of old; Drs. Keith and Bantock and Mr. Tait will have none of it. The latter said to me, "I sold out all my right, title, and interest in Listerism, with my tea-kettle to Battey."

So far as I know, the best statistics yet obtained in ovariectomy in the United States belong to Dr. Battey, of Georgia, and Dr. John Homans, of Boston, Mass., both of whom operate under the carbolic spray, and in apartments kept especially for abdominal operations. I make special mention of the fact that these gentlemen use the carbolic spray, for the reason that Dr. Emmett says in his last edition, page 715, "I do not know of any prominent operator in this country who now uses the spray"—evidently an oversight.

I do not use the spray myself, but look upon the entire Lister system, less the spray, as firmly grounded in the surgical mind. Cleanliness and Listerism can never be separated, for "Listerism is the gospel of cleanliness"; without the latter you cannot have the former.

The year has wrapped up in its eternal folds one whose name is synonymous with the surgery of women; one whose reputation is immortal, who in America at least, stood next to McDowell; beloved by his own countrymen, and honored by the entire surgical world. No eulogy of mine can increase his fame. I speak of *the great, the good, the pure, the noble, the generous* Marion Sims. Like McDowell, he possessed a genius for origination, and will share with him the admiration and plaudits of future generations.

ORIGINAL ARTICLES.

CHOLERA.

Read in the Section of Practical Medicine, Materia Medica and Physiology, at the Annual Meeting of the American Medical Association, 1885.

BY JOHN H. HOLLISTER, M.D.,

OF CHICAGO.

The fact that this disease so prominently commands the public attention everywhere, renders its discussion at the present hour peculiarly appropriate. The probability, according to precedent, that it may again traverse the western continent invests it with a special interest, and by every motive that may appeal to us we are called upon as members of the medical profession to counsel the most efficient means for preventing its progress and controlling its severity.

In the discussion of this subject, especially upon controverted points, I shall not presume to speak as an authority, my aim being, rather, so to present the essential points as to elicit free and full discussion on the part of the able representatives of the profession here convened.

The topics germane to this discussion, it seems to me, are mainly these: *Firstly.* As to the origin of cholera; *Secondly.* Its pathology; *Thirdly.* Its manner of propagation; *Fourthly.* The means for its control; *Fifthly.* Its treatment.

To these several subjects, with the brevity which my paper compels, let me invite your kind attention. Over the origin of cholera and its dissemination—after a hundred years of rigid investigation, in which time tomes of literature upon the subject have appeared, there still hangs a mystery which invests it with fearful interest. And since, in the footsteps of the epidemic invasions, such appalling fatality has followed, compared with which the wastes of wars are insignificant, it were strange indeed if the laity did not turn with deepest concern to the medical profession, imploring its utmost effort to stay the progress of the destroyer, or, failing in this, to lessen its fatality.

To the proper presentation of the subject a brief historical summary seems essential:

Evidently cholera is not a disease of recent origin. It has been prevalent, doubtless, upon the banks of the Ganges, for untold ages. Nor there alone. It was accurately described by Hippocrates. It is a matter of mention by Aretius of Cappadocia in his early writings. As early as 1669 Sydenham speaks of its prevalence in London. Huxham also refers to it there in 1741. According to French authorities, it was prevalent in Paris in 1773, and again in 1780. Lebeque describes cholera as breaking out with violence in Hindostan in 1762, and states that 800 Europeans and 30,000 natives perished in a brief period of time. Dr. Paisley, of the East India service, writing from Madras in 1774, states that cholera was then not only endemic in India, but that with peculiar meteorological conditions, to which I shall have occasion to refer, it was epidemic among the natives.

M. Sonnerat, travelling in India between the years 1774 and 1781, states that cholera was not only prevalent upon the Coromandel coast, but that at one time it assumed not only a malignant, but an epidemic form. Time will permit me but in briefest manner to refer to its appearance among the British troops in Bengal, 1781 and 1782, and in the same year in the squadron of Sir Edward Hughes at Madras.

April, 1783, is memorable for its outbreak in Hurdwar, upon the Ganges, where, in eight days, 20,000 pilgrims perished. From 1787 to 1790 inclusive, we have the record of its prevalence in Arcot and Velore, and among the Bengal troops at Seringapatam. Bell, who is quoted as an authority upon the subject, is of the opinion that the pestilences which, from time to time, prevailed in India, and which were attended by an almost incredible fatality, were but successive outbreaks of cholera, of which but meagre reports were preserved by native historians.

