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Abortive Treatment of Typhoid Fever.

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ABORTIVE TREATMENT OF TYPHOID FEVER.

Much has been written in reference to the abortive treatment of typhoid fever. Most of the literature of the subject has concerned itself chiefly with the advocacy of different drugs, and the statement of clinical results obtained by them. The profession will not test these methods extensively until they are shown to rest on a rational foundation of facts of anatomy, physiology and pathology. My purpose is to make a beginning, at least, in this direction; to state the facts that should give both encouragement and direction to our efforts. While I shall not omit a statement of my method, I count it of secondary importance. The facts that have guided me to it are guiding me still, and have, I believe, still better things in store.

The difficulty of proving beyond question the success of an abortive plan of treatment of a given disease is greatly increased by the following facts: that in order to merit the name, the treatment ought to arrest the disease at an early period in its ordinary clinical history; that the symptoms pathognomonic of most diseases occur late in their course; that therefore a successful abortive treatment will arrest most diseases before the development of symptoms that allow of a positive diagnosis, and while there is yet room for a possible doubt as to the identity of the disease whose short course we attribute to an abortive plan of treatment.

The demonstration of the success of an abortive treatment of typhoid fever presents these difficulties in an extraordinary degree, because the symptoms recognized in the past as absolutely pathognomonic all occur late in its history.

The difficulty can be overcome and an absolute demonstration reached only by the following methods:

1. Correspondence of results reached in a very large number of cases that present strong reasons in their symptoms or lineage, or both, for believing them to have been typhoid fever.

2. Post-mortem appearances obtained in the event of death from some other cause, during convalescence from what we believe to have been aborted typhoid fever.

3. A possible aid may be had by a closer study of the early symptoms, that will allow us to attach more diagnostic authority to some of them than we have in the past; perhaps, also, by new methods of research to discover new pathognomonic symptoms that occur so early as to be always present even in aborted cases.

Such observations will themselves need to demonstrate their reliability by long and repeated trial before we can use them with absolute confidence in establishing the claim of any proposed abortive treatment of the disease.

The question at once presents itself. How far have these difficulties been overcome? The reply will be found along the line of the three methods proposed for overcoming the difficulties.

A large amount of evidence has been gathered bearing upon the possibility of arresting the progress of cases giving reasonable ground, in symptoms or lineage, or both, for believing them to be typhoid fever. This evidence will have varying weight with different minds.

Our German confrères have done much, by extended observation, to establish to their own satisfaction the utility of what they are pleased to call a "specific" treatment by calomel or sodium. Niemeyer finds the argument so convincing as to state "that after the accurate observations of Wunderlich we can scarcely doubt that by this remedy (calomel) we may

in some few cases cut short the disease." Sansom and Wilkes, in England, Henry, Peabody, Nelson and White, in our own country, are also among those who should be gratefully remembered for their research in this direction.

I am not aware of any published record of observations bearing upon the second method—post-mortem appearances in supposed successful cases of aborted typhoid fever, in which death may have occurred during convalescence, from some other and unrelated cause.

To the third method of a more exact determination of the significance of early symptoms, contributions are occasionally coming in, the most recent, Ehrlich's urine test, being probably also the most valuable.

The present status of the problem may perhaps be fairly stated thus: It will require the utmost diligence and care on the part of the profession for many years to come, in order to add to the extended observations of the past sufficient data to make a final settlement of the question possible.

Results already reached by observers in Germany, France, England and America afford so strong a suggestion of the possibility (some of us will perhaps say, the probability) of a favorable solution of the problem, that duty as well as interest should hold us all to careful observation and record of all facts bearing upon it. It is this double impulse that prompts me to what I have to say.

That I am not advantageously placed for original investigation, in a locality where typhoid fever is rarely seen, is doubtless true, and might justly seal my lips against attempting an argument that rested chiefly on clinical facts. It does impel me to come even to an argument based chiefly on acknowledged facts of anatomy and pathology with much modesty and with great deference to the views of those whose clinical opportunities have been greater.

Let me state briefly at the outset those conclusions

to which my own mind is tending, to which I shall hope to win your assent.

1. That there is a modified form of typhoid fever, seen in localities provided with a pure water supply, whose development is *slow*, whose *earliest symptoms* are *pulmonary*, and which deserves more careful study than it has received.

2. That there are early symptoms of typhoid fever to which we may attach much, if not a pathognomonic, value, a value greater than that we were accustomed to assign them a few years ago.

3. That certain facts of anatomy and pathology point not only towards the reasonableness of methods already employed for the so-called abortive treatment, but also of adding to those methods other expedients.

4. That the cases so far observed in which these methods have been followed, though insufficient to demonstrate finally their power to abort typhoid fever, are sufficient to strongly suggest their usefulness, and to demonstrate their harmlessness.

5. That these measures have a wide application outside of typhoid fever in febrile affections, especially in those difficult to classify.

We consider first, then, the evidence of the existence of a form of typhoid fever modified by conditions frequently obtaining in communities provided with a pure water supply.

The *avenues* by which the typhoid germs gain entrance to the body are the respiratory and alimentary tracts. Lodgment in either of these tracts involves danger. Lodgment elsewhere is probably harmless.

We may well believe that the initial lesions will be different, and developed with different rapidity, according as the vehicle by which the germs enter, is the respired air or the ingesta—whether fluid or solid. In the latter case the germs mingle at once with dead organic material, and in the lower bowel with fermenting fecal material—so finding the *best* conditions for rapid development. Here, too, is found

that arrangement of blood-vessels and lymphatics which gives most favorable conditions for rapid absorption, having indeed this for its office.

Manifestly, germs entering by the respiratory tract will find much less favorable conditions both for increase and absorption, for here are in health no fermenting materials, and structure is adapted to interchange of gases rather than of fluids.

Explanation of the fact that typhoid fever may be contracted through germs presented with respired air has been offered by the statement that some of the germs lodging in the pharynx and mouth are swallowed with the saliva. We may admit as at least equally probable, that after primary infection of the bronchial mucous membrane, germs contained in phlegm which is coughed up, but fails of expectoration and is therefore swallowed, may carry infection to the alimentary tract. Pathology adds its testimony to the fact of infection through the lungs.

