

the addition of a few drops of an alcoholic solution of acetic acid to the mixture at the end of a period of digestive action.

The following experiments may be cited as indicative of the results obtained in a large series of tests:

TABLE OF RESULTS

Thirteen test-tubes containing 2 c.c. casein solution and 0.5 c.c. trypsin solution, to which increasing quantities of milk serum were added and decreasing quantities of 0.85 per cent sodium chlorid solution. These tubes were incubated at 37.0 degrees C. for three-quarters of an hour, when the alcoholic acetic acid was added drop by drop, about six drops of the acetic acid to each tube:

Milk Serum.	NaCl.	Sodium Benzoate.	Sodium Sulphite.	Results.
0.0 c.c.	1.0 c.c.	Nearly complete digestion
0.1 c.c.	0.9 c.c.	Nearly complete digestion
0.2 c.c.	0.8 c.c.	Nearly complete digestion
0.3 c.c.	0.7 c.c.	Nearly complete digestion
0.4 c.c.	0.6 c.c.	Slight precipitate formed
0.5 c.c.	0.5 c.c.	Slight precipitate formed
0.6 c.c.	0.4 c.c.	Definite precipitate formed
0.7 c.c.	0.3 c.c.	Definite precipitate formed
0.8 c.c.	0.2 c.c.	Definite precipitate formed
0.9 c.c.	0.1 c.c.	Definite precipitate formed
1.0 c.c.	0.0 c.c.	Marked precipitate formed
0.0 c.c.	0.5 c.c.	0.5 c.c.	Definite precipitate formed
0.0 c.c.	0.5 c.c.	0.5 c.c.	Slight precipitate formed

Increasing quantities of milk serum exerted an ascending effect in inhibiting the tryptic digestion of the casein, indicating that some preservative had been added to the milk. The addition of 0.5 c.c. sodium benzoate solution (concentrated) produced a slightly greater inhibition of the tryptic digestion than did an equal quantity of the milk serum, while 0.5 c.c. sodium sulphite solution (concentrated) produced approximately a degree of inhibition of the tryptic digestion equal to that produced by an equal quantity of the milk serum.

I have also attempted to ascertain the reliability of the antifermentative test in the detection of preservatives in meat. The hacked meat is placed in a meat-press and the blood and juices expressed. The main difficulty in applying the test to the meat juices lies in the fact that normal blood serum exerts an antifermentative effect. I have attempted to overcome this influence by heating the meat juice and recovering the watery part of the juice for the tests. The tests with this final product have not been satisfactory because, apparently, the major portion of the preservatives is retained in the coagulated albumin of the blood serum, and in consequence the tests made on the watery part of the meat juice were never very decisive.

The evidence which scientific investigators have collected all shows that the addition of chemical preservatives to food substances is injurious to the consumer in several different ways:

1. The use of preservatives permits the use of food substances that are of inferior grade or that are not perfectly fresh.

2. The food preservatives when taken into the body must be eliminated subsequently; this operation entails extra labor on some special organ or organs, and this extra labor will sooner or later lead to direct injury.

3. Many of the food preservatives are direct poisons and their constant ingestion induces chronic poisoning with its concomitant effects.

4. The food preservatives have an antifermentative effect on the different digestive ferments, especially on trypsin, and thus retard digestion and interfere with assimilation.

5. The food preservatives act on the food substances in such a manner as to render them less easily digested or even wholly indigestible, and make it necessary for the body to deal with partly digested food or deprive it altogether of nutrition, especially of nitrogenous food.

6. In consequence of the use of food preservatives digestion and assimilation are interfered with, and the general nutrition of the body suffers through organs that are overtaxed and injured in attempting to eliminate the poisonous chemical substances, and in attempting to deal with imperfectly split food substances that may be in such a state as to preclude their utilization, especially on account of the lack of digestive ferments, because of the loss entailed through the destructive action of the food preservatives.

SUMMARY

Contrary to my expectations the use of the antifermentative test for the detection of preservatives in foods does not give results that are satisfactory in every particular. The employment of this test must be carried out in such a manner as to eliminate normal antifermentative effects in the food substances, and attempts to remove these normal antiferments may lead to the simultaneous removal of the preservative.

In view of our knowledge of the detrimental effects of chemical food preservatives there is no more reprehensible practice than that of permitting their use in foods in any quantity whatever. The better class of manufacturers and dealers have set us an example, which we should follow, in the fight which they are making against the enactment of laws that in any particular permit the use of chemical substances for the preservation of foods where their use is not absolutely necessary.

We should not allow the greed of unscrupulous manufacturers and dealers to overrule our knowledge of the evil effects of the use of food preservatives. The medical profession owes it to itself to uphold the laudable position assumed by Dr. Wiley in his crusade against food adulteration, and any course which falls short of this will stultify the profession in the eyes of educated humanity of to-day and for all time to come.

