

THE CONTRIBUTION OF NEISSER TO THE ESTABLISHMENT OF
THE HANSEN BACILLUS AS THE ETIOLOGIC
AGENT OF LEPROSY

8 AND THE SO-CALLED HANSEN-NEISSER CONTROVERSY

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There is uncertainty about what role Albert Neisser played in establishing the leprosy bacillus, *Mycobacterium leprae*, as the etiological agent of the disease—not in its original discovery, the credit for which goes without question to Hansen. Most people today are quite unaware that Neisser comes into the picture at all; in this connection he has been virtually forgotten.² On the other hand there have been a very few who regarded his role as the principal one, and Neisser himself is said always to have felt keenly that he was not accorded his proper place in the picture. In the present historical note an attempt is made to examine the evidence and determine what credit rightfully belongs to Neisser.

It is most difficult today to realize the skepticism and opposition that Hansen had to face, first when he tried to introduce the idea that leprosy is contagious, and later about the nature and significance of the rod-like objects he observed under the microscope in material from nodules.

As regards the bacillus as the causal agent, the first difficulty was the absence of precedent. As Hansen himself wrote later (4), the teaching that bacteria cause disease was then in its infancy, and no chronic disease was known to be of bacterial nature. In his earlier contacts with leprosy patients, he occasionally saw one that, "agreed strongly with my idea that leprosy was a disease with a specific cause and not one due to a mode of life or that was inherited." So he had the conviction that there must be a contagious element, and he was driven to hunt for it, although he had never seen any pathogenic bacteria.

When he demonstrated the leprosy bacillus—in wet preparations and at best imperfectly colored by osmic acid—his chief (and father-in-law) Danielssen was not and would not be convinced. The consequences of Hansen's discovery are commented on by Jeanselme (6):

The discovery of the specific bacillus of leprosy by Hansen ruined many a [cherished] hypothesis, and reduced to the status of secondary causes many etiological factors to which previously a preponderant role had been attributed.

² Neisser is remembered chiefly as the discoverer of the gonococcus, his name perpetuated in *Neisseria*, the name of the genus of bacteria to which the gonococcus belongs.

Nevertheless, a number of leprologists did not at first appreciate the significance of this decisive discovery. The authority of Danielssen and of Boeck, who attributed leprosy to multiple and disparate origins, remained great, and Hansen had much to overcome to obtain recognition of the specific agent. Neisser in Germany (1879), Brocq (1885), Leloir (1886) and Ernest Besnier (1887) in France, contributed to the triumph of the idea of contagion.

As for Neisser's part in the matter, John Henry Richter, a great-nephew of Neisser who wrote an appreciation on the anniversary of his birth (9), pointed out that in a single year, 1879, at the age of 24, Neisser discovered the gonococcus and demonstrated the leprosy bacillus by newly-introduced staining methods. He stated that of the two accomplishments Neisser always regarded the second as the more important. Neisser acknowledged that Hansen had been the first to see the bacilliform bodies, but felt that it was his own merit to have brought forward convincing proof of their relationship to leprosy. As things turned out he felt slighted, and for the rest of his life bore resentment that he had not received the recognition that was due him. The matter engendered some heat between the two men at the time, but only between them.

In 1874 Hansen published his observations of the rod-like bodies, as part of a report of work done the previous year (2).³ He was much less definite about the nature of these rods than he was about other bacteria found in ulcerated lesions and elsewhere. Then in the summer of 1879 Neisser—a youthful 24 when Hansen was a settled 38—visited Norway to study leprosy. He returned to Breslau with a lot of material that had been given him by Hansen and immediately proceeded to apply recently devised staining methods, and with them, he demonstrated the bacilli as had not been done before. He evidently read his paper at a meeting of the Silesian Society for Natural *Kultur* in October, and it was published in sections shortly afterward (7). Promptly thereafter Hansen, who had remained silent since 1874, published an article in German, also in his own language and also in English (3). Neisser wrote again on the sub-

³ The pertinent part of that 1874 publication has been translated and reprinted in THE JOURNAL (2). Among other dates occasionally mentioned are 1868 and 1869. Hansen began to work in the leprosy hospitals of Bergen in 1868, and presumably began such laboratory work as he could. His first publication was in 1869, in the *Nordisk Medecinsk Arkiv*, but that one is so unimportant in comparison with the 1874 report that the latter is taken as the first actual record of his observations of the leprosy bacilli. The date 1873, often mentioned in this connection, was the year in which Hansen—according to the usual assumption—made the observations recorded in 1874. In the nineteenth century there was a tendency to accredit work as of the time it was done rather than the time of publication.

