

CABELL (J. L.)

With the Compliments of the Author.

ETIOLOGY OF ENTERIC FEVER.

BY

J. L. CABELL, M.D.,

OF UNIVERSITY OF VIRGINIA.

EXTRACTED FROM THE
TRANSACTIONS OF THE AMERICAN MEDICAL ASSOCIATION.



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IN one of Dr. Parkes' latest Reports on Hygiene, that for 1873, such prominence is given to the rival theories of Budd and of Murchison, as to imply that one or the other, or otherwise the two conjointly, suffice to cover the entire ground as to the external conditions of the production of enteric fever. With judicial fairness Dr. Parkes cited the strongest arguments on either side, and contrasted them with the weak points of the rival doctrine. He concluded "by expressing the opinion that neither the views of Budd nor of Murchison can be considered as wholly proven, and that in our present state of knowledge, each must be accepted as limiting the conclusions of the other." The special object of this paper is to inquire whether enteric fever, as it occurs in this country, may not depend upon other external causes than those which are assigned by either of these theories. Such an inquiry must proceed on a careful study of the natural history of the disease, by which method we may hope to be eventually able to eliminate every unessential circumstance, and fix our regard upon the efficient agencies in its production. When definite conclusions in this direction shall have been reached on trustworthy grounds, we may then be in a position to scrutinize, with better hopes of success than we can now do, the various speculative doctrines which relate to the nature of the infectious material itself.

In the mean time we may assume the substantial truth of some form of the germ theory, as a legitimate scientific hypothesis to be provisionally held, subject, however, to future verification or correction. If such a doctrine were not already well nigh demonstrated, its provisional assumption on a few striking analogies might serve a very useful purpose in suggesting lines of inquiry and guiding our observations. Prof. Huxley has well said that

there are periods in the history of every science when a false theory is not only better than none at all, but is a necessary forerunner and preparation for the true theory. And Dr. Burdon-Sanderson, after avowing his belief that it is only by the investigation of particular cases, by clinical observations and pathological experiments, that any progress can be made in the inquiry whether there is or is not such a thing as a *contagium vivum*, deprecates the charge of inconsistency for using most of the time at his disposal rather in the discussion of general views or speculations, than with the description of new facts, urging that "to the man who is occupied in investigation the speculative faculty is quite as essential as the critical or sceptical," and that on occasions it may be proper to occupy ourselves with the relations of facts about which we may be supposed to be agreed rather than with the facts themselves.

Just two years ago Dr. Klein electrