turn out to have nothing to do with food, but would be found to have an infectious cause. He had argued, successfully and correctly, against Arthur Looss. He had
suggested the word *haemozoin* for the product of haemoglobin degradation by malaria parasites. Despite opposition from Ross, who preferred the name *Plasmodin*, haemozoin it remained. It was hardly an important issue, but Ross was in error, as the malaria parasites were not the only organisms that could produce haemozoin.

At the seventy-third annual meeting of the British Medical Association, in July 1905, Sambon had presented a largely theoretical paper on the aetiology of pellagra, but pellagra was not then high on the agenda of tropical medicine experts. But by 1907 the Americans had started to take an interest in pellagra. At that moment, Sambon had every reason to be pleased with his progress in his adopted land.
Chapter 8. Zeists and Anti-Zeists: the Problem of Pellagra

Pellagra is today known to be a nutritional deficiency disease caused by a diet poor in the vitamin niacin (Vitamin B3) or the amino acid, tryptophane. The disease is characterised by an unpleasant, though conveniently mnemonic, trio of symptoms in ‘d’: dermatitis, particularly affecting areas of skin exposed to the sun; diarrhoea, and dementia. Since pellagra can be fatal, the fourth ‘d’ is death. Niacin is found in a variety of foods, including meat, fish, legumes, and some cereals. The cereals include maize (Indian corn, *Zea mays*), but—crucially for this history—niacin in maize exists in a chemically bound form that is not released by ordinary methods of preparation or cooking.

Pellagra was first recognised by the Catalan physician Gaspar Casál in 1735. Sambon (“so thorough and competent a student of the literature of pellagra”\(^1\)) believed that it must have existed long before this, but that it had often been confused with other skin diseases such as eczema, leprosy, or scurvy, and that the earlier Italian names ‘mountain scurvy’ or ‘Alpine scurvy’ (see reference 23) denoted pellagra. The first published description was by François Thiéry in 1755, though this author recognised Casál’s priority.\(^2\) The first recorded use of the name ‘pelagra’ [sic, but Frapolli was writing in Latin] was by Francesco Frapolli in 1771, though later the present spelling became the usual form.

The date when maize was first introduced from the Americas into Europe remains uncertain. As with syphilis, some authors suggest it accompanied the returning Columbus and his crew in 1493. Sambon, referring to the early-modern Italian health pundits, Michele Savonarola (1385-1468) and Bartolomeo Boldo (15??-1616), thought that maize was being used, sparingly, as a food since at least 1554.\(^3\) There is no doubt that its cultivation increased
in southern Europe and North Africa in the sixteenth and seventeenth centuries as its utility as a food came to be appreciated.

In Italy, maize was traditionally eaten as polenta,⁴ (fig. 8.1) boiled flour either eaten as a porridge, or allowed to cool and then baked as bread. And while numerous theories purported to explain pellagra, many medical men, including Casál, and the Italian physician Gaetano Strambio (1752-1831)

Fig. 8.1 La Polenta. Pietro Falca (called Pietro Longhi.) In the Ca. Rezzonico, Venice. Courtesy of the Fondazione Musei Civici di Venezia.
increasingly conjectured that the disease was somehow connected to maize. By the time Sambon was giving thought to pellagra, zeists\(^5\) had explained the association in four ways at least: maize was wanting in proper nutritive value; or it contained toxins that produced pellagra; or it became contaminated by bacteria or moulds which were either pathogenic, or produced harmful toxins. Regardless of the mechanism, maize was certainly involved somehow or other, though no one could explain why indigenous Americans who subsisted on a diet of maize did not develop pellagra. (Indigenous South Americans had learned to boil their maize with lime, a process known as nixtamalization.\(^6\) The alkali released the bound niacin. The navigators who brought maize back to the Old World omitted to bring back this essential processing technique).

By the end of the nineteenth century, the doyen of Italian zeists was Cesare Lombroso (fig.8.2) the controversial physician and criminologist. Lombroso

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\(^5\) zeists: People who study the nutrition of foodstuff.

\(^6\) nixtamalization: A process where maize is boiled with lime to release niacin.
believed that *Penicillium* and *Aspergillus*\(^7\) species caused pellagra, when and only when grown on spoiled maize. His one worthwhile contribution was to recognise that pellagra and poverty were linked, but not in a directly causal relationship. Lombroso died in Turin on 19 October, 1909, so he missed the aetiology wars that were about to begin.

Pellagra, sometimes called the Italian leprosy, shared some unpleasant associations with leprosy, which today is called Hansen’s Disease (HD). These associations went beyond the occasional confusing of the two diseases. As patients with HD were in earlier times collectively called ‘lepers’, so those with pellagra were called ‘pellagrins’ or pellagrosi. Special hospitals, leprosaria and pellagrosaria, provided for the two groups of patients. The word ‘loathsome’ was applied to both diseases.\(^8\) Leprophobia and pellagrophobia abounded, the latter especially in the United States where schools excluded children who came from HD or pellagra families. (In May 1915, the Shreveport, Louisiana, Board of Health designated pellagra a communicable disease. Children were excluded from school, and pellagrins were forbidden to prepare food or attend public meetings.) Early symptoms of both diseases were easily missed, or, if suspected by the patient, were hidden from prying eyes. Under-reporting was one inevitable result, leading to suspect statistical data. Sambon himself considered the Italian statistics “grossly erroneous.” He quoted the physician-politician Nicola Badaloni (1852-1945):

“Badaloni, speaking in the Italian parliament on March 3, 1908, said: It is stated that pellagra has greatly diminished in Italy. But I have no great faith in the accuracy of this assertion, and there are many reasons which incline me to hold the contrary opinion. However, it is a certainty that, if you base your estimate upon the statistics concerning the number of individuals to whom are extended the benefits of those measures that weigh upon the budgets of the communes and the provinces (and they
are strictly accurate statistics), you may find reasons for the support of the belief that
pellagra is greatly declining."

As the nineteenth century progressed, pellagra was becoming a problem for a
number of countries of southern Europe and North Africa. In 1863, Thomas
Peacock, physician to St Thomas’ Hospital, (where, among other duties, he
supervised the skin clinic) reported on the apparent relationship between
pellagra and lunacy in Venetia. In the five year period 1857 to 1861 the San
Servolo Hospital (for male lunatics) on the Venetian lagoon had admitted
1314 patients. Of these patients, 411 (31%) were pellagrins. A later report
understood the Italian census of December 1877 to show that almost 9% of
insane people in the Italian population were pellagrins. In the three years
1876 to 1878, some 3,300 people died in Italy by suicide. Of these dead 250
(7.6%) left notes blaming “the tormenting sickness of pellagra.”

The compilers of the 1881 census estimated that there were over 100,000
pellagrins in Italy. Of greater concern, by 1889, while the number of
pellagrins was diminishing in the province of Venetia and Lombardy, the
worst affected of all the Italian provinces, their number was increasing in
Umbria and Latium. By 1905 pellagra had appeared in Abruzzo, where the
disease was previously unknown.

In 1902, accepting Lombroso’s hypothesis, the Italian Government introduced
a law that required crop diversity, sanctions against producers and purveyors
of spoiled maize, the exchange of better quality for spoiled maize, the
construction of pellagrosaria, the building of ovens to dry maize, and the
distribution of salt at low prices (lack of salt, largely due to the imposition of
exorbitant taxes, was considered to contribute to the development of pellagra).
Despite the law, pellagra continued in Italy, albeit at a diminishing incidence,
a phenomenon observed before regulation had begun.
Pellagra was also found in Roumania, the number of notified cases increasing from 33645 in 1901 to over 100,000 in 1906. Other countries affected by pellagra included parts of Spain, the Austrian Tyrol, Greece, Egypt and Morocco. In all these countries, except Spain, maize was an important source of food. At the 1898 meeting of the British Medical Association, Fleming Sandwith presented in abstract a paper on pellagra in Egypt, a disease he had first recognised there in 1893. He later noted, “The abstract of my paper led to no discussion, and room was not found for the lengthy paper in the published accounts of the meeting.” Tropical medicine specialists had not yet developed an interest in pellagra. Sambon, however, had heard plenty about pellagra in his native Italy and wanted to learn as much as possible about it.

Sambon’s growing faith in germ theory and a belief that epidemiology could provide answers to how diseases spread within a community prompted him to take a critical look at zeist theories. During trips to Italy in 1900, 1903 and, for the Wellcome collection in 1904, he took the opportunity to discuss the problem of pellagra with Italian medical colleagues.

In July 1905, at the seventy-third annual meeting of the British Medical Association, Sambon expounded to the tropical diseases section a novel hypothesis for the aetiology of pellagra. He showed that the area of pellagra endemicity did not exactly map onto the area of maize culture. Furthermore pellagra unquestionably occurred in areas where maize was not cultivated. Zeists tried the trick of saying that pellagra in non-maize cultivated areas was “pseudo-pellagra”, or sometimes *morbus miseriae*, a proposition he discounted. He noted that, in Italy, pellagra was spreading from its traditional home in the north of Italy (where maize formed a staple of the diet,) to more southerly areas of the country, including the area around Naples, where maize was less important,. It was the agricultural labourers who were most at risk.
City dwellers, even in areas of high pellagra endemicity, seemed immune. Sambon suggested that it was in the maize fields that the peasant came into contact with a specific agent of pellagra, perhaps through the agency of a biting fly. In the Section’s later discussion on beriberi, another nutritional deficiency disease, Sambon’s enthusiasm for insect-vector theme led him to concur, without any evidence, with a suggestion that beriberi, might be spread by bed-bugs.\textsuperscript{18}

As we have seen in chapter 6, Henry Wellcome proposed sending Sambon to Italy in January 1906, in the company of Thompson. This would have interfered with Sambon’s duties at the School. Sambon therefore asked Wellcome if, as part of his researches for the historical exhibition, he might do some work on the aetiology of pellagra. If Wellcome agreed, the School would allow Sambon to find a substitute for his lectures. Otherwise, he would not be able to go on working for Wellcome. Wellcome was persuaded.

Sambon’s pellagra hypothesis received little support, though at least one Italian, Professor Camillo Terni was reported to have said at the third pellagra conference in Milan in 1906 that he was “inclined to accept Sambon’s opinion.”\textsuperscript{19} The hypothesis received some recognition with an occasional report of the beneficial effects of arsenic preparations—used experimentally in the treatment of some infectious diseases, notably syphilis, visceral leishmaniasis, (‘kala azar’) and sleeping sickness—on the course of the disease, but pellagra was still of little interest to mainstream tropical medicine doctors. Manson made no mention of the disease until his fourth edition, published in 1908, while Castellani and Chalmers devoted less than five pages to the subject in their 1910 \textit{Manual of Tropical Medicine}.\textsuperscript{20}

In 1906, the British biochemist Frederick Gowland Hopkins reported that the principal protein of maize, \textit{zein}, contained no tryptophane. Young rats fed on a diet containing zein and a plentiful supply of carbohydrates, fats and salts
failed to grow; once the zein was replaced by pure casein, the rats grew well. It was clear to Hopkins that proteins might not only always support life, and that some molecules in protein served purposes other than tissue repair or providing energy. This work was not picked up by either Sambon or the Italians.

Pellagra was now spreading within parts of the British Empire, including South Africa, Egypt, India and the West Indies. The Colonial Office appeared uninterested. Pellagra was also spreading in some of the southern states of the United States of America. Despite Sir William Osler’s confident assertion that the disease did not occur in that country, George Niles believed that Union soldiers imprisoned in Southern prison camps during the American Civil War suffered from pellagra, among other afflictions. After the war, pellagra probably occurred in the South, but was misdiagnosed by physicians, including, Niles admitted, himself. James Wood Babcock (see below) thought that pellagra had been present in the South Carolina State Hospital for the Insane since its opening in 1828.

In 1902, Henry Fauntleroy Harris, an Atlanta Ga physician, diagnosed what he mistakenly claimed was the first case of pellagra in the United States. Little notice was taken. Five years later, Dr George Searcy published an account of an outbreak of an acute disease he said was pellagra. It had occurred in the Mount Vernon Hospital for the Insane, an institution for coloured patients in Mobile, AL. According to Searcy, a similar disease had affected three or four patients each summer since the asylum had been moved from Tuscaloosa in 1901; but in 1906, eighty-eight patients were affected, and 57 died, a mortality of 64%. Searcy was a zeist. Samples of the meal used in the hospital, said to be “the best western meal,” were examined by a Washington plant pathologist who declared it to be unfit for human consumption. Warning bells now started to ring.
Shortly after, Babcock described pellagra patients in his institution, the South Carolina State Hospital for the Insane. Determined to make certain that he was on the right track, Babcock visited Italy in the summer of 1908, accompanying Senator Benjamin (“Pitchfork”) Tillman, democratic senator for South Carolina, as his personal physician. (Tillman had suffered a stroke, and had been advised to take a break in Europe.) What Babcock saw in the pellagrosaria and lunatic asylums of Italy convinced him that the disease he was seeing in South Carolina was indeed pellagra. The South Carolina board of health sought help and advice from Surgeon General Walter Wyman, the head of the US Public Health and Marine Hospital Service. Claude Hervey Lavinder was assigned to this duty.

Among Lavinder’s first duties was the preparation of an English language monograph. The completed *Pellagra A Precis*, was submitted to the Secretary of the Treasury on 24 July, 1908. Most of Lavinder’s discussion of etiology relates to maize in some way, but he added that some observers (unidentified) considered pellagra to be an infectious disease. He briefly mentions “pseudo-pellagra” only to add Manson’s dictum, that “the disease is pellagra when it fits in with the orthodox theory…but when this is not possible it becomes pseudo-pellagra.” He writes that a good diet is essential in management, but then obfuscates by saying that some advocated the use of quinine, arsenic or strychnine; and that atoxyl had been highly recommended. He had not yet met Sambon.

On his return to Columbia, SC, Babcock arranged a local meeting to discuss the problem of pellagra. At this meeting, held in the Asylum, the two positions—zeists and antizeists, typified by Sambon’s thesis — were drawn up. Most of the papers were anecdotal in content, but the conference did highlight the fact that pellagra was a problem in some of the southern states.
A year later, in early November 1909, a National Conference on Pellagra was held in Columbia. The first paper was to be read by Sandwith. He was unable to attend in person, but his paper strongly supported the zeist position while Julius Heyward Taylor, also an asylum physician and a friend of Babcock, was much impressed with Sambon’s thesis. Captain Henry Nichols of the US Army accused Sambon of making a stir “with his free-lance methods of theorizing about different diseases,” but Taylor was having none of this. He later said of Sambon’s achievements, “there is sufficient proof of the man’s almost unerring judgment, certainly sufficient to make us read with unconcealed impatience the occasional slighting references in American print to his suggestions regarding the probable protozoal nature of pellagra.” Of more immediate interest to the British were papers from Jamaica and Barbados. Dr D. J. Williams, the Medical Superintendent of the Kingston Asylum, suggested that pellagra had probably been present for at least twelve years and that maize, damaged or not, was not the sole cause of pellagra. In a subsequent report, he wrote that he had excluded maize from the diet of his patients, and had even added extra salt, but still pellagra persisted, proving, to his mind, that pellagra had nothing to do with either maize or salt. The paper from C. G. Manning, the superintendent of the asylum at Bridgetown, Barbados, discussed the *Psilosis pigmentosa* (Manning’s own coinage) at the asylum and argued that this disease was not pellagra. His clinical and post-mortem descriptions leave little doubt that it was.

An interesting paper on *Pellagra in Yucatan* (Mexico) was read by Dr G. F. Gaumer. He noted that pellagra was unknown in the region until 1884. In 1882, the local maize crop was destroyed by a plague of locusts. To avert a famine, maize was imported from the United States. The maize was brought as ballast, and frequently became wet and mouldy. Pellagra increased in Yucatan, and only diminished after 1891, when the maize crop recovered. Between 1901 and 1907, the crop again failed, more maize was imported, and
pellagra increased. The paper perhaps brought more comfort to zeists than to their opponents.

The Americans quickly got to work. Entomologists, bacteriologists, epidemiologists and clinicians all took an interest. Two U. S. commissions were appointed to investigate the disease. The first, the Illinois Pellagra commission was established in November 1909 by the Illinois State Board of Health after George Zeller reported that since August 1909, “130 well defined and diagnosed cases of pellagra” had been “officially recognised” in the State asylum at Peoria. Among the members of this commission were US Army Captains Joseph E. Siler and Henry Nicholls. Dr Howard Taylor Ricketts, one of the great names in American pathology, was a member, but in May, 1910 he died at 39 of typhus while investigating a major outbreak of that disease in Mexico. The second group, the Thompson-McFadden commission, was established in 1911, and initially looked at the epidemiology of pellagra in Spartanburg County, North Carolina. Siler was a member of this commission too.

The enthusiasm with which the Americans entered the fray can be judged from the number of pellagra papers they published. Sandwith noted that, in 1907, six medical men in America wrote on the subject: the following year, thirty-five papers were published, in 1909, 150 papers and in 1910, 100 papers.

Perhaps stung into action by Sandwith’s involvement with the Americans, Manson wrote to Ross on 5 November 1909, suggesting that money from “the Committee” (the Advisory Committee of the Tropical Diseases Research Fund of which Ross had recently been appointed a member) be used to fund an investigation into the aetiology of pellagra, especially in the West Indies. He added that
“I am very anxious for various reasons to give Sambon a chance in this, for although he has many of the faults of genius, he is a genius, remarkably industrious, when he sets about a thing & full of original ideas & I am sure would struggle till he got a solution. Poor fellow, he is cursed with the want of cash, which has handicapped him for years & I fear that, if we can’t get him something soon, he will be lost to Tropical Medicine, & the loss, I know, would be a serious one. He is a magnificent worker, with ideas ahead of his time, & perhaps that’s the reason, or one of the reasons why so many affect to sneer at him…But everyone is down on him, because he is not like other men.”

Despite his self-confidence, Sambon probably felt the need to refresh his knowledge of the clinical features of pellagra, and suggested to Ross that he go to Italy first, then to the West Indies. Ross agreed. Sambon was delighted, writing to Ross to thank him “heartily” for the good news and his support.

Around the beginning of 1910, a Pellagra Investigation Committee was established in Britain. Among the members of the committee were the physician and pharmacologist Sir Thomas Lauder Brunton, Manson, Ross, Sandwith, Cantlie, Leiper and Sambon; Sir William Leishman of the Army Medical College, Netley and Fleet Surgeon Percy W. Bassett-Smith of the Royal Naval Hospital, Haslar; the editors of the Lancet and the British Medical Journal, the Marquis of San Giuliano, the Italian ambassador, and Dr F. G. Melandri, the physician to the Italian Embassy. Commercial interests included Henry Wellcome and the London and Italian Chambers of Commerce. Cantlie was appointed treasurer and honorary secretary. An Advisory sub-committee, consisting of Manson, Ross, Sir William Leishman, and Ernest Austen, the British Museum (Natural History) entomologist, and others was also established.

At the Investigation Committee’s first meeting, in January 1910, Sambon explained his views on the aetiology of pellagra. The committee proposed to
send Sambon to Italy to see if his hypothesis stood up to scientific scrutiny.
The Colonial Office promised to contribute £150 towards the £600 the committee estimated might be needed for the investigation, but only if more money could be obtained from other sources. The Tropical Diseases Research Fund offered £150, but the public response was less than the committee had hoped. This prompted a critical comment in the editorial pages of the British Medical Journal:

“It will be a reproach to this country if a discovery pregnant with far-reaching possibilities is suffered to be lost, or to pass to another nation for the lack of a little money—the merest fraction of what is thrown away in the formation of a “free library,” which encourages a taste for the reading of worthless novels.”

Sambon had other worries at the time. Ross had asked him to write a chapter on malaria in Italy for his book, The Prevention of Malaria, but Sambon had got behind, and was, according to Hilda, to whom Ross had written, harassed and worried. Sambon, with his classical knowledge and antiquarian interests, believed that malaria had been endemic in early Rome. Ross disagreed: he supported W. H. S. Jones’ belief that malaria entered Rome in historical times—possibly brought to Rome by Hannibal’s Carthaginian troops in 218 BCE—and that it ultimately contributed to the decline of the Roman Empire. Sambon wrote to Ross,

“Good gracious! Have your Archaeologists not read Plautus, Cato, Cicero, Varro, Caesar, Virgil, Horace, Ovid, Livy Seneca, Valerius Maximus, Celsus, Columella, Pliny, Juvenal, Suetonius and the other Latin authors who mention the endemic: have they never seen any of the coins, engraved gems, terracotas, and other monuments from ancient Italy which now fill all the European museums; have they never gone further than Kingsley’s “Heroes” and Hawthorne’s “Tanglewood Tales” in their studies on classical mythology? I have read all the Latin authors, I have lived for years in Italy and I have had every opportunity of studying the subject thoroughly both from the medical and archaeological side. I maintain most emphatically that in Italy, from the earliest time of which we have any record, man has had to battle fiercely against malaria.”
It was not a letter calculated to endear its author to Ross. Sambon happily gave up work on the chapter and, by 15 March, 1910, he was in Italy. Angelo Celli contributed the section on malaria in Italy for Ross’s book.

In an unusual move towards transatlantic cooperation, Siler, who was studying at the London School at the time, was invited to join Sambon. Sambon decided to set up his headquarters in Bergamo, in the foothills of the Alps, and informed the local authorities of his intentions. On arrival at Bergamo railway station, the two men were met by a large crowd including members of the medical fraternity and members of the local pellagra commission. In the surrounding areas, Sambon found numerous cases of pellagra, many of whom lived in close proximity to torrents that teemed with Simulium (Blackflies). Fortuitously, Lavinder and Dr Rupert Blue—the latter was to succeed Dr Walter Wyman as Surgeon-General of the United States in 1912—were also in Italy at the time, so Sambon invited them to join him.

Sambon’s group worked in the Province of Bergamo from 26 March 1910 to 9 May, 1910, but also visited—it could only have been hastily—the Provinces of Milan, Brescia, Padua, Rome and Perugia.

In April, Sambon sent a wire—which he later much regretted—to his committee saying “The pellagra field commission has definitely proved that maize is not the cause of pellagra. The parasitic conveyor is the Simulium reptans” (Fig. 8.3). Sambon recognised that pellagra was not contagious in the generally accepted sense—he was well aware, for example, that pellagrin wet-nurses never transmitted the disease to the infants they suckled, carers in pellagrosaria never developed pellagra and the disease, unlike syphilis, was not passed down from mother to child—so he had to promote the idea of transmission by an indirect route. Insects seemed very appropriate. In Sambon’s mind, his hypothesis had now become a theory. Siler, almost ten years younger than Sambon, seems to have been impressed, and reported to
the Pellagra Investigation Committee that “the Field Commission” had done excellent work.44 Lavinder and Blue left the party before Sambon had finished his work. Meanwhile the Colonial Office was being slow with its promised financial contribution. The Pellagra Committee complained that “unless this was forthcoming at an early date the Committee must recall Dr Sambon at a moment when the Committee believe that useful work is being done.”45 Members of the committee dug deep into their own pockets, and the Colonial Office paid its contribution.

On his return to London in mid-June, 1910, Sambon got to work on a report of his findings for the Pellagra Investigation Committee. The first fifty pages of the report consist of a polemic against zeism and what he called “the Lombrosian theory.” In his opinion, this theory had, for thirty-six years, prevented any progress in the elucidation of pellagra. He denounced the Italian law of 1902, saying that it was ineffective, and subject to misdiagnosis and manipulation. Sambon noted that pellagra in Italy was often precipitated

Fig.8.3 *Simulium reptans*, drawn by A. Terzi. Taken from Castellani and Chalmers, 1913.46
by an antecedent infection, such as enteric fever, malaria, whooping cough or rheumatic fever. In women, pregnancy and parturition were common predisposing causes. He dismissed any idea of contagion or hereditary transmission, but noted the frequency of concurrent infection with hookworms, which, he believed, contributed significantly to the anaemia of pellagrins. He devoted a great deal of time and space to his hypothesis that pellagra is an insect-borne disease, and concentrates on Simulium species.47

Sambon’s report reads less like a scientific paper and more like The Tour of Dr Sambon in Search of the Picturesque as he hurries from one place to another to see yet more pellagrans, or to disprove the existence of pellagra in areas where it was said to be prevalent: “I shall never forget the return journey from Costapennacchio down the narrow, precipitous and slippery paths, over the boggy fields and across the swollen streams, whilst the lightning flashed, the thunder roared and the rain came down in veritable torrents.” He peppered the report with a large number of references, mostly to Italian authors, many from the eighteenth century. Perhaps the most extraordinary omission, for a paper that purported to be epidemiological, (“The principle object of the expedition to Italy was the study the epidemiology of pellagra” Sambon wrote) was the complete absence of any epidemiology. There are no tables, no statistics, no maps, and no discussion about diet, poverty, occupation, age, gender, climate or other factors that might be, or had been held to be, relevant in the aetiology of the disease. Polemics apart, the report included numerous case histories.

The way Sambon deployed his material caused Ross to write irascibly to Manson, “I have already read Sambon’s report in the Journal of Tropical Medicine. It seems to be another case of that old error of doctors of insufficient random sampling.” After further criticisms of Sambon’s methods, Ross adds, “Much less had he any authority to write to the lay press stating
positively that Simulium carries the disease. This sort of thing produces the worst possible impression. It is not science.”

Manson replied “I am sorry…that you do not support Sambon who with all his failings, and they are many, has a certain capacity for research and much enthusiasm in certain directions.” Ross replied that his main objection was that Sambon seemed to have jumped to the conclusion that his Simulium thesis was proved, while, to Ross’s mind, Sambon scarcely had enough evidence for a reasonable working hypothesis.

At the end of his trip, Sambon brought back to London a 14 year old Italian girl from Perugia, whose mother had committed suicide as a result of pellagra, for further investigation. Though the child improved dramatically, in the spring of 1911, and again in April 1912, she developed symptoms—weakness, vertigo, strange behaviour and a ‘furfuraceous’ [floury] rash—which Sambon attributed to recurrences of pellagra. To his mind, these recurrences were very significant, and analogous to the recurrences that occurred in certain forms of malaria. Carmichael Low examined her blood, but could find nothing very abnormal except an increase in small lymphocytes, a common finding in the disease.

At the British Medical Association meeting in London in July, 1910, Sambon repeated his claims about pellagra being a protozoal disease, apparently spread by Simulium reptans. Lavinder spoke at the meeting in appreciation of Sambon’s work in Italy.

Shortly after the Association meeting, Lavinder returned to America. He immediately wrote an efficient, if somewhat tabular, article about Sambon’s theory, ending with the words “The theory of Sambon has interested me greatly, and it seems that now, when this theory has been elaborated so much more in detail, it is well worthy of serious attention at the hands of American students and investigators.” Later, he would write that Sambon’s work was
“hypothetical, and lacking scientific demonstration.”\textsuperscript{52} The Italians meanwhile established a commission to investigate Sambon’s hypothesis.\textsuperscript{53}

The Colonial Office still waited for Sambon to get to the West Indies, but he was not too enthusiastic about this\textsuperscript{54} as he had other plans.
Chapter 9. A damp squib

In 1911, Sambon’s situation was about to change. As Manson had noticed, he was always short of a penny or two. He had been advanced £123 by the publishers John Bale, Sons & Danielsson Ltd for a book, provisionally entitled *Geographical Distribution of Disease and Acclimatisation*, to be an updated and extended version of his earlier manuscripts on these subjects, but he had got behind, and seemed unwilling or unable to finish it. Bale wanted their money back. Henry Wellcome now stepped in. He offered to pay off the loan, on condition that Bale hand over “a complete set of the proofs of the proposed book, all the original drawings made for the book and all the rights of reproduction of the book and illustrations, together with Dr Sambon’s gold medal…” Henry Bale, after a little quibbling, agreed to the conditions, with the exception of the medal.¹

At the same time, Wellcome offered Sambon a position with the Wellcome company. He would be employed as a researcher in the Wellcome Physiological Research Laboratory, in the historical research department, and “in such other capacity as I may desire.” If Sambon accepted, he would be subjected to the usual strict conditions that Wellcome insisted on for all his employees. Part of the contract required him to proceed to “Austria-Hungary Italy and Spain” for a period not exceeding three months from about the 1st of August, for further investigation of the pellagra situation. When travelling overseas, he would be entitled to first class steamer fares, but second class railway fares, except in Spain, where he would be entitled to first class fares. Though Wellcome insisted that Sambon devote his entire energies to the company, he was allowed to continue lecturing at the London School and at the Livingstone College. Sambon accepted.
While these discussions were taking place, Sambon hurried off to the British Medical Association meeting in Birmingham, where, he told Thompson, “a Yankee is going to read a paper on [pellagra]…If the American pellagrologist…is a corn man, we should have a battle royal.” Much to his regret, Herbert Cole, of Mobile, Alabama, had no interest in maize, but was convinced that blood transfusions could cure pellagra.