Actually, according to Dr. Reidar Melsom of Bergen (personal communication) the time when Hansen first observed his bacillary forms is uncertain. Neisser indicated 1868 and 1873 in the 1879 report. According to a letter from him to Charles S. Butler, published by the latter in his book *Syphilis s. Morbus Humanus* (1936), the 1874 report was finished in 1872, so according to that, 1873 was not the year when he made the observations. In the letter to Butler he mentions 1870 or 1871. He himself was not able, later, to fix the date.

ject (8), exhibiting some rancor; from Hansen we have nothing more about the matter except certain statements from his memoirs (4). Another article published in 1882 (5) dealt with animal inoculations; the priority question was not referred to.

In 1917 Czaplewski, of the Cologne Academy of Practical Medicine, made an attempt to establish Neisser's claim for main credit (1). He begins:

Albert Neisser is gone—one of the great! With Robert Koch he lived as a leading spirit in the rebirth of medical science through bacteriology Had he accomplished nothing other than the discovery of the gonococcus, that alone would have assured him immortal fame. Yet we are indebted to him for the discovery of yet another important organism, the lepra bacillus. While the discovery of the gonococcus is universally and unconditionally accredited to him, the discovery of the lepra bacillus is contested, his share in the discovery being passed over or discounted. It is understandable that Neisser experienced personal hurt over this, which he could not dismiss. He valued his work on the discovery of the lepra bacillus more than that on the discovery of the gonococcus. Now he is gone. We stand sadly at his grave. Since the possibility of the hoped-for recognition of his discovery during his lifetime is gone, we want at the very least to award to the dead that which is rightly his.

Thus the opening paragraph. Two comments are in order: 1. He states his conclusion, then sets forth to prove it. 2. We note at one point the word "share" in the discovery. Czaplewski does not go the whole way, only 90 per cent. He then devotes five and one-half pages to Neisser's 1879 article (7), from which we quote directly:—

The disease, about which I want to present some information briefly, would at first seem to be one of interest principally to the specialist in dermatology. However, if I do not deceive myself, the opposite is the case in a broad sense, since the conclusions to be drawn may apply to other diseases as well, conclusions which up to the present time we have been inclined to reject.

Our theme is the etiology of *Lepra arabum*, leprosy, or Elephantiasis graecorum The etiology of this disease is no more determined today than it was three centuries ago. Even during recent decades writers on the subject have vacillated between climatic and social factors, or between heredity and contagiousness, or have incriminated both as being of importance. Even the idea of a combined intoxication such as lepro-syphilis is not altogether abandoned.

How fruitless these discussions have been is clearly shown by the fact that the two most outstanding contemporary experts, Danielssen and A. Hansen of Bergen, both studying identical material, maintain contradictory views. Danielssen denies contagion in favor of heredity, while Hansen maintains it to be a purely contagious process and rejects the hereditary factor.

Yet even the investigators who have supported contagion have so far advanced our knowledge but little. Carter thought he found *Schizomycetes* Later Klebs [1873] reported, "In freshly extirpated nodules groups of bacilli were readily demonstrated, which were quite different in form and arrangement from those of other diseases." However, to me the most significant are Hansen's published 1868 and 1873 findings.⁴ In preparations of fresh nodules he saw motile rods in large numbers. Earlier he had observed yellowish granular masses in older skin nodules, spleen, testis, and retina, which were possibly zoogloal groups.

⁴ Neisser does not say that the findings were *published* in those years. See Footnote 3, above.

Cultures and animal inoculations of these yielded no results, hence he had not been able to draw the conclusion that he had before him the virus of leprosy, although he believed that the disease was a contagious process.

Thus the question stood when my colleague E. Lesser and I undertook a trip to Norway last summer to obtain material for the study of leprosy. Through the extraordinary courtesies of the Norwegian scholars we saw almost all the cases in the hospitals in Trondjhem, Molde, and Bergen, about 600 in number. We studied the possibilities of origin and course of individual cases, and left with the impression of a noncontagious process, something on the order of a constitutional lupus or of syphilis as Virchow interpreted them. Hansen's personally demonstrated preparation of motile bodies had not convinced us of the presence of bacteria.

