

With retroversion there must *always* be some prolapsus and congestion. The congestion is a result of the displacement and will only disappear with the exciting cause. The congestion causes increase in weight, and this increase in weight causes a tendency to drag the whole structure downwards. If a pessary be now applied, there must be more or less pressure upon the bow from the very nature of things. The ligaments are relaxed and will not hold the uterus in place, handicapped, as it is, by an abnormal weight. The pessary has been measured properly, with such expansion of the rami that the rectum may not be disturbed; what will result? The super-sensitive uterus is pressing hard against an unyielding body, which body itself, thus encroached upon, is crowded into the vaginal walls. This condition, the general one, and not regarded as especially significant of danger by the majority of practitioners, may become alarmingly aggravated by distension of the rectum or bladder, by sudden physical or mental shocks, or by the changes engendered by menstruation. These conditions, in whole or in part, may be beyond the control of the woman, and are liable to occur to anyone and at any time. No amount of present mechanical ingenuity can meet the possibility of a future unfavorable contingency. Neither can any man predict what accident of environment will fall to the lot of his patient. Moreover, no physician can satisfy himself thoroughly of the sensitiveness of any uterus, save only after the lapse of weeks. If it be urged that the pessary should not be applied until after the congestion has disappeared, the answer naturally occurs, that such a waiting would be continuous with the life of the patient, and would terminate only at her death. The congestion depends upon the dislocation: the dislocation depends upon a relaxation of the ligaments. Upon what does the weakened condition of the ligaments depend?

Just here is where all that I have written so haltingly and with such a sense of my own shortcomings, culminates. Go to the foundation; let the uterus alone for a time. Build the woman up, mentally and physically. Then reduce uterine congestion by medicated tampons; restore it by the postural treatment. Keep it in place, if necessary, by tampons. The primary steps in the treatment being fulfilled, in the hands of a thoroughly scientific physician, a perfectly fitting pessary may be worn, but only after other indications have been met. For myself I cannot look upon them as desirable inventions, and I never make use of them. Anterior and posterior ulceration of the vaginal walls may result from the long use of a pessary. Infectious muco-purulent products may be absorbed by the lymphatics or veins. Dr. Kelly (*Philadelphia Medical News*, October 18, 1884,) has recently reported a death caused by a pessary; others of similar nature are on record. I maintain that while these sporadic cases may now be regarded as curiosities of medical literature, yet they are dangers that *may* occur in the practice of even the most intelligent physician. No instrument can claim to be a scientific one which shall entail any possible danger in its use.

Conservative gynecology does not necessarily

mean that surgical interference is interdicted, but it does mean that only such treatment shall be made use of as shall be the one which shall be the best calculated to foster the vital energy of the patient, and shall restore to the nearest possible normal, the abnormality.

THE ETIOLOGY OF TUBERCULOSIS.

BY H. F. FORMAD, M.D.

OF PHILADELPHIA, PA.

Presented in Section on Practice of Medicine and Materia Medica of American Medical Association, May 8, 1884.

Dr. Formad opened the discussion on this subject, but refrained from presenting it for publication, as a similar paper, by himself, and giving his views in full, had just been published in *THE JOURNAL OF THE AMERICAN MEDICAL ASSOCIATION*, April 26, 1884.

He said, in the remarks which he read before the section, that he was not at liberty to present any additional matters upon the subject, being about to publish, in conjunction with Professor William Pepper, Provost of the University of Pennsylvania, a monograph on tuberculosis, in which the whole of his research and experiments, upon which his views were based, will be related with full details.

Dr. Formad's paper elicited the following

DISCUSSION.

DR. AUSTIN FLINT, SR., thought that if tuberculosis was an infectious disease, produced by a parasite, its contagiousness would follow, as a matter of course. The presence of a parasite had been demonstrated. The great preponderance of evidence is in favor of the view that it is uniformly present in products recognized to be tuberculous. The bacillus is not found in products which from other characters are considered to be not tuberculous. The conclusion which he had reached was that there was some essential connection between the presence of this parasite and the tuberculous disease. In regard to the contagiousness and inoculability of tuberculosis, he thought that all testimony was in favor of this. Other causes may cooperate, and undoubtedly do cooperate in the product of tubercle, but the presence of the specific parasite is essential.