We can hardly fail to trace a growing tendency among recent authors to give greater prominence than before to the changes in the respiratory tract. A more or less severe bronchitis, with not infrequent enlargement of the bronchial glands, is fast gaining deserved recognition as one of the most frequent lesions. I must content myself with only three quotations from prominent authorities in support of this statement. Loomis says: "So constantly is catarrhal bronchitis present in this fever that Dr. Stokes proposed to call typhoid fever, bronchial typhus." Niemeyer says: "We find changes in the respiratory organs in all cases. The typhoid laryngeal ulcer already described is not infrequently found. There are always signs of an extensive catarrh even in the smallest bronchi, marked by dark redness of the mucous membrane, and scanty tough secretion. The bronchial glands are swollen, vascular, and occasionally have a medullary appearance, such as we shall describe for the mesenteric glands." Flint says: "The

enlargement of these glands (bronchial) in some cases is considered as corresponding in pathological character to the enlarged mesenteric glands." This is a fact full of diagnostic significance for us, and of special importance where a well-protected public water supply reduces to a minimum the danger of infection from fluids through the digestive tract, and leaves open only the much smaller opportunity of infection through the respired air.

Three conclusions are inevitable from these facts:

1. That in cases where germs are received first into the respiratory tract, the early symptoms should be bronchial rather than abdominal.

2. That in such cases the symptoms should be slow in development, owing to the conditions unfavorable to rapid increase and absorption of the germs.

3. That such cases are likely to lack in their early history many of the typical symptoms of the disease, *i. e.*, to be atypical, and that room should be given for such cases in our consideration of early symptomatology and diagnosis.

We may add one more statement, viz: that in communities provided with a pure water supply, this is likely to be the most frequent type of the disease, a consideration of much importance in the symptomatology.

If any question the possibility of infection through the lungs they must do it in the face of the fact that most, if not all, recent writers concede it on the double ground of clinical experience and pathological observation.

The testimony of the inflamed bronchial mucous membrane and the enlarged bronchial glands is not easily gainsaid.

SYMPTOMATOLOGY.

We turn next to symptomatology, and especially to the question whether there are early symptoms to which we need to give more prominence than in the

past. Whatever concerns diagnosis may find place here, for if we are to claim abortive treatment of typhoid fever, we shall need to establish reasonable, if not positive evidence that the disease whose short course we attribute to the treatment is indeed typhoid, and therefore one whose natural history is a course of several weeks' duration.

We find ground for diagnosis of disease from:

1. Its lineage, both as regards its parentage and offspring.
2. From symptoms observed during its course.
3. From post-mortem appearances.

Typhoid fever is a disease that may give us evidence from all three sources. *Aborted* typhoid fever, except as death may come in early period of convalescence from some unrelated cause, can gather its evidence from only the first two sources, lineage and symptoms.

The discussion of diagnosis, so far as it depends upon lineage, leads immediately and necessarily to the question of etiology. Is it ever of spontaneous origin? This we have not now time to discuss, and must content ourselves with a statement of the following postulates:

1. No recent prominent writer, with possibly one exception, ventures to positively affirm the negative as more than an individual opinion.
2. All recent and prominent writers mention both views with respect.
3. The strongest opponents of the spontaneous origin have in many instances to confess their inability to trace the relationship they claim between a given case and its parent.
4. Several of the most prominent recent writers are zealous champions of the spontaneous origin of the disease from putrescent organic matter.

Loomis admits that there are strong advocates of both sides, though himself not a believer in spontaneous origin. Murchison stoutly upholds spontane-

ous origin. Reynolds' "System of Medicine" makes this statement, "We must therefore conclude that this disease has a spontaneous origin in putrescent matter." Wilson, while himself a dissenter, admits the weight that must be given to such an advocate as Murchison.

The latest utterance from high authority I have been able to find is that of Dr. Janeway, who, in the course of a discussion in the New York Medical Society, last fall, defined his position as one of willingness to admit the possibility of its arising *de novo* from filth fermentation, but inability to admit it as more than a possibility.

In view of these facts, are we not justified in asserting: 1. That it must be said of both views: Not proven. 2. That inability to trace a suspected case to a preceding case need not deter us from a diagnosis of typhoid fever.

Three considerations must control our discussion and practical estimate of symptoms.

1. That we have to do with a disease whose pathognomic symptoms occur late in its clinical history.

2. That its early symptoms are generally obscure and not infrequently attend other diseases.

3. That if we shall ever arrive at a successful abortive treatment we shall see only the earlier symptoms, and even these modified by this treatment. If asked to give the most characteristic symptoms, we should doubtless answer: the eruption, and abdominal symptoms of tenderness and diarrhœa, and the peculiar temperature curve. If intestinal hæmorrhage is present it doubtless strongly confirms the diagnosis, but as that occurs in only one in twenty of well authenticated cases we can hardly class it among the symptoms to be expected, even in a case following the natural history of the disease. Eruption and diarrhœa rarely occur before the early part of the second week. Abdominal tenderness is often present among the earliest symptoms.

How many of the symptoms generally considered as most essential to a positive diagnosis we shall find in aborted cases, if such exist, will depend upon how early we have begun the treatment and how rapidly it proves effective.

In the cases that have fallen under my observation, eruption has not been infrequent, and abdominal tenderness quite constant. Diarrhœa has been of rare occurrence. The temperature curve we should also expect to find materially modified by successful abortive treatment, though retaining certain strong points of resemblance; quite enough to suggest, if not establish its parentage. We must, therefore, if admitting an abortive treatment, undertake a careful and correct estimate of the value of other and early symptoms.

Under the former expectant or symptomatic treatment, we found our most urgent motive to early diagnosis in prophylaxis in the interest of others than the patient. If an abortive treatment is accepted as possible, our motive is strongly reinforced by interests to be guarded in the patient's behalf, as, if successful, the treatment must begin early.

It therefore behooves us, in the future, to give the closest attention to the symptoms that will afford us, if not positive, at least reasonable, grounds for early diagnosis. A careful review of the statements of Loomis, Flint, Bartholow, Wilson, Niemeyer, and the "Systems" of Pepper and Reynolds, must lead us to assent to the following conclusions, which will also doubtless find verification in our recollection and experience:

We must limit ourselves to those symptoms that are sufficiently characteristic to be an aid in early differential diagnosis.

The onset is generally insidious. The frequency with which patients are, at first, unwilling to believe themselves seriously ill, and interpret the symptoms as those of a cold, is referred to by many writers.

Headache, often of great severity and obstinacy, but sometimes slight, is rarely absent.

Epistaxis is referred to by all writers as a usual symptom, varying from so slight a degree as almost to escape notice to profuse hæmorrhages.

Tenderness of abdomen, sometimes quite general, sometimes most apparent in right iliac fossa, is universally mentioned, and if to it is added ileo-cæcal gurgling, it gains still more significance.

Constipation is probably more frequent than diarrhœa during the first week, though several writers assert a tendency to more than the usual response to a mild laxative. There remain the three symptoms which, I believe, deserve more weight than we have been accustomed to give them, viz.: cough, temperature, and Ehrlich's urine test.