At our last stop in Bergen we were most liberally provided with material, a part of it alcohol-fixed. Four freshly removed nodules were preserved in alcohol. An autopsy increased our store by material from internal organs, as well as skin specimens. At the same time I made, smeared and dried coverslip preparations of tissue juices according to Koch's method.

Having quickly returned home with this wealth of material I immediately began to study it, and to my intense surprise found everywhere bacilli in large numbers, in all 14 pieces of skin and nodules, in the liver, spleen, testes, lymph nodes, and cornea. These rods appeared to be something previously unknown, even as they did to those to whom I had previously shown preparations on the 3rd of September.⁵ The singularity of their appearance awakened the hope that further investigation might bring light to an obscure question.

This is as much as need be extracted from Neisser immediately. Most of the remainder, with the exception of the last paragraph, is concerned with descriptive details which are not controversial in nature. Czaplewski roughly summarized that as follows:

Neisser succeeded in this discovery by virtue of employing techniques in staining and microscopy introduced by Weigert and by Koch. With the help of these methods Neisser described the lepra bacilli most exactly, especially their appearance and distribution. He left the questions of spores or granular forms relatively open The distribution of bacilli within cells, and the formation of globi, were both expressly described, particularly in the skin and cutaneous nodules, and also in internal organs where the infiltration of bacilli was commonly accompanied by extravasation of red cells and formation of blood pigments.⁶ Outside of the skin, Neisser found bacilli most abundant in the testis: the lumens of the seminiferous tubules were filled with golden-yellow crumbly masses (Hansen), which proved to be compact masses of lepra bacilli. Bacilli were less numerous in the cornea, still fewer in the liver and spleen, and absent from the kidney and from the specimens of ulnar nerve. They were also absent from skin specimens from cases of purely anesthetic skin without gross or microscopic changes.

Of importance are his words: "I was not able to recognize these minute organisms in unstained sections. They appeared most beautifully with fuchsin and gentian violet stains . . ." [They were found] "in astonishing numbers in both fresh and fixed material of various organs from various cases of leprosy in the Norwegians. Further, these were the only organisms found, aside from the usual surface bacteria. They occurred everywhere that a pathological process was present, or in process of formation, and nowhere else." Neisser rejected the view that the organisms were an "accidental secondary infection in a favorable culture medium."

⁵ Presumably in Norway.

⁶ This observation suggests some unusual complication, not directly related to leprosy.

Czaplewski continues with Neisser's argument for an etiologic significance, which is not in dispute and need not be detailed, and then quotes the final paragraph of the Neisser article:

Finally, may I repeat that previous authors, notably Hansen, have worked and expressed their views in the same direction. I, for my part, have concerned myself with the demonstration of a wholly specific type of bacterium and its distribution in the organism, which will be found by anyone who seeks it.

Hansen, shortly after the appearance of Neisser's 1879 article—having, he says (4), received an "invitation officially" from a German journal to make his discovery known—wrote again (3), for the first time in a language other than the Norwegian. This 1880 article is entitled "Bacillus leprae," a term not used by him in print, so far as we are aware, before this occasion. Freely rendered, he said:

I had not intended to publish my investigations on this subject yet, but now feel compelled to report what I have accomplished up to the present time in my work on this infectious agent. A few years ago I showed my preparations and communicated my opinions on the parasitic nature of leprosy to a Swedish physician, Dr. Eklund. In a recent brochure, "Om Sptelska", he refers to the specific causative agent of leprosy as something which he himself had discovered in the form of a micrococcus. In addition, Dr. Neisser of Breslau, who spent a part of this last summer in Bergen for the purpose of studying leprosy, has just published the results of his investigations of the preparations which he obtained here. He, too, found the preparations filled with bacilli, which he regarded as species-specific and the causative infectious agent of leprosy, a view shared by the bacteriologists Ferdinand Cohn and Dr. Koch. I am making this report now, partly to maintain my priority in this matter before a scientific public larger than the Scandinavian alone, and partly to bring my work up to date with additional details omitted from the 1874 report to the medical society in Christiania because of the then incompletely proven results.

Then Hansen writes a bit on matters which show he had been busying himself somewhat with cultures and was plagued by contaminants, which he thought might be present in the culture materials as a "peculiar poison" evoked by the disease, but not as etiologic agents. He records failure to infect rabbits, and gives seven pages of protocols of notes made in 1873. Then he continues:

Up to the present time my efforts to produce good and convincing preparations have been futile. In only one case did I think I had achieved a completely satisfactory example. As I reported, the rod-shaped bodies appeared more distinctly in osmium-treated nodules.