Sambon started work for Wellcome on 1 August, 1911. Shortly after, accompanied by Albert Chalmers and Blanchard, he visited France and Spain to look into the epidemiology of pellagra. Almost immediately on their return, Sambon and Chalmers set off on an extensive tour through southern Europe, visiting Hungary, the geographical Transylvania, Roumania and the Austrian Tyrol, returning to London in November.

On 12 December, 1911, Sambon and Chalmers showed the Pellagra Investigation Committee a structure they had found in the nucleus of a cell from the nervous system, taken shortly after death from pellagra. Manson, who chaired the meeting, said there was much to be said for Sambon’s theory, and urged further investigation.

At a meeting of the Tropical Medicine Society on 15 December 1911, Hugh S. Stannus gave a paper on pellagra in Nyasaland, and Sandwith a paper called “Pellagra in Thirty-Five States of America.” In the discussion, Sambon remarked that the nature of the structure found by him and Chalmers was uncertain, although an un-named medical journal (it was in fact the British Medical Journal, see reference 2) had reported their discovery as a “microscopic organism.” He added, “Premature information is undesirable, and it is also to be regretted that on another occasion a telegram I had sent home was published in an incomplete and misleading form. [He meant his telegram announcing that Simulium was the transmitter of pellagra.] One lives in fear and trembling of reporters.” Prophetic words indeed, but perhaps
Sambon was being disingenuous? At the same meeting, Chalmers said that, though pellagra in southern Europe was restricted to hilly country close to streams, maize should not be forgotten: the Roumanians who ate maize got pellagra, the Bulgarians, who did not eat maize, did not get the disease.\(^5\) The anonymous author of the *British Medical Journal* article of 16 December (reference 3) added that “on inquiry, this legend was proved to be false.”\(^6\)

Profiting from the experience of Ross, Bruce and others, Sambon was beginning to realise that if his insect-borne hypothesis for pellagra was correct, he would have to do something to prove it experimentally. He talked over his ideas with Chalmers, who was supportive. He obtained permission from Wellcome to go to Egypt, which Chalmers was about to visit, but Chalmers quickly realised that Cairo would not be a good place for the experiment. Sambon thought that patients were rather scarce in Transylvania, Professor Victor Babes, in Roumania, was rather hostile to Sambon’s hypothesis, so Italy it would have to be (he does not say what objections there might be to Austro-Hungary and the Tyrol as sites for his experiment.) Sambon was suffering at the time from an undefined illness (“as I was at the time rather uneasy about my liver” Manson, who visited him on 13 January, examined him, but “could not make, or would not mention his diagnosis.”\(^7\)) By the following week, though still tired, he decided he was fit enough to travel. After a couple of days in Paris, he arrived in Milan on 24 January 1912, then went to Turin and Florence and then onto Rome.

In each city, he called on as many important medical men as he could. In Florence, he met Professor Alessandro Lustig, the head of the national Commission for the Study of Pellagra, who promised to arrange a meeting of the commission, to which Sambon would be invited. In Rome, Sambon had an interview, organised by his father, with King Vittorio Emanuele III, who took “the greatest interest in my researches, and asked me to keep him
informed.” As a result of all these discussions, Professor Luigi Devoto of Milan offered the Pellagra Investigation Committee the use of his ‘splendid’ laboratories, and the Pellagra Commission of Cittea di Castello, Perugia, the use of its pellagrosarium and its patients. These offers came at a cost, however. In all the places he visited, Sambon never forgot his second (or, as Wellcome would probably have it, his first) duty of collecting material for the Historical Medical Exhibition (HME) which Wellcome was now proposing to open the following year.

Sambon outlined his plan of action in a note to George Pearson, the general manager of Burroughs Wellcome:

“The experiments will be made on animals and man. It will be necessary to breed large numbers of simuliedae [sic] in special insectaria. A certain number of patients (say about sixty) will have to be collected and brought to the Pellagrosario of Citta di Castello others to the Clinica del Lavoro in Milan...We shall require at least three professional assistants – one of these I propose to take from London...A certain amount of money will be necessary for many incidental expenses; animals for experiment, collecting of simuliedae, remuneration to patients used for experiments, travelling expenses (3rd class) etc., etc...To the expenses mentioned above must be added expenses for entertaining delegates and other persons connected with the work...”

Pearson was beginning to get irritated with Sambon. He wrote, “In view of the very generous way in which Mr W has already dealt with Dr Sambon in respect to pellagra work, I think Dr Sambon is treating Mr W very unfairly in respect to the HME work...”

Even so, the Pellagra Investigation Committee and Pearson both agreed that Sambon should go to Italy in March 1912, and should finish by the end of April, so that he could devote all his time to the HME. Chalmers was to join him in Italy. Pearson initially suggested that Sambon should be paid his usual
travelling expenses of fifteen shillings per day. At this point Wellcome put his foot down. He was quite willing to continue paying Sambon his London salary, but if no-one else could be found to pay the travelling expenses, then “he had better drop it.” Wellcome added as an aside that if Sambon had gone to the West Indies, as originally planned, those colonies would have picked up the tab. It seemed only fair, since Italy stood to gain the most from Sambon’s investigations, that they should make some contribution, as the Spanish Government had done earlier. As Pearson put it:

“Mr W felt that he had more than done his share towards the expenses of this Pellagra work [he had contributed £700] and he was also keenly disappointed in Dr Sambon’s disregard for our instructions to report to us regularly respecting his work and movements—he never let us have an address to which we could write him the whole time he was away.”¹¹

Sambon refused to accept the Wellcome conditions. He could not be expected to cover the expenses for patients, hospital accommodation, and the rest, and he could not be certain he would be finished by the end of April. If Wellcome was insistent, the only alternative would be to go to the West Indies and carry out his experiments there. With Manson’s encouragement, Sambon then entered into discussions with the Colonial Office, and by mid-March, it was agreed that he would go to the West Indies.

Pearson attended a meeting of the Pellagra Investigation Committee on 20 March, (Wellcome was on a visit to the Sudan) at which Sambon now proposed that he should go to Italy, not the West Indies. The committee considered that he could complete the work in Italy by mid-May or early June, and on his return would devote all his energies to the HME, even to the extent of leaving the writing of reports to Chalmers. Manson reminded the committee that he had said that Sambon would go to the West Indies. He had secured a promise of £300 from the Colonial Office to cover Sambon’s
expenses. If he now told the Colonial Office that Sambon was not going to the West Indies, he could see that he would have considerable difficulty in obtaining grants from the office in the future.

To Sambon’s regret, Pearson now suggested that Sambon should devote himself entirely to pellagra work, and leave Wellcome free to make whatever other arrangements he wished to complete the HME sections for which he had engaged Sambon, but Pearson later softened his stance. He wrapped up the matter by undertaking to give Sambon leave of absence on full pay to the end of May, or the first week of June at the latest, provided that, on his return, he devote himself entirely to HME work, not even writing a report on his Italian expedition. Pearson then agreed to deal with the financial side of the bargain.

Chalmers, sweating it out in Cairo since January, was becoming confused. He had originally expected to meet Sambon in March in either Rome or Perugia. He had later received a cable saying that Sambon was headed for Barbadoes [sic], which he, Chalmers, thought was not a good idea, and now was Sambon’s trip to be scrapped because of money? He sent the Pellagra committee a cable saying “Think Sambon should be supported economically. Observations here favourable parasite fly theory Willing give services and contribute small sum Italian expenses if necessary,” a generous offer which slightly put the other members to shame after Manson had appealed to them to do what they could to raise the necessary funds.

Thompson then interviewed Sambon, making him sign an agreement to abide by the conditions set by Pearson. Thompson reduced the number of patients to thirty four, and removed one Italian assistant from the list. He offered Sambon eight pounds for entertaining. Sambon thought this quite insufficient, but Thompson did not budge.
Sambon left for Rome on 2 April 1912, and met up with Chalmers there. Unfortunately administrative and bureaucratic delays meant that they did not get to Perugia—where, to Sambon’s pleasure, Blanchard was waiting for him—till 21 April. In Perugia, Sambon discovered that butterfly nets suitable for catching small flies could not be bought locally, so he asked Thompson to send out a pair, while explaining:

“I hope that the long delay in London, and the further delay in Rome will not wreck my chances, and of course we shall do our very best to succeed, but I must say that the obstacles in my way have been most unfortunate. However all is well that ends well, and I have no other feeling than one of gratitude for being enabled to attempt somehow to solve myself the great pellagra problem.”

If, as Robert Burns wrote, the best laid schemes o’ mice an’ men gang aft agley, then poorly prepared, ill-laid schemes will tend to fare even worse. On 24 May Sambon wrote to Thompson,

“We work from sunrise to late at night & we are always on the move. Having been unable to carry out the work we intended for lack of suitable material we have settled other important points and brought the whole question of pellagra nearer its solution. We are now settling a most important point, & I hope you will allow me two or three more weeks.”

What these important points the two had settled he never said.

Despite Manson’s gloomy assessments of how the Colonial Office might react to Sambon’s Italian trip, he managed to persuade them that, once Sambon returned from Italy, he might be free to go to the West Indies. The Colonial Office accepted the delay. However, “owing partly to other work, and partly to the discovery of endemic pellagra in the British Isles,” the trip had to be postponed. The Americans meanwhile were forging ahead.
The Illinois Commission largely investigated the state’s mental hospitals and asylums. Its report, published in 1912, noted that pellagra was mainly found among chronic patients, many of whom were enfeebled and poorly nourished. It also noted that the employees of the hospitals never contracted the disease. Numerous microbiological investigations were undertaken, with rather mixed and confusing results. Monkey experiments were all negative. A meta-review of *Simulium* species concluded that *Simulium reptans* did not occur in North America. In a later paper, Stephen Forbes, the state entomologist who wrote the review, admitted that, whether Sambon’s thesis was correct or not, it had provided an opportunity for a great deal of entomological research. The Commission was unable to find any evidence that either sound or spoiled maize was in any way responsible for pellagra, adding “If one adds to these direct observations, the keen analysis by Sambon of the foundations upon which the maize hypothesis rests one cannot but feel that the arguments in its favour are extremely slender.” Despite all the negative microbiological evidence, the Commission concluded:

1. Pellagra is a disease due to infection with a living micro-organism of unknown nature.
2. A possible location for this infection is the intestinal tract. [Investigators had noted marked differences in the faecal flora of pellagrous and non-pellagrous patients]
3. Deficient animal protein in the diet may constitute a predisposing factor in the contraction of the disease.
4. The number of cases of known pellagra renders this disease a decided menace to the public health of this State.
5. Careful search for, and investigation of, suspected cases outside the State Hospitals for the insane is extremely desirable in view of experience elsewhere.

Ricketts must have turned in his grave.
Sambon, who was surely cheered by this, and other reports, many of which were supportive of his position, spent some of the latter part of 1912 investigating reported pellagrins in Britain. He and Chalmers travelled through Eastern Scotland and were pointed to several instances of the disease. Sambon believed that pellagra could be traced back to at least 1860 in these parts.\(^{17}\) He had by now achieved a certain status as the pellagra expert in the British Isles. Doctors wrote to him describing suspicious symptoms; he was always happy to travel, sometimes long distances, to see the patients and give an opinion (fig. 9.1).

Fig. 9.1 Two girls suffering from pellagra. Watercolours by Amedeo Terzi. Courtesy of Wellcome Images.

In an extensive article, *The Natural History of Pellagra*, he quite shamelessly promoted what was by now, in his mind at least, a theory of the transmission of an infectious agent of pellagra by *Simulium* flies.\(^{18}\) There is nothing in this
report to indicate what “important points” he and Chalmers had settled in Italy in April/May 1912.

The second triennial meeting of the (American) Association for the Study of Pellagra was held at Columbia in October 1912. Sambon’s hypothesis was discussed in a number of papers. The association resolved, among other things, that the ultimate cause of pellagra was unknown, but that in view of the incrimination of spoiled corn, measures should be taken by the proper authorities to prevent its sale and consumption as food. The association agreed that there was no evidence to show that pellagra passed directly from man to man, and there was no specific remedy, despite some enthusiastic reports on the benefits of various arsenical preparations. Pellagra was not notifiable in most of the States of the Union, despite which Lavinder had put together some figures that he suspected must be an underestimate. He concluded that between 1907 and 1911, pellagra affected at least 30,000 people in the USA, with a fatality rate of 39%. This was, he said, a major public health problem.

At a meeting in London on 21 February 1913, Sandwith again spoke on pellagra, and said that recent developments in beriberi had caused him to wonder “anew’ whether pellagra was another deficiency disease. He was, however, wary of raising new ideas, quoting an un-named German author, who said that

“the field of pellagrous etiology has been the playground of scientific fledglings, whose intellectual judgment is befuddled because they have no real comprehension of the fundamental question, and therefore attempt to introduce into the etiology of pellagra all sorts of things, from bald banalities to the most wonderful fantasies.”

It might also have been a gentle rebuke to Sambon, who was present at the meeting. Sandwith told his audience that the protein of maize, zein, contained no tryptophane, which had been shown experimentally to be essential for
normal growth. He postulated that the damaged or inferior maize eaten by Italian and Egyptian peasants contained a “complex organic body” which, by virtue of minute chemical changes, could not be used by the body in “the normal chain of living processes.” Sambon disagreed: “Sandwith, by espousing the deficiency of nutrition theory, had stepped backwards about two hundred years.” Sambon affirmed his belief in the infectious nature of pellagra; he emphatically denied saying that Simulium reptans was the only possible carrier of the infection, and he now suggested that other blood-sucking midges might be involved.

Up to this time, it had been generally thought that disease was caused by the action of something—miasma, divine intervention, climate, over indulgence, poison, germ, or any other of a long list of potentially harmful substances or events.

In the audience at the February meeting was a young Polish biochemist, Kazimierz (‘Casimir’) Funk (1884-1967). Funk came to London in 1910 to work at the Lister Institute of Preventive Medicine, after several years working in biochemical laboratories in Paris, Berne and Berlin. In the 1890s Christiaan Eijkman had observed and experimented on polyneuritis of fowls, which is a good model for human beriberi; and Eijkman’s colleague Vorderman had established by epidemiological investigations of the East Indies prisons, that beriberi was less likely to occur in individuals who ate brown, rather than polished, rice. Funk, in 1911, isolated from rice polishings the substance that prevented polyneuritis in fowls. The following year, he also isolated the same, or similar substances with anti-beriberi properties from yeast.

Funk made the important intellectual breakthrough that beriberi was due to a lack of something; this something, found in the diet, he named vitamine. It took a little while before the name, respelled vitamin, became generally
accepted. He then suggested that a number of other diseases, including scurvy and pellagra, might be due to a deficiency of a dietary something. Funk admitted that the evidence for pellagra being a deficiency disease was thin on the ground, but suggested investigations along the lines of those proposed for beriberi, in other words, perhaps pellagra was due to “over-milled’ maize, in which case the cure could be found by “undermilling.” He also suggested that changing maize for potatoes might be advantageous.