Thus, while it is constantly asserted that epidemic cholera originated at Jessora, a large and populous town sixty-two miles north of Calcutta, in August, 1817, and from thence swept over western Europe as the first great epidemic, yet, from the records referred to and from others equally reliable, it is evident that cholera had

been prevalent in India from a period antedating historic records, and doubtless successive plagues, of which we have only imperfect account, were but successive outbreaks of this disease—sometimes counting their victims by millions. The often repeated statement that the first great epidemic, as it is called, originated at Jessora, needs a moment's consideration. While it is asserted that it originated in Jessora August 28, 1817, it is certain that it was prevailing, even before this date, in various and distant parts of Bengal. In July it had appeared in Sunergong. In July it it was even prevailing as an epidemic in the distant provinces of Behar and Dacca. On the 11th of July it appeared in the city of Patana, three hundred miles northwest of Calcutta, spreading from there during the month of August to all the adjacent villages. On the 23d of August, five days before the invasion of Jessora, it was raging upon the further shores of the Bay of Bengal, and only a week later upon the high and distant tracts of Monghyr. I have trespassed a moment upon your time to show that the assertion of its origin at Jessora needs correction.

In the briefest manner possible, let me outline the march of this first epidemic, as it is termed, in its progress westward. After its fearful ravages in 1817, it seemed, during January, 1818, to be nearly extinct; but in February of that year, it again developed with great violence, spreading eastward over Aracan. In 1819 it devastated Maritius and in 1820 had reached the Isle of Bourbon. Of its progress south along the Indian Ocean, I have not time to speak. In its western course it had reached Bombay in 1818. During the two succeeding years it entered Muscat. Ascending the Persian Gulf, it visited the seaport towns on either side, and from thence spread over Persia and Asiatic Turkey. Its ravages in Persia were arrested by the cold winter following 1821. In 1822 and 1823 it skirted the Caspian Sea to the mouth of the Volga, where its progress was again arrested by the rigorous winter of 1823. All Europe trembled with apprehension, but at this point for six years its western march was stayed. Here and there it appeared in Persia during these years, as it had done in India, but up to 1829 it had not crossed the Bosphorus. But in 1829, it began to appear in the eastern provinces of Russia. In 1831 it had reached the Baltic, and on June 15th of that year it first appeared at St. Petersburg. In August it had reached Berlin and in September it entered Vienna. On the 11th of September it was at Hamburg, and in October, at Sunderland on the east shore, it first made its appearance in England.

In June, 1832, it crossed the Atlantic, appearing first, as is commonly asserted, in Quebec, and was soon prevalent along the line of travel. Such had been the tide of immigration that more than thirty thousand persons had landed in the Canadian provinces during the preceding three months, many impoverished and badly cared

for, and among these cholera assumed extreme malignity. The disease spread rapidly along the upper lakes. It appeared with great fatality among the United States troops in command of General Scott in transit to the seat of the Indian war. With them it reached Chicago, and westward over the prairies it seemed attendant upon the troops and all who had communication with them, until it reached Fort Armstrong, at Rock Island. Thence descending the Mississippi, it visited all the river towns until it reached the Gulf. Crossing the St. Lawrence in the meantime it overran the state of New York, and during the summer of 1832, nearly every state in the Union suffered from its invasion.

Of the epidemics that reached this country in 1848, in 1854, in 1865, and in 1873 it may be said that their manner and methods of advance were so markedly similar that the general history of one invasion would outline them all. From a careful survey of these histories it seems safe to make the following deductions:

I. In America, cholera is not an endemic disease.

II. That it is only communicated from foreign countries, where and when it is there epidemic.

III. That it is not transported by ocean or atmospheric currents.

IV. That it may be in every instance, traced to infected ships arriving from infected countries, and bearing passengers developing the disease, or materials that had been previously in contact with cholera patients.

V. That it has never spread inland from infected ports, except along the lines of travel, and in such manner as that it might be communicated by travelers—or the transfer of contaminated materials. It is not known, I think, to have appeared in any locality where absolute isolation had been secured. If this be true, by so far as the avenues of travel and traffic may be closed, by so far we have it in our power to arrest the progress of the disease.

No questions command the attention of the medical profession in both hemispheres at the present time as do those of the causation of cholera. In the solution of these questions, the comma-bacillus is at this moment the pivotal point of controversy.

Koch and his followers are confident that this specific bacillus is the producing cause. He affirms that in the 100 cases of autopsy from the examinations of which he derives his conclusions, in every case he found these bacilli present in the intestines and that he did not find them in any other organs than those diseased; that they were not present in discharges from healthy persons, nor from persons affected by other diseases than cholera.

He reasons also, from the fact that by the inoculation of healthy organisms with specific bacilli a specific disease may be produced, as in the instance of the bacillus tuberculosis, that

therefore it is rational to conclude that cholera is developed in like manner.

In furtherance of his views he affirms that in the beginnings of the disease the comma bacilli are few in number; that as it advances the colorless and odorless evacuations contain enormous quantities of comma bacilli, while other microgerms are notably diminished, and that as the excreta become more feculent they diminish in numbers, and that finally, with returning health they disappear. On the other hand, the French commission, of which M. Straus was chairman, at the conclusion of its investigations in Egypt reported that while finding these microgerms in the cases examined, in some of them so few were present as to lead the commission to the conclusion that cholera was not attributable to the presence of these microbes even in cases in which they were most abundant.

Again, Dr. T. Lewis, who has been for many years a resident of Calcutta, and who has made the evacuations of cholera patients for years an especial study, asserts that the so-called comma bacillus is identical in form with one which may be found in the mouth of a healthy person. This affirmation the followers of Koch are inclined to deny. The crucial test of inoculation with comma bacilli, eliminated from all possible associate infection, and the production of cholera by such inoculation must be fully demonstrated before the seemingly too confident assertions of Koch can be accepted as verities. Far be it from me to speak disparagingly of these investigations. As never before the advocates and the opponents of the germ theory of disease are marshalled in conflict. The smoke of the battle is yet too dense for us to determine where final victory shall rest, but one thing is certain. From such prolonged and skilful investigations by such able observers we shall date a new era in the advancement of medical science.

Whether or not we reach the conclusion that the comma bacillus is the producing cause of cholera, it is evident that there are conditions which favor the development of the disease, without which its propagation seems impossible. The essentials to such development seem to be *first*, warmth; *second*, atmospheric moisture; and *third*, the presence of organic substances in process of decomposition.

So far as I know the disease has rapidly diminished and even disappeared from localities where the temperature fell, and remained below 32° F. It has been the common observation that a warm and moist atmosphere was present when it attained its greatest severity, and that according to the excess of earth and water pollution in that proportion and in such localities it has attained extreme malignity.

The reports of surgeons in the India service are explicit upon this point. From the Bengal Reports I quote from a review the following extract: A disorder possessing the principal characters of cholera appears to have prevailed more or less

endemically during the hot and rainy seasons of every successive year, in the lower provinces of Hindoostan, but chiefly limited in attacks to those whose constitutions had been debilitated by poor, ungenerous diet and by hard labor in the sun, and who were badly clothed, and frequently exposed in low and foul situations to the cold damp air of the night. It rarely occurs during the dry months, Europeans are scarcely ever affected by it, and the better classes of natives are rarely subject to its influence. A high temperature, a moist atmosphere, and accumulated filth seem essential to its development, but once, under these favoring influences, it assumes an epidemic form, such is its malignancy that it is thenceforth no respecter of persons, carrying devastation wherever it may chance to go. As in a conflagration, its beginnings are the matter of chief concern, and the removal of favoring agents for its production before the deadly work commences. We should be concerned for the prevention of cholera, even more than for its cure.

It seems presumptuous, in passing to this next topic, that I should discuss the pathology of cholera in the presence of this learned assemblage, since you yourselves are conversant with all the facts so far as they are yet revealed. And yet, the consideration of other points renders it necessary that we briefly refer to the more manifest abnormalities attendant upon this disease.

We find the intestinal canal the seat of special lesion, analogous in appearance to that of intestinal catarrh. Where death has occurred in the cyanotic stage the mucous membrane is thickened and oedematous, the intestines distended with fluid, turbid by reason of the countless myriads of micro-organisms developed in the decomposing fluids. Necrosis of tissue is general, and the villi of the intestines are largely divested of epithelium.

The mucous, submucous, and connective tissues are impacted with lymph cells. The blood having largely lost its serum, fails of free circulation, and capillary stasis everywhere obtains. Its corpuscles become viscous and adherent, as we would anticipate. One prominent result is the failure of the proper function of the kidneys. Urea is unduly retained. Its toxic effects upon the nervous centres are manifest. Its conversion into ammonia renders the blood strongly alkaline. In cases not immediately fatal we may have resultant pathological conditions in great variety, involving mucous and serous membranes and glandular organs as well, in acute inflammations with their varied sequences. From the inspection of those who died in the algid cyanotic stage, it may be difficult to discover other lesions than those of the intestines; but in protracted cases it is difficult to say what organs may not be affected, or to what extent they may be involved.

With reference to the manner in which cholera is communicated, very great differences of opinion have prevailed. Such have been the immunities of physicians and attendants upon the sick,

that many whose opinions we are bound to respect, even to-day, stoutly affirm that cholera is not contagious and that cordons are useless.

But the preponderance of conclusion is otherwise, and everywhere the conviction more and more obtains that isolation is *the* safeguard from cholera invasion.

The view now nearly universally accepted is that cholera is developed by a specific poison—whether or not a living organism—and that this poison is contained in the alvine evacuations of those suffering from the disease, and that if these are properly disinfected, we may care for our cholera patients with perfect impunity.

By far the greater number of observers incline to the view that the germ theory is true, and that the destruction of these germs as they leave the body would stamp out cholera.

It seems to be conclusively demonstrated that after a period of time varying from a few hours to as many days if cholera dejections are exposed to warmth and moisture they so contaminate the air for limited distances that those who are susceptible and come within the infected area are smitten with the disease.

Soil and water may be so polluted with these discharges as to become fruitful sources of contagion.

The infection may be imprisoned for a long time and be carried long distances in clothing which has been thus soiled and dried, to be again liberated by the washerwoman, to develop the disease afresh. It is a fact long since recognized that those who dwell in the midst of filth, whose foods and drinks are of impure and unwholesome qualities, those most exposed to immediate contact with sinks, closets, drains and cesspools, to water courses and surface exhalations, where cholera discharges have been deposited, are first and most to suffer in the outbreak of an epidemic. And were these poverty-stricken and filthy people controlled to better sanitary conditions, by so much the facility for cholera dissemination would be diminished.

If it be true, as I strongly incline to believe, that the sources of cholera are limited to the intestinal evacuations of cholera patients, and that it is not otherwise communicated, then it becomes a matter of utmost importance as to how these shall be disposed of. If, by proper disinfection they may be sterilized and rendered absolutely innoxious, then indeed we have in hand the key to the question, How shall cholera be controlled?

If we have definitely settled the location of the death-producing cause, and if we are possessed of germicidal or viricidal agents which can destroy the contagious element, then but one farther question confronts us, and that is, How shall we practically apply the remedies in our hands, as to control the disease with germicidal treatment?

Here, gentlemen, we are brought face to face with a question of no ordinary magnitude. It

is none other than this: How shall we in times of epidemics bring under proper sanitary surveillance the countless thousands of people who are constantly traversing every sea and every land as fast as steeds and steam can bear them, and how shall we adequately supervise the commerce of the two hemispheres.

This is not the problem of the hour, but of the age. Neither legislation nor executive action was ever so taxed as they *will be* in the final settlement of this question. Whether we endeavor to estimate the value of human lives or the unnumbered millions of money involved in a widespread epidemic of cholera, we are prompted by motives which have scarcely a parallel, to the utmost of our effort, to *educate* our rulers and the *masses* of the *people* to the exigencies of the hour.

It may be that the devastation of our fair land by this destroyer must again *be*, before the nation will adequately meet the issue; *but* so surely as cholera *shall* from time to time visit our shores, *as it will*, so surely will the American people sooner or later devise the means that shall prove effectual in the control of cholera. In the accomplishment of this result nothing less than wise, efficient international sanitary legislation will meet the exigency.

On the part of our government our prime concern is with our marine interests. A sanitary cordon of divine construction—a sea wall 3,000 miles in breadth, is for all time interposed between us and that pestilent *hotbed* on the banks of the Ganges.

The surveillance of our ships means the surveillance of our shores, the timely control of travellers and of infected merchandise means for all our broad land immunity from the disease. *This* is the ideal toward which our utmost effort should be directed.

And when international effort fails, and the pestilence is upon us, then it seems to me that nothing short of national sanitary control will avail to arrest its progress. With national legislation, state and inter-state laws should blend and harmonize. The needs discovered and fully appreciated, the counsels of eminent jurists should prevail. In the presence of this question which appeals, as almost no other can, to universal philanthropy, all sectional and selfish interests should sink into insignificance. There should be timely, efficient, and harmonious action among the entire sisterhood of states. Each state should dominate in all its municipalities and in each municipality its local authorities should exercise a rigid surveillance of all its citizens. House-to-house inspection should be made and often repeated; ample power should be appropriately vested for the abatement of all nuisances and the *control* of all who ignorantly, stupidly, or maliciously transgress the laws of health.

This ideal may not and probably will not be so fully realized as to prevent the advent of cholera in the near future. If not, what yet remains? I say unhesitatingly, go to the city of

New York, and *learn and remember and practise* the teachings of its Metropolitan Board of Health.

Let there be a like sanitary inspection by a corps of trained men instant and alert to attend upon each and every reported suspect, with as efficient organization for the practical application of disinfectants, with the entire force subject to one central command as *instant and implicit* in obedience as though under military control and martial law. After witnessing the results which obtained in New York in 1866, I think no man would question either the wisdom or the efficiency of their action. The same general means which can accomplish so much for our cities should be wisely and efficiently applied along all lines of travel, whether coastwise, by river, or by rail. A like absolute oversight should secure the cleansing and disinfection of all our cities and their suburbs until the hamlets in the open country even are thoroughly renovated.

Another important duty of the hour, it seems to me, is this: To properly acquaint the masses of the people with the relative values and efficiency of disinfectants, and to give simple and sufficient directions as to the manner and extent of using them.

If the germicidal theory be true, doubtless signal failure would come from a too implicit trust in the use of carbolic acid, the odor of which so satisfies one as to the fact of its veritable presence and yet so signally fails to meet the requirements of a germicide. If the acids and chlorine and sulphur are potent agents for disinfection, as they are, no time should be lost in acquainting all our communities with the minute details of obtaining, preparing, and using them for the purposes of successful disinfection.

But when preventive means have failed and we come to meet the disease face to face, at the bedside of the patient, what yet remains for us *to do*? If there be knowledge, if there be skill, if there be at command remedial agents that can *avail*, surely there shall be no withholding at such a time as *this*. To this end the experience of the medical profession is willingly, nay, gladly laid upon the altar for the good of a common humanity.

Having a vivid remembrance of the consternation which possessed our people in 1832, and a somewhat extended professional acquaintance with cholera, as it appeared in this country in 1849, in 1854, in 1866, and in 1873, may I be pardoned, as I conclude this paper, if I presume to offer a few suggestions as to the treatment of this disease—suggestions for which I do not claim originality, but which have been helpful to me in many emergencies.

Let me say at the *outset* that the inspirations of *hope* and of *confidence* are prime necessities. They lift the man to life, while fear is swift to bring destruction. The beginnings of this disease must be quickly under our control if we

would hope to save our patients. The commencement of intestinal disturbance should be a matter of immediate concern, before choleraic discharges are developed. If it be possible to rightly diagnose the case our remedies will then be found of most avail.

A hot bath for fifteen minutes, the quick envelopment of the entire body in dry, hot flannel immediately after bath, the stimulation of the cutaneous surfaces with dry mustard and with friction under cover; artificial heat so applied as to secure, if possible, free perspiration; these all conspire to divert the circulation and save from intestinal congestion. From this time on the patient should be compelled absolutely to the recumbent position.

The drinks should be bland and mucilaginous. Cold water, notwithstanding the immoderate thirst, should be used but sparingly. It will be found that hot drinks will often serve the purpose best. Nothing now serves better to allay vomiting and quench the thirst than moderate draughts of hot water. All food should be in liquid form and limited in amount. No solid food, under any circumstances, should enter the alimentary canal. The crisis will be passed in a few hours, and during that time there should be a suspension of digestion.

Acidulated drinks, not alkaline, should be administered. They are usually grateful to the patient and germicidal as well. Of all remedies to be administered internally I hesitate not to say that opium heads the list. It brings, as nothing else can do, that quietude both of mind and body so essential to success. It lays its hand effectively upon the digestive organs, lessens their sensibility and insures their rest.

Skilfully, cautiously, but efficiently administered, opium is the sheet anchor in which more than in all else we rest our hope. In the hour of extreme exhaustion internal stimulation may be wisely associated with opium. Of the stimulants camphor is the one most valuable. Capsicum is sometimes of service; these rather than alkaline carbonates, or alcohol. The introduction of salines and the hypodermic injection of water to replace the losses are but fanciful delusions, I think, and they are highly impracticable.

Quietude of mind and body, the maintenance and equalization of temperature, the diffuse circulation of the blood, and absolute rest for the bowels—these are the essential requirements, and to these all other medication should be secondary and subsidiary. The crisis passed, the convalescent is to be treated as in each instance the exigency may require. With this brief reference to the origin, the pathology, the propagation, the control, and treatment of cholera, I desire to give place for the discussion which it is the purpose of this paper to elicit.

DR. AUSTIN FLINT, SR., said that in opening the discussion on epidemic cholera he would

limit himself to an enumeration of the several aspects under which it is important that the subject be considered, and to a very brief expression of the views which seemed to him most consistent with our present knowledge. The several important aspects which the subject offers relate, in the first place, to the etiology and pathology of the disease; and in the second place, to prevention and treatment.

Is epidemic cholera an infectious disease? This question at once suggests itself. In other words, adopting the sense of the term infectious, as this term is now generally used, does the disease involve in its causation a specific agent capable, under favorable conditions, either within or without the body, of indefinite self-multiplication? The affirmative answer to this question will probably be accepted without the need of discussion. Is the specific cause of epidemic cholera a micro-organism; that is, is cholera a parasitic disease? I do not hesitate to express the belief that the affirmative answer to this question is sustained by logical evidence which is almost, if not quite, conclusive. I use the term "logical evidence," in contradistinction to demonstrative proof. This evidence is two-fold: *First*: No other explanation harmonizes so satisfactorily with the facts pertaining to the transportation and diffusion of the special cause, the duration of epidemic invasions, and the successful measures for prevention. *Second*: Inasmuch as it has been demonstrated that the specific causes of certain of the infectious diseases are micro-organisms, it is fair to conclude, reasoning by analogy, that other infectious diseases arise from similar causative agents.

Has a specific micro-organism, standing in a causative relation to epidemic cholera, been discovered? This question is likely to give rise to considerable discussion, and it is probable that different opinions will be expressed. My belief is that the evidence in behalf of the validity of the discovery by Koch is so strong as to encourage an expectation of its being, with time and fuller researches, demonstratively established. I do not propose to enter into a discussion of Koch's discovery. I shall content myself with stating the fundamental requirements for the acceptance of its validity.

First.—The so-called comma bacillus must be invariably present in cases of epidemic cholera. This condition seems to lack but little in the way of demonstrative proof. The negative results are so few that they may be fairly supposed to be referable to insufficiency or unskilfulness, or bias on the part of the observers.

Second.—The comma bacillus must be wanting in the healthy body and in other diseases than epidemic cholera. With regard to this requirement, there is some discrepant testimony. There seems, however, good reason to distrust adverse testimony on the ground that the organisms found in the healthy body and in other diseases than cholera are not identical with the cholera

bacillus, although morphologically similar. The characteristics incident to cultivation appear not to have been sufficiently taken into account by those who claim to have found this organism in the mouths of healthy subjects, and in the excreta of those affected with sporadic cholera, dysentery, or other diseases.

Third.—Can a disease identical with epidemic cholera be produced in animals by inoculating them with pure cultures of the cholera bacillus? With regard to this question, it is to be borne in mind that the negative results of inoculation do not constitute positive evidence against the parasitic doctrine, whereas successful results, however few in number, constitute absolute proof of the doctrine. This proof appears to have been obtained, but, in view of the liability to error in experimentation on animals, further researches in this direction are desirable.

Assuming the existence of a specific cause of epidemic cholera, irrespective of the doctrine that this cause is a micro-organism, certain interesting questions relate to the pathology of the disease. Does this special cause produce its primary effect upon the blood, the intestinal affection being secondary thereto, or is the primary effect upon the alimentary canal, and the changes in the blood and in the tissues elsewhere, secondary? Are the striking symptomatic phenomena of the disease attributable to the loss of the serum of the blood, or are these phenomena due to other unknown pathological conditions? Is there ground for the doctrine advocated by George Johnson, that the intestinal dejections denote a salutary elimination of a *materies morbi*? If the specific cause be a micro-organism, in what way does this produce its pathological effects? Does it act *per se* directly upon the solids and fluids in causing these effects, or does it produce a toxic product to which these effects are attributable, as conjectured by Koch? These and other pathological questions open up too wide a field for me to enter upon in these remarks.

Passing to the practical aspects of the subject, the most important are those which relate to the prevention of cholera. First in importance are the measures for the protection against importation of the specific cause of the disease. How far shall quarantine detentions and disinfecting measures be carried? What are the most efficient agents and processes for disinfection? These are questions of great importance as bearing on the protection of communities against the disease, and also as affecting commercial interests. The public naturally and properly look to the medical profession for information as the basis of legislation and municipal acts. To prevent the diffusion of cholera whenever it obtains a foothold in any community, is an object obviously of vast importance. In connection with this object, the question arises: How is the special cause of the disease disseminated? We know that it may be conveyed in drinking water. It seems impossible,

however, that this is the only mode of conveyance. That the special cause may be transported in the atmosphere is probable, if not certain.

Another question connected with the prevention of the disease relates to the choleraic dejections. It may be assumed that these contain the special cause, but is this the only source of the infective agent? The multiplication of the infective agent doubtless may take place outside of the body. Hence arises the question, What are the local conditions essential for the existence and increase of this agent after it leaves the body? Connected with this question is another, namely: What are the most efficient measures for the destruction of the special cause outside of the body? To what extent and under what circumstances is the special cause disseminated by means of fomites? This is a question the importance of which is obvious. As bearing on the prevention of the diffusion of the disease, an important question yet to be decided is whether the dejections in the cases of so-called *cholérine*, prevailing coincidentally with cases of epidemic cholera, contain the special choleraic agent? If the answer to this question be in the affirmative, it is evident that disinfection of the dejections and other precautions are just as important in all cases of simple diarrhœa, prior to and during the prevalence of an epidemic as in well marked cases of cholera.

With reference to these several questions concerning the diffusion of cholera, it is needless to say that as much knowledge as can be obtained of the nature of the special cause of the disease is desirable. But, irrespective of any more knowledge in this direction than we now possess, experience has furnished facts of vast importance. Experience has demonstrated that epidemic cholera may be stamped out by disinfecting and other measures adopted to prevent the diffusion of the disease. For proof of the correctness of this assertion, I refer to the history of epidemic cholera as it occurred in the city of New York in 1866 and 1867. The disease in that city in the years just named was stamped out by efficient measures of disinfection and purification adopted by the Metropolitan Board of Health under the direction of the Sanitary Superintendent, the late Edward B. Dalton. The facts pertaining to the history of cholera in the city of New York in those years are contained in the volumes published by the state of New York in 1866 and 1867, entitled "Report of the Metropolitan Board of Health." For an account of the measures pursued and the successful results, I may refer to an article recently published in the *Philadelphia Medical News* of April 18, 1885, headed "On the Stamping out of Epidemic Cholera in the City of New York in 1866 and 1867." Another effectual method of preventing the diffusion of epidemic cholera is to employ prophylactic measures whenever the premonitory symptoms are present. Epidemic cholera is generally preceded by a painless diarrhœa. Experience has demonstrated

that almost invariably the arrest of this premonitory event prevents the development of epidemic cholera. The difficulty in the endeavor to carry out this method of prevention is to insure a prompt arrest of the premonitory diarrhœa in all cases. For this end, house-to-house visitations daily, or twice daily, during an epidemic, by competent medical inspectors, in order to ascertain whether any of the occupants are affected with diarrhœa, and to furnish proper instructions for relief in all cases, constitute an important measure of prevention. It is probable that this measure alone, if faithfully carried out, would suffice to restrict very much the diffusion of the disease.

There is reason to believe that the removal of filth of all kinds from dwellings and their surroundings, avoiding polluted drinking water, and, in short, attention to hygiene in all its public and private relations, are highly important with reference to the prevention of cholera. But it is to be considered that purely unsanitary conditions will never lead to the development of this disease without the presence of the special cause. Moreover, the latter may give rise to cholera under conditions which apparently are not unsanitary. Important, therefore, as may be appreciable unsanitary conditions in the etiology of the disease, the specific causative agent is of greater importance. Were it a matter of election whether this agent should be destroyed or these conditions removed, there could be no doubt as to the choice. Of the particular conditions which promote either the dissemination or the efficiency of the special cause of cholera, we have as yet no positive knowledge; we can only direct sanitary measures against everything which may reasonably be supposed to be unsanitary.

Finally, the therapeutics of epidemic cholera offer a wide scope for inquiries. One thing is certain, namely, in the existing state of our knowledge, measures for the prevention of the disease are of vastly greater importance than any known therapeutical measures. Experience, as I believe, warrants the assertion that of all remedies in the first stage of cholera, that is, prior to the state of collapse, opium has no competitor. I believe, also, that after the occurrence of the symptoms which denote collapse, opium is to be used with very great caution. The drugs and various therapeutical appliances which have been recommended in the collapse stage of cholera are innumerable. Intravenous injections, to which attention has been recently directed, were tried and found unavailing in the epidemic of 1832. Not one of the countless methods of treatment has stood the test of experience. With the knowledge which we now possess of the cause, the pathology, and the treatment of epidemic cholera, it is perhaps a fair statement that a patient in the advanced stage of the disease has as good, if not a better, chance of recovery, if nothing potential be done in addition to maintaining

absolute rest and the introduction of fluids as freely as the stomach will retain them.