DR. WILLIAM H. WELCH agreed with Dr. Flint. Dr. Formad had spoken of tuberculosis produced by inoculation of other substances than the bacillus. If bacilli be the sole cause, the condition can, of course, be produced in no other way. Koch, in his experiments, had taken every precaution to prevent error. He had, with better methods of investigation, found the bacillus in every case. When the difficulties of the process required for the detection of the bacillus is considered, it is not at all wonderful that at times it should not be discovered. The essential point of Dr. Formad's objection is whether or not tuberculosis can be produced by other causes than the bacillus. The weight of evidence is in favor of the view that tuberculosis can be produced by the inoculation of no other substance than the bacillus tuberculosis.

DR. GEORGE M. STERNBERG, U. S. A., had not been

able to reach any definite conclusion in this matter. He had performed some experiments, but they had not been conclusive. He thought that the suggestion of Dr. Formad, that the bacillus might act as a local irritant, was worthy of consideration. He suggested that possibly the reason why tuberculosis had appeared in animals which had been inoculated with other matters than the bacillus, might be that the animals had developed the condition from being kept in unsuitable quarters.

DR. R. S. FITZ, Massachusetts, thought that the pith of Dr. Formad's remarks lay in the statement that he had produced tuberculosis without the bacillus. It may be that he considered that tubercles were not really such.

DR. JAMES TYSON, Philadelphia, had hoped that Dr. Formad would have produced some carefully detailed experiments. When Koch's theory was first advanced, he thought it would share the fate of previous theories of this kind. Instead of that it had been constantly gaining strength. The only way that the matter can be settled is by a repetition of the experiments to decide whether Koch or whether Formad is right. One point in regard to the contagiousness of tuberculosis and to its infectiousness. Dr. Flint thought that one would follow from the other. Tuberculosis may be infectious, and yet not be contagious. Malarial fever is an infectious disease that is due to a specific cause, but it is not contagious—that is, communicable by contact. It may be proven that tuberculosis is infectious before it is shown that it is contagious.

DR. JANEWAY related a number of cases which he thought went to show tuberculosis was contagious.

DR. C. DENISON, of Denver, Col., said there were three considerations which had not been mentioned by any of the speakers, but which seemed to him to have an important bearing on the subject under discussion. The points he would make were favorable to a definite etiological relation between the bacillus of tubercle and tubercular lesions, because they in part supply the conditions essential to such relations:

First—The argument based upon the analogy between tuberculosis and croupous pneumonia as an infectious disease. The speaker called attention to the intimate relation both of these diseases have to "malaria," so called, the sections of the United States which are most malarious being fruitful in both acute tuberculosis and croupous pneumonia. The idea that croupous pneumonia is infectious, is much strengthened by studying the geographical distribution of that disease. The speaker had noticed, during eleven years residence in Colorado, the remarkable infrequency of true croupous pneumonia there, as compared with his previous experience in Connecticut. He believed that if mistakes could be corrected in the death statistics in regard to pneumonia in the 1870 census, and catarrhal, broncho- and pleuro-pneumonia—which are the forms which prevail in Colorado—could be eliminated from the statistics, the result would be that croupous pneumonia would be consigned almost wholly to malarious districts, and in some proportion to malarious element, festered as it is by *warmth and soil moisture*. In

support of this point, the speaker referred to the percentage of deaths from pneumonia to all deaths as given in the U. S. census of 1870. Of course all other forms besides the croupous variety were included, yet the twelve highest percentages in order by States were as follows: Arkansas, Texas, Alabama, Kansas, Mississippi, Florida, Georgia, Missouri, South Carolina, Kentucky, Virginia, Tennessee, and these ranged twelve (12) per cent., while the twelve lowest percentages in order from least to greatest were Montana, Idaho, Washington, Wyoming, Oregon, Colorado, New Mexico, Minnesota, Wisconsin, California, Michigan and Vermont, with an average of $4\frac{1}{2}$ per cent. of deaths due to this disease.

