dyspnea, which gradually becomes more and more intense.

In diphtheria, the exudate in the vast majority of cases makes its initial appearance upon the tonsils, later extending to the pillars of the fauces, the uvula and even to the posterior pharyngeal wall; later, in some cases extending into the posterior nares or downward into the larynx, trachea and bronchi. In croup, the membrane invariably first appears in the larynx, from this point extending upward or downward or both. In diphtheria, we generally have a characteristic glandular swelling, which is absent in croup. Constitutional symptoms are prominent in the former disease and absent in the latter. In croup, all the symptoms are rapidly increased in severity, the dyspnea becoming intense. Cyanosis soon appears, and death generally closes the scene.

Should the diphtheria be laryngeal in character, there would not be such a wide divergence in the symptoms. Still the fever would be higher than in croup, glandular swelling would occur and the constitutional symptoms be marked.

Two of the cases of croup in my own practice, to which I referred, had absolutely no rise of temperature at any time, and in all four of the cases no glandular swelling existed and no symptoms characteristic of diphtheria were present.

In the earlier portion of this paper we have seen that there occurs a true diphtheria, highly contagious and very fatal; and a so-called pseudo diphtheria not contagious and much less fatal than the former affection. We have seen that the etiology of these two diseases is very different.

Is it not possible, nay, even probable, that these same conditions prevail with reference to the laryngeal mucous membrane? I would answer the question affirmatively, and would further strengthen this opinion by studying the history of diphtheria and croup as they occurred in this country during the present century, and to a certain extent by the study of the clinical manifestations of the two affections.

The conclusions, then, which I have reached are as follows:

1. That there are two forms of membranous laryngitis; one a true diphtheria, produced by the Klebs-Löffler bacillus and the other non-contagious membranous laryngitis, produced, if you please, by streptococci, staphylococci, etc.; and that both of these types are very fatal.

2. That membranous croup as we have heretofore understood it, is a disease of much less frequency than was formerly supposed.

3. That it being impracticable to make a bacteriologic examination of the membrane in many cases, and as this is the only method by which we may be absolutely positive of our diagnosis, the same precautionary measures should be taken to prevent a possible spread of the disease as would be adopted in a case known to be diphtheria.

Those who believe that a non-contagious membranous croup does not exist will hardly be favorably impressed with the idea of carding the house, isolating the child, and causing any other children who may be members of the family, to remain from school for several weeks, but in the light of our present knowledge upon the subject it is the only safe method to pursue, unless a careful microscopic examination by a competent bacteriologist has demonstrated the absence of the Klebs-Löffler bacillus.

ON LOCAL ANESTHESIA PRODUCED BY INTRACUTANEOUS INJECTIONS.

BY DR. CHOLEWA.

BERLIN, GERMANY.

[The following paper was sent to me by the writer, my old friend, the well-known aurist of Berlin, Germany, with the request that I look it over and have it published here. He was induced to write the essay, not only because he found the method described of great value in the practice of his own specialty, but out of gratitude to Dr. Schleich (the originator of it), who performed an operation upon him that under ordinary circumstances would have required a general anesthetic. This otherwise very painful surgical procedure was carried out with perfect comfort to the patient, by employing the local "infiltration-anesthesia" method, which he describes.—CASEY A. WOOD.]

In the winter of 1891, a young surgeon gave a discourse on infiltration-anesthesia before the Berlin Medical Society which excited great interest. That is saying a great deal, since this Society, the greatest and most famous in Germany, consists mostly of professors in the University as well as the heads of the great hospitals in almost equal numbers. He who aspires to address this scientific body must be very sure of his subject, for he will have to suffer a most merciless criticism.

Prof. Virchow is the President of this Society, and who does not know the sharp tongue and the caustic wit of this gentleman, who has so often promoted as well as humiliated scientific efforts? But this young physician was master of his subject. A numerous array of cured patients surrounded him and were introduced to the audience by him, in order that his colleagues might with their own eyes see, and with their own ears hear, and investigate. He did not discuss the ability to operate without pain in the various branches of minor surgery only, but asserted that capital operations could be done by his method. For example, he exhibited one case in which an ovarian tumor weighing many pounds was removed and the woman did not faint once during the long operation. An old man showed scars on his thigh which were left by a carbuncle extending from the trochanter of the femur to the bend of the knee, and yet this old man had in this case undergone a very severe operation without the use of one single drop of chloroform! The applause, the enthusiasm, especially among the younger members of our profession, was general; it rose to rapture when a young colleague showed the assembly his finger, to which a short time previously this method had been applied by the author, after which a deep but painless incision was made into a well developed whitlow.