Cough, at first dry and hacking, is insisted on by several writers as an aid in many cases. My own view of its importance where we may fairly infer that infection, if present, has occurred through the respiratory tract, has been sufficiently stated. I question whether the spasmodic cough often seen in such cases does not depend upon pressure of enlarged bronchial glands upon the bronchi or upon the recurrent laryngeal nerve. This view would seem to be supported not only by the character of the cough, but also by the altered physical signs at right apex that often attend such enlargement.

Flint states that "a slight or moderate cough is almost invariably present." Niemeyer says: "In most, if not all cases, the cough and mucous expectoration betray the bronchial catarrh." Wilson says: "Bronchial catarrh is of sufficiently common occurrence to acquire a certain amount of diagnostic significance." Loomis writes: "Slight bronchial catarrh can hardly be regarded as a complication, it is so much a part of the clinical history of the disease."

Temperature.— In typical cases, the well-known temperature curve is observed early in the disease, if

not from its beginning, but atypical cases are so frequent that we need to inquire whether there are not other features of temperature that will afford valuable aid. This, Wilson believes, we have in a marked tendency toward a constant morning remission, and this my own observation strongly confirms, as my cases have followed this rule even during the decline of temperature resulting from treatment. How widely atypical the temperature may be is shown by the outbreak among the German troops at the siege of Paris, where many fatal cases occurred without rise of temperature. Wilson calls attention to another feature of the temperature that he deems characteristic, viz.: that it is very labile, easily elevated by trifling causes, as first solid food, etc. He also makes much of sweating as a diagnostic sign.

Ehrlich's urine-test, though perhaps needing further confirmation, promises to be most useful.

We turn next to the discussion of anatomical and pathological facts that suggest the reasonableness of methods previously advised for the so-called abortive treatment of typhoid fever, and which suggest modifications of those methods and additions to them. The facts most significant in reference to our present purpose are: the period of the disease at which the various intestinal lesions occur; the location of the areas of intestinal ulceration; the relation of the large mesenteric glands to those areas.

The period of occurrence of the intestinal lesions calls for only brief statement. Enlargement of the solitary glands may be observed as early as the third or fourth day in some instances, and continues till twelfth or fourteenth day, when ulceration begins, and may continue till the twenty-eighth day. This statement is significant chiefly as indicating the narrow limits of the period during which an abortive treatment that depends solely upon intestinal antisepsis can have any hope of proving effective.

As to *location of lesion*, the duodenum escapes ul-

ceration; the jejunum is very rarely affected; the ileum is most frequently the sufferer, and ulcerative lesions are frequently confined to the last four feet of the ileum, while the largest ulcers are almost invariably found in the last six inches of this portion of the intestine, *i. e.*, in the part just above the ileo-cæcal valve. It is, indeed, not infrequent, in cases presenting few ulcerative lesions, to find them limited to this small portion of the intestine. In the large bowel, the most frequent seat of ulcerative lesions is in the cæcum. If found beyond that, they grow less frequent as we go downward. The important points to establish in this connection are three:

1. Tendency toward such lesion increasing in the small intestine as we recede from the point of entrance of the *bile*—an antiseptic fluid—a fact that suggests the possibility, at least, that this natural antiseptic fluid proves generally effective in the upper part of the small intestine, and that therefore the addition of another antiseptic might so far extend the effect that the entire intestinal tract should escape injury from the bacillus. It is true that it is only a suggestion of such a possibility, and not a demonstration; a suggestion, however, of such force as to make it a duty to address ourselves diligently to the task of testing the possibility by all safe means.

2. The ulcerative tendency culminates at a point remote from the point of entrance of the bile, and behind an obstruction constituted by the ileo-cæcal valve, and analogous to the obstruction afforded by a stricture of the urethra, behind which the congestion, inflammation and ulceration that maintain a gleety discharge so often intrench themselves.

3. The large intestine developing the ulcerative tendency most in the cæcum, where fæcal matter is most easily long retained, and progressively less as we go toward the rectum, where evacuation of contents is most frequent and immediate.

The therapeutic significance of these facts lies

chiefly in the suggestion that whatever we can safely do to reduce this natural obstructive tendency of the ileo-cæcal valve, and to keep the bowel comparatively free from long retained fæcal matter, will contribute to the recovery of our patient, and to the prevention of the worst features of the disease, dependent on ulcerative lesions.

That the mesenteric glands most involved are always those immediately related to the ulcerated areas, is the fact that most concerns us in their pathology; a fact that has, as yet, proved rather suggestive than productive in my own thought.

Besides these pathognomonic, pathological lesions, there are other minor ones, of not infrequent occurrence, not considered pathognomonic, and yet of perhaps greater significance than we have been accustomed to think. These changes concern the upper portion of the alimentary canal and the respiratory tracts. The large swollen tongue, indented by the teeth; the swollen lymphatic follicles in tonsils and at base of tongue, and the not infrequent ulcerations found in pharynx, œsophagus, larynx and trachea, are referred to by most recent authorities as coincident with, and analogous to, the intestinal changes. These minor lesions afford important therapeutic suggestions as to addressing an antiseptic abortive treatment to the *entire* digestive tract from mouth to anus, and if possible to the respiratory tract as well.

It is also a suggestive fact that, even when no abortive treatment is attempted, cases are not infrequently found in which the first authorities (Loomis, Pepper, Reynolds, Wilson, Bartholow) accept without question the view that the solitary and Peyer's glands, after the enlargement observed in the early period of the disease, undergo resolution without ulceration, a fact that disposes finally of the objection often advanced against abortive treatment, that if the poison once gains entrance to the system, the disease must run

through all its peculiar phases both of symptoms and pathological change.

Having these suggestive facts of the pathology of the disease, let us endeavor to build upon them a reasonable

THERAPEUSIS.

The movement to find an abortive treatment for typhoid fever has arisen in connection with the theory of the intimate relation of a specific germ to the disease. That view may be now fairly regarded as established, and, if true, it is only rational for us to search for some agent with which we may directly combat the germ. Indeed, we should be lacking in our performance of duty did we, as a profession, fail to make such search most diligently. The profession have not been forgetful of this duty, and as a result, the day seems near its dawning when we can set before ourselves more rational and definite aims in our therapeutics.

A therapeusis based upon the germ theory must, in view of our present imperfect knowledge, set before itself *two* aims:

1. To destroy the germs of the disease, or arrest their development.

2. To combat the ptomaines which are developed during its course.

We consider first the conflict with germs. This conflict we may well believe must be conducted on two fields:

1. On the surface of mucous membranes with germs that have not yet entered the tissues.

2. In the tissues with germs that have gained access to them.

The first field we find in the respiratory and alimentary tract. The possibility of the application of efficient antiseptic therapeutics to the bronchial mucous membrane is still open to question. Inhalation of antiseptic vapors or of oxygen, and internal ad-

ministration of antiseptic drugs which are eliminated by the lungs, as, for example, eucalyptol, are as yet our best and very uncertain agents.