2. The argument, based upon climatic conditions, which favor germination, *i. e.*, the chief characteristics of a given climate which is likely to produce tuberculosis as compared with those of a climate giving approximate immunity from the disease. The consideration of the geographical distribution of phthisis, in our own country, leads to the conclusion that the humidity and temperature of the atmosphere are very closely associated with the greatest prevalence of consumption. Assuming that the statement is admitted, without presenting many possible proofs, that *warmth* and *moisture* are most important conditions for germ development, both within and without the human body; in fact, that these attributes aid in furnishing a favorable soil for the lodgment and growth of the bacilli of tubercle; assuming these things as known, the additional fact that the opposite conditions, *cold* and *dryness*, predominate as immunity is secured, is mentioned as a most important announcement which has been lost sight of in the histological discussion of tubercle. The speaker presented humidity charts of his own device, graphically illustrating by seasons the dry and moist halves of the United States. The moisture of the atmosphere was shown in four shades of blue, and the dryness by four shades of red colors, each shade representing an equal division of the whole climate, and being made up of the combined cloudiness and the relation and absolute humidity of the air as shown by all the signal service records for the year.¹ It was thus plainly seen that the greatest humidities were in localities admitted to be fruitful in tubercular disease, and the least humidities in localities known to give the greatest immunity from such diseases. Elevation coming in, in harmony with this general conclusion to increase that immunity. Thus warmth and moisture, the friends, are put in striking contrast to cold and dryness, the foes, to germination.

3. The opponents of the bacillus, as the germ cause of phthisis, have assumed unwarranted liberties in their negative judgment, since they have taken no account whatever of the *eliminating power natural to every living thing*. The speaker had never heard one of these opponents admit that that, which, under more favorable conditions, might have proved infectious, had been eliminated by a system in a vigorous state of health, a condition antagonistic to the de-

¹ These charts are preliminary drafts of a seasonal climatic map which, with an annual climatic map of the United States, are being prepared by Messrs. Rand, McNally & Co., of Chicago.

velopment of the bacillus of tubercle. Yet there is reason to believe that this is a frequent occurrence in the lives of many of us, especially those physicians who are often brought in contact with active phthisis. How better can we account for the fact, mentioned by Dr. Sternberg in his remarks, that certain guinea pigs, when allowed plenty of liberty in a grassy field, kept fat after tubercular inoculation, and finally when killed the bacillus was found in all of them? Is it not reasonable to account for this result different from that usually obtained, where animals are confined in close quarters, by the counteracting influence of the power elimination due to healthful living?

There seems to be a law in nature, that the lower the forms of life, so much more ample are the possibilities, both of multiplication and destruction, of these forms. Pasteur is said to claim that under the most favorable conditions possible, one parent bacillus anthracis can multiply to three millions in twenty-four hours. Certainly a startling statement, yet, if true, of much importance as indicating the great destruction of germs in their infancy, or the unsuitableness of the soil they fall upon, in order to stem such prolific increase. If any such reasoning holds good with the bacillus tuberculosis, it is not hard to conceive that those conditions of health, which are the heritage of the normal man, are the great destroyers of the bacillus. Indeed, the further we investigate this subject the more there seems to have been ample chance for infection, wherever the bacillus of tubercle is found. There is a lack of conclusiveness or force to any argument against the supposed etiological relation of these bacilli to tubercle or phthisis, which takes no account of this possible destruction,—the eliminating or counteracting power of healthful life processes. On the other hand, the cleansing and health-restoring influence of fistulæ, issues and counter-irritations; of hydrocarbons, nervines, alteratives, antiseptics, etc., and of complete respirations in a cool, dry, rarified atmosphere, favoring an increased exhalation of moisture over that inhaled, and constant use of the contaminated lung; in fact, *all practices* that we know to be remedial to phthisis, point to a positive and progressive character of disease, the etiology of which has no better or more consistent explanation than that afforded by the bacillus of tubercle.

DR. W. T. BELFIELD, Illinois, could not agree with the theory that tuberculosis was due to bacteria. He believed that it was not due to bacteria, but to internal causes, or to conditions in which the patient was placed.