He goes on with some descriptions of that preparation, and then:

In my previous communication I reported in greatest brevity that I frequently found small rod-shaped bodies in the granuloma cells, and, indeed, routinely on adequate search, whereas they were never found in blood samples. In the basis of recently repeated investigations I am wholly unable to confirm Dr. Eklund's micrococci.

The article concludes with a short paragraph—a contradictory one, unless it was an addendum—noting his application of Koch's staining method to sections of a nodule, confirming thereby the presence therein of numerous stained bacilli.

If this sounds like a weak article, it is because it is weak. Little new is recorded, and that inadequately. There is no evidence of more than a trivial amount of fresh work. We can but wonder whether, between 1874 and 1879, Hansen had really appreciated the importance of his observations.

If we now turn to Neisser's 1881 article (8), we find him saying that he is not happy about the prematurity of its appearance, and then giving a whole page of explanations. Evidently material for study was extremely limited in quantity, and obtained from Grenada. Then he mentions the 1880 publication of Hansen (and those of others) and says:

I hereby declare to these authors . . . that I have never claimed for myself the priority of having been first to see bacteria in leprosy . . . and it would seem that this statement is carefully worded.

Neisser then expresses doubt that Hansen's bacilli of 1873-1874 amounted to anything. Certainly he says that they were not adequately demonstrated. (It has been seen, from a quotation given above, that Hansen had already said as much.) And he adds that Hansen's work was regarded with little or no respect even by his own colleagues, whereas he himself (Neisser) had reported "a specific type of bacterium, etiologically related to all the lesions of leprosy." A footnote reads:

When I was in Bergen in July and August of 1879 Hansen did, it is true, hold the personal (or, as he called it, subjective) view that the "rod-shaped structures" played a role in leprosy, a view which appeared objectively compelling to absolutely nobody at all. Indeed, even his Bergen colleagues granted no significance to his findings, although acquainted with them for years. Some were firm opponents of the idea of contagiousness of leprosy, and I recall very well Danielssen's ironic query, "If Hansen had shown his bacteria to me, too." There was no talk of a "bacillus," and extremely little of staining or culture technique.

Neisser notes Hansen's 1880 publication:

And this to obtain for himself a priority which I had granted him in two places in my article, which was scarcely eight pages long . . . And so on, devoting considerable space to expostulating about Hansen and his publication.

This 1881 article of Neisser's must be acknowledged to be an outstanding definitive description of the relations of the bacilli to the lesions and of their etiologic import. It runs to 29 pages, with one plate of illustrations of historical quality only. His descriptions of beaded forms of bacilli as spores does not detract from the importance of the article.

To return to Czaplewski, for a last glance: He gives much space to Neisser's report of 1879, goes on with much drawn from Hansen's 1880 article, and then with Neisser's of 1881. Czaplewski reports accurately—which is not to say that he reports impartially. His position was taken from the start, and he quotes to his advantage and to further that position as best possible. In his article he compares Neisser and Hansen

as though they had had the same tools to work with, which seems not correct. Toward the end he states:

Armauer Hansen had probably seen bacteria in leprosy and maintained their specificity, but he had not proved it.

With this last we heartily agree. It is an accurate statement of the case. The real point is the importance of Neisser's work (and it was extremely important), not the belittling of Hansen's. Yet when one looks back at Neisser's first article, and begins to realize the situation, one can understand how Hansen must have felt about it. He could scarcely have failed to be infuriated.

Neisser started out by discrediting Hansen, who had willingly shown him everything he could and had surely been instrumental in providing Neisser's material. Far from genuinely giving Hansen any credit Neisser spent much effort to assert the importance of his organisms as against Hansen's. Hansen must have felt the impudence sorely, especially from a person much his junior in years and experience—and a German at that—hence his quick answer in the 1880 article. And Neisser, having asserted himself, must stick to it and find himself sorely abused by Hansen, who had indeed abused him not at all.