How many since this never-to-be-forgotten evening have been treated with this method; how many have been freed from the danger of chloroform and from their own dread of employing it! Over and over again this method of infiltration-anesthesia has been employed in hundreds of cases, simply because surgeons prefer infiltration-anesthesia to the anxiety of a chloroform-narcosis, the dangers of which stand in no proportion to the importance of the operation.

In what respect does this method differ from other local anesthetics?

Inasmuch as the description of the first reception of this method by the medical world was witnessed by myself, in explaining it I beg permission to use the words of the master who discovered it. In the spring of 1892, Dr. Schleich took the opportunity, at the annual meeting of the Head Surgeons at the Langen-

beck Haus, to lay before the assembled colleagues the management of his method in the following words:

INFILTRATION-ANESTHESIA AND ITS RELATION TO
GENERAL ANESTHESIA.

The efforts hitherto made to perform painless operations by means of local anesthesia have not been very successful. To all the methods of local anesthesia hitherto employed, was opposed a danger in one or another direction. Anesthesia produced by vaporization of ether, or mixtures of ether, brought with it the danger of gangrene of the frozen tissues, to which was added great pain, especially of the inflamed parts during the freezing, thus considerably diminishing the advantages of the process. The dangers of poison were opposed to the general use of cocain, that most excellent of all local anesthetics. To this must be added that the pricking with the Pravaz needle and the act of injection itself, especially into the inflamed parts, was so painful that the procedure was considerably robbed of its value. By the employment of the ether-spray as a preliminary to anesthesia by means of cocain injections, I succeeded to some degree in avoiding this evil. The surface of the skin above the part to be made anesthetic became sufficiently insensible by the ether-vaporization to render painless the introduction of the needle. The contents of the syringe entered first into the skin (intracutaneous), and not under the skin (subcutaneous). If I am not mistaken, it was our colleague, H. Schmidt (of Stettin), who first suggested the necessity of these primary intracutaneous injections. I have for many years very frequently performed operations by this species of cocain anesthesia, and have reported several cases. Nevertheless, after the application of solutions from 1 to 5 per cent., as soon as a maximum dose of 0.005 cocain had been reached, frequently even below this point, the most serious symptoms of intoxication have taken place. This occurred even after the application of from two to five small syringefuls, and even when, by previous compression with Es-march's bandage, the circulation was stopped as long as the narcosis lasted. Medical periodicals are full of reports of cocain intoxication, a large percentage of which have had a fatal issue. Owing to this state of things, it was impossible that local anesthesia could successfully compete with "general" narcosis.

I have, therefore, undertaken the task, in a series of trials on myself, on my assistants and the attendants of my clinic, to settle where precisely lay the limit of the efficacy of cocain. These and the experiments following, the results of which I had the honor of laying before the Medical Society of Berlin last November (1891), were all performed in such a manner that the real question, whether a given fluid be an anesthetic one or not, was decided within the intracutaneous limits of the skin. We made the punctures as parallel as possible to the surface of the skin immediately under the papillæ, until a white and slightly elevated blotch rose, resembling the sting of a gnat. The reason that the investigations of this subject, so carefully made by Liebreich and his pupils, as well as by Levin and Kanewsky, did not immediately lead to the discovery of any new facts, was that the aforesaid authors applied the fluid under examination *subcutaneously*.