DR. E. O. SHAKESPEARE, of Philadelphia, after an extended review of the matter, concluded that tuberculosis was often an infectious disease; that often it was due to a specific, pathogenic, parasitic agent, and that so far it has not been demonstrated that anything besides the tubercle bacillus can produce the disease.

DR. G. E. SMYTH, of Green Castle, Ind., remarked that the discussion of all scientific subjects should be strictly non-partisan in character. In order that the physician or scientist may be impartial in investigat-

ing or discussing a subject like the one under consideration at present, it is absolutely necessary that all preconceived opinions should be discarded. He who has formed or expressed an opinion is disqualified from serving upon a jury at law. The theorist in medicine, who has preconceived opinions, is not a safe investigator. The day of theories in our profession is past. Facts are what we are in search of now. One fact established by indisputable evidence is worth a thousand theories however plausible they may be made to appear.

Koch's discovery of the bacillus tuberculosis has, in my judgment, been subjected to very unfriendly and, I might say, unjust criticism; not so much by the author of the paper under discussion at present as by others.

There seems to be much confusion existing in the minds of the profession in regard to the exact meaning of the terms contagion, infection, etc., the terms being frequently used as synonyms. All contagious diseases are infectious, but all infectious diseases are not contagious. Infectious diseases should be divided into three or more classes or divisions, based upon the manner in which the poison is conveyed from one patient to another, its absorption into the system, together with its subsequent behavior.

The first class under this method of dividing these diseases should include the purely contagious diseases—or those during the course of which the poison is sufficiently matured to be conveyed directly to an uninfected party who is susceptible to its action—of which small-pox, measles, scarlet fever, typhus fever, are good examples.

The second class, which is of equal if not greater importance, is not strictly contagious, because the infecting material has to undergo a period of development or growth after it escapes from the body of the patient, before others can be infected by it. To this division belong such diseases as typhoid fever, epidemic dysentery, Asiatic cholera, etc.

There is still another division which is purely miasmatic in character and although strictly infectious, cannot be communicated in the same manner as the diseases mentioned in either of the other divisions. In this division will be found such diseases as malarial fevers, croupous pneumonia, acute articular rheumatism, cerebro-spinal meningitis, etc. Perhaps other divisions or classifications will be added to these, as our knowledge of the subject is extended, and one may be found for tuberculosis and other chronic infectious diseases.

Let us assume now that all the infectious diseases, both acute and chronic, are caused by the introduction into the system of low but living micro-organisms. Whether they constitute the real contagion or are simply carriers of something still more subtle which has heretofore escaped our attention, is not germane to the argument.

It is necessary that this contagious material be brought into contact with a fruitful soil. This simply means that the person who receives it possesses what is known as a predisposition either inherited or acquired, and for which we may at some future time

be able to give an anatomical explanation as we can now do with many surgical diseases.

Clinical observations have established the fact beyond the possibility of a doubt that the predisposition, or what is the same thing, the susceptibility to the action of the poison of certain infections, is very great in some families and almost entirely absent in others.

This fact has been demonstrated frequently in large tenement houses, especially in New York city, where whole families have been destroyed by such diseases as diphtheria, scarlet fever, etc., while other families, occupying the adjoining apartments upon the same floor and equally exposed, have escaped without a case.

The predisposition to croupous pneumonia is strong in some families, and almost, if not entirely, absent in others. Some families are exempt from this disease while in others it appears in some member in every generation. The same is true in regard to articular rheumatism. There are doubtless present many practitioners who have had these diseases appear in certain families through several generations, while other families under their care have escaped them entirely.

It is not every person that can be infected with the poison of any given disease. Many persons will not have small-pox or measles. So it is with tuberculosis. Many persons cannot be infected with the disease, at least in the ordinary way in which infectious diseases are communicated. If consumption is infectious, it perhaps should be classified with the local infectious diseases, such as syphilis, leprosy, etc., the system at large becoming infected subsequently from the primary local lesion, thus accounting for many recoveries in patients where the predisposition is slight and the power of resistance being great, enabling them to throw off the disease.