The alimentary tract we can deal more directly with, and through a larger variety of agents. We may approach it through two avenues—the mouth and the anus; in both cases our agents are likely to lose in effectiveness as we depart from the point of introduction, while that introduced into the rectum will be limited to the large intestine. As far down as the ileo-cæcal valve we must depend solely upon antiseptics introduced by the mouth, and our aim should be to use antiseptic measures along all the tract—mouth, œsophagus, stomach and intestine.

The mouth and throat may be reached by a mouth-wash or gargle frequently employed. The œsophagus, stomach and intestine we may consider together, as they must all be reached by some agent which is swallowed.

Would intestinal antisepsis be useful if we could effect it? We may find an affirmative answer in the following facts:

a. Even in health, a considerable portion of fæcal matter consists of micrococci and micro-bacteria, and that poisonous ptomaines are developed as a result of their activity.

b. The effect of nature's disinfectant fluid in the intestine, viz.: the bile and its apparent effect in restraining development of the germ in the upper portion of the small intestine.

c. The results of clinical experience.

May we safely attempt such intestinal antisepsis? Clinical experience must here again give us our only reliable answer. That we may safely take considerable amounts of some antiseptic materials into the digestive tract is well established, and if doubt exists, it must concern the question whether these amounts are sufficient to be efficient. Fortunately, we have a test for that in the presence or absence of phenol

in the urine. It is a constant product of intestinal putrefaction, and is eliminated by the urine. Dr. Henry has demonstrated that during the use of thymol phenol disappears from the urine—a proof of effective intestinal antisepsis. Perhaps no more striking proof of the possibility of intestinal antisepsis could be afforded than this action of thymol:—It does not lower temperature when no fever exists; its effect is uncertain, and at best slight, if fever is not associated with disorder of alimentary tract; it proves an active antipyretic in typhoid fever where such disturbance, dependent upon germs, is at its height. Phenol here, too, disappears from the urine during its use.

Another clinical fact going to prove intestinal antisepsis to be both possible and safe is the large class of antiseptics that are effective agents for the arrest of diarrhœa dependent on germ development. Acknowledged advocates of this view are found in large numbers among the first names in the profession.

Other agents than germicides may aid in limiting germ production in the bowel. *Evacuants* may aid us by preventing long retention of the intestinal contents. Certain facts of clinical experience are suggestive of the very practical value of evacuant measures in typhoid fever. Loomis says: "A mild diarrhœa through the entire course of the fever is a favorable rather than an unfavorable symptom." "When the diarrhœa is present in the early period of the disease, it is better to let it alone." The advantage of an active laxative in the initial stage of the disease is too generally conceded to admit of doubt —an advantage shown in reduction of the number of microörganisms, and in reduction of temperature.

If opportunity of treatment is offered early in the disease, the problem of controlling development of the germs in the tissues may not be so difficult, and may not require a charging of the whole body with an antiseptic sufficient to be destructive in every part. The problem will be a simpler one, if we can

find special hiding and breeding places of the germ within the body, where the bacilli are rapidly developed, and from which they, or the poisonous ptomaines which they produce, or both, are poured out into the blood in increasing quantity, till its power of disposing of them is exhausted, and the whole system succumbs to their influence. That such is the fact I think we must admit, if we would give a rational explanation of the phenomena often observed.

What are these hiding and breeding places? My answer rests in part on some well-recognized facts of anatomy and physiology, and in part upon some not irrational, but, I will admit, theoretical, inferences from these facts. In the ordinary process of absorption from the intestine, two carriers wait in the intestinal wall to convey the nutritive products of digestion into the circulation, viz.: the capillaries and the lymphatics. Material in solution, as the peptones, enters by way of the capillaries; material in suspension, as the emulsioned fat, by the lymphatics, aided, doubtless, by the peculiar structure of the epithelial cells, whose root-like processes of adenoid tissue extend to, and are continuous with, the wall of the lymph vessel in the centre of a villus. The closely packed rods on the surface of the epithelial cell first entangle the foreign body, and the amœboid activity of the cell conducts the entangled mass along the cell tissue and its root-like processes, till it reaches the cavity of the lymph vessels, so determining its entrance into a lymphatic, rather than a capillary.

We may fairly infer that this is also the pathway of the germs, as they would correspond more to the suspended oil globules than to the dissolved peptone. A few germs, perhaps, enter the capillaries, but we may well suppose that a fluid containing as much oxygen as the blood, with which it parts readily, would be capable of promptly disposing of a small number of such germs.

On the other hand, germs received into the lym-

phatics find a medium much less inimical to germ development, as it contains only a slight trace of oxygen. As these pass along the lymph vessels to the point of entrance into the general circulation, they are obliged to pass through, we might almost say to be filtered through, the pulp of the mesenteric glands, and many of them would be almost inevitably entangled there, and find, if not an ideal, at least a favorable, place for development, comparatively removed from oxidizing agents. Those carried on through the glands to the general circulation may be destroyed by the well-oxygenated blood.

Favorable as is the condition in the mesenteric glands for germ development, a much more favorable condition is found in the closed lymph follicles that constitute the solitary glands and Peyer's patches—entirely closed sacs, with no lymph current running through them, as have the mesenteric glands, to molest developing germs; the products of the glands escaping probably partly into the capillaries, and partly into the lymph sinuses that lie beneath them.

My view, supported, but, I will admit, not proved, by the peculiar lesions of the disease, is that the germs, finding their first place of lodgment and increase in the contents of the bowel, find a second such point of lodgment and increase in the closed lymph sacs of the solitary glands and Peyer's patches and in the lymph glands of the mesentery, their rapid increase there giving rise to inflammation and enlargement of the glandular structure, and to a constantly increasing rate of discharge into the blood, till that fluid is no longer able to destroy the germs or their products, the ptomaines, and the constitutional symptoms, characteristic of the disease, result.

If this be true it has an important bearing upon the possibility of abortive treatment. If intestinal antisepsis be possible we certainly have an early opportunity of aborting the disease, while germs are confined to the contents of the bowel.

If my view of the part played by the solitary, Peyer's and mesenteric glands be true, we have a second opportunity, after the germs have gained access to the tissues, but are practically confined in their development to these lymph structures, and the blood is yet able to dispose of such as are discharged into it. In partial support of such a view, let me quote from Loomis: "It is scarcely questioned that the typhoid poison, to a great extent, gains entrance to the system through these glands and lymphatics, and here produces the primary irritation."