The Thompson-McFadden Committee, based in Spartanburg, proposed to give its first progress report on 3 September, 1913 and had invited Sambon to attend. If he accepted the invitation, he would have no excuses for not visiting the West Indies. He accepted.

The timing of the American visit pleased him as it would allow him to attend the Tropical Medicine section of the International Medical Congress (August 6th to 12th, 1913) at which Manson, who had just retired, was to be presented with a gold medal. Blanchard used the opportunity to hail Manson as “the Father of Tropical Medicine” (Fig.9.2). It was at this meeting that Wellcome’s Historical Medical Exhibition was opened, to great acclaim (see chapter 6.)

The book illustrator, Monro Scott Orr, also used the occasion to draw a cartoon in which the protagonists were not, as might have been expected, Manson and Blanchard, but rather Blanchard and Sambon (fig. 9.3). Orr’s son William, who later became a research veterinarian, and was a friend of Sambon’s, introduced Orr to Sambon. It has been suggested that Sambon asked Orr to draw the cartoon, which refers to Blanchard’s writings on steatopygia (large, fatty buttocks) and partial albinism.

Orr’s depiction of Sambon as an Italian bandit was taken from Terzi’s 1903 cartoon (fig. 3.5). The general design of the cartoon parodies one by Captain
Fig. 9.2. The Tropical Medicine Section of the International Medical Congress, London, 1913. Sambon is standing behind Manson. On Manson’s left is Blanchard. Alphonse Laveran, distinguished by his white beard is the gentleman 3rd from the right in the front row. Courtesy of the International Federation for Tropical Medicine.

Frederick Marryat. Marryat’s cartoon was engraved by George Cruikshank, published on 10 October 1818 and entitled ‘‘Puzzled which to choose’’!! or the King of Tombuctoo offering one of his daughters in marriage to Capt._ anticipated result of ye African Mission.’ Marryat was alluding to the proposed expedition of 1818 to find Timbuctoo, in which Marryat was to accompany the 28 year old Joseph Ritchie. The whole mismanaged affair mutated into the disastrous Ritchie and Lyon expedition of 1819. Ritchie died, and Lyon was forced to turn back while still far from his goal. The cupid comes from an engraving, *Love and Beauty*, of, dubiously, 1822. Beauty was the ‘‘Hottentot Venus,’’ Saartjie Baartman, a woman of the Khoikhoi people from South Africa’s Eastern Cape who, because of her steatopygia, had been put on prurient public displays in England, Ireland, and France (where she
Fig. 9.4 M S Orr. “An Italian Bandit offering a French Gentleman the piebald one of three ‘hottentot’ women.” Courtesy of the Wellcome Library.

1 Raphael Blanchard. 2 Louis Sambon. 3 Partial albinism. 4 ‘Steatopygia’ or the Venus Hottentot. 5 Sir James Cantlie. 6 Sir Patrick Manson. 7 ? William Orr, the artist’s son. 8 ? James Gilchrist, a cousin of William’s. 9 Rickard Christophers. 10 Sir William MacGregor. 11 Robert Leiper. 12 Sir Ronald Ross.
died in 1815). Ross, notoriously fond of threatening libel actions\textsuperscript{26} seemed to have ignored Orr’s Tomboctoo cartoon, or at least to have had the sense not to take legal action over the image (no 12 in the key to figure 9.4) of his head on a pike.

When he saw the book is not known, but Sambon must have been exceedingly pleased to read the dedication in Stewart Roberts’ book on Pellagra, published in St Louis in 1913: “To that long line of physicians and scientists from Casál, through Lombroso to Sambon, and those who shall come after them…” \textsuperscript{27} Now that the HME had been officially opened, to great acclaim, Wellcome had no reason not to allow Sambon to visit the Americas. Besides, he had announced at the opening of the HME that Andrew Balfour had returned from Khartoum, and would head a new entity to be called the Wellcome Bureau of Scientific Research. Sambon would start work at this new laboratory on his return from the Americas. Sambon sailed from Southampton on the S. S. Philadelphia, arriving in New York on the 30 August, 1913.
Chapter 10. The Americas.

On his arrival in New York, Sambon was interviewed by reporters. He told them his theory. The New York Times called him “Professor Louis Sambon, Head of the Tropical School of Medicine,”¹ an error he seems to have done nothing to correct.

He then made his way to Spartanburg, an important textile town in the northern part of South Carolina, which was the headquarters of the Thompson-McFadden Pellagra Commission.

South Carolina’s newspaper The State hailed Sambon as “the world’s greatest authority on pellagra.”² The meeting was held in the conference hall above the Merchants & Farmers Bank. Sambon spoke first. A summary of the Commission’s progress report was read later that evening, not in the conference hall, but at the Spartanburg Country Club. The report showed that in their study area pellagra was two and a half times more prevalent among whites than among non-whites, affected women three or four times more often than men, was an urban disease (the residents of cotton-mill villages) rather than a rural one and affected the poorer sections (“people of insufficient means”) of the community. The investigators could find no difference in the diets of pellagrins and non-pellagrins, but noted the low levels of consumption of fresh meat among the “working classes” generally. There was nothing to suggest that corn (maize) products were the causative agent of pellagra.

An associated report from the US Department of Agriculture’s Bureau of Entomology noted that Simulium was of “comparatively moderate abundance” in Spartanburg County. Its biting habits tended to make it only a minor nuisance to man, and its local attacks on man were largely confined to field
workers. The report added “Had Sambon’s theory not been advanced, these flies could hardly have attracted suspicion of any connection with pellagra in this country.”

After describing the various laboratory tests that had been undertaken (including the injection of monkeys with blood from pellagrins, with no detection of any abnormal micro-organisms) the authors concluded that corn (maize) was an unlikely cause of pellagra; that pellagra was most likely an infectious disease, transmitted somehow from person to person; they discounted the idea of Simulium, but offered reasons for thinking that stable flies (Stomoxys species) might be involved, and that this should be further investigated. Crucially, the Commission recognised that the diet of many people in the affected areas was very poor but thought this was merely a predisposing factor. As to the cause, the commission had reached no conclusion, agreed that the disease was not directly contagious, though in all probability infectious, discarded the ‘Lombrosian’ theory, and supported Sambon’s hypothesis.

After the conference, Sambon spent ten days in South Carolina, visiting Charleston, the near-by Johns Island, and other places where pellagra was endemic among African Americans. He eagerly sought Simulium flies, but failed to find any. He returned to New York, and on 13 September sailed direct to Panama where he met General Gorgas (fig. 2.5). He learned that pellagra was well known in Panama, occurred largely in women of West Indian and African descent, but had not been reported in children. Sambon considered that this last was probably more apparent than real. Accompanied by Siler and Alan Jennings, an American entomologist, he then visited British Guiana before travelling to the islands, including Trinidad, Barbados, Antigua, and Jamaica, (in whose lunatic asylum, in 1898, Dr McCormack recorded the first instance of pellagra in the western hemisphere). He was
fascinated by the volcanic geology of the Lesser Antilles, and made extensive notes, which were later published.  

Sambon found numerous cases of pellagra in the hospitals, asylums and rural districts. He reported anecdotal evidence of pellagra in Cuba and the French West Indian colonies of Martinique and Guadeloupe. He arrived back in England on 6 January 1914.

Manson, who had retired from the London School, was working on the fifth edition of his ever popular *Tropical Diseases A manual of the Diseases of Warm Climates*. He now chose to support Sambon in an unusual manner. The frontispiece of all four previous editions of his textbook had been a colour plate showing different stages of the malaria parasite. For the fifth edition, the frontispiece consisted of a colour painting by Terzi of an English patient with pellagra (fig.10.1).

![Frontispiece for Manson’s 5th Edition of *Tropical Diseases*. Courtesy of Wellcome Images.](image)
In the chapter on pellagra, Manson discussed both zeist and anti-zeist ideas, but dismissed nutritional deficiencies in a few lines:

“Maize stands high as regards alimentary value. Insufficient nourishment may bring about inanition and marasmus, but never causes specific lesions like those of pellagra. Entire populations who live solely on rice or potatoes remain quite free from this disease, although these foods are far inferior to maize in nutritive value.⁵

Sambon barely had the time to sharpen his pencils to start work on his reports (Wellcome had insisted that there be two reports, a short one for the Colonial Office, and a more detailed one for him,), before Balfour (fig. 10.1), now his superior, left London for an extended trip to South America and the West Indies.⁶

Fig. 10.1 Sir Andrew Balfour. Courtesy of Wellcome Images
Balfour expected, but, much to his annoyance, never received, copies of the early drafts of Sambon’s report. (Balfour travelled by steamer, train, canoe, mule and road from Barbados to Grenada, Trinidad, Venezuela, Colombia, Panama, Jamaica and Cuba before returning via New York and arriving at Liverpool on 17 July 1914. Balfour was perhaps being a little hopeful.) When he finally returned to London, he was irritated by how little progress Sambon had made, and started to push Sambon to complete his report. The delays, Balfour said, led to problems with the Colonial Office.  

On 4 August, 1914, Great Britain declared war on Germany and Austro-Hungary. Six days later Sambon’s good friend Henry Johnston-Lavis, hurrying back to Britain, was killed in a motor-car accident in the South of France. Sambon, pushed to write his report, was unable to get to the funeral.

Some of the delays in producing the reports were due to Terzi, who was asked to do the illustrations, including coloured drawings of British and other pellagrins, and of *Simulium* flies. Some of the flies had not been fully identified, leading to further delays. There were problems over how much Terzi was to be paid for the illustrations. Terzi himself was not without his faults: on an earlier occasion, in 1909, he had tried to palm off his failure to produce some diagrams for a report, for which he had been paid, by suggesting that his friend and supporter Sambon must have stolen them. He later admitted that he had never completed the diagrams. The colour plates of patients were to be printed by a firm in Edinburgh, but, as they explained, there were delays “due to the way in which our staff have responded to Lord Kitchener’s appeal” [of August, 1914, when Kitchener appealed for 100,000 men to fight the war.]

The second report of the Thompson McFadden Commission was presented in June 1914. It found that in the Spartanburg area being investigated, pellagra
was rare in infants, but became increasingly common in children between the ages of two and ten, before declining. The disease occurred most frequently in the white population, and was more common in women than men, but the gender disparity disappeared in old age. The authors concluded that these results suggested the home as the place where the disease originated. This suggestion received support from the finding of a close association of cases with pre-existing cases in the same house. An intensive study of food found no evidence of an essential pellagra-producing, or pellagra-preventing, food. Pelagra also seemed to be rare in houses with water closets and more common in houses with inadequate or insanitary privies. Simulium was again shown to have nothing to do with the disease. The report, inconclusive in many ways, provided little comfort for Sambon.

On a number of occasions after August 1914 Herbert Read of the Colonial Office wrote to Balfour to find out when Sambon’s report would be available. Initially Sambon was expected to produce two reports, a “General and Preliminary report” for the Colonial Office, followed, probably much later, by a full report, which would be in the form of a book. When neither report appeared, Balfour started to threaten Sambon, who was outraged by Balfour’s attitude. He complained of being bullied and treated like a boy. He asked for permission to work on a paper on the human bot-fly, or warble-fly, *Dermatobia hominis*, that he had come across on his travels. Balfour agreed, telling the Colonial Office that, in addition to the pellagra report, Sambon was working on a report on health conditions in the West Indies, of which the warble-fly report would be the first part. Perhaps Balfour had not yet appreciated that Sambon’s writing could be extremely prolix. Sambon admitted that his observations on the flies, made in Cartagena, British Guiana and Trinidad, had been “quite casual” yet he used the opportunity to write an extensive review. The review, illustrated with black and white illustrations by
Terzi, makes fascinating reading, and shows off Sambon’s erudition and the depth of his bibliographic research. It reads as a labour of love, but was of little relevance compared to the work on pellagra, which was falling further and further behind. It was, however, forwarded to the Colonial office.

Sambon was again in financial trouble. Hilda was pregnant. Money was tight. Three years earlier he had borrowed £91.10.0 from Henry Wellcome, to be repaid out of his salary, or from the sale of some of his manuscripts and geological and zoological specimens. As a surety, he lodged some jewellery, presumably Hilda’s, with Wellcome. In addition, the money Wellcome had advanced for the Bale proofs had not yet been repaid. Unfortunately, Wellcome did not buy any of Sambon’s manuscripts or specimens, as the parties could not agree on a price. Three years later, the debts had not been repaid. Joseph Collett Smith, the Secretary of the Wellcome Foundation, wrote to Balfour in August 1914, suggesting that it was now time for the debt to be settled. He suggested that £10 a month should be taken out of Sambon’s salary. Balfour, not unreasonably, wanted to have nothing to do with the affair. As far as he was concerned, it was a private matter between Sambon and Wellcome, though he did say that he considered Sambon’s position indefensible and reprehensible. He suggested, however, that he might buy some of Sambon’s books, which could be useful to the Bureau. Wellcome could then use the account to settle some of the debt. Balfour added,

“I am afraid if the question comes up now, there will be trouble. I have succeeded, with some difficulty, into getting Dr Sambon into line – He is working very diligently and, so far as I can see, giving of his best. Any such application will assuredly send him off his balance and interfere with his report for the Colonial Office and his book on pellagra."

A month later, Collett Smith wrote to say that the matter would be held over for a more suitable occasion. When, if ever, that was is not clear.
In December 1914, Balfour wrote to the Colonial Office to say that there would now be no preliminary report, “It is the full general report which will be sent, and it is of an exhaustive nature”.

Unfortunately, Sambon strayed from the path of diligent work. On 8 February, 1915, Balfour wrote to Sambon, saying that there were no personal feeling involved, but he regretted that Sambon seemed unable “to conform to those methods and habits of work without which no institution of this kind can make any progress.” He terminated Sambon’s appointment, and offered him three months’ pay in lieu of three months’ notice. Sambon replied:

“…I have worked continuously as many hours as it was physically possible Taking most of my meals at my desk.