Neisser had something of great importance to present. Was it necessary for him to be personally concerned with Hansen's status in the picture? He certainly was deeply concerned about that from the beginning, and it is difficult not to interpret the introductory remarks in his 1879 article as purposely designed to eradicate Hansen from the picture, even before he got around to getting himself in. He did not succeed, and the palm for the original discovery has been awarded to Hansen. Neisser's complaint evidently received little if any attention at the time, even from his own colleagues, who very likely saw the matter quite clearly and in its true light. Czaplewski's article, also, seems never to have evoked comment of note, or to have been given much attention; it was of course published in the parlous times of World War I. The controversy is all most unfortunate, for it very likely tended to obscure the fact that Neisser made an important contribution in confirming, establishing, and extending Hansen's discovery.

Hansen naturally dealt with this matter in his memoirs (4), but briefly and without heat—as well he might. An available partial translation⁷ has been searched for anything pertinent to this matter. There are passages that are of interest, and they follow here either in condensed form or as direct quotations from the translation.

⁷ This translation was made about 1944 by Dr. Pearson, of Louisiana State University, at the request of the late Dr. George W. McCoy, who gave it to Mr. Stanley Stein, editor of *The Star*, published at the U. S. Public Health Service Hospital at Carville, Louisiana.

In 1868 Hansen, having graduated at the age of 25 from the medical school of the University of Kristiania (now Oslo) in 1866 and then interned for a year at the Rikshospitalet there, began to work in the leprosy hospital in Bergen, under Danielssen. Bothered greatly at first by the concentration of misery there, he soon became interested in his patients and began to do autopsies; pathologic anatomy, he said, was his favorite study. "Here there was overwhelmingly much to do, and I was always full of plans of all that I should investigate but never got time to do." However, he soon produced a first *Arbeit* on leprosy,⁸ and having received a stipend for travel he went to Germany for a year (1870-71). The principal thing gained there was an acquaintance with Darwin's publications and admiration of his methods of investigation. Hansen mentioned one minor incident presumably because of later happenings.

I found something that I thought was new, but it was not possible to get him [Max Schultz] interested; but I noticed a few months after I came home that some German had written it up. Then I began to realize the Germans' zeal to be the first to describe things.

In his earlier work he had become convinced that the views of Danielssen on the etiology of leprosy were "not well founded." Danielssen was "truly a learned soul, but he had a tendency to draw conclusions quickly without applying the necessary criticism." He was severely critical of others but not of himself, "a very common human trait."⁹ Hansen recognized that if one has worked hard to achieve a result, as Danielssen had, it is hard to give it up even if the arguments against it are overwhelming.

This happened to Danielssen. His spirit was elastic and young for his age, and I was young and eager, and we argued nearly every day about certain questions. . . . There was no one who could stimulate me better, and I hastened to work and obtain support for my ideas.

With an untiring drive for work, Hansen would stay for long hours at the microscope. Time after time he would think he had found something, only to become convinced he had not, and so he had to begin anew. All he could say was that in the leprosy nodes he had found bacteria-like forms.

Then came Weigert's and Koch's discovery of new methods of staining bacteria, and I naturally began to pursue them, but I was not successful. When I was working with this, Professor [sic] Neisser came to Bergen from Breslau to study leprosy and I showed him what I had found, and hoped that he who came directly from the place

⁸ Presumably this was the 1869 publication that has been mentioned. According to Melsom, however, he was awarded in the same year the King's Gold Medal for a work presented to the university, which was *not* printed. In 1871 he received a monetary award for his study of the normal and pathological anatomy of the lymph nodes, which was printed.

⁹ The fact that Hansen had for a time been Danielssen's son-in-law probably did not help. According to Melsom, that marriage lasted less than a year, the lady dying of tuberculosis. Hansen married again later. Incidentally, it is often said that, in his turn, Hansen's successor Lie was Hansen's son-in-law. Melsom says that was incorrect. Lie married twice, but neither time to a daughter of Hansen.

where Weigert and Koch [worked] could help me, but at that time he was not any better than I was.

Back in Breslau with material obtained from Hansen, Neisser was successful, but,

In the meanwhile I was also successful, having written to Koch and received good advice. I did not think it was necessary to hasten to publish my results, as I found that I still had much to do before I could maintain with certainty that the bacteria I had found were truly the cause of leprosy. This was not true of Neisser; he published at once, but was so honorable as to tell what I had demonstrated to him. He did not refrain, however, from saying that Danielssen had asked him with irony if Hansen had shown him his bacteria.

I took this very quietly because I had already published my findings in the *Magazin for Laegevidenskap*. But Danielssen became angry, especially because he had regarded my findings with irony, and rebuked me for my indolence, for as he saw it there was an attempt to steal my discovery.