This fact gives two most useful suggestions to one who aims at the abortive treatment of typhoid fever:

1. That though it were proven to be unsafe to introduce into the general circulation a germicide in quantity sufficient to be effective there, still he need not limit his antiseptic measures to the intestinal tract, but may make the effort to pursue the germs as far, at least, as their first place of hiding and increase in the tissues, viz., the lymph structures of the alimentary tract.

2. That probably the most effective agent for such pursuit would be a fatty germicide, if that can be found, or failing of that, a germicide so united with a fat that in the process of digestion it will be retained in the fat when the latter becomes emulsioned, and would be therefore absorbed by the lymphatics, as the fat is.

It is also possible that an insoluble germicide finely triturated, might prove effective. Or, that accompanying a germicide which is soluble in fats, with a diet specially rich in fats, would gain the same result.

The prospect of obtaining a germicidal effect in these tissues would seem to be excellent, as these organs lie directly in the path of absorption from the bowel, and as the lymph current is at best a very slow one, allowing more time for the action of a germicide in the gland tissue, before it should be carried on into the general circulation.

The structure of the spleen, closely allied as it is to that of lymph-glands, suggests that it would be likely to prove a place of hiding and increase for the germs, after they had once been thrown into the blood faster than that fluid could dispose of them, and this seems to be the case as shown by its constant enlargement, a change subsequent to that in the intestinal and mesenteric glands; and by the enormous number of the germs found there in fatal cases by microscopic examinations.

The possibility of pursuing the germs to this point turns upon the possibility of safely introducing a germicide into the blood in sufficient quantity to prove effective, unless it should be found that the spleen, tolerant as it is of some forms of hypodermic medication, should be tolerant of some effective germicide given in that way.

It appears, therefore, that there are three periods in the disease during which we may address ourselves to germicidal, and so abortive, treatment of the disease:

1. While germs are limited to contents of the intestinal tract.

2. After they have gained access to the lymph structure of the intestine and mesentery, but while the blood is yet able to dispose of all the germs thrown into it from the glands.

3. After the production of germs has passed the limit of the blood's power to destroy them, and there occurs a general infection of the body.

Manifestly the difficulty of attaining our aim increases as we advance from the first to the third of these periods.

Whether a single remedy suited to all stages can be found; and if not, what choice of remedies we had best make for each stage; whether in treating each stage later than the first, we must carry over into it the remedies of the preceding stages, are

problems that will require close observation and most patient research.

So much for the first of the aims we set before us, viz., combating the germs of the disease.

The second aim is to combat their product—a ptomaine or several ptomaines, which, we cannot yet assert.

The ptomaines are the latest, and a most welcome and suggestive addition to our knowledge of the causes of disease. They open up a wide field for surmise and investigation, and give more promise of leading us to rational treatment of disease than has any fact brought to our notice for several years past.

A brief statement in reference to them, which will serve to freshen recollection in our thought, will not be amiss. Perhaps we may best present such a statement in the form of the following postulates:

1. As vegetable cells, by an activity peculiar to themselves, develop a material known as an alkaloid, so animal cells in normal activity develop materials known as animal alkaloids, or ptomaines.

2. That ptomaines are also developed under other conditions, viz., in the normal processes of digestion, and under all circumstances where bacteria and putrefactive changes are active in the body.

3. That these ptomaines are numerous, the kind developed depending upon the character of the cell that produces them, the species of bacteria at work, and the stage of putrefaction.

4. That many diseases are attended with the development of a ptomaine peculiar to the disease, and which, if introduced into animals, causes promptly the development of symptoms allied to, if not identical with, the symptoms of the original disease.

5. That the ptomaines produced differ with each disease so far as investigated.

6. That these ptomaines produce symptoms affecting chiefly the heart, lungs, and cerebro-spinal system.

7. That the active agent producing the symptoms is not the germ, simply as a foreign body, but the ptomaine (or ptomaines) resulting from the growth of the germ and the changes it effects in albuminoid material of the tissue or of the intestinal contents.

8. That typhoid fever and lobar pneumonia are among these diseases.

9. That from cultivation of typhoid fever bacillus, a ptomaine has been derived which proved fatal to small animals in twenty-four hours.

10. From the urine of both typhoid fever and pneumonia patients two extracts have been obtained, either of which will produce death, but whereas one leaves the heart in diastole, the other leaves it in systole.

11. That several facts point to the conclusion that these ptomaines are thrown off by the excretions, and that such elimination, if rapid, is sometimes followed by a prompt disappearance of symptoms.

Such a fact is the sudden termination of symptoms after profuse sweating, as in intermittent malarial fever and in pneumonia. Such also is the remarkable success had in the treatment of yellow fever by hypodermic injections of 1/6 to 1/4 of a grain of pilocarpine muriate, reported by Dr. E. Habersmith. The profuse sweating and catharsis or emesis, or both, was followed by return to health in forty-eight hours.

This ptomaine enemy we may combat in the direction of preventing its formation, of destroying it by effecting its decomposition, by using strictly antidotal remedies and by hastening its elimination from the system. Doubtless the best, indeed perhaps only way of preventing its formation is by destroying or rendering inert the germs of disease, whether in the intestinal tract or the tissues. Therefore any germicide that we use effectively for *that* purpose will at the same time effect *this*.

The destruction of the ptomaine after it has once appeared in the system is something that we know

little about, too little as yet for intelligent discussion, and we therefore dismiss it with this simple mention, and the suggestion that we may find encouragement to our effort in this direction in the fact that some at least of the ptomaines have an exceedingly unstable chemical equilibrium, and may therefore offer themselves easy victims to an agent that invites their constituents to more stable combinations.

The antidotal treatment we have probably, to a slight extent, unwittingly followed in the use of digitalis for heart-failure.

Elimination of the poison we may perhaps hasten in vigorous patients by profuse sweating. The remarkable results gained by Nelson with veratrum viride are perhaps attributable, in part at least, to the drug acting in one or both of these methods. It suggests another possible advantage of keeping the large bowel free from accumulation by large enemata, in addition to those already referred to. Especially is this the case when we remember that the large intestine is in health the place of most rapid development of bacteria and formation of ptomaines, and also an organ capable of carrying on active absorption. This is a field that waits our study, and seems to offer a rich harvest to the wise and patient seeker.

Having laid this foundation of many facts of anatomy, physiology and pathology, and I hope not materially weakened it by some theoretical considerations drawn from them, I am ready to present in detail the methods of treatment I am accustomed to follow.

No precaution, in effort to procure good ventilation, pure air, proper diet and careful nursing, is omitted. In cases presenting evidences of primary infection through the respiratory tract, I have sometimes employed creosote inhalations with Beverly Robinson's oro-nasal inhaler, with apparently good results, in at least diminishing cough.