You have treated me in a way that I never expected from a man of your standing, ability and equity—after all, I am a colleague of yours in medicine, science and arms and, like yourself I have been honoured by governments and Scientific bodies…”

Two days later Balfour wrote to Wellcome to tell him that he had, “with some regret, but a genuine feeling of relief” terminated Sambon’s appointment with the Wellcome Bureau. After complaining that he was driven to presenting Sambon with an ultimatum, he wrote:

“Frankly he is impossible, and no one with any self-respect or sense of discipline or knowledge of the value of properly regulated work can deal with him. I have put up with a great deal & would have with much more for your sake if only my own comfort and peace of mind had been concerned. It became however quite another matter when the Bureau was made ridiculous in the sight of the Colonial Office….Sambon came home in January 1914. Before I left for South America I arranged that he should send me batches of his M.S. which I intended to revise and return so that no time should be lost. Nothing was sent me. On my return I accepted his somewhat lame excuses and urged him to fresh efforts outlining the plan of campaign and insisting on the necessity for system and methods. I gave him no other
work to do, and from his promises and protestations was led to think that he had at last turned over a new leaf & was going to play the game. On August 6th the Colonial Office enquired as to whether Sambon’s report would be ready that month. After seeing Sambon and hearing what he had to say, I replied that he hoped to send in his general and preliminary reports within a month…Needless to say, there was no report forthcoming though I kept hammering at him. He had no other work to do. I gave him every assistance and facility…[but no reports were produced and] on February 3rd I wrote Sambon telling him that unless his Colonial Office report, which, after all was only a fragment of his main report was in my hands by noon on February 8th I would terminate his appointment here.”

Wellcome was unwell, and had travelled to southern Europe and North Africa for convalescence. His annotation on Balfour’s letter was unforgiving: “I’m sure you have been more than patient and forbearing and have spared no effort to save him from himself. This unfortunate affair does not upset me in any way.” He then goes on to complain about the atrocious weather he had encountered during his convalescence.

What Sambon’s “lame excuses” were is not recorded, but it is possible to get a glimpse of his problems. He had originally hoped to write a book about pellagra, in which his theory would have pride of place. But 1914 was a difficult year, and, more important, his theory was beginning to crumble. Funk published his influential book on the vitamins, and included pellagra as a disease due to a vitamin deficiency. Funk’s other three avitaminoses—the word is his own coinage—were beriberi, scurvy and rickets. Sandwith, in his presidential address to the Society of Tropical Medicine and Hygiene in October 1915, again repeated his belief that pellagra was a nutritional deficiency disease. An American physician, Joseph Goldberger, was thinking along the same lines.

Goldberger was a physician with the US Public Health Service. He had worked on the transmission of typhus, and on Rocky Mountain Spotted Fever,
so was a reasonably knowledgeable microbiologist. He had an interest in pellagra (in 1911 he had attempted unsuccessfully to transmit pellagra to rhesus monkeys using blood and spinal fluid from pellagrins) and had read the literature. Early in 1914, he succeeded Lavinder as the US Public Health Service’s chief pellagra investigating officer, by which time he was already convinced that pellagra could not be an infectious disease;\textsuperscript{14} nurses and attendants in the hospitals and asylums never contracted the disease, even though many of them lived in the wards with the patients, an observation that even Sambon accepted. Neither contact in any sense of the word, nor insect transmission, could explain the “peculiar exemption or immunity” of the staff. What did distinguish the staff was their better diet, and he was convinced that diet explained the disease. He then suggested that a nutritious diet, one in which cereals and canned foods were replaced by meat, milk and fresh vegetables, would prove beneficial. Goldberger’s dietary prescription was moderate: half a pound of lean meat per twenty-four hours, a pint and a half of fresh milk, four eggs at least and pea or bean soup given freely. He wrote: “The comparatively recent studies that have definitively established beriberi as a “deficiency” disease...in a sense hardly dreamed of before, have given a new and added significance to the more recent studies of nutrition and have opened a practically virgin field to the student of epidemiology and therapy.”\textsuperscript{15}

In 1914-1915, Goldberger carried out a number of dietary interventions in orphanages and asylums, showing that diet could prevent, and sometimes cured, pellagra.\textsuperscript{16} As if this was not enough, in 1915 he induced pellagra in six out of eleven prison volunteers by placing them on a restricted diet, rich in carbohydrates and almost entirely devoid of fresh protein and green vegetables.\textsuperscript{17} He died, on 17 January, 1929, with the larger investigation unfinished. Niacin was isolated from liver by the American biochemist Conrad Elvehjem in 1937.\textsuperscript{18}
Shortly after losing his job at the Wellcome Bureau, Sambon found employment as a medical officer at a military convalescent hospital, Holmleigh, in Harrow, established under the auspices of the Voluntary Aid Detachment of the British Red Cross. The hospital, described in a nursing journal as “a charming little place” was managed by Charles Thompson, the curator of the Wellcome collections, who lived in Harrow. Sambon continued to lecture at the London School, but numbers of students (and staff) were dwindling. By January 1918, only two students were enrolled, but with the cessation of hostilities, numbers soon picked up again. He was not at the School when, on the evening of 19 January, 1917, some fifty tons of TNT ignited in the Brunner, Mond factory in Silvertown, West Ham, killing seventy-three people. Numerous windows at the school—which in 1917 was still at the Royal Albert Dock—were blown out by the blast, which could be heard as far away as the Sussex coast. He also spent a considerable time during the war years trying to establish how fodder crops might be turned into food for human consumption.

Sambon’s report on pellagra in the West Indies was finally presented to the Colonial Office in February 1915, in the form of a preliminary report of 18 pages, and a ‘further’, but incomplete, report of 43 pages. In the short report, he states that the purpose of the journey was to establish whether the disease reported as pellagra in the West Indies was indeed pellagra. On this point he had no hesitation in stating that it was. There was some confusion in the local doctors’ minds between pellagra and sprue. He devoted a considerable amount of space to a discussion of sprue, emphasising that, in sprue, though the symptoms included diarrhoea and a sore mouth, the skin lesions so typical of pellagra did not occur. He noted the common association of pellagra with a variety of intestinal worms and protozoa, and other non-intestinal diseases. As in many of his papers, he introduces irrelevancies: he saw a patient suffering from both pellagra and “ulcerative granuloma of the pudenda” but
then side-tracks to write about smears he had seen from such a patient. He examined a number of patients in the leprosy hospitals, but failed to find any evidence of pellagra. His report makes no mention of flies.  

In the addendum to the report, he describes all the places he visited, and the pellagrins he saw. There is little useful epidemiology to be found in these pages, apart from the existence of pellagra in many places. He devotes twenty-three pages to a discussion of “Food and Other Etiological Theories.” This consists largely of a polemic against zeism, coupled with quotes from those who support his theories. Of the people of Barbados he says, “I was surprised at the variety of their fare and the excellence of their cuisine,” but says nothing about the diet of those suffering from pellagra. Apart from a quote from Blanchard (“it is urgent to elucidate an important question of parasitology and epidemiology in which natural history will probably determine the issue by the discovery of the transmitting agent— insect or acarid”) there is no more mention of Simulium or other biting insects. Nor are there any references to Goldberger or Funk.

In February 1918, Sandwith died in Bournemouth, where he had gone to recuperate after two difficult years with the army in Egypt. Though he disagreed at times with Sambon (whose hypothesis he had labelled ‘brilliant’), his arguments were always concise, polite and well-delivered.

By the end of the Great War, it was accepted that pellagra was not an infectious disease, but was connected to nutrition, probably through the substances now renamed vitamins. Sambon, however, was never quite prepared to let go of his hypothesis. He stated in 1918 that pellagra in Barbados was only contracted in places where a blood-sucking midge could be found, and:

“The majority of physicians still cling to the belief that [pellagra] is a food-disease, due either to unsound food or food of deficient quality, but they might just as
fatuously continue to hold that scabies is a food–disease…and for consistency’s sake they should also continue to uphold the old belief that the ingestion of arnuts or earth-nuts (*Bunium flexuosum*) generates lice in the head.  

At a meeting of the Royal Society of Tropical Medicine and Hygiene on 21 May, 1920, pellagra was again the focus of discussion, but Sambon did not attend.

As late as 1922, he wrote that his epidemiologic researches throughout Southern Europe, the southern states of America, and the West Indies, as well as the realisation that pellagra was “endemic throughout the British Isles, led to the overthrow of the maize theory,” which was probably correct, but for the wrong reasons.

In the 1919 book *Pellagra* Harris, the Atlanta, (GA) physician who claimed to have diagnosed the first case of pellagra in the United States, launched a blistering attack on Sambon:

“Without any practical acquaintance with pellagra worth speaking of, and clearly ignoring the enormous amount of work that had been done on the etiology of this malady, Sambon some years ago halted long enough, on a flying trip across Italy, to inform an expectant world, even by telegraph, that he had discovered the cause of pellagra. Before leaving England he in some way succeeded in having a “pellagra commission” formed, though his writings do not make clear just how this was done…It would appear that he left his own country obsessed with the idea that pellagra is an infectious disease transmitted from man to man by some biting insect, and with admirable quickness of decision he immediately inculpated a wholly supposititious parasite and assumed that it is transmitted by one of the small gnats of the genus *Simulium*…”

Charles Bryan writes disparagingly of Sambon’s hypothesis, accusing him of ‘obsessional’ behaviour. “It will be argued here that Sambon’s intricate speculation more than anything else slowed Americans’ scientific assault on
pellagra, leading to thousands of deaths that might have been prevented." To place the blame entirely on Sambon seems unfair. He was neither the first, nor the only person to suggest an infectious cause. Even if the spoiled corn theory had prevailed, bacteriology was bound to play a large part in trying to isolate an aetiological agent. Others might argue that the clash of ideas about the causes of pellagra stimulated a considerable amount of useful scientific research in a large number of scientific fields; as Lavinder pointed out in a 1911 paper, there were globally numerous epidemiologic quirks associated with pellagra that raised more questions than provided answers.

Why the Americans in particular should be so harsh about Sambon is difficult to understand, particularly as their medical care for the underprivileged left much to be desired. Others had suggested hypotheses which were later discredited without receiving the censure Sambon had to endure.

Sambon had had enough. Eager for a new direction, he turned to the study of cancer.
Chapter 11. The Elucidation of cancer

Only six weeks after the end of the Great War, Sambon met Henry Wellcome, who suggested that he write an article on the history of artificial limbs. This might involve a trip to Italy. Sambon had already contacted Balfour about publishing some of his work on parasites of snakes, and Balfour seemed agreeable. However, when he now asked Balfour whether he could do any work for the Bureau (the Wellcome Bureau of Scientific Research), Balfour was certain. He could offer Sambon a room for study, and the use of the library, but that was all.¹

Sambon was not interested in artificial limbs; he preferred the parasites. He now decided it was time to refresh his credentials, which had lapsed since the publication of his pellagra paper for the Colonial Office in 1915. He chose as the vessel for this effort the newly established magazine *West Africa*, published weekly in Fleet Street with the financial support of two important Liverpool shipping and trading companies, Elder Dempster Shipping Ltd, and John Holt and Co., Ltd.

Sambon reminded his readers of his expertise. He was the one who had suggested that health had nothing to do with climate, but a great deal to do with parasites. He had deduced that blackwater fever had nothing to do with quinine. He had argued successfully against Sir David Bruce’s opinion that the transmission of trypanosomes by tsetse flies was a purely mechanical phenomenon. *Schistosoma mansoni* became Sambon’s schistosomiasis. He mentioned all the parasites and other creatures he had discovered. More, he had found endemic pellagra in Great Britain. Perhaps pellagra had been similarly overlooked in West Africa? In Panama, much had been done to protect the health of the inhabitants. If Panama, why not West Africa?
Considering that he had never been to West Africa, it was a remarkable *tour de force*.\textsuperscript{2}

The lights may have come on again after 11 November, 1918, but the post-war years started badly for Sambon. His good friend Raphaël Blanchard died suddenly on 8 February, 1919 at sixty-one. Around the same time, Terzi produced a cartoon of Sambon (fig.11.1) that Lise Wilkinson called “somewhat unkind.”\textsuperscript{3} If, however, the cartoonist’s art is to emphasise and exaggerate salient physical characteristics, then Terzi has not been unkind.

Fig. 11.1 Cartoon of L W Sambon by Amedeo Terzi, 1919. Courtesy of Wellcome Images
Sambon still sports his luxuriant moustache, but he has now developed a significant paunch. This is perhaps not surprising since Sambon had a reputation as an excellent cook—he gave occasional lectures on the historical aspects of cookery—and a connoisseur of fine wines, especially the better Tuscan wines. Among his favourite artefacts was a lump of solidified wine from Pompeii. He used to say that if it could be liquefied, it would make a fine drink. Sambon befriended a young Indian physician, Amolak Ram Mehta, who came to England in 1920 to obtain degrees in public health and tropical medicine. Sambon invited Mehta to dinner, and, unaware that Mehta was teetotal, offered him some fine Tuscan wine, saying, “a day without wine is like a day without sunshine.” When Mehta refused the wine, an appalled Sambon went upstairs, woke six year old Arthur, and pressed the glass of wine on the child. Who happily downed it.4

Sambon followed up his West Africa article with a lecture at the Royal Society of Medicine on 14 June 1919, about sanitation in the tropics.5 This was based on his travels through the West Indies, and ignored the fact that he had no practical experience of the subject whatsoever. He did, however, make an interesting proposal: a school of tropical medicine of a new kind, a floating school, on which students could be brought to tropical places, see actual tropical diseases in their natural state, and have opportunities to investigate the sanitary problems associated with these diseases. The West Indies could be an ideal place. He added that he had made a similar suggestion to institutions in both Italy and France, which were broadly supportive of the idea (though he does not say where, or when, or to whom, he made the suggestions.).

In mid-July 1919, a committee, with Andrew Balfour as chairman, was set up to consider the proposal. Balfour’s Sudan experience had included the deployment in 1907 of a small, but effective floating laboratory, the first in the world of its kind. The laboratory, a small barge towed by a tug,
appropriately named *Culex*,(fig. 11.2) was based at the Wellcome Tropical Research Laboratories in Khartoum. Not unreasonably, Balfour now preferred the floating laboratory scheme, while Sambon wanted the ship merely as a transport. An editorial in the *British Medical Journal* chose Balfour’s scheme; recognised that either scheme would be expensive; and thought that the Colonial Office might properly be expected to contribute. It did not offer to do so, and the idea faded away.

Fig. 11.2 *Culex* towing the Wellcome floating laboratory. Courtesy of Wellcome Images

Balfour had been succeeded as Director of the Wellcome Laboratories in Khartoum by Sambon’s friend and erstwhile companion Albert Chalmers. Sambon experienced another blow when Chalmers, on a round-the-world trip, developed acute hepatitis in Calcutta, and died on 5 April 1920. He was fifty years old.

The Albert Dock site for the school and hospital was now proving inadequate, as well as inconvenient for students and teachers to get to. Balfour joked that
the journey from Fenchurch Street resembled the apocryphal Scottish elder’s description of a heretical brother as “like a November day, short, dark and dirty,” except most of the time it was not short.6

The Endsleigh Palace Hotel, at 25, Gordon Street, close to Euston Station, had been requisitioned during the war as a hospital for officers. With assistance from the Red Cross, which donated £70,000, the hotel was converted into a new School and hospital (fig.11.3). The opening ceremony was performed on 11 November 1920 by the Duke of York, (later King George VI) who had earlier in the day attended the unveiling of the Cenotaph. The earliest patients were veterans of the War.