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THE PARASITOLOGY OF SYPHILIS *

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Syphilis has long been considered an infectious disease, but until recent years the discovery of the causal agent has eluded the most careful investigation. There has hardly been a pathologist or syphilographer of note who has not attempted to unravel the mystery. It would be impossible here to enumerate all the alleged causes that have been reported. Lassar has said that during the past twenty-five years at least 125 causes have been assigned to syphilis. It is evident that the great majority of these theories were very short-lived. There are three, however, that deserve mention. The bacillus which Lustgarten described in 1885 occupied the stage for several years, being defended by so good an observer as Weigert, but later it was shown to be identical with the smegma bacillus. The next appearance was Neissen's so-called syphilis bacillus. Recently Neissen¹ has published the

* Read in the Section on Dermatology of the American Medical Association, at the Sixtieth Annual Session, held at Atlantic City, June, 1908.

1. Neissen: *Der Syphilis Bacillus*, Leipzig, 1908.

results of his fifteen years' work in the form of an extensive atlas profusely illustrating the remarkable transformation of his organism from the vegetable to the animal kingdom and back again. It is remarkable with what persistence Neissen has pursued his phantom. His is a case of "love's labor lost," for his results have never been confirmed.

Somewhat on the same order has been the work of Siegel,² who, in 1905, described his *Cytorrhycles luis* along with cultural and inoculation experiments. Siegel's work attracted a great deal of attention, and, while the majority of observers failed to confirm Siegel's findings, a few, notably Merk,³ Freund⁴ and Jancke,⁵ did confirm them. As a court of last resort, Schaudinn was selected to pass judgment on the cytorrhycles, which he did very shortly, failing to confirm Siegel's work and announcing his own discovery of the *Spirochæta pallida*.

At the present time there is probably no one who believes in the cytorrhycles, except Siegel and his pupils, and they were until recently busily engaged in explaining the cytorrhycles as one of the developmental stages of the *Spirochæta pallida*.

Schaudinn and Hoffman⁶ announced their discovery in May, 1905; and it is remarkable with what unanimity confirmatory observations were reported from all quarters. The bibliography on the subject is already enormous.

The organism, as described by Schaudinn, is an extremely slender thread, closely wound in the form of a spiral similar to a corkscrew. In length it varies from 7 to 21 microns. The ends are pointed and provided with flagellæ. It stains with most of the anilin dyes, but only very faintly; the best staining method is probably that of Giemsa, which stains the organisms a pale red. In tissues the best method of staining is that of Levaditi.

There have been many attempts made to grow the spirochete on artificial media, but they have been uniformly successful. Recently Schereschewsky⁷ has reported successful results. Levaditi and others have reported a seeming increase in the number of organisms when grown in collodion sacs in the peritoneal cavity of rabbits. They have also been obtained in pure culture after inoculation into the anterior chamber of the eye of rabbits and other animals.

This brings up the question of inoculation experiments. Metchnikoff and Roux,⁸ in 1903, demonstrated the susceptibility of the anthropoid apes to syphilis. Haensell,⁹ in 1881, and Klebs,¹⁰ Siegel¹¹ and Schultz¹² had all reported successful inoculation experiments, but on account of the absence of secondary manifestations or subinoculations their results are doubtful.

At the present time, however, there are numerous observations showing the pathogenicity of the spirochete. Finger and Landsteiner inoculated monkeys with spirochetal material and transmitted the infection from animal to animal through forty-five different monkeys.

Metchnikoff and Roux, Neisser,¹³ Hoffman and Brüning,¹⁴ Mühlens¹⁵ and many others have made successful inoculations and recovered the spirochete.

Bertarelli¹⁶ was the first to demonstrate the susceptibility of rabbits. His results have been confirmed by Scherber,¹⁷ Groef and Clausen,¹⁸ Kraus and Volk¹⁹ and many others. Bertarelli and Tomaszewski²⁰ carried the spirochete from rabbit to rabbit, and later to monkeys and guinea-pigs.

Hoffman²¹ and Brüning have obtained positive results in dogs. It seems probable that most of the lower animals are susceptible to inoculation with the spirochete; only the higher monkeys, however, show evidence of secondary syphilis, the lower animals reacting only by the production of a local lesion at the site of the inoculation. Neisser²² has shown, by experiments on several hundreds of monkeys, that, in spite of the fact that the lower monkeys do not develop evidence of general syphilis, the virus is distributed throughout the body.

In the face of so much confirmatory evidence it would seem useless for any one to deny the specificity of the spirochete; and yet there have been many who have done so.

It is manifestly impossible for the spirochete to pass through a Pasteur filter; yet Jancke,²³ a pupil of Siegel's, reports a successful inoculation with syphilitic material which had been passed through a Berkefeld-Chamberlain filter. This is in direct contradiction to the results obtained by Klingmüller and Baermann,²⁴ Metchnikoff²⁵ and Siebert.²⁶ Since Jancke did not find any spirochetes, nor did he make any subsequent inoculation experiments, it is possible that from the syphilitic toxins injected he obtained a local syphilitic, analogous to the toxic tuberculids obtained by Zieler,²⁷ von Pirquet and others.