The digestive tract receives antiseptic attention throughout.

Frequent use of an antiseptic mouth wash, generally Dobell's solution with Listerine added, has been only recently employed, and I do not feel justified in urging it on the ground of any positive results demonstrated yet, but on purely theoretical considtions derived from pathological facts already noted.

To this antiseptic wash is added occasional doses of $\frac{1}{100}$ gr. of hydrarg. iodid. rubr. in 1 per cent. trituration with sugar—this to be used three to six times a day, allowed to melt on the tongue, and no fluid to be taken after it for several minutes. The antiseptic effect of this is doubtless felt in the mouth, œsophagus, stomach and bowel. In order to avoid the danger of salivation this is not used after the second or third day, unless very infrequently.

The main dependence for intestinal antisepsis as far as the ileo-cæcal valve, is sodium salicylate in doses of 10 grains every two to three hours, given either in capsules or Wyeth's compressed tablets. I am well aware that it has been pronounced by some high authorities to be not only useless, but also positively injurious; attention has been called to the fact that it is eliminated by the kidneys and may irritate those organs. I am grateful for the suggestion, and always watch the urine of my patients closely, while using it, but have yet to meet any unpleasant result from its use. The same may be said of the statement that it depresses the heart, diminishes respiratory movements, causes gastric disturbances, increases diarrhœa and tendency to hæmorrhage. I have watched for, but have not observed them, as in any way troublesome results. The only precautions in methods of administration that I have found necessary, have been to take a few swallows of some fluid nourishment before the capsules, and the addition, in some cases where the patient's circulation is

poor even in health, of a few small doses of tr. digitalis.

There is a large class of available antiseptics. I have used sodium salicylate not because I know it to be the best, but because I have found it safe and efficient. Other agents that call for very favorable mention are Dujardin-Beaumetz's solution of carbon bisulphide, thymol urged by Dr. Henry, *beta*-naphthol advocated by Dr. Bouchard; naphthalin advocated by Dr. Peabody seems to have a good record. In using it we need, however, to remember that it is objectionable in cases in which lung symptoms are prominent. In well selected cases I am disposed to think it may prove more effective than sod. salicylat., as Dr. Peabody finds it reduces temperature to normal in 24 to 48 hours, a more prompt result than I dare claim for the soda salt.

Salol I have been led to be suspicious of, as, if used in sufficient quantity it needs to be closely watched in reference to its effect upon the kidneys. Two 10 grain doses have, in my experience, once caused suppression of urine.

The iodine-carbolic acid of Dr. Sansom, of London, also advocated by Bartholow, has in my hands proved very disagreeable to patients, and much less efficient than sodium salicylate.

Sulphurous acid, much praised by some, and the phenic acid treatment of Dr. Déclat, have served me well in a few cases, but not as satisfactorily as the sodium salicylate.

My third therapeutic agent is calomel, given in one large dose at the outset, if the case is seen before a free diarrhœa has set in. If seen later on, a dose of equal parts of castor oil and syr. rhei has the preference, as an efficient but soothing laxative, which tends to limit its own action. If abdominal palpation and percussion give no sign of accumulation in the bowel, even this would be omitted in cases with free diarrhœa. The use of laxatives in the disease

has been much inveighed against. In Reynolds' System we find the statement, "Calomel should be avoided, its action is too irritant." One cannot but wonder, prior to personal experience, whether the danger is not overstated by this author, when reading of the results gained by the Germans, as shown by statistics covering hundreds of cases, and of Dr. Bartholow's large confirmatory experience. We will rather assent to the statement of that sage among medical teachers, Dr. Loomis, who says: "The greatest caution must be observed in the use of cathartics in any stage of the fever." The German mortality, reduced from 13.2 per cent. by the expectant treatment to 8.8 per cent. by an active calomel treatment continued during the first week, may go far towards relieving our minds of apprehensions of evil effects from one full dose at the outset. Bartholow comes strongly to our support with advocacy of 7½ to 10 grain doses every alternate night till the third or fourth day. Dr. Peabody, who believes that for two years past he has aborted many cases coming under observation during the first ten days of the disease, employs as an initial measure a 10 grain dose of calomel. Niemeyer, Liebermeister and Wunderlich also lend the support of their strong names to the treatment.

Apart from my own confirmatory experience, I cannot but conclude from the testimony of others that the measure is at once safe and useful. I remember to have heard Dr. Fordyce Barker give his approval to such an initial dose as a wise rule.

My evacuant agent for the remaining course of the disease, after this initial dose of calomel, or castor-oil and rhubarb, and also my chief reliance from beginning to end of the disease for an antiseptic agent below the ileo-cæcal valve, is a daily large enema of an improvised decoction of chamomile flowers and borax. The nurse is instructed to add a small handful of chamomile flowers to 3 pints of water, and

allow it to simmer, without boiling, on the stove for one hour; then to strain it through cheese-cloth to remove any woody fibre that might prove irritating; add a teaspoonful of borax and administer as an enema, giving all that the patient can be persuaded to take. At the second or third using, if not from the outset, an adult will generally take all. Retention for a few minutes is not essential, but doubtless increases the beneficial effect. At times, no little tact is required to induce the patient to take the necessary amount. In difficult cases, the Sims position, and momentary pauses in the administration, will facilitate the process. A slight amount of discomfort during the giving of the enema is not unusual, but need not deter us.

Some writers give urgent warning against the use of such enemata. Wilson says: "Large enemata are attended with danger arising from their liability to set up energetic peristaltic movements which may extend to the lower part of the ileum." "The constipation of enteric fever is most safely treated by the daily administration of small enemata of strong warm soap-suds or of thin gruel." Much of the acknowledged force of this argument as applied to advanced cases in which there is a probability of ulceration having occurred, is lost, when we apply the treatment in the early history of the case, where the abortive treatment has its best opportunity.

In treating advanced cases the size of the enemata should be reduced. The advantage of it in early treatment is enhanced by the "peristalsis extending to the lower part of the ileum," as the measure proves in that way not only an evacuant agent for the large bowel, so removing obstruction from before the fæcal matter lying just above the ileo-cæcal valve, but also as an active evacuant of this matter in the lower portion of the ileum, at the point of most frequent ulceration.

The vast importance of any agent that will give

safely a daily evacuation of the material tending to collect here, so preventing not only the irritation resulting from such collection and retention and consequent fermentation in the early history of the disease, but greatly diminishing the amount of absorption of ptomaines and septic material likely to occur through the abraded surfaces of the ulcers—the importance of such an agent is, I believe, incalculable. We may, perhaps, give emphasis to it by calling to mind the fact that, while the bladder in health allows of almost no absorption, in diseased conditions resulting in ulceration some topical applications have to be most cautiously used, in order to guard against the danger of absorption through the abraded surfaces.