![Image of Endsleigh Palace Hotel](image.png)

Fig. 11.3 The Endsleigh Gardens School of Tropical Medicine and Hospital for Tropical Diseases. (Now the University College, London, Union). Courtesy London School of Hygiene and Tropical Medicine.
In May, 1921, Sambon’s father Giulio (Jules), who had been living with Louis in Hampstead, died after a long illness. He was 84. He left some of his collection of Roman coins to Louis, who sold them at auction in November 1925 (making £1,124) to pay off some of his debts. Less than a year after his father’s death, on 9 April, 1922, Sir Patrick Manson, Sambon’s friend, mentor and supporter, died. Sambon was grief-stricken, but even at this critical juncture, he managed to court dissent.

According to Manson’s obituary in the *Times*, modern tropical medicine was born when Manson showed filariae to be transmitted by mosquitoes, and later discoveries of the part played by these insects in the transmission of malaria and yellow fever could be attributed to Manson. Ray Lankester was having none of this, and wrote accordingly to the journal *Nature*.

Lankester had from the very beginning openly expressed his disbelief in the mosquito transmission of filaria story, prompting a vigorous and well-argued letter from Sambon in *The Times*. Not himself a medical man, Lankester also believed that tropical diseases were all parasitic and should therefore be investigated by parasitologists, not mere medical men.

Sambon leapt to Manson’s posthumous defence. In a detailed letter he described Manson’s role in the discovery of the mosquito/malaria cycle. He claimed that he, Sambon, had been “almost daily” at Manson’s house when Ross was doing his work in India, “where I had the privilege and good fortune of being able to follow step by step the unfolding of one of the most wonderful chapters in tropical medicine.”

Ross was outraged. He immediately drafted a strongly worded letter to the editor of *Nature* in which he belittled Manson’s role in the discovery of the mosquito/malaria cycle. Ross claimed the priority for the idea of using bird malaria as a model. Many of Manson’s ideas about the transmission of
tropical parasites had, Ross said, been wrong. Thus Manson had been mistaken in supposing that filarial and malaria parasites might be carried by mosquitoes into drinking water, and that sleeping sickness was due to \textit{Filaria perstans}. Attacking Sambon directly, Ross wrote:

“[T]here is a large class of writers, especially among medical men, who seem to think that a guess is as good as a proof and who hasten to publish... any speculation, however bizarre or commonplace, which happens to occur to them. If the guess turns out to be wrong, a veil is decently drawn over the failure; but if by good fortune it proves right, the ingenious or ingenuous speculator proceeds to lodge a claim on the discovery...Tropical medicine is full of such attempts.”

The pellagra/fly story was such a speculation. Furthermore he suggested that Sambon was merely promoting himself by saying that the Sambon and Low experiment in the Roman Campagna laid the last brick in the mosquito theory. It was an unkind, bad tempered and revealing letter.\textsuperscript{13} Fortunately for his reputation Ross was persuaded not to send it. Even the published letter expressed his outrage that Sambon had been allowed, without his knowledge or consent, to see his letters, and said he could not accept any of Sambon’s conclusions.\textsuperscript{14}

In a further obituary notice, Ross again belittled Manson’s role in the malaria work, and claimed that Manson was surrounded by ‘\textit{claqueurs}’ (hired applauders) and ‘\textit{poseurs}’ among whom he named Sambon.\textsuperscript{15} Ross was, of course, correct on one point: Manson was not the first person to have suggested that mosquitoes played a role in the transmission of disease. In 1807, Dr John Crawford of Baltimore had suggested that mosquitoes played a role in malaria; in 1848, Dr Josiah Clark Nott, of Mobile, Alabama suggested that mosquitoes had a role in the transmission of yellow fever, an idea given practical reinforcement in 1853 by Dr Louis Daniel Beauperthuy, a Frenchman working in Venezuela. And in 1883 Albert Freeman Africanus
King (1841-1914), an English born American physician, proposed on theoretical grounds that mosquitoes transmitted malaria. Manson’s ideas and deductive work, however, were essential in getting the hypothesis accepted, then proved. Ross grudgingly admitted as much.

In 1923, the year after Manson’s death, Ross published his memoirs. He mentions, very briefly, the Roman Campagna malaria/mosquito experiment, largely to disparage it. “It is impossible to estimate, from such details as are given, the actual chances of infection in the locality for unprotected persons, and therefore to gauge the scientific value of the experiment.”

Sambon had had his say, and now proposed to concentrate on an idea that had been germinating in his mind for a long time: the causes of cancer.

Awareness of cancer was nothing new. As Sambon was fond of reminding his audiences the disease was known to the ancient Egyptians, and to doctors in classical Greece and Rome. For almost two thousand years, humoral theory had dominated ideas about the causes of cancer (generally considered, like melancholy itself, to be due to an excess of black bile); but new ideas were beginning to surface. In the seventeenth century, two physicians practicing in Holland, Zacuto Lusitano (1574-1642) and Nicolaes Tulp (1593-1674) (fig.11.4) independently proposed that cancer was contagious. They had observed that breast cancer sometimes occurred in the same household.

Germ theory provided fertile ground for similar ideas. In 1887, Sir James Paget said, “I believe that micro-parasites, or substances produced by them will some day be found in essential relation with cancers and cancerous disease,” and condemned the use of the word ‘metastatic.’ In 1899, two eminent surgeons, Jackson and Barling, independently and explicitly promoted the view that cancer might be due to parasites, an idea supported by Sambon.
1. By 1901, the pathologist George Adami could spell out, though not necessarily agreeing with them, the reasons proposed for an infectious cause of cancer:

2. The increase in cancer in “civilised communities” in the past four decades was out of proportion to any factor except the gradual spread of an infection.

3. Cancer was often found to be localised, to low-lying areas, to certain villages, and even to certain houses (whether or not those were blood relatives).

4. Cancer and tuberculosis were symptomatically analogous: both had a primary site, and both were prone to dissemination or metastasis. Why should they not be aetiologically analogous?

5. Careful examination of malignant tissue increasingly had shown the presence of both intra and extra-cellular bodies of unknown, but possibly infectious origin.

6. If microbes were responsible for some cancers, their ability to do so was limited.\textsuperscript{22}
In the late nineteenth and early twentieth centuries, a number of investigators showed that it was possible to transplant some cancers of rats and mice within the same species. This suggested the possibility of an infectious cause, though little attention was paid to these results. In 1902, Danish investigators showed that a mouse cancer could be repeatedly and indefinitely transplanted into successive generations.

The first Egyptian Medical Congress was held in Cairo in December 1902. Here, examples of ‘tumours’ resembling epitheliomas were demonstrated in association with the disease schistosomiasis. In 1908, two Danish scientists, Vilhelm Ellermann and Olaf Bang, showed that cell-free filtrates of chicken leukaemia cells passed on the disease to other chickens. Three years later, the American Peyton Rous showed that cell-free filtrates of tumour extracts from Plymouth Rock chickens with sarcomas (a form of malignant disease) could pass the disease to healthy chickens. An infectious cause for cancer seemed increasingly likely.

While dissecting wild rats for evidence of tuberculosis in 1907, the Copenhagen pathologist Johannes Fibiger came across three animals with stomach tumours. He found within the tumours a roundworm (a nematode), which he named *Spiroptera neoplastica* (later renamed *Gongylonema neoplasticum*.) The larval forms of this nematode were found in cockroaches. Fibiger postulated that the rats ate the infected cockroaches, and that some (a high proportion) of those infected with the larval nematode developed tumours. In 1913, after several years of careful research, Fibiger published his findings in the *Berliner Klinische Wochenschrift.*

A leader in *The Times* of London commented on Fibiger’s work:

“There is at least no ground for supposing that either the rat or the cockroach plays any important part in the propagation of cancer to human beings, or that their liabilities to the disease or to its causes are more dangerous to mankind than those of
other animals or insects… The work of Professor Fibiger will no doubt be followed to its results by himself and by other observers; but so far it has neither added, nor does it promise to add, to the resources of the healing art."

Fibiger was, controversially, awarded a Nobel Prize for this work in 1926. Later research showed that rats developed cancers when fed on a diet deficient in Vitamin A. Gongylonema had nothing to do with cancer.

Tapeworms are flat worms that generally live and deposit their eggs within the digestive tract of the host. If the eggs are swallowed by an appropriate intermediate host, a larva develops from the eggs. The larva migrates out of the digestive tract and forms a cyst in other tissue, such as liver or lung (thus hydatid cysts are the larval form of the dog tapeworm.) In 1906, the French researcher Amédée Borrel (1867-1936) suggested that rats infected with some forms of larval tapeworms developed sarcomas in their livers. This work was later confirmed by American researchers Frederick Dabney Bullock and Maynie Rose Curtis, who showed in 1920 that feeding young rats with eggs of the cat tapeworm Taenia taeniaeformis (at the time called T. crassicollis) led to the formation of liver sarcomas in a high proportion of the animals. The Times may have purported to doubt the value of all these investigations, works, but others, including Sambon, were not so cynical.

If Sir Patrick Manson had been Sambon’s mentor and hero in tropical medicine, then Louis Pasteur was his hero in germ theory. The centenary of the birth of Pasteur (fig. 11.5) fell on 27 December 1922. Sambon celebrated the occasion in two ways: he wrote a long article, Pasteur and His Work Historically Considered, which was not only a panegyric to Pasteur, but allowed Sambon to show off his antiquarian erudition and understanding of the history of medicine; and he travelled to France for the celebrations.

Dole, Pasteur’s birthplace, held its celebrations on 27 December. Paris and Strasbourg, for no doubt excellent Gallic reasons, celebrated in May, 1923.
Sambon, always the good European, travelling from Paris to Strasbourg, met Dr Giorgio (‘George’) Ghetti, a physician from the town of Faenza, some 50 kilometres south east of Bologna. Ghetti told Sambon that the prevalence of cancer in the entire region (the Romagna) was particularly high and climbing. Ghetti suggested a joint investigation of cancer in the region by British and Italian physicians: the Italians would provide the material and the hospital facilities, the British the money. The opportunity was too good to be missed.

While in Strasbourg, Sambon met both Fibiger and Borrel, (the latter Professor of Bacteriology at the University of Strasbourg at the time) which no doubt stimulated his interest even further.
The British Empire Cancer Campaign (BECC) was launched at the beginning of June, 1923, with the stated aim of coordinating and supporting research on cancer throughout the empire. Though Italy was not in the empire, Sambon approached the Campaign managers for support. The Campaign authorities regretted that, “in view of the purpose and title of the Campaign” they were unable to offer financial support, but agreed to grant him £50 towards expenses, on condition that “the results [of his investigations] were not communicated or published in any form, either in England or abroad, without the previous written authority of the Executive Committee of the British Empire Cancer Campaign.” Sambon agreed.

He spent five weeks in the Romagna. He admitted that he inclined to the parasitic explanation of cancer, but was determined to approach his study with an unbiased mind. On his return to London, he prepared an interim report, which he sent to the BECC requesting permission to publish. The BECC wanted the full report before making any decisions, and this only arrived in early January, 1924. It was sent to two acknowledged experts for comment. William Sampson Handley was a renowned Middlesex Hospital cancer surgeon; Walter Sydney Lazarus-Barlow was the Director of the Middlesex Hospital Cancer Research Laboratories.

Handley wrote:

“Dr Sambon’s report shows him as a literary artist, an accomplished parasitologist and a learned antiquarian, but much of the report is connected with cancer by threads of extreme tenuity. Upon the subject of the origin of cancer he appears in the report rather as an advocate than as an investigator. He believes that the cockroach is the medium whereby a cancer parasite, at present hypothetical, is transferred to man. The evidence in favour of this view is ably marshalled, but Dr Sambon’s visit to Italy has not produced any fresh facts in favour of it, except his observations on the presence of cockroaches in several cancer houses visited.”
After further methodological criticisms, Handley suggests that the report should not be published by the Campaign, as it contained too much extraneous material. Lazarus-Barlow suggested that no “useful purpose would be served by publishing the report at the present time.”

Sambon was keen to know what was happening, and complained of being kept in the dark. “The report contains new and original information and should have been published forthwith,” he wrote to the Secretary of the Campaign. To speed things up, he spoke to John Bale, Sons & Danielsson Ltd, publishers of the *Journal of Tropical Medicine*, who offered to publish his paper on “‘Cancer Research’ written on behalf of the British Empire Cancer Campaign.”

He received no reply from Campaign headquarters until April 1924, when he was given, hesitantly, permission to publish. The reply stipulated that the only mention of the BECC should be an acknowledgement of financial support. Curiously, by the same letter, Sambon was offered a grant of £250 for him and an assistant to travel to Italy, in order to collect pathological and entomological material bearing on his Gongylonema theory, and to bring it back to London for further investigation.

If Sambon had read Ross’s criticisms of his 1910 pellagra paper, he had now almost entirely forgotten them. Once again, his long report took the form of a travelogue, describing where he went, with whom, the patients he saw, the doctors with whom he discussed the problem. Quoting local statistics—this at least was an improvement on his 1910 report—he wrote that cancer, and especially stomach cancer, had increased steadily in the region in recent years. Interspersed with the travelogue are details about cancer in antiquity, about the unequal distribution of cancer in the region (he is led to consider the existence of “cancer-houses”, even cancer streets), about the similarities between cancer, as he was seeing it, and certain infectious diseases; and about
the parasites which might be responsible for cancer. He came across several instances of animal cancers occurring in close association with human cases, which seemed to strengthen his belief in a parasitic aetiology.

Even cursory inspections of dwellings in which cancer had occurred “invariably” revealed the presence of cockroaches and, often, other food associated beetles; if not in the house, then in the out-houses at least. This was particularly significant to him, for these vermin frequently harboured Gongylonema parasites. However, he then added a note of caution. It seemed unlikely to him that Gongylonema would cause cancer by itself. He postulated the existence of an ultra-microscopic organism, that, activated by the worm, invaded the tissue cells, and gave rise to malignant change. 33

Some of the natural history work for the Italian study was conducted by Harry Arnold Baylis, a zoologist at the British Museum (Natural History), and an expert on nematodes (who had some years earlier written scientific descriptions of marine worms collected by the Terra Nova Antarctic Expeditions of 1910-1913). Baylis examined a number of specimens of Gongylonema from Italy. He was uncertain about the exact taxonomic status of the specimens from different hosts. He conducted an experiment, assisted by Sambon’s daughter Juliet, in which two rats were successfully infected with Gongylonema from ruminants, causing him to doubt whether different species of the worm were indeed associated with specific hosts.