We tried these experiments first on ourselves; those authors experimented chiefly on the skin or the mucous membrane of animals. During these "intracutaneous" trials of the various solutions of cocain, first appeared the surprising fact that solutions of 1-5000 in water were able, within the limits of the infiltration, to produce a complete anesthesia, so that punctures, cutting, scratching and scraping could be done absolutely without pain. Henceforth this fact may be justly considered as indisputable, after my having proved it in several hundreds of operations. After the discovery of this fact it was a natural inference that the cocain could be altogether left out of the fluid, and the same results were obtainable with pure water and solutions of salt. We found, indeed, that pure distilled water is able to produce perfectly anesthetic blotches. But here an essential difference appeared, namely, the process of injecting water into the substance of the skin itself with the formation of water blotches is painful, and that to no slight degree. Not until from a half to a minute after the injection does pure anesthesia take place, and then is quite as perfect as after the injection of cocain of from 1-5000, or in stronger solutions. The result was that distilled water, according to the meaning of Liebreich, is an "anesthesia dolorosa;" that is, the commencement of anesthesia (the paralysis of the nervous substance), is preceded by a stage of pain, a hyperesthesia of the nerves. This hyperesthesia was an obstacle to the use of pure water for operating purposes, although the refrigerating of the water to zero (Cels.), as well as the use of the ether-spray, employed in the above-mentioned way, reduced this stage of irritation to a minimum, so that I was several times enabled to perform with pure distilled water, such operations as opening furuncles or carbuncles, removal of small ganglia, etc.

Further experiments revealed that a number of substances were practically available as anesthetics without producing irritation. First, as strong a solution of common salt as 0.6 per cent. produces no anesthetic effects worth mentioning. On the contrary, it was found that a further diluted solution of common salt, namely, a 0.2 per cent. solution, by itself, especially in lower degrees of temperature, produces a perfectly prompt anesthesia in the region of the infiltration. I may here, perhaps, remark that what is true of the skin also refers to other tissues of the body; for instance to the subcutaneous layers, the muscular layers and the periosteum. The 0.2 per cent. solution of common salt produces not only anesthesia, an effect which it possesses in common with water, but the process of injection is painless. This proves that it is the infiltration itself that produces the anesthesia.

The anesthetizing fluid need not be itself anesthetic. The proportion of common salt in a solution is important, as witness the following experiment: The smallest effective dose of cocain—weaker injections caused pain—was 0.2-100, or 1-5000. This watery solution of cocain produces anesthesia and makes other injections painless. The same dose of cocain containing 0.6 per cent of common salt is painful and is not able to produce anesthesia, while the same solution wherein the amount of common salt is not greater than 0.2 per cent. again affords perfect anesthesia; indeed one gram of cocain in ten liters of a 0.2 per cent. solution of common salt is capable of producing complete anesthesia, or in other words,

I may use half a liter of my anesthetic solution before the maximum dose of 0.05 cocain is reached.

Now, when you consider that for most operations, as for example the amputation of the breast, about fifty to eighty grams of the solution are employed, you will agree with me, that there can be absolutely no question of danger of poisoning from this method of anesthesia, especially as the dose is never administered all at once, but is extended over the time occupied by the whole operation. If I may be permitted to suggest a theory to explain the incontestable fact of anesthesia by the thoroughly penetrating infiltration of "indifferent" fluids, I must not omit to say that cocain, common salt and water are not the only materials that are able in this way to produce anesthesia. You will obtain as complete a state of anesthesia by using a solution of 3 per cent. sugar or of 0.1 per cent morphia or of 3 per cent. potassic bromid, or of 1 per cent. methyl-violet, or of 2 per cent. caffein, and so on. I may mention that the efficacy of all of these materials, to which may certainly be added a great many others, will be increased by dissolving them, not in water, but in a 0.2 per cent. solution of common salt.

How is this fact to be explained? I assume that the physiologic solution of common salt of 0.6 per cent. on account of its similarity with the composition of blood serum is highly irritating when presented in excess to the sensitive nervous tissues.

Water, on the other hand, is sufficiently different with respect to the lining tissues and its normal fluid to modify the nervous substance; it first produces irritation, and afterwards paralysis of the nerves and anesthesia. I surmised that between these two effects, the indifferent one of the 0.6 per cent. solution of common salt and of the distilled water (differing from the composition of the fluid of the lining tissues) there must be a territory within which a weaker solution of common salt would be able to produce anesthesia without the irritation to nervous tissues resulting from the use of both stronger and weaker solutions. This border land was found in a 0.2 per cent solution of common salt in distilled water. This is the very anesthetic fluid which I employ in my operations; the addition of 1 gram of cocain to 10,000 or 5,000 grams of this solution facilitates the practical application of the solution without entailing even a shadow of danger. One gram of this solution contains only 0.0001, that is a tenth of a milligram, of cocain. The maximum dose of cocain is not reached till 500 grams of the solution have been used. It must be remembered, also, that during the operation more than half of the fluid employed runs out again or is wiped off.