In the meantime I had received an official invitation to make my discovery known in a German journal, and as far as I can see that article has for all times established my credit for finding the etiological element of leprosy which in the literature is called the Hansen bacillus.

And that is all Hansen said of the matter. If he had any animosity toward Neisser in 1880, he exhibited none thirty years later.

In conclusion, it can be said that the rods which Hansen observed and described were *Mycobacterium leprae*. His credit position rests on his 1874 report, and we believe it a valid credit. He saw the organisms. His techniques were too poor for their satisfactory demonstration, but satisfactory techniques had not been elaborated at that time.

Neisser's demonstrations were the first convincing ones of the bacilli themselves, and the first satisfactory evidence of their relationship to the lesions of leprosy. It is interesting that Neisser appeared to consider his ability to convince Koch and Cohn on the matter as important as convincing himself, but he was young at the time. He should be given a secure place in the history of leprosy for confirming and extending Hansen's observations.

There is a historically significant difference between the approaches of the two men to the problem. Neisser's consuming interest in microorganisms as causative agents of disease was a drive which did not exist in Hansen. To Hansen leprosy was the thing, its etiology being but one of its many important features. It is obvious that his relative inactivity in the study of the bacilli in the five years after his 1874 report renders his 1880 article of no scientific importance.

The vast bulk of the literature of leprosy is devoid of suggestion of controversy in this matter. It is Hansen's, not Neisser's, bacillus. And it should be pointed out that much of this literature was written by men personally acquainted with both parties, completely familiar with the matter. To us, at least, this large volume of negative evidence has weight.

RESUMEN

Repásanse en este trabajo los datos disponibles acerca de la participación que tuvo Albert Neisser en el descubrimiento del bacilo de la lepra y en el establecimiento de su relación con las lesiones de la enfermedad. Hubo una vez alguna disensión entre Hansen y Neisser y dicen que el último siempre resintió el hecho de que no le concedieran la gloria que merecía.

Hansen observó, y en 1874 describió, formas parecidas a bastoncillos en las lesiones leprosas, correspondiéndole así una prioridad indudable en el descubrimiento del bacilo. Su observación de esas formas fué, sin embargo, muy defectuosa y no resultó convincente a otros—incluso Danielssen, su jefe—y manifiestamente, poco más hizo en el asunto. No se habían elaborado todavía técnicas satisfactorias para el descubrimiento de bacterias de ese género. En 1879, Neisser fué a Bergen de Breslau y obtuvo material al cual aplicó los nuevos métodos de coloración, demostrando así los bacilos y su relación con las lesiones histológicas. En su comunicación, mencionó las observaciones de Hansen, pero aparentemente más para desacreditarlas que para reconocer su mérito. Hansen replicó prontamente con un aserto de prioridad, y Neisser mostró entonces su irritación. Al fallecer Neisser, Czaplewski reclamó para él casi toda la gloria y ese aserto ha sido recientemente repetido por un tal John Henry Richter, Pariente de Neisser.

El estudio de los trabajos originales, que se citan aquí ampliamente, conduce a las siguientes conclusiones:

1. Hansen observó, y en 1874 describió, los bastoncillos que son los *M. leprae*. Vió los microbios, pero carecía de medios para observarlos satisfactoriamente.

2. Las observaciones de Neisser, con nuevas técnicas, de los bacilos en tejido obtenido en Noruega constituyen las primeras convincentes de los bacilos mismos y de su relación con las lesiones leprosas. En la historia de la lepra, hay, pues, que prestarle reconocimiento a Neisser por haber, con sus demostraciones, confirmado y extendido las observaciones de Hansen. Su aporte fué importante e indispensable, pero no comprendió el verdadero descubrimiento del bacilo.

3. Existe una diferencia importante históricamente en la forma en que esos dos sujetos abordaron el problema. Para Hansen, lepra, la enfermedad, era lo real, no siendo el factor etiológico mas que una de sus muchas importantes características, y por eso quizás fué que hizo tan poco en ese campo dado a partir de 1874. En cambio, Neisser abrigaba un interés devorante en los microbios como agentes causantes de enfermedades, impulso ese que no existía en Hansen.

En la vasta literatura de la lepra, hay bien poca indicación de controversia. Es el bacilo de Hansen, no el de Neisser—y mucha de la literatura de aquella época fué escrita por sujetos que conocían a ambos individuos y estaban al tanto de lo que había sucedido.

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