Robert Leiper, the London School helminthologist, and Baylis got into a spat over the taxonomy of these nematodes. Leiper argued that none of the Gongylonemata found in domestic stock was associated with cancer in these animals, and the same applied to the few human cases infected with the parasite. He then categorically stated that “There is not the slightest scientific foundation for Dr Sambon’s belief that gastric cancer, stated to be very prevalent in certain houses in some villages in northern Italy, is due to
invasion with gongylonema associated with the presence of rats and cockroaches in these so called “cancer houses.” The editor of the *British Medical Journal* thought that Leiper’s investigation had “destroyed one of the few apparently solid pillars supporting the “cancer house” hypothesis.” The suspicion, however, remained.

Sambon used the BECC grant to travel to Italy and to Iceland, where stomach cancer was said to be common at the time, perhaps related to the widespread consumption of home cured meat and fish. In December, Sambon delivered a lecture to the *Institut Pasteur* in Paris (in French) on the epidemiology of cancer, in which he extended his remarks about cancer and parasitism. He probably felt that in Paris he would be less likely to be attacked for his opinions.

If Sir James Cantlie (see below) is to be believed, Professor Albert Calmette (1863-1933), the assistant director of the Pasteur Institute, wrote to Sambon the following day to thank him for the lecture, and added,

“We are fully disposed to enter into the views which you have declared to us, and to collaborate with you in the researches which obviously it would be extremely useful to carry out in different countries on the aetiology of cancer, and particularly on the part played by parasites in its genesis, a role which has been so well demonstrated by the researches of Professor Borrel in this Institute, Professor Fibiger of Copenhagen, Bullock and Curtis in America, and by your own.”

Despite Calmette’s approval, and for reasons that are not clear, the lecture, which was to have appeared in the January 1925 edition of the *Bulletin de la Société de Pathologie Exotique*, was never published.

One person who was impressed by Sambon’s hypothesis was Charles Edwin Brand. Brand, a journalist who was said to have worked as a diver and salvage expert on the sunken German fleet at Scapa Flow, wrote under the pen
name “David Masters.” Much of his oeuvre is devoted to under-water topics, diving and submarines especially, but he dabbled in medical matters. In 1925, he published *New Cancer Facts*, a small book aimed at the general public, which consists of a eulogy of Sambon and his theory of cancer. The preface was written by Sir James Cantlie (fig. 11.7).

![Fig. 11.7 Sir James Cantlie. Courtesy of Wellcome Images](image)

Cantlie (1851-1926), like Manson a graduate of Aberdeen University and a surgeon by training, had been persuaded in 1888 by Manson to join him in Hong Kong. He became Dean of the newly established Chinese School of Medicine in that colony. Among his first students was Sun Yat Sen, who in 1912 became the first president of republican China. Cantlie returned to London in 1896, and assisted Manson in his efforts to establish a school of
tropical medicine in London. He was appointed a visiting surgeon to the new
School in 1899.

Cantlie also achieved a degree of fame in 1896, when his erstwhile student Dr
Sun, now on the run from Chinese authorities for revolutionary activities, was
Cantlie, despite some difficulties—Scotland Yard was not interested—
managed to get the attention of Prime Minister Lord Salisbury, who obtained
Dr Sun’s release.39

In his preface to the Brand book, Cantlie either was being kind to Sambon,
whom he had known well for more than twenty-five years, or did not
appreciate the limitations of Sambon’s work. He called Sambon “our
foremost epidemiologist…No living man knows as I do the inner life of
Sambon, his struggles to live and carry out the work for which he is so
eminently fitted…Sambon has braved even starvation for the sake of Science,
but what a splendid record of work in archaeology, geology, medical
entomology, protozoology, helminthology and epidemiology.” The British
Medical Journal book critic thought the David Master’s book could do no
good, but could do harm, and “Such a jumble of new cancer opinions, (not
facts) is especially unfair on Dr Sambon whose careful epidemiological
studies deserve much better company.”40 Cantlie, it has to be said, had been
declining in health since the death of his wife Mabel in 1921; he died on 28
May, 1926.

Impressed with Sambon’s investigations, a “Westmorland Field Commission
for Cancer Research” was formed under the chairmanship of Sir Samuel
Haslam Scott, 2nd Baronet, of Yews, Windermere, whose family business was
the Provincial Insurance Company of Kendal. The researchers decided to
carry out an extensive epidemiological study of cancer in the county, though
they give no details of how exactly this was to be done. Their preliminary
results showed that cancer in Westmoreland occurred in clusters, of villages, hamlets or houses. The authors, one of them Gerald Holroyde, who would write Sambon’s obituary for the *British Medical Journal*, suggested that the frequent occurrence of cancer in husband and wife was not “coition-cancer”, but evidence of conjugal house infection, though they do not say how they arrived at this conclusion.

Despite the effects of increasing age, (fig. 11.8) Sambon went to the Lake District, though for how long is not clear. His name is not included in the list of authors, who write, “Dr Sambon, who is a naturalist as well as a physician and archaeologist, has left no stone unturned in his comprehensive study of cancer conditions in the English Lake District.”41

![Fig. 11.8, Sambon in later life. From G. von Olpp.](image)
A proposal for Sambon to head a cancer research facility in the Isle of Man in 1927 came to nothing. His parasitic theory of cancer was being attacked by influential researchers from around the world. The Second Resolution (“The statement of practical facts and sound working opinions”) at the American Mohonk Cancer Symposium in 1926 began “The causation of cancer is not completely understood, but it may be accepted that... for all practical purposes cancer is not to be looked upon as contagious or infectious.” At the International Cancer Conference in London in 1928 James Ewing, professor of pathology at Cornell Medical School, expressed his categorical opinion that the parasitic theory was a waste of time.

Sambon later extended his studies to cancer in antiquity. In mid-1931, he sent a draft of his paper to Martin A. C. Hinton, Deputy Keeper of Zoology at the British Museum (Natural History). Hinton wrote to Sambon “You indeed do well in drawing attention to that vast store of learning and experience which is enshrined in ancient literature. Today unfortunately, few can spare the time or have the ability to ransack that store.”

Six weeks later, Sambon was dead. In Paris for a cancer meeting, he collapsed while walking down the Rue Jacob on 31 August, 1931. Hilda and Arthur, who had accompanied him to Paris, had a brief moment at his side at the Hôpital de la Charité, before he was declared dead. He was sixty-five. His daughter Laura later wrote to Hinton to thank him for his words of sympathy and offers of assistance, and added “We are greatly hoping that Father has left sufficient notes for his last great report on cancer to be published...It would be dreadful if all that work and time were to be wasted...”

Laura found a sympathetic writer in Alexander (later Sir Alexander) Haddow, a young Scottish cancer researcher, whose wife Peggy was a close relative of Monro Scott Orr. Haddow saw Sambon’s notes “through the kindness of Sir
Samuel Scott, Bt,” and presented a paper on Sambon’s historical records of cancer in antiquity to the Royal Society of Medicine in February 1936.  

Sambon was carrying three books on cancer with him when he died. The books were brought back with his body, and since he treasured them, they were buried with him in Hampstead cemetery, a short distance from his home in Fordwych Road. Sambon’s extensive library of early medical and scientific books was treated more kindly, being sold at auction for £1695. His remains lie not far from those of Lord Lister (1827-1912), whose introduction of aseptic surgery revolutionised the practice of surgery.

Sambon’s nemesis, Sir Andrew Balfour, then only fifty-seven, had committed suicide at the end of January 1930 while being treated for depression at the Cassel Hospital, Penshurst, Kent. What Sambon thought at this time is not recorded. Sir David Bruce died 3 months after Sambon, on 27 November, 1931, while Ross outlived him by thirteen months.

It is unclear why Manson chose Sambon to be a lecturer at the London School, given Sambon’s lack of experience in the tropics. What Sambon did have, however, was fluency in French and Italian. What is also clear is that Manson held Sambon in high esteem, though many other Britishers sneered at him, presumably because of his ethnic background. Ross and Bruce, both military men, disliked him intensely. George Nuttall, an avid supporter of Ross in his campaign against Grassi had written to Ross in March, 1903, “They [Italians] seem to be all alike, Sambon I take to be as bad a schemer as Grassi.”

Sambon was, if not obsessed, certainly much taken with, germ theory (which he chose to call parasite theory, since disease could be caused by a variety of organisms, both large and small.). He tried to find parasitic causes for many diseases, even suggesting that diabetes would some day be found to be an
infectious disease. He was quick to delve into matters that he was scarcely qualified to discuss. If some of his parasitic ideas were far-fetched, he stressed throughout his professional life in England that human disease should not be seen in isolation. To truly understand human disease it was equally important to understand animal and plant disease, and the study of comparative pathology should never be neglected. He was always interested in the plague-rat-flea story (and suggested that domestic cats and dogs might play a part in the spread of plague), and had hypothesised in 1908 that diphtheria might be a zoonotic disease, perhaps spreading from birds.

Sambon was undoubtedly vain, flamboyant and vociferous in his opinions. He had excellent insights into many matters of tropical medicine, yet he lacked the perseverance, analytic and mathematical skills to prove his opinions. Whether he deserved the sobriquet of “pseudo-scientist”, allegedly given him by Carmichael Low, (and no doubt readily endorsed by Ross and Bruce) or whether he was “le savant estimé” of the French, or “un valente milite della scienza”—a talented soldier of science—as his Italian obituary said, is for the reader to decide.
Bibliography: Abbreviations used.

Am. J. Insan.: American Journal of Insanity
Am. J. Nurs.: American Journal of Nursing
Arch. Parasit.: Archives de Parasitologie (French language)
Brit. J. Nurs.: British Journal of Nursing
Dtsch. Med. Wochenschr.: Deutsche Medizinische Wochenschrift (German language)
J. Am. Med. Assoc.: Journal of the American Medical Association:
J. Lit. Sci.: Journal of Literature and Science
J. Trop. Med.(Hyg.): Journal of Tropical Medicine (and Hygiene)
Med. Chir. Trans.: Medico-Chirurgical Transactions
Med. Hist.: Medical History
Mosq. Syst.: Mosquito Systematics
Public. Health Rep.: Public Health Reports
Riv. Malar. Soc: Rivista di Malarologia
Trans. (Roy.) Soc. Trop. Med. (Hyg.): Transactions of the (Royal) Society of Tropical Medicine (and Hygiene):
LS(H)TM: London School of (Hygiene and) Tropical Medicine
BMA: British Medical Association
Notes and References.

Introduction.


Chapter 1.

1 The School became known as the London School of Hygiene and Tropical Medicine in 1924.
2 London School of Hygiene and Tropical Medicine (LSHTM) archives, GB0809 Ross 147 60 03, a letter from Read to Ross, 20 January, 1910.
5 It is possible that the elder Louis was a supporter of Charles X, the last Bourbon King of France, who was forced to abdicate following the July Revolution of 1830.
9 The origin of the name Westenra is obscure, but may be Dutch. Why Louis was given this name is not known.
11 The French had no doubt that Louis junior was French. Le Figaro, 1 July, 1914, page 4, speaks of him as a Frenchman, brought up in Italy.
12 Morton J. V., Solidified Wine 2,000 Years Old. Glimpses of Life in the Ancient City of Pompeii. Dr Louis Sambon on Wines and Food: An Interesting Interview. The Wine Trade Review, 15 April, 1923. In a wide ranging interview, Sambon spoke on a variety of subjects, including his childhood.
It is not clear which school he refers to. Though Hoddesdon was granted a charter in 1559/1560 to establish a grammar school, it failed to flourish and ceased to exist by the end of the century.


Snowden F. M., Chapter 1 describes conditions in Naples at the start of the 1884 epidemic.

At this period, cholera referred to any watery diarrhoea. Infantile cholera and cholera morbus (or nostrus) referred to non-bacteriologically defined watery diarrhoea occurring in infants or adults. Asiatic cholera was the disease endemic in India, which is caused by the Vibrio cholerae. Cholera infantum and cholera morbus were later shown not to be caused by Vibrios, and the terms went out of use.

Snowden F. M., op. cit., page 374. See also The Times of London, 25 September, 1884, for a vivid description of the conditions under which the poor lived in Naples.

Axel Munthe, the Swedish doctor and writer, was a volunteer during the epidemic. The appalling conditions in the lower city are described in his Från Napoli (Letters from a Mourning City,) Translated from the Swedish by Maud Valérie White, John Murray, London, 1887.

Gazzetta Ufficiale del Regno D'Italia, N 014 del 19 Gennaio,1886, page258.


At the Armstrong-Pozzuoli works, a division of the Armstrong armaments conglomerate, producing heavy guns for the Italian navy.


St Bartholomew Hospital Archives SBHMS/S/1/3. On 9 July, 1888, Sambon signed the Student Signature Book, undertaking to uphold the rules of the hospital. The books were usually, but not always, signed at the start of attendance at the medical school. His residence was given as Naples, though the book noted that he was, at the time, living in Notting Hill.


Johnston-Lavis H. J., 1918, op. cit. Naples was extensively damaged during this eruption (and over 200 fatalities were recorded.). Funds for the 1908
Olympic Games due to be held in Rome were diverted to the reconstruction of Naples, and the games were moved to London.


29 Anon., The Times (London), 30 March 1894, page 5, noted that there were over 7,600 registrations, compared to 5,725 at the 1890 Berlin conference, and 350 at the first such congress, held in Florence in 1869.

30 Rome and the Medical Congress. The Tablet, 14 April, 1894, pages 6 and 7.

31 Lanciani R., Archaeology and Medicine in Rome, Lancet, 1894:143;954.


36 Italian military records for the Naples region were destroyed during the Second World War. Whether Sambon ever got to Eritrea or Abyssinia is unknown.


Chapter 2.


21Adams F., *The Seven Books of Paulus Aeginata. Translated from the Greek with a commentary embracing a complete view of the knowledge possessed by the Greeks, Romans, and Arabians on all subjects connected with medicine and surgery*. Printed for the Sydenham Society by C. and J. Adlard, London, 1847. Volume 1, pages 16-17


Chapter 3.


2Jones’ philanthropy may have been compromised. He was complicit in the crimes committed by the regime of King Leopold II in the “Congo Free State”, his ships exporting guns, ammunition and explosives to the country, and bringing back rubber and ivory collected by forced labour. See Hochschild A., *King Leopold’s Ghost. A Story of Greed, Terror and Heroism in Colonial Africa*. Houghton Mifflin Company, Boston & New York, 1998.