In minor operations I make use of a solution of 1-5000 grams, and for dressings recommend the employment of a solution from 1-1000 grams.

I advise two "stock" solutions: One of 1 gram of cocain to a 1000 grams of water (Solution A) and the second of 2 grams of common salt to a 1000 of water (Solution B). Parts of solution A are mixed for use with five or ten times as much of fluid B. Both fluids are easily sterilized.

The cause of anesthesia by infiltration is not a simple one; several factors are at work; first the pressure of the injected fluid and the removal of the blood from the infiltrated tissues. After a properly performed injection, they appear perfectly white. That is, instead of the tissue juices the foreign mix-

ture is incorporated in all the lymph vessels and the areolar spaces, while the blood is gradually forced into the neighboring vessels. But besides this pressure and anemia of the tissues, temperature plays a prominent part. The proof of this is very simple. All my solutions of common salt of from 1-1000 to 10,000 have the best effect at zero, Celsius.

Every edema of the skin would produce anesthesia of the parts, every swollen limb would become insensible, did not the injected fluid, which causes this pathologic edema, contain the same amount of common salt (0.6 per cent.) as the serum of the nervous fluid. If one produces an artificial edema with other fluids—with or without a little common salt—the whole region of the artificial swelling will become insensible and will enable one to operate without causing pain. This is briefly the principle of infiltration-anesthesia, which in practice has gained such a widespread application. This proceeding is, in the first place, beneficent to the patient, for whom the danger of the operation is diminished in the same degree as we are able to spare him the general narcosis.

The danger of an operation, in so far as loss of blood is concerned, scarcely exists, thanks to the advanced progress of our operative technique, for which we are indebted chiefly to our great masters of the pre-antiseptic era, and not the least to him whose immortal name has been given to this house. Lister has enabled us to make the dangers of infection depend upon our sense of duty and our carefulness; the danger of poisoning by the use of antiseptics we have banished by our efforts to reveal the laws of asepsis. What remains, then, to be done? To diminish the dangers of narcosis. What is wanted is sufficiently shown by the ever recurring recommendation of new and improved inhalation-anesthetics, such as ether, ethyl-bromid, penthal, etc. Among these, local anesthesia claims the first rank.

For operations of medium importance, in which I include simple uncomplicated laparotomy without extensive adhesions, amputations of the breast, and removal of large tumors from other situations, there can now be no question of the efficacy of infiltration-anesthesia. It will be the task of the future to render this matter of proceeding, in principle unassailable, practical for surgical purposes in still more serious and extensive operations. I have so far operated upon 521 patients painlessly and without a sign of danger. Among these I might mention such operations as nephrotomy, herniotomy, removal of sequestra, amputations of the mammae, removing glands from axilla, and laparotomies. It must never be forgotten that chloroform narcosis increases the danger of the operation. This danger is incalculable. Even the most circumstantial statistics can not tell us anything definite as to whether the danger be great or small for the individual. But how many dangerous cases of asphyxia of this or that kind, even of the deaths happening hours or days after the use of chloroform come to our knowledge, but are never in our statistics!

"General narcosis" is not the ideal method for the patient or for the physician; if I operate upon one part, why should I deprive the individual wholly of his consciousness and force him to give himself helplessly into my hands? especially if, as frequently happens, I am obliged to deprive him of consciousness not in the form of a quiet, delightful sleep, but too

often only after a struggle followed by anxiety about the eventual danger of the narcosis.

Of 537 operations, I have used chloroform in only 16 instances, and then in cases where an especial exigency demanded it as, for instance, insurmountable fear, hysteria, the personally expressed wish of the patient and failure of local anesthesia.

I now consider myself no longer justified in making use of general narcosis in my operations, unless the method of infiltration-anesthesia has been previously tried. To perform operations under chloroform or other narcosis, when they are certainly practicable with one or other form of local anesthesia, I must, from the standpoint of humanity, denounce as absolutely unjustifiable.

Königgrätzer Strasse, 47.

ARTIFICIAL HYPEREMIA IN THE TREATMENT OF DELAYED UNION.