Another short-lived objection to the spirochete theory was the question of the identity of the so-called silver spirochetes obtained by the Levaditi²⁸ method of staining. They were held to be nerve fibrillæ, cell borders, fibrin, etc. The introduction of the dark-ground illuminator effectively put an end to this controversy.

Neisser ably sums up the question of the specificity of the spirochete as follows:

As long as we are not in a position to obtain the spirochete in pure culture, and with such a culture produce syphilis in the lower animals, the proof of the specificity of the *Spirochæta pallida* depends essentially on the demonstration of its constant presence in all syphilitic lesions and its constant absence in non-syphilitic lesions.

What are the facts in the case?

It may be said that the spirochete can be demonstrated in all chancres, and in their regional adenitis,

2. Siegel: München med. Wehnschr., 1905, lii, 1321.
3. Merk: Wien. klin. Wehnschr., 1905, xviii, No. 36, p. 926.
4. Freund: München med. Wehnschr., 1905, lii, No. 38, p. 1819.
5. Jancke: München med. Wehnschr., 1905, lii, No. 45, p. 2183.
6. Schaudinn and Hoffman: Arb. a. d. k. Gsndtsamte, 1905, xxi, No. 2, p. 527.
7. Schereschewsky: Deutsch. med. Wehnschr., 1909, xxxv, No. 19, p. 835.
8. Metchnikoff and Roux: Etudes expérimentales sur la syphilis, Ann. de l'Inst. Pasteur, xvii, No. 12.
9. Haensell: Arch. f. Ophth. (Graefe's), xxvii, p. 93.
10. Klebs: Arch. f. exper. Path., 1879, v, 161.
11. Siegel: München med. Wehnschr., 1905, lii, 1321.
12. Schultz: Med. Klin., 1905, i, No. 19, p. 466.

13. Neisser: Deutsch. med. Wehnschr., 1905, xxi, No. 19, p. 748.
14. Hoffman and Brüning: Berl. klin. Wehnschr., 1905, xlii, No. 46, p. 1450.
15. Mühlens: Deutsch. med. Wehnschr., 1907, xxxiii, No. 30, p. 1207.
16. Bertarelli: Centralbl. f. Bact., xl, 64.
17. Scherber: Wien. klin. Wehnschr., 1906, xix, No. 24, p. 726.
18. Groef and Clausen: Deutsch. med. Wehnschr., 1906, xxxii, No. 36, p. 1454.
19. Kraus and Volk: Wiener. klin. Wehnschr., 1906, xix, No. 21, p. 620.
20. Quoted by Grouven: Med. Klin., 1907, lii, No. 26, p. 774.
21. Hoffman: Deutsch. med. Wehnschr., 1907, xxxiii, No. 14, p. 553.
22. Neisser: Die experimentelle Syphilis-Forschung nach ihrem gegenwärtigen Stand, Berlin, 1906.
23. Jancke: Med. Klin., 1907, lii, No. 17, p. 486.
24. Klingmüller and Baermann: Deutsch. med. Wehnschr., 1904, xxx, No. 21, p. 766.
25. Metchnikoff: Ann. de l'Inst. Pasteur, 1904, xviii.
26. Siebert: Deutsch. med. Wehnschr., 1905, xxxi, No. 41, p. 1642.
27. Zieler: München med. Wehnschr., 1908, lv, No. 39, p. 2046.
28. Levaditi: Compt. rend. Soc. Biol., 1905, lvii, 652.

and in all condylomata and mucous patches. Noeggerath and Staehelin²⁹ found them in the circulating blood. Rolshoven³⁰ found them in the blood in thirty out of forty cases; in one case, three weeks before the eruption. Hoffman³¹ produced a successful inoculation of a monkey with blood from a man whose syphilis was of six months' duration and who had a profuse secondary syphilis. Bandi and Simonelli³² obtained the organisms in the blood from a patch of secondary erythema. Veillon and Girard³³ found them in the capillaries of an erythematous patch. Jacquet and Sevin³⁴ found them in all secondary lesions. They have been found innumerable times in papular syphilids. Spitzer³⁵ found the organisms in a case of ulcerating syphilid of eight years' duration, also in a gumma of the scalp. Doutrelepont and Grouven³⁶ and Tomaszewski,³⁷ Schandinn,³⁸ Hoffman, Neisser and many others have found the spirochete in gummata. They have been found by numerous observers in the placenta and umbilical cord. They are found in the great majority of cases of congenital syphilis and in all organs and tissues of that condition. Meyer³⁹ examined eighteen macerated non-syphilitic fetuses and failed to find any spirochetes.

They have been found many times in various internal organs in acquired syphilis; notably in the heart and blood vessels, especially in cases of aortitis and endarteritis, in the liver in syphilitic hepatitis, and in the kidneys in syphilitic nephritis. In spite of the increasing evidence of the syphilitic etiology of tabes and paresis, I have never heard of the *Spirochæta pallida* being found in the central nervous system in acquired syphilis. Gaucher and Merle⁴⁰ found the organisms in the cerebrospinal fluid of a patient with cerebral syphilis. Neisser⁴¹ in a case of secondary syphilis found the spirochete in the spinal fluid. It is remarkable, however, how few observations there are on this subject.

To sum up, it may be said that the *Spirochæta pallida* has been found innumerable times in all the lesions of every stage of syphilis. In fact, the presence of the organism is the cause of the lesions. In studying these lesions one is struck with the marked variation in the number of organisms present. As a general rule, it may be said that the more acute the lesion the greater the number. The organisms, as a rule, occupy the intercellular lymph spaces, although it is not uncommon to find them within a cell. They are more numerous in the center of the lesion, diminishing toward the periphery, but extending even into the non-inflammatory area. A remarkable circumstance noticed by all observers is the vast number of organisms in congenital syphilis and the slight evidence of inflammatory change.

The discovery of the *Spirochæta pallida* has resulted in an entirely new pathogenesis for the disease. It has been found in the experimental work that the most successful method of inoculation is by rubbing the virus into a scarified area of the skin; this in addition to the fact that the genital region of animals is the most sus-

ceptible part of the body may account for the sexual transmission of the disease and the relatively small percentage of extragenital infections.

Given an inoculation, the infection for a time is purely local; during the period of incubation the virus proliferates until the irritation becomes sufficient to cause the tissues to react by the production of a local inflammatory area, which tends to necrosis, but undoubtedly may resolve. From this local focus a dissemination of the infecting organisms takes place, the most usual route being by way of the lymph channels, although there is no doubt that at times the dissemination is hematogenous. Wolters⁴² and others have demonstrated sections of chancres showing the spirochete in the blood vessels. This opens up an interesting and important point, as to the time at which this dissemination occurs. There is not much doubt but that this varies within wide limits. Neisser⁴³ in a large series of experiments attempted to solve this question by excising the inoculated area at varying intervals of time. His results were not conclusive. He found that in six cases excision at periods from ten minutes to twelve days prevented dissemination; on the other hand, in ten other cases excision was useless, even when done as soon as eight to fourteen hours after inoculation.

In whatever manner and at whatever time dissemination takes place, it is eventually by means of the blood stream that the spirochetes are distributed throughout the body. They do not remain in the circulating blood, but locate in the tissues. They show a predilection for certain tissues and organs, notably the skin, lymphatic tissues, the circulatory system, glandular organs, and, as Neisser has shown, the bone-marrow. Wherever they locate they may cause an inflammatory reaction, which is more or less characteristic, aside from the spirochetal content.

From the results obtained by the Wassermann⁴⁴ reaction, it is evident that pathologic changes are much more frequent in the internal organs than we have heretofore suspected.

As dermatologists, we are particularly interested in the cutaneous localization of the spirochete. We no longer recognize a toxic syphilid; all the cutaneous manifestations of the disease represent foci of spirochetal growth in which the spirochetes may persist for long periods, and from which a new local or general dissemination may occur. This remarkable quality which the spirochetes possess of lying dormant for long periods, their presence seemingly causing no pathologic changes, accounts for many of the characteristic late secondary eruptions, such as the annular, circinate and corymbose syphilids.

This tendency of the infection to become latent accounts in part for the marked chronicity of the disease. Füresz⁴⁵ examined lesions from twenty-four cases to determine the effect of mercurial treatment on the presence of the spirochetes. In spite of the fact that the cases had from ten to fifty-five inoculations, spirochetes were found in seventeen cases. They could be found as long as the least grade of infiltration was present. In fact, the organisms have been found at the site of a previous eruption.

The question of immunity has taken on new life since the discovery of the spirochete. It has always seemed rather illogical that a syphilitic should be immune to

29. Noeggerath and Staehelin: München. med. Wchnschr., 1905, lii, No. 31, p. 1481.

30. Rolshoven: Med. Klin., 1907, iii, No. 33, p. 989.

31. Hoffman: Berl. klin. Wchnschr., 1905, xlii, No. 46, p. 1450.

32. Bandi and Simonelli: Centralbl. f. Bact. u. Parasitenk., xl, 64.

33. Veillon and Girard: Compt. rend. Soc. biol., 1905, lvii, 652.

34. Lesourd: Ann. de dermat. et de syph., June, 1905.

35. Spitzer: Wien. klin. Wchnschr., 1905, xlviii, No. 31, p. 822.

36. Doutrelepont and Grouven: Deutsch. med. Wchnschr., 1906, xxxii, No. 23, p. 908.

37. Tomaszewski: München. med. Wchnschr., 1906, liii, 1301.

38. Levy-Bing: Le microorganisme de la syphilis, Paris, 1907.

39. Meyer: Centralbl. f. Bact., 1. Abt., xvi, No. 4.

40. Gaucher and Merle: Compt. rend. Acad. d. Sc., 1909, iii, 29.

41. Neisser: Abstr. in Berl. klin. Wchnschr., 1909, xvi, No. 20, p. 934.

42. Atlas der aetiologischen und experimentellen Syphilis-Forschung, Berlin, 1908.

43. Neisser: Die experimentelle Syphilis-Forschung, p. 80.

44. Wassermann: Berl. klin. Wchnschr., 1907, xlv, No. 1, p. 12.

45. Füresz: Med. Klin., 1907, iii, No. 35, p. 1045.

heterogenous virus and not immune to his own virus. The experiments of Finger and Landsteiner and Neisser show that, strictly speaking, there is no immunity. The apparent immunity, such as expressed by Colles' and Profeta's law, has been shown by the results of the Wassermann reaction to be due to a latent syphilitic infection. This is also true of 47 to 57 per cent. of syphilitics after the usual treatment.