Another consideration adds to our estimate of the value of this and of intestinal antisepsis carried on through the mouth: Wilson has pointed out most clearly that the fever has two distinct movements: the first or primary fever, due to specific germ infection; the second, an imitative or hectic fever from ulcerations, sloughing and resorption. Even if the impossibility of destroying or retarding development of the specific germs in the intestinal tract or the tissues, or of combating the ptomaine which appears coincidently with their development and activity, should be demonstrated, there yet remains here, in conflict with the causes of this septic fever, a large field for abortive treatment of typhoid fever. We might well hope to limit the symptoms to the type represented in the class of cases grouped by most writers under the head of abortive typhoid, in which, from failure of the enlarged solitary glands and Peyer's patches to ulcerate, we have the course of the fever limited to that due to specific germ infection.

The chamomile and borax I have employed, not because it is necessarily the best, but it is simple and seems to be efficient. It combines in each of its constituents agents that are mildly antiseptic and

astringent. Stronger antiseptic solutions could doubtless be safely used. In future cases I shall be disposed to use the solution of carbon bisulphide, recommended by Dujardin-Beaumetz. He says of it: "For six months I have been giving this carbon bisulphide water in typhoid fevers in doses of from 5 to 10 tablespoonfuls a day according to the intensity of the diarrhœa, and I have obtained most satisfactory results from the point of view of intestinal antisepsis. Nor have I seen any untoward accidents from its employment. It has proved to be possessed of efficacy above all other medicaments." It is true he mentions its use by the stomach alone, but I should not hesitate to use it largely diluted as an enema, as well.

Other promising therapeutic agents for enemata are oxygenated water, solution of peroxide of hydrogen and, as supplementary to, rather than substitute for, fluid enemata, gaseous enemata of oxygen.

This is the treatment that I commend to your favorable consideration. It is *new* only in the particulars of attempted antisepsis extending through the entire alimentary tract and to the lungs, and in the use of large and antiseptic daily enemata.

Does this treatment abort typhoid fever? I cannot answer positively, because, as we have seen, typhoid fever rarely, if ever, admits of an early positive diagnosis. I believe that it does:

1. Because the method commends itself to me as rational, and not unlikely to give such a result.

2. Because competent observers are persuaded that by like means—less the enemata, which I believe to be a most potent agent—they have frequently aborted pronounced cases of the disease (Peabody, Henry, Sansom).

3. Because my own experience is sufficient alone to justify such a conclusion to my own mind.

It must be a matter largely of judgment, and therefore I cannot find fault with those that dissent. I

have grown familiar with the answer that many objectors make to the second and third reasons offered for "the faith that is in me." "There was an error in diagnosis; the cases, if arrested, were certainly not typhoid." I heard it first from Dr. Alonzo Clark in the course of one of his lectures on typhoid fever. "Gentlemen, if you arrest a fever you have diagnosed as typhoid, that fact is proof of your error in diagnosis." I question whether the day is not passed when fair-minded men will venture to assert that as more than an opinion. I trust the argument, with such record of the experience of others as I have been able to offer, would commend itself to you for trial, without evidence from my own experience.

I have already so far trespassed upon your time that I am impelled to offer only a few illustrative cases to meet the demand of this practical time, that asks every one who advances a theory to establish it by evidence. The demand is perhaps more reasonable, since the method has some new features, clinical evidence of the value of which cannot be had as yet, unless offered by my cases. As the early positive diagnosis of typhoid fever is of at least questionable possibility, I will narrate the symptoms and leave each one to draw his own inferences. One group of seven cases in the same family, using the public water supply, which was doubtless pure, so that we might look for infection through respired air, will be representative.

Case 1.—Young man, much indisposed for over over a week before I saw him. Severe cough which did not yield to ordinary remedies. Seen December 25. Frequent chills through the night; temperature 102°; headache; marked abdominal tenderness, most marked in right iliac fossa, with moderate tympanites and distinct gurgling. Eruption on abdomen fourth day after seen. No looseness of bowels. Morning temperature 2.5° less than evening. Temp. steadily declined during treatment; normal on sixth day.

Second and third cases taken ill December 30, five days after the first was seen. *Case 2* had no marked chill. First temperature observed 101.3°; headache; nose-bleed. Eruption on abdomen (three spots) the fifth day after treatment was begun; tenderness over abdomen quite general, most marked in right iliac fossa; gurgling slight; marked morning remission; temperature normal on fifth day.

Case 3.—No distinct chill, but frequent chilly sensations, headache, nose bleed, slight abdominal tenderness, spots of pneumonia; highest temperature 104°. Temperature had fallen nearly to normal on sixth day, when pneumonia set in; confined to bed for two weeks, no eruption observed.

Case 4.—Chill; temperature 102.5°; marked abdominal tenderness, tympanites and gurgling: nose bleed every day. This patient was ordinarily subject to nose bleed once in a week or ten days, but had never before suffered from it with such frequency. Temperature normal on fifth day.

Case 5.—Severe chill; marked tenderness of right iliac fossa with moderate tympanites; highest temperature observed 102.75°; headache; no eruption. Temperature normal on sixth day.

Case 6.—Headache severe; no nose bleed, no abdominal tenderness or gurgling, no eruption. On bed but not undressed most of three or four days. Highest temperature observed 100.75°. Normal on the fourth day.

Case 7.—Suffered least; no chill, no abdominal tenderness, headache, no nose bleed; temperature 100.5°. No eruption. Normal temperature on fourth day. A partial explanation of the lightness of the symptoms in the last two cases may be that they had been previously using 1 per cent. trituration of hydrarg. iodid. rubr. as a prophylactic measure.

The temperature of all showed a marked morning remission, and all felt, during and after the fever, a degree of prostration out of proportion to the other symptoms. Enlargement of spleen was marked in

No. 1 and slight in No. 2. All had a troublesome cough, as might be expected, considering the probable avenue of infection, the lungs. Nos. 1 and 2 sweat profusely during convalescence.

Case 8.—The patient was exposed to typhoid fever during his care of a friend who died with well-marked symptoms of the disease, viz., headache, epistaxis, tympanitis, abdominal tenderness, ileo-cæcal gurgling, diarrhœa, and as typical an eruption of typhoid fever as it has ever fallen to my lot to see.

The friend was not under my care, but I saw him a few times at the request of his physician. Careless attention of those nursing him left his linen often soiled with the discharge, and the air of the sick-room foul.