It is admitted by the author of the paper under discussion that the bacillus tuberculosis is nearly always present in or during the tuberculous process, that it is diagnostic of the process to a very great degree, and he also admits that they are sometimes the cause of the disease by being lodged in the lungs, and by their presence exciting the tubercular process. These are dangerous admissions for the author to make. It is not claimed by the most earnest and ardent supporters of the germ theory of disease, that the mere presence of the germ is sufficient to account for the phenomena of infectious fevers. It is the propagation and growth of the germ, with the consequent oxidation and destruction of the tissues, which gives rise to the subsequent phenomena. If the bacillus tuberculosis is ever the cause of consumption, may not subsequent investigations prove that it is always so? This is a question of such vast importance that time will not admit of its discussion.

Clinical observations seem to go for naught with the author of this paper. This virtually takes the case out of the hands of the greyheaded veterans of the profession, no matter how careful and painstaking they have been in their observations. It turns the entire case over into the hands of the specialist on microscopy. I have always attached great im-

portance to the natural history and the clinical appearances of all diseases. Valuable information is certainly gained in this way. I was convinced early in my professional life that there was a probability that tuberculosis was an infectious disease by the manifestations of the disease in several instances, one or two of which I will relate.

Two brothers, members of a tuberculous family, and whom I shall designate as A B and C D, aged respectively 26 and 29, painters and glaziers by occupation. A B contracted consumption in 1865, and died after an illness of eighteen months. He was nursed by the wife of C D, who (the wife) was a member of a family consisting of thirteen children, all of which are still living. No case of tuberculosis has ever occurred in any branch of this family for the last three generations. This includes uncles, aunts and cousins. The wife of C D, who nursed and cared for A B, contracted the disease, and died in about ten months, while C D, the husband who is strongly predisposed to the disease, escaped and is still living.

W. B., aged 22 years, and whose occupation, previous to enlistment in the army, was that of a farmer, returned from the army at the close of the war of the rebellion, with well-marked symptoms of consumption, which terminated his life in a few months. He was a member of a family consisting of father and mother and eleven children (seven girls and four boys). The paternal and maternal grandparents had been pioneers in the early settlement of the county, and both branches of the family were entirely free from tubercular affections. I investigated the history of the family carefully myself, and am personally acquainted with it for three generations back. They were farmers in good circumstances, well fed and clothed, and comfortably housed. Two of the sisters of this patient were his constant attendants.

They both contracted the disease and perished from its effects, and in turn were nursed by other members of the family who contracted the disease and also died. This process was continued until seven of the children were slain by the disease, and finally the father, aged nearly seventy years, took the disease and died, the only members of the family who escaped being the mother, who was an invalid and confined with chronic rheumatism in a distant part of the house and unable to render any assistance in nursing, one son and one daughter who were married and living several miles distant, and one younger son who was too young to be of service as a nurse, escaped and are still living.

Many other cases have fallen under my notice similar to these, and perhaps there is not a practitioner within the sound of my voice but who has had a similar experience. These are stubborn facts, they are seen every day by the general practitioner, and, unlike the bacillus, they don't require the aid of the microscope. If physicians would report these cases, so that they could be tabulated, an array of clinical facts could be produced which would be valuable if not convincing.

The world owes a debt of gratitude to Koch for his discoveries in this field of observation. So are we indebted to his critics. It is by continual agitation

investigation and discussion that we finally arrive at the truth.

No great discovery or improvement has ever been accepted unquestioned by the medical profession. It was twenty-five years after Harvey demonstrated the circulation of the blood before it was accepted by the profession at large, and it is recorded in history as a fact that no member of the profession who was then over 40 years old ever gave in his adherence to the doctrine. The same is true in regard to Jenner's discovery of vaccination.

In 1809, when Ephraim McDowell, of Kentucky, demonstrated the practicability of ovariectomy, he was assailed by every prominent surgeon then living, and the operation was compelled to fight for a full half century, and finally established itself as a legitimate operation, but over the dead body of every prominent surgeon who was practicing at the time it was first proposed.