Admitting the etiologic rôle of the spirochete in syphilis, the question naturally arises: Of what value is the organism for diagnosis? Aside from the Wassermann reaction, the demonstration of the spirochete is the most reliable means at our disposal for the diagnosis of syphilis; as it is, these two factors give us practically absolute control of the diagnosis of syphilis in all its stages.

The greatest field for usefulness of the demonstration of the spirochete is in the diagnosis of primary syphilis; here it is far more reliable than the Wassermann reaction and much simpler. The spirochete can be demonstrated in every chancre and its contiguous enlarged lymphatic glands. I believe with Rona⁴⁶ that the finding of the spirochete permits a positive diagnosis to be made earlier than any other symptom. The Wassermann reaction in primary syphilis is not nearly so reliable and is infinitely more difficult of performance. The percentage of positive reactions increases in direct ratio to the duration of the infection; therefore the statistics of different observers vary greatly. When one considers the duration of the chancre, one finds that a positive Wassermann reaction is unusual before the third week, and that after that time the percentage of positive reactions increases up to the time of the general eruption, when it is practically 100.

Hoffman, Blumenthal and Roscher⁴⁷ all hold that in primary syphilis, diagnosis by means of the spirochete is simpler, quicker and more certain than the Wassermann reaction.

After the primary stage, the Wassermann reaction is so much more reliable that I have substituted it for the demonstration of the spirochete in doubtful cases.

The great drawback to the more widespread use of the demonstration of the spirochete for the diagnosis of primary syphilis has been the difficulty of staining the organism. Since the introduction of the Reichert dark-ground illuminator this is no longer a valid reason for the non-use of this most reliable means of diagnosis. By means of this instrument we are in a position to diagnose syphilis from the moment the initial lesion is first noticed. The method is so reliable, so simple and consumes so little time that there is no reason why it should not supplant all other methods of diagnosis.

As seen with the dark-ground illuminator the *Spirochæta pallida* is so characteristic that there is absolutely no difficulty in identifying it. The chief characteristics are as follows:

1. *Size*.—The spirochetes vary in length from 7 to 21 microns, being from one to three times the diameter of a red blood cell. It is not uncommon to see the organism longer than this, but I believe such forms to be composed of two or more individuals.

2. *Shape*.—They are seen to consist of an extremely slender thread closely wound in a corkscrew form, the windings being very acute. In fresh preparations the windings are absolutely regular, but as the specimen

gets older the organism changes form, the most frequent change being an irregularity or obliteration of the windings in the central portion.

3. *Position of Ends*.—The ends of the pale spirochete are sharp and terminate on the periphery of the spiral and not in the center as in the other forms of spirochetes. This peculiarity is very characteristic, but is seen only when the organism rotates on its long axis.

4. *Motility*.—When the specimen is freshly prepared the organism is very active and possesses the following motions: (A) A rotation on its long axis in either direction; this motion is very rapid, but not necessarily accompanied by a change of position; as the specimen gets older this motion grows less. (B) It progresses from place to place, but not so rapidly as the other forms of spirochetes commonly met. (C) It has a quick and spasmodic bending or twisting motion. This form of movement increases as the specimen gets old, and at times one sees an organism bent in the form of a circle, resembling somewhat a crenated red blood corpuscle. It is not uncommon to find two organisms joined end to end. Again, one sees two or three organisms lying side by side.

The number of organisms found varies in the different lesions. They are most numerous in chancres, condylomata and mucous patches and vary from two or three in a field to sixty or eighty. Patients who have received some treatment show fewer organisms than the untreated ones. A few days' local treatment with calomel or other mercurial preparations causes a marked diminution in the number present in the chancre, although they are very numerous in the enlarged inguinal glands.

I have been using this method for two years and have never failed to find the organisms in chancres, mucous patches and condylomata. In our first communication,⁴⁸ Dr. Corbus and I reported finding the spirochete in mucous patches in three cases in which the syphilis was from one to three years old. Recently we found them in a mucous patch in which the infection was of eight and one-half years' duration. Neilsen⁴⁹ reports a case with a papulo-erosive syphilid of the mouth in which he demonstrated the spirochete nine years after the infection. Hoffman has reported a similar case and Papée⁵⁰ has reported one thirteen years after the initial lesion. These observations are important in view of the fact that some authorities are apt to belittle the contagiousness of syphilis after the fifth year.