Read before the Mahoning County Medical Society, April 9, 1894.

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In 1875, Nicoladoni¹ published an article upon Dumreicher's method of treating threatened pseudarthrosis. The underlying principle of the method was the production of hyperemia at the seat of fracture by means of compression above and below that point. The method demanded the use of graduated compresses, extra splints, etc., and was quite complicated. It does not seem to have come into general use, as Bruns² says he only knows of five cases where it was successfully employed. H. O. Thomas,³ of Liverpool, recommends a similar method, combining with it, percussion. He endeavors to produce severe irritation at the seat of fracture by vigorous percussion—sometimes in anesthesia. After this he seeks to retain the congestion by elastic constriction above and below the part percussed.

In a paper read before the German Surgical Society in 1887, Helferich⁴ proposed to employ artificial hyperemia, produced by elastic constriction, in the treatment not only of delayed union, but in that of bone cavities produced by operative procedures, and in cases of infantile paralysis, congenital dislocation of the hip, and other conditions in children where an increased growth in the length of the bone is desired. I have had no experience with this method in other cases than those of delayed union. The object of this paper is to once more bring this comparatively unknown but very efficient method before the profession, and to report my experience with it in a limited number of cases.

Helferich proves, by citing cases where an increased blood supply, caused by some pathologic condition produces a thickening of bone in adults, and a lengthening, or thickening or both in children, that hyperemia plays a prominent part in the formation of new bone. He gives warning, however, that it will not cause the production of callus but will only increase it when started by some other natural or artificial means.

To illustrate the technique of his method, Helferich takes a fracture of the humerus for an example. He first applies a flannel bandage from the fingers to a point about an inch and a half below the fracture; at the same distance (an inch and a half) above the fracture he applies a piece of rubber bandage or tubing tightly enough to retard the return of the

venous blood but not to interfere with the arterial circulation. This produces a marked congestion in the parts surrounding the fracture and left uncovered by the bandages. At first the constriction is only allowed to remain a short time, but the duration of its application is rapidly increased until it is soon left on day and night. I have found its almost continuous application for a period of ten days sufficient in the cases in which it has effected a cure.

During the use of this method, the fracture should be properly immobilized. In the case of the upper extremity, this is easily accomplished by the use of splints. In fractures of the leg, a plaster-of-paris dressing is employed and the constrictor applied at its upper margin. To admit of the swelling and congestion at the desired point the plaster dressing is applied quite snugly, the part where the congestion is desired being either heavily padded with cotton or large fenestræ being cut in the dressing. In fractures of the femur the Buck extension is used, the flannel bandage and constrictor being applied as in the case of the humerus.

This method may be employed alone in cases of delayed union, but is only of use in the treatment of pseudarthrosis subsequent to some operative procedure. The time of its application after an operation depends upon the condition of the wound. As a rule it should not be applied until union of the soft parts has taken place, still small granulating spots offer no contra-indication for its use.

This method should not be used where there is any tendency to a varicose condition of the veins. Helferich says it will not produce this condition in normal vessels.

Helferich reports eight cases "in which at the time a fracture is usually healed there was very little or no callus." Six were fractures of the leg and two of the thigh. By the use of this method alone he accomplished a cure in each case in a few weeks. He also reports four cases—three of the leg and one of the forearm—in which it was employed subsequent to the nailing together of the fragments. All resulted in cures.

Six cases which were treated by this method have come under my observation. Four of these occurred in Volkmann's clinic at Halle, Germany, during my connection with that institution, and two in my own practice. Of the four hospital cases three were of the humerus and one of the radius. The fractures of the humerus were all healed, that of the radius was not benefited. One of the cases of fracture of the humerus was of particular interest in showing the utility of the method. The patient, a man about 40 years of age, had two years before sustained a compound fracture of the humerus which resulted in pseudarthrosis. Six months subsequently the ends of the fragments were resected and the bones wired together. His condition was not improved and in another six months the operation was repeated. He derived no benefit from the second operation, and at the end of a year returned to the hospital where, for the third time, the resection and wiring was done. As soon as the incision had healed the elastic constriction, as described here, was applied. In ten days a marked callus could be felt and a few weeks later he was discharged with the fracture firmly united.

A subsequent operation upon the case of fracture of the radius, mentioned above, revealed the fact