6Seamen’s Hospital Society, *The London School of Tropical Medicine. Report for the year 1899-1900.*

7Philip Bahr changed his name to Manson Bahr in 1921, at the suggestion of his father-in-law.


13Ghesquier D., op.cit., gives an excellent account of the work of Galès, and the controversy his work caused. The French for itch is ‘Gale!’


Adrien-Achille Proust was the father of novelist Marcel Proust.


Chapter 4.


Letter from Ross to Manson, 27 May, 1896.

Bynum W.F., and Overy C, op. cit., page 348. Ross sent Manson, in Edinburgh, a telegram on 8 July, saying “Duct of gland traced directly into mosquitos proboscis.”

Sambon L. W., Tropical and Sub-Tropical Diseases. United Empire, The Royal Colonial Institute J. 1920:11;420-447.

Bynum W.F., and Overy C., op. cit., Letter from Ross to Manson, 6 July, 1898, pages 334-337.


de Kruif Paul, The Microbe Hunters, HBJ Publishers, San Diego, CA, 1926. This book’s chapter on the Ross-Grassi controversy is unkind to both Ross and Manson. The chapter was suggested by the British protozoologist Clifford Dobell, who had an intense dislike of Ross. In the English edition, the chapter was omitted, following threats of legal action by Ross. A chapter on David Bruce was also omitted.

LSHTM archives, GB 0809 Ross 58/01. Letter from Ross to Sambon, 14 July, 1913.

LSHTM archives, GB 0809 Ross/58/04.


LSHTM archives, GB 0809/Ross/77/24.
The Scotsman, 30 July, 1898, page 8, has an English translation of Koch’s presentation to the German Colonial Association on blackwater fever (Schwarzwasserfieber.)


Sambon was not the first to make this suggestion. Harford C. F., Blackwater fever, Brit. Med. J., 1920:2;802, claims to have suggested this in 1892.


The railway crosses the eastern part of the Roman Campagna.

This occurred before any arguments over priority between Ross and the Italian malariologists.


Sir William Watson Cheyne (1852-1932), a student and junior colleague of Lord Lister, and a pioneering bacteriologist in his own right, was one of a number of civilian surgeons who offered their services to the British Army during the Boer War. He left Britain in January 1900, returning in July of that year.

Low G.C., A Recent Observation on Filaria Nocturna in Culex. Probable Mode of Infection of Man. Brit. Med. J. 1900:1;1456-1457. Though Italian researchers initially claimed that they had seen filarial in the proboscis of mosquitoes, they later admitted Low’s priority.

Corvallis (OR) Gazette, 1 June 1900. (www.chroniclingamerica.loc.gov.)


Humphreys of Knightsbridge was a prolific provider of prefab and corrugated iron buildings. In 1907, they provided the material for Shackleton’s Antarctic Expedition hut, which was still standing, though in need of repair, in 2009.

6 years later, Sambon reported that the coin was from the time of the emperor Domitian (Emperor, 81-96 CE), a claim he repeated in 1918.

Ferro-China: an alcoholic solution of iron and cinchona, developed by Felice Bisleri as a ‘blood tonic’ in 1881.
Chapter 5.

1The RAMC was born in 1898 (www.army.mod.uk/medical-services) before Bruce first heard of Sambon.
5Quoted in: Christy C., The Epidemiology and Etiology of Sleeping Sickness in Equatorial East Africa with Clinical Observations. Reports of the Sleeping Sickness Commission of the Royal Society, No 6, 1903.
This society was founded in 1868. It merged with the Royal Society of Medicine in 1907.


Stanley H.M., *In Darkest Africa, or, The quest, rescue and retreat of Emin, Governor of Equatoria*. Sampson Low Masters, Searle and Rimington, London, 1890. Volume 2, pages 8-10. The descriptions given are not sufficiently detailed to confirm that sleeping sickness was being discussed by Stanley and his porters.


Le Dantec, 1900, op. cit., page 761.


In 1883, Lewis was appointed Assistant Professor of Pathology at the British Army Medical School at Netley. He died of pneumonia supposedly after accidentally inoculating himself with a bacterial culture.

Both the names and the spelling of the names of the trypanosomes have been subject to much change. *T. evansi* and *lewisi* were sometimes spelled *evansii* and *lewisii*, while *T. brucei* was originally written *brucii*.


Sambon L. W., A Case of Trypanosoma in a European at the Seamen’s Hospital, Royal Albert Docks, London. *J. Trop. Med.* 1902:5; 330-331. Though William Leishman had seen the organism that causes kala azar, (visceral leishmaniasis,) in 1901, he did not publish his findings until 1903.


30 Uganda at the time was not a colony, but a British Protectorate, and therefore the responsibility of the Foreign Office, not the Colonial Office.

31 LSHTM archives, GB 0809 Ross/79/15. Letter from Daniels to Ross.


37 Christy, another man described as irascible, died in 1932 after being gored by a buffalo he had shot and wounded.


39 Spinal tap, or lumbar puncture was introduced into medical practice in 1891 by the German surgeon Heinrich Quincke (1842-1922).


Manson made these comments in the course of a speech at the London School of Tropical Medicine on 7 December, 1903, reported in *J. Trop. Med.*, 1904:7;10-14.


LSHTM archives, GB 0809 Ross/145/01/26. Letter from Nuttall to Ross, 3 March, 1903.


Émile Brumpt (1877-1951), a French parasitologist, and young colleague of Blanchard, who had recently returned from the French Congo, was present at this meeting. He supported Sambon’s arguments.

H T Bulstrode was an important member of the Local Government Board. After his death at the early age of 52 in 1911, his wife Mary Beatrix (nee Nunns), achieved some fame for her travels on camel through Mongolia.


LSHTM archives, GB 0809 Ross/77/29. Letter from Bruce to Ross


The discovery of trypanosomes in animals in the South Sudan in 1904 gave rise to great concern. The following year a Sudan Sleeping Sickness Commission was established to examine and report on the situation along the southern border with the Congo Free State and Uganda. Andrew Balfour, then the Director of the Wellcome Tropical Research Laboratories in Khartoum was an important member of the commission.


Chapter 6.

2 Frogerais A., Un siècle de machines à fabriquer les comprimés (1843-1950). andrefro47@yahoo.fr.
3 Church R. & Tansey E. M., op. cit., pages 125-127.
4 For various reasons, the Medical History exhibition was delayed year after year, and only opened in 1913, to great acclaim.
5 A letter from Dr Daniels to Ross in 1912 suggests that Sambon was paid ten guineas per session.
6 Wellcome Library, WR 2/9/9.
8 The late Lise Wilkinson suggested that it was Amedeo who accompanied Sambon on this, his first trip, but the evidence is quite clear that it was his older brother. Wilkinson L. Illustrations from the Wellcome Library: A J E Terzi and L W Sambon: early Italian influences on Patrick Manson’s “Tropical Medicine”, entomology, and the art of entomological illustration in London. Med. Hist. 2002:46; 569-579.
11 “Lasciate ogni speranza voi ch’intrate”, (All hope abandon, ye who enter here). Dante, Divina Commedia- Inferno, Canto 111, line 9. Part of the inscription over the entrance to Hell.
12 Aleardo subsequently achieved considerable fame and success as a poster artist, particularly for the Italian music publisher, Ricordi. He died in 1943.
13 Wellcome Library, WR2/9/28.
14 Wellcome Library, WR2/9/34.
15 Julius Arthur Sambon died in November 1958 when the flying boat on which he was a passenger crashed into the Atlantic.
17 Wellcome Library, WR2/10/18. St Anthony’s fire was an extremely painful burning sensation of hands and feet associated with ergot poisoning.
Chapter 7.

3Dioecious-having distinct male and female individuals.
9Sonsino P., Discovery of the Life History of Bilharzia haematobia (Cobbold). The Lancet 1893:2; 621-622.
Holscomb R. C., The West Indian Bilharziosis in its relation to the Schistosomum Mansoni (Sambon, 1907), with Memoranda in Ten Cases. U. S. Nav. Med. Bul, 1907:1; 55-80. A “Passed Assistant Surgeon” was an American naval officer who had passed the examinations and requirements for promotion to Surgeon, but whose promotion had to wait until a vacancy arose.


Groves, op. cit., page 236.


Grove, op. cit., page 239.


Chapter 8.


4In classical Rome, polenta was made from barley. If English observers are to be believed, in 1590, polenta was still made from barley. By 1764 polenta was made from Indian corn (“very nourishing and agreeable”. Smollett T. *Travels through France and Italy*. Web edition by eBooks @Adelaide, University of Adelaide, 2014, Letter XX.)

5Zeist: one who held that pellagra was related somehow to maize.

6*Nixtamal* is the Mexican Spanish word for the dough obtained after maize has been slaked with lime.

7Common microscopic fungi.


14Marie A., op.cit., page 63.


17A disease due to privation.


27 Meal’ was, in the USA, a term for coarsely ground maize flour.

28 The nickname came from a speech in which Tillman threatened to go to the White House and “poke old Grover [President Grover Cleveland] with a pitchfork”.


30 Conference on Pellagra Held Under the Auspices of the State Board of Health of the State of South Carolina At the State Hospital for the Insane October 29, 1908. The State Co. Printers, Columbia SC, 1909.


Nathaniel Hawthorne’s *Tanglewood Tales for girls and boy: being a second wonder-book*. Ticknor, Reed and Fields, Boston, 1853, was a book of Greek myths for children.


LSHTM archives. Minutes of the Pellagra Investigation Committee, 21 May, 1910, GB0809 Ross/147/60/31.

LSHTM archives. Minutes of the Pellagra Investigation Committee, 21 May, 1910, GB0809 Ross/147/60/


The news spread to the United States. “GNAT CAUSES PELLAGRA” the *New York Times* reported on 14 May, 1910. “Committee on Disease in Europe says Corn Is Not to Blame.”

Bynum W.F., and Overy C.,*op.cit.*, pages 478-481. In addition to his scientific skills, Ross was a very competent mathematician, who understood the niceties of statistics better than Sambon.


Wellcome Library WR2/11/27. In a letter to Thompson from Rome, 2 February 1912, Sambon wrote, “as you know the West Indies are not to my liking.”
Chapter 9.

1. The provenance and fate of this medal is not known.
2. Albert John Chalmers had worked in West Africa and Ceylon (Sri Lanka). He came to England in 1911 as a member of the Ceylon Coronation Contingent, and resigned his commission after the coronation.
6. Chalmers had seen a copy of the of the Tropical Society report, and was very concerned that it was misleading and wrong, and liable to give rise to a great deal of criticism.
10. Wellcome Library, WR2/11/32, letter from Pearson, 6 March 1912
11. Wellcome Library, Letter from George Pearson, 6 March 1912. WA/HMM/CO/Ear/844


In late 1912, Ross threatened Manson after the latter had written a testimonial for Dr William Prout to succeed Ross at the Liverpool School. Ross said that the testimonial impugned his, Ross’s, skills as a teacher and practitioner of tropical medicine.


Chapter 10.

1 *New York Times*, 1 September, 1913
6 Balfour and his wife Grace sailed from Southampton on 14 January, 1914.
7 Some of the delays were also due to Sambon travelling to Paris to give lectures on pellagra at the Institut Pasteur and the French Academy of Medicine in late June, early July, 1914.
9 The statistical techniques necessary to sort out the role of the different foods were not available at the time.

11 Wellcome Library, WA/BSR/BA/sta/B21. The whole of this sorry saga is contained in these manuscripts.


18 Elvehjen said that vitamins should, if possible, be obtained from natural foods, being cheaper, more palatable, and in a better balance with other essential factors. Quoted in: https://en.wikipedia.org/wiki/Conrad_Elvehjem. He might well have been appalled at the massive overconsumption of synthetic vitamins by well-nourished individuals in the western world today.


21 Today called Granuloma inguinale, or Donovanosis.


24 Bryan C., op. cit., page 86.


Chapter 11.

1Wellcome Library, WA/BSR/BA/Cor/B/43
2Sambon L. W., The Future of West Africa. West Africa (magazine), 3 August, 1918.
5Sambon’s paper was not published in the Proceedings of the Royal Society of Medicine.
9Sambon L. W., Sleeping Sickness. The Times, London, 3 August, 1908, page 16. Though the letter started with an account of Castellani’s discoveries in sleeping sickness, the bulk of the letter was taken up with an account of filariasis.
12In his letter 21 March, 1898 to Manson, Ross, wrote “What an ass I have been not to follow your advice before & work with birds. “ Bynum W. F. & Overy C., The Beast in the Mosquito: the Correspondence of Ronald Ross & Patrick Manson, Rodopi, Amsterdam, 1998, page 291.
Manson P., On the Nature and Significance of the Crescentic and Flagellated Bodies in Malarial Blood. *Brit. Med. J.*, 1894:2; 1306-1308. In this article, Manson suggested that the mosquito, or a similar blood-sucking insect, must be the means by which malaria was transmitted.


Andreas Vesalius (1514-1564) deserves the credit for taking the first steps against humoralism. In his anatomical dissections, he was unable to find any evidence of ‘black bile.’


Spanton W. D., Some Notes Concerning the First Egyptian Medical Congress. *Birmingham Medical Review*, April, 1903

Wellcome Library, SA/CRC/N.2 (Box 80), Report by Lazarus-Barlow, dated 24 January, 1924, and an undated report by Sampson Handley.

Wellcome Library, SA/CRC/N.2 (Box 80), Letter from the Medical Secretary, British Empire Cancer Campaign, to Sambon 3 April, 1924.


Lazarus-Barlow W. S., A Note on Cancer in Iceland. Fourth Report from the Cancer Research Laboratories. Macmillan and Co. Ltd, 1905, pages 273-275. Lazarus Barlow noted that cancer was apparently rare in Iceland, but of 115 cancers notified between 1890 and 1900, 58 were of the stomach.


The notice of the lecture and the proposed publication date appeared in the Bulletin de la Société de Pathologie Exotique, 1924:17; 854-855.


Hinton’s name crops up occasionally as an alleged conspirator in the Piltdown Man fraud.


Natural History Museum Archives. Letter from Laura Sambon to Hinton, 12 September, 1931. DFZOO/232/6/13/15/3.

Haddow, born in 1907, was only six years old at the time of the Orr cartoon.


Some authors have suggested that Sambon worked for a time in Central Africa. This seems to be a misunderstanding of Sir John Kirk’s introductory remarks at the Geographical Society meeting of April 1898 (see chapter 2).

LSHTM archives, GB 0809 Ross/145/01/26.

Whether type 1 diabetes is a purely autoimmune disease, or whether viruses play a part is still the subject of considerable debate.


Anon.,Notizie Dr Louis Westenra Sambon. Rivista di Malariologia, 1931:10; 668-669.
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