The person who later became my patient laid out the body of his friend and complained bitterly of the foulness of the body and linen. Eight days later he suffered from serious malaise, chilliness, and slight headache, but did not call medical aid for a week, during which time his symptoms grew constantly worse, and thirst, feverishness, abdominal tenderness and entire loss of appetite, developed. When I first saw him temperature was 104°. Headache intense, abdominal symptoms of tympanitis, tenderness and gurgling well developed, and patient had had epistaxis. Next day eruption on abdomen appeared. Temperature steadily declined to normal on fifth day.

Case 9.—Also assisted about the patient who died, and had symptoms corresponding nearly in time of development with Case 8, but of less severity—viz., quite pronounced chill, headache, temperature 102.6°, abdominal tenderness, pain and tympanitis with ileo-cæcal gurgling, but no eruption. Seen at same date as preceding. Temperature normal on fourth day. Both 8 and 9 had slight, dry cough.

Case 10.—Occurred in a locality where well-marked cases existed at same time. Patient was seen on

third day of decided symptoms. Temperature 103.4°, severe headache, dull countenance, epistaxis very slight, abdominal distension and tenderness, bowels constipated, eruption appeared distinctly one week after first observation. Patient was at times slightly delirious. Temperature declined to normal on twelfth day. I might multiply cases did time allow.

The objection will doubtless be raised that the leading hospitals have tested and discarded as useless the abortive treatment of typhoid fever. To this two replies at once suggest themselves:

1. That typhoid patients are rarely received into a hospital till the disease has passed beyond the period best adapted to an abortive plan of treatment.

2. That the methods thus far tried have lacked what I believe to be a most essential factor, viz.: the large antiseptic and astringent enemata.

I can hardly close without commending the treatment as remarkably effective in the irregular forms of fever, dependent, as doubtless many of them are, upon germs and resulting ptomaines. What I would specially urge is not the use of sod. salicylate by the mouth, or of borax and chamomile by enema, or of creosote vapor for inhalation, but of some efficient antiseptic by all these methods.

The statements of Drs. Peabody and White lead me to think it not unlikely that later trial of naphthaline and sulphurous acid may change the estimates of their value, derived from a few cases in my own experience.

Permanent reduction of typhoid temperature to normal in 24 to 48 hours, which they report, I have not yet seen in well-marked cases by the use of sod. salicylat. It is most earnestly to be hoped that these gentlemen will give us more detailed accounts of their methods and results.

What agent or agents will prove best can be shown only by long-continued and most careful tests. That the results already reached put upon us all the duty

of aiding in the settlement of the question, is a conviction that, individually, I cannot escape.

Wilson states that "typhoid fever destroys more lives that could be saved than any other acute disease whatever. We who accept the abortive treatment of the disease, as he does not, repeat the statement with an added and sad emphasis, and find ourselves urgently impelled to persuade all to test a measure in our thought so full of promise. No problem more occupies the thought of the profession to-day, none so busies its laboratories with earnest research, none gives such promise of yielding for us better therapeutic resources and for our patients a more certain recovery from disease.

It behooves us to take a part in the movement—at least to study closely its results and if possible to contribute to them.

I append an instructive letter from Dr. Horace C. White, kindly put at my disposal by Dr. H. O. Marcy, of Boston, and would also refer the reader to Dr. Peabody's statement of his own and his friends' experience, as given in the published proceedings of the Association of American Physicians.

Ehrlich's urine test is admirably stated by Dr. A. Jacobi, in the same transactions.

Dear Doctor:—In answer to your request for a statement of my experience in the abortive treatment of typhoid fever by the use of sulphurous acid, I am obliged to write only a hasty reply.

The *British Medical Journal*, of Dec. 3, 1870, contained an article on the administration of sulphurous acid in typhoid fever, by G. Wilks, of Ashford, Kent, in which after giving his experience in its use he says: "I will state distinctly what I claim for the sulphurous acid in typhoid fever; that it arrests the further development of the fever poison, and by continuing this arrest long enough, exterminates the fever. Briefly, it is an antidote."

In 1871, I began the use of this remedy when practicing in Maine, in a locality where typhoid was prevalent and severe. My expectation was more than realized, and my results would justify the claims of Dr. Wilks, in nearly every case. I have

continued to use it for the past 16 or 17 years and have lost none of my confidence in it.

Since my removal to the suburbs of Boston where we have much less typhoid fever, I have used it very extensively in scarlatina, diphtheria and kindred diseases, with equally good results. In fact, there is no remedy that I feel more confidence in prescribing than this. It has never failed me in but one case of scarlatina, and I lost no confidence in the remedy in that case, as the child was already in *articulo mortis* when I was called. It has never failed me in diptheria, except in a very few cases which were complicated with croup, or in other words, where the membrane was located far down the trachea.

I know these are strong statements, but I think I have used the medicine long enough (nearly 17 years) to have been able to have eliminated the enthusiasm which attaches itself to a new remedy.

And now allow me to answer the question: Why is not the sulphurous acid as potent a remedy in the hands of others as it has been in mine? The answer to this question is not difficult. 1st. A misconception of what the medicine is. What is labeled sulphurous acid, is a solution of sulphurous acid gas in water. Being chemically SO_2, it is often supposed to change to sulphuric acid SO_3, by exposure. This is not so. In fact, it is difficult to change SO_2 to SO_3, but when left in a partly filled, or not sealed bottle, the gas escapes, leaving an almost inert substance. As a consequence, prescriptions are seldom filled with the medicine of proper strength, unless the druggist is accustomed to its use. I have had no small amount of trouble on this account even with those we generally consider very reliable druggists. In fact, I seldom get the medicine of proper strength on prescription until the druggist has been labored with.

2d. My confidence in the remedy and in its safety leads me to prescribe it freely, and also to disinfect the rooms at least twice a day with the gas by burning sulphur, believing that if the germs of disease can be destroyed by fumigation, as has been the practice for many years, that the air can be kept free from germs or sterilized by frequent use of the gas.

But I am wandering from the subject. In typhoid fever, when called early, I expect a decided remission of the fever in from 24 to 48 hours, and the fever is kept a very mild type if not completely aborted ever after. I give it mixed with syrup and diluted with water, also by inhalation. I will close this hastily written letter by quoting the closing words of Dr. Wilks' paper: "Very possibly, I have failed to convince you of the advantages of using this drug; but let me again remind you of the plain fact that of 173 cases of this fever occurring in our practice during the past fourteen months, two only died, and those two did not take the acid; for the one it was not prescribed, the other

was a drunkard and would take nothing. Of the 171 who took sulphurous acid not one lost his life, and there were few who were not convalescent within fifteen days of commencing the treatment. Surely such a result will induce you to try the medicine for yourselves when opportunity offers."

Very sincerely yours,

HORACE C. WHITE.