DR. N. S. DAVIS, being called on by the Chairman, said that he had studied the epidemic cholera in all its aspects. He had encountered the disease at the bedside, both in hospital and private practice, in all its stages and phases. He had personally witnessed its beginning, progress, culmination, and decline in seven epidemic cholera seasons. The simple clinical facts thus coming under his own observation were wholly irreconcilable with the popular theories of the present day, concerning the importation of its specific cause, its propagation in the form of bacilli or germs from the evacuations of cholera patients, and the consequent contagiousness of the disease.

He had studied the cholera evacuations under the microscope with much patience and care, both in 1866 and in 1873. He always found an abundance of vibrios, bacteria or bacilli, especially after the evacuation had been kept from twenty-four to forty-eight hours. He had still, in his library, pencil sketches of microscopic fields made in those years, containing as exact representation of Koch's comma bacillus as can be seen in any of the cuts that have appeared in the medical journals of the last two years. But on extending his microscopic examinations to the serous discharges of sporadic cholera morbus and to the thin evacuations of the children having summer diarrhœa, he also found germs so similar that he could not differentiate the one from the other, and consequently regarded them as accompaniments rather than as causes of the choleraic disease. He did not think the epidemic cholera any more contagious (in the sense of being communicable from one individual to another) than is intermittent fever. But, like the latter disease, its origin and spread depend upon the local sanitary conditions of the soil and water, combined with persistent high atmospheric temperature. He was well aware that these views would be regarded as heresy by most of those present, and did time permit he would be most happy to give in detail the facts on which they were founded. But the very few minutes allowed to each speaker in these discussions precluded the statement of anything more than naked conclusions.

DR. G. F. JENKINS, of Iowa, has noticed that a difference of opinion prevails regarding every so-called contagious disease. He had even heard men doubt the contagiousness of scarlet fever. He felt, however, that the profession, as guardians of the people, should give them the benefit of the doubt, and defend them in every way. He did not think that any one should stand up and talk against preventive measures. For treatment he would recommend the injection of salt water under the skin in different parts of the body.

DR. SCHAUFFLER, of Kansas City, hoped he had misunderstood Dr. Davis in saying that

cholera is not imported. He supposed if any question was settled it was that the cholera is an Asiatic disease. Its home is on the Ganges River, and it travels only as rapidly and along such lines as human beings convey it. It can not arise *de novo* in New Orleans or in Chicago. Dr. S. was in New York as a student in 1866 and 1867 and he bore enthusiastic witness to the thoroughness and successfulness with which Dr. Dalton carried out his sanitary measures at that time. People were turned out of their infected houses and the buildings with their contents so thoroughly fumigated that the disease was crushed out. He thought such stringent measures were easier then because the people were accustomed to bayonet rule just after the war. Such methods might not be so easily prosecuted now. When a boy Dr. Schaffler witnessed an epidemic of cholera in Constantinople, where it raged with terrible violence. The thirst was insatiable and tormenting, but he noticed that those who resolutely refused to drink, though suffering agonies of the damned, got well, while those who drank water died. He would therefore take exception to Dr. Flint's allowance of fluids.

DR. JOSEPH JONES, of New Orleans, said that he was also in New York during the cholera epidemic of 1866, and that he entertained a high opinion of the labors of Drs. Dalton and Elisha Harris.

DR. M. K. TAYLOR, of Fort Sill, Indian Territory, said that he had had considerable experience with cholera since 1832. During the attack of 1872 he was in north Alabama with some troops, and great good was accomplished by strict isolation and proper hygienic measures. For treatment he said, "In the early stages we possess no remedy comparable with opium." But he agreed with Dr. Flint that opium is not so valuable in the last stages.

DR. J. T. WHITTAKER, of Cincinnati, said that he knew that cholera is a disease which multiplies in the body and is transmitted to other bodies. No diseases consisting of chemical changes in the system do thus multiply or are thus transmissible. In regard to the comma bacillus, however, no one claims that it is proven to be the bacillus of cholera. Neither Koch nor any of his followers have positively stated that they think that the comma bacillus is the definite cholera bacillus. It is a well known fact, however, that mild cases of cholera, apparently simple diarrhœas may contain the germs of the disease and therefore should be treated as rigorously as a severe case, especially in regard to disinfecting measures.

DR. N. GAY, of Columbus, Ohio, said: "If you find a man in black collapse, and he can still pass a little water, put your ear to his chest; if you can still hear the regular tic-tac of his heart, work with him. If, however, the heart sounds are obscured you need not trouble yourself any further with him. He is gone."