Considering the simplicity and reliability of the dark-ground illuminator for the diagnosis of syphilitic lesions it is very strange that the method has not had a more universal application. We can explain this circumstance only by assuming that the profession has not a proper appreciation of the necessity for an early diagnosis. An early diagnosis permits us to institute a logical treatment; such a treatment should consist in the removal or destruction of the infected area at the earliest possible moment and the immediate institution of energetic mercurial medication.

In commenting on the results of excision in his experimental work, Neisser⁵¹ says:

These results are not very satisfying. In spite of them, however, I think now, as before, that excision should be done wherever possible. The value of excision is not affected by

48. Harris, Frederick G., and Corbus, B. C.: The Clinical Value of the *Spirochæta pallida* in the Diagnosis and Treatment of Syphilis. *THE JOURNAL A. M. A.*, Dec. 5, 1908, li, 1928.

49. Neilsen: *Monatsh. f. pract. Dermat.*, xlviii, No. 2, p. 5.

50. Papée: *Monatsh. f. pract. Dermat.*, xlviii, No. 8, p. 347.

51. Neisser: *Die experimentelle Syphilis-Forschung*, p. 81.

46. Rona: *Wien. med. Presse*, 1907, No. 34.

47. Hoffman, Blumenthal and Roscher: *Med. Klin.*, 1909, v, No. 7, p. 241.

these experimental results, since under all circumstances it must be useful to get rid of such an important focus of poison, as the chancre.

Hoffman says:⁵²

Since the microscopic examination now allows us to diagnose the chancre early, we must try the effect of excision or destruction of the chancre at the earliest possible time, even if the experimental work does not permit us to expect to cure the disease. Still, the removal of such a large amount of virus, perhaps, may make the disease milder, especially when combined with energetic mercurial treatment.

The list of adherents of the method of excision and early treatment of syphilis is a growing one and includes such names as Neisser, Hoffman, Hallopeau, Volk, Scherber, Rona and Lukaszewicz. All authorities are agreed on the question of antispirechetal action of mercury, but the work with the Wassermann reaction has shown us that it is necessary that the treatment of syphilis should be more energetic and more individual than has been the custom. In fact, I believe that the control of the treatment, by means of the Wassermann test—the so-called biologic treatment—is a great advance in the therapeutics of syphilis.

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ABSTRACT OF DISCUSSION

DR. G. A. DESANTOS SANE, New York City: Our knowledge of the *Spirochæta pallida* has progressed remarkably within the past two years, since Edward Spitzer, of Vienna, first showed the value of the dark-ground illuminator in recognizing this organism. There is still, however, a great deal to be learned, not only about the biologic character of the spirochete, but also about the practical aspects of the diagnosis of syphilis. Dr. Harris is very pessimistic on the subject of staining. The only method I know of that will give satisfaction in a fair proportion of cases is that of Schereschewsky, which was described in the *Centralblatt für Bakteriologie*, October, 1907. This author was the first to recognize the difficulties of staining the *Spirochæta pallida*. They are twofold. First, the difficulty in spreading the smear in such a way that the erythrocytes will be sufficiently far apart to get the dye directly on the spirochetes. Second, the difficulty of avoiding precipitation in this delicate stain.

In order to obviate this, the following technic has been suggested by Schereschewsky: The clear serum from the sore, etc., obtained by means of a small curette, is spread on the slide and fixed with heat. The cubic centimeters of a 0.5 per cent. solution of pure glycerin and 13 drops of Giemsa's solution are heated in a test tube, which must be absolutely clean. The slightest trace of acid in the tube causes a precipitate, and renders the stain useless. Contact of the slide with any grease or acid, or traces of dye cause a precipitate. The slides, therefore, should never come in contact with the fingers, but should be stained on clean glass supports. The heated stain is allowed to remain for ten minutes, and is then washed off. This method has been used in a large series of cases by White and Avery, and with it they found the spirochete in 100 per cent of chancres. I have found the method very satisfactory when all the precautions mentioned were used. I was able to find the spirochete in chancres in 100 per cent. of the untreated cases, and these results were verified by control experiments with the dark-ground illuminator. After treatment with mercury, however, the number of cases in which the spirochete was found was reduced to 50 and even 25 per cent., according to the extent of the treatment. The dark-ground illumination method takes less time and is more satisfactory than the staining method, but it is not always convenient to have the dark-ground condenser at hand.

This brings up the question: With what shall we dilute our drop of secretion in preparing it for the dark-ground

illumination? I have obtained the best results by using either water or salt solution. This point may seem a trifling one, but if the results without water and with water be compared, it will be found that the spirochete is three times as distinct after using water. It swells up, becoming more refractive, brilliant and distinct. The refractive index of the serum is such that dark-ground illumination does not show the spirochete so distinctly in serum as in water. This dilution process has one disadvantage, namely, that the spirochete loses its motility more quickly, but the organism can be kept alive for a sufficient time to make a satisfactory examination. This observation has also been made by Commandon.

Sometimes, if the motions of the spirochetes are watched for some time, one can see two of them come together, join heads or tails, and wind round each other for a part of the way, and sometimes for the entire way. Sometimes one of the organisms seizes a bit of protein matter and floats about with this tiny ball, just as with a headlight. These peculiarities are, as yet, unaccounted for, and much research is needed in this field.

DR. HOWARD FOX, New York: I should like an expression of opinion about the different apparatus used. For the past year I have tried both the Reichert and the Leitz dark-field illuminators. The instruments that are placed on the stage of the microscope such as the Reichert and Leitz (condenser B) are certainly much more convenient and cleanly than those inserted from beneath and that take the place of the Abbe condenser. I have used the Nernst lamp of from 50 to 60 candle power and it seems to me a perfectly satisfactory source of illumination. Does Dr. Harris for ordinary use consider the small arc light preferable to the Nernst lamp?

DR. MAXIMILIAN HERZOG, Chicago: Doctor Harris has not been the only or first one in Chicago who examined primary and secondary luetic lesions for spirochetes. I took up this work three years ago, on my return from the Philippine Islands, where I first became interested in it. I can confirm the observation of those who claim that the spirochetes are found in the great majority of cases in the primary and early secondary lesions. My first work was done before the dark-field condenser had been recommended and I had no great difficulty in finding the spirochetes in stained specimens, although it may take a great deal of time to find them in some instances, particularly in the late secondary lesions. Subsequently, I used the dark-field illuminator, but I generally found that if I could not detect the spirochetes after a prolonged search of stained specimens, I could not find them with the dark-field illuminator. I have frequently used a 50-candle power stereopticon incandescent lamp, in front of which I have a dark shield, and I employ the ordinary condenser, which is put on top of the stage. Dr. Saxe mentioned round, luminous bodies which he has found in connection with the spirochetes. These are probably due to optical effects and I would not attach much importance to the theory that the spirochete possibly feeds on these round bodies.

DR. WILLIAM S. GOTTHEIL, New York City: I should be the last to deprecate these newer methods of diagnosis, and yet I think that we are perhaps attributing a little too much importance to them, to the exclusion of the older and established methods, very much as was the case with the surgeons and the Roentgen ray. I had occasion, a number of months ago, to see a physician with a lesion of the finger which looked very much like a chancre. I sent him, not to a tyro, but to an expert, who examined the secretion from the lesion, and the report came back, "no spirochetes." The doctor returned to me a week later and said: "I know that this is an initial lesion." A second bacteriologic examination was made, and again the report came back, "no spirochetes." One week after that he developed a pigmented papular syphilide. I mention this case only that it may serve as a note of warning against the neglect of the ordinary and customary and long established and satisfactory methods of chancre diagnosis.

DR. DOUGLASS W. MONTGOMERY, San Francisco: The assertion, I think, was made that amputation of the primary sore should be done to rid the patient of that much infection. I can not see very well how that would rid one of any considerable amount of infection. The primary sore is usually very

⁵² Hoffman: *Deutsch. med. Wchnschr.*, 1905, xxxi, No. 4, p. 1712.

small, no larger than a papular syphilide, and if the assertion made by Dr. Harris would hold good, it might be considered proper to excise every papule of a papular syphilide in order to get rid of so much more infection. That would be an equally logical proposition, and as a result, the patient would certainly be well tattooed. I would ask: What grounds are there for amputating the chancre with the idea of ridding the patient of that amount of infection?

DR. M. S. HEIDINGSFELD, Cincinnati: I concur with Dr. Gottheil that we are in danger of placing too much reliance on comparatively new tests. While the value of the laboratory tests can not be questioned, they must still be regarded in the light of aids to clinical diagnosis rather than as absolute methods. The reports that emanate from the laboratory and which receive widespread attention are apt to lead the profession into serious error. I have known the diagnosis of syphilis essayed purely on laboratory tests in cases of suspected initial lesions which at the time were clinically negative and failed to show any confirmation after months of careful observation. I do not believe that the laboratory tests in syphilis have yet reached the degree of perfected development that they overshadow the clinical features in importance. With reference to the syphilometer: I found the syphilitic regulation test of two drops to be entirely too small to convey any degree of accuracy. The manufacturers kindly supplied me with a half pound of the taurin solution and when I repeated the test, using one or two grams of clear serum in place of one or two drops, they became uniformly positive in character, irrespective of the syphilitic or non-syphilitic character of the case.

DR. F. G. HARRIS, Chicago: In regard to the method of demonstrating the spirochete, to which several of the speakers referred, I worked for a long time with the staining method, with uniformly poor results. My organisms were indefinite in outline, and I was unable to identify them positively. We must have a live organism in order to make a positive diagnosis, an organism which is not deformed.

In my work, I employ the Reichert apparatus, which I prefer on account of its convenience. I use an arc light, although one may also use a Nernst lamp; with the new model of the Reichert apparatus the ordinary inverted Weisbach light may be employed.

To obtain the material, I simply irritate the chancre or mucous patch and collect the serum in a capillary tube, from which it is then blown on a clean cover glass, which is inverted on a clean slide. The specimen must be as thin as possible. If the preparation is too thick or the slide is too thick, the rays of light will not be focused at the proper point.

The characteristic movement of the spirochete in a fresh preparation is rotary, like that of an auger-bit, and that is one thing that we do not see in the stained specimen. The windings are absolutely regular in a fresh specimen, but later become somewhat irregular. In the stained specimen few of the organisms are characteristic, most of them have lost their regular windings, due to the traumatism of preparation.

As to Dr. Montgomery's question whether or not it would be proper to excise all the possible foci of infection in secondary syphilis, he misunderstood the purpose in view in amputating the original chancre. With the dark-field illumination method we are able to make the diagnosis from the very beginning of the chancre. I have found the spirochete in chancres one day old. In such cases, by amputating that focus and putting the patient on immediate treatment, we hope to abort the syphilis. Scherber, in the case referred to by Dr. Pernet, amputated the chancre and gave the patient two courses of injections and there were no further symptoms during the three years that he was kept under observation. By means of the dark-field illumination method we can make the diagnosis of syphilis very early, and the reasons for amputating the chancre are mainly for the purpose of preventing dissemination, just as the surgeon would remove the primary source of any other kind of infection. We remove the primary and chief focus of infection by excising the chancre, and depend on the mercury to destroy any spirochetes that are left behind. Why should we wait until the body is permeated with them?

DR. MAXIMILIAN HERZOG, Chicago: I do not think that there was any possible mistake about the character of the organism that I demonstrated in Chicago in the shape of photomicrographs. The smears had all been prepared from typical primary lesions, the cases in fact were followed up to the secondary stage. It would indeed have been strange if I should have had the misfortune to find only the *Spirocheta refringens* in typical primary lesions. Such a claim need hardly be discussed.

REGENERATION OF THE CORNEA *

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It has long been known that the epithelium of the cornea would regenerate itself after being destroyed intentionally or by accident.

Haller¹ as early as 1763, in his "Elementary Physiology of the Human Body," asserts that regeneration of the cornea takes place, and even the oldest ophthalmologists² assume that regeneration of corneal tissue occurs. In fact, Desmarres³ states that, in experimenting on transplantation of the cornea, he has noted an increase in length of the corneal flap, and even a true regeneration. Mauchart,⁴ Mead and Larry,⁴ Wardrop,⁴ Gulz,⁵ and Malgaigne⁶ resected corneal scars, and found that healing took place with little or no scar. What is extremely interesting in the light of our present knowledge of the effect of an opium derivative, dionin, is the work of Muehlbauer⁷ and Beger,⁸ who resected the superficial layers of the cornea and succeeded in obtaining a clear cornea following the application of laudanum. Desmarres was unsuccessful in his attempt to verify these results, since the rabbits on which he operated contracted a severe purulent inflammation (infection?), resulting in heavy scars.

It is a matter of common observation that corneal wounds following cataract extractions heal so that it would be difficult to find the line of incision. Also numerous corneal opacities clear in time, where the question might easily arise as to whether there might not be a true regeneration of transparent corneal tissue in these cases.

The first systematic research for actual proof of corneal regeneration was made by F. C. Donders⁴ in 1846, who suspected that it occurred, but did not think that there was sufficient proof to establish the fact. He accordingly conducted a series of experiments on twenty-two rabbit eyes, from which he resected a flap of cornea from one-third to two-thirds of its thickness, including generally about one-half of the area of the cornea or more. He examined six eyes microscopically and gives a tabulated report showing (1) the thickness of the cornea cut away, (2) the thickness of the uncut

* Read in the Section on Ophthalmology of the American Medical Association, at the Sixtieth Annual Session, held at Atlantic City, June, 1909.

1. "Vulnerato ex succo effuso pellucida renascitur, ut coram video, et late deleta nova redit, omnino ut epidermis." Haller, *Elementa physiologiae corporis humani*, Lausanne, 1763, 363.

2. Mauchart, B. D.: *De ungue oculi pure inter lamellas corneae* (defender C. F. Bilget), *Diss. Med.*, Tübingen, 1742; in Hallerus, *Alb. Disputationes chir. selectae*, 1, 394, Lausanne, 1755; abstr. in Donders, Note 6.

3. Desmarres: *Ann. d'ocul.*, 1843, x, 5.

4. Donders, F. C.: *Untersuchungen über die Regeneration der Hornhaut*, *Holländ. Beitr. zu d. Anat. u. Physiol. Wissensch.*, 1846-8, 1, 387 to 400.

5. Gulz: *Oesterr. med. Wchnschr.*, 1842, No. 2.

6. *Ann. d'ocul.*, Fl. Cuniere, 1845, xlii, 211.

7. Himley: *Die Krankheiten und Missbildungen des menschlichen Auges und deren Heilung*, 1843, ii, 56.

8. Ueber die Verwundbarkeit des Auges und seiner Haute, nach Versuchen an Thieraugen, *Ztschr. f. d. Ophth.* (von Ammon's), iii, Nos. 2 and 4.