

from the animal tissues. Recent experiments on this topic by Martin\* and others appear to indicate that the toxic power of the albumin is slight, when the culture has an acid reaction; virulent when it is alkaline. The virulence is reduced by exposure to 58 C. for two hours and removed by boiling at 100 C. for twenty minutes.

We now pass on to another step that was made by Behring, an assistant in Koch's laboratory. In 1891-2 he found in the blood of immune animals a something non-bacterial, that neutralized the otherwise toxic products of the Loeffler bacillus. What was it? Behring† held to the idea that the blood serum contained this *anti-toxine*, as it was

About one year ago Drs. Klemensiewicz‡ and Escherich tested the theory by experiments on guinea pigs. They also succeeded in getting some blood from patients convalescing from diphtheria, and extracting the blood serum, found that it arrested the poisonous action of the diphtheritic cultures that had previously been inoculated upon guinea pigs.

But about nine months ago, Kossel§ was, so far as I can learn, the first to test this *anti-toxine* on the human being. During last March and April eleven children were treated. Their ages varied from three to eleven. Four underwent tracheotomy. Of these four, only one died. Of the eleven, nine recovered. These cases were undoubtedly severe ones, but the mortality was light, and in the tracheotomy cases *unusually* light. In the previous year, in the same institution, of 32 cases only eleven survived. Hence a fatality of 65% under ordinary treatment, as compared with 18% under the anti-toxine. So far, then, the results of this new mode of treatment appear to be most encouraging.

Efforts are now being made by Behring|| to concentrate this blood serum or make an extract of it; while Aronson¶ claims that he has already succeeded in so doing, but has not had sufficient opportunity for testing it. And here the matter rests.

\*Martin, *Annales de l'Institut de Pasteur*, Nov. 1, 1892, p. 335.

†Behring, *Zeitsch. fur. Hygen*, Pa., 12, 1. p. 10.

‡Behring, *Deutsch. Med. Woch.*, Jan., 1893.

§Klemensiewicz, *Centralbl. f. Bakt.*, 13, 1893.

¶Kossel, *Deutsch. Med. Arch.*, April 27, 1893.

||Behring, *Deutsch. Med. Woch.*, June 27, 1893.

¶Aronson, *Deutsch. Med. Woch.*, July, 1893.

Now, while we appear to have all the proof that is requisite to place true diphtheria among the contagious diseases, there is a difference of opinion, as to whether it may not also be autochthonous, that is, self-generating, or indeed miasmatic. Gottstein\* concludes from his experience, that if 100 susceptible individuals are exposed only 21, or 21% will take it, while in measles 99% would take it, and in scarlet fever 30%. The sources of contagion whether direct or indirect, are held to be, primarily, the membrane as spit up or coughed or sneezed up, and so inspired or swallowed directly or indirectly through the medium of infected garments, or instruments or vessels that have been contaminated, by milk, perhaps even by flowers taken from the coffin of diphtheritics. It is certain also, I think, that several of the domestic species animals suffer from a communicable form of this disease, and that both in them and in mankind it may be propagated from chronic cases or from convalescents after the acute stage has been passed. In this connection Dr. L. Emmett Holt's views are especially interesting and valuable. He writes me: "Diphtheria is certainly much less contagious than measles, scarlet fever or small pox."

"I have personally known of but two physicians contracting the disease from patients. In three cases only have I seen parents or nurse take the disease from children. I believe this to be very rarely the case if proper precautions, such as the regular use of an antiseptic gargle and nasal spray, are employed."

This is the view commonly held by most of our clinicians. In this connection, it should be borne in mind that there is a vast difference in the liability to the reproduction of a disease, according as we view it from the standpoint of the bacteriological laboratory, or our actual every-day experience. Under special conditions, which the experimenter can regulate, he may reproduce a disease like diphtheria in 95% of his laboratory experiments, while in our clinical experiences it may not be reproduced by the ordinary processes of nature in 10%, because the conditions are unfavorable. That so low a percentage as 10 or 20% contract diphtheria after exposure, does not, however, invalidate the theory that it is exclusively caused by contagion. But it indicates that

\*Gottstein, *Berl. K'in. Woch.*, 25, 1893.