DR. H. C. ERNST, Massachusetts, agreed with Drs. Welch and Fitz, and then went on to describe the advantages and disadvantages of the different staining processes which had been recommended, and then called attention to the fact that in performing these investigations nothing less than an immersion lens, with some sort of sub-stage illuminating apparatus, would give reliable results.

DR. WILLIAM PEPPER, Philadelphia, had observed similar cases to those described by Dr. Janeway, but his observations had led him to a directly opposite conclusion. He did not believe in the contagiousness of tuberculosis. In the experiments which Dr. Formad had performed, great care was taken to place the animals under the best hygienic surroundings; some were sent to the country, and all were given plenty of fresh air. He thought that there were many other ways of explaining the connection of the bacillus and tuberculosis than the assumption that the bacillus held a casual relation to tuberculosis.

DR. TRAILL GREEN, of Easton, Pa., did not believe that tuberculosis was a contagious disease, and referred to the fact that working in grindstone factories would soon induce tuberculosis in perfectly healthy individuals.

In closing the discussion, DR. FORMAD said that he was highly gratified, and felt thankful to the section that so much time had been allowed to a debate which had been so interesting and instructive to him. He did not see fit to comment upon the remarks of the many prominent gentlemen who participated in the discussion; yet he felt somewhat disappointed, for he had hoped that more stress would have been laid upon facts than on views, and that some accounts of active personal work would have been elicited. He thought that Koch had not made out his case fully, and that it brought others into misconception to rely upon his statements in regard to the etiological relations of his bacillus, and that it would be well to await further developments in a question of such grave importance, instead of jumping with Koch to hasty conclusions. To the merits of Koch's discovery in regard to the diagnostic value of his bacillus in tuberculosis, Dr. Formad did full justice.

A CASE OF CALCULUS OF THE URETHRA.

BY I. H. DUNNING, M.D.,

OF SOUTH BEND, IND.

J. S., a farmer, consulted me on August 4, 1884, for the removal of a stone from the urethra. He stated that he was 62 years old, and that for a number of years he had suffered intensely at times in consequence of gravel; that he had at times passed small gravel, but had no suspicion of the presence of a stone in the bladder. Some time in January, 1884, a stone, which he thought to be as large as a hickory nut, had entered the urethra from the bladder, and passed down and lodged in the penis, just behind the glans. At this point it remained and gradually increased in size until he presented himself for treatment.

The penis then measured seven and one-half inches at its point of greatest circumference, which was three and one-half inches posterior to the meatus. The whole penis was much enlarged and of a dark livid hue. Several openings, readily admitting the passage of a small probe, were found leading from without inwards into the urethra posterior to the stone. The external orifices of these false passages were slightly excavated, and presented an almost gangrenous appearance, and from them the urine was oozing constantly. The patient stated that two or three weeks previous to this time he had noticed that but little urine escaped from the orifice of the urethra, and thinking that the stone had plugged the passage, he one day made a hole with a gimlet through the center of the stone parallel with the urethra; but he found that even then the urine could pass through the orifice only drop by drop. He tried to remove the whole of the calculus, but failed.

On August 7, assisted by Drs. Hitchcock and Gist, of this city, the stone was quickly removed. An incision one and one-half inches long was made in the urethra, from the orifice backward. The stone was crushed and removed in fragments. A pouch was found, in which the stone had rested, of sufficient size to hold fully one ounce of water. The weight of the stone was afterward found to be 300 grains. The walls of this pouch were dense and hard. An unsuccessful attempt was made to pass a small bougie from the bottom of this pouch into the urethra. There was doubtless a communication between the pouch and the urethra, but it was so small or so well concealed that it could not be found. With a view of making a free passage for the urine, I cut down upon and into the urethra just in front of the scrotum, and passed a grooved director forward along the urethra until the pouch was reached; then along the groove of the director a sharp-pointed probe was passed through the posterior wall of the pouch, the end of the director being made to follow it, after which the opening was enlarged with a probe-pointed bistoury. Having no suitable silver tube with me, a portion of a soft rubber catheter was passed through the pouch into the urethra, beyond the posterior extremity of the incision, and made fast. An effort was made to pass a small bougie into the bladder, but it was unsuccessful, as it was impossible to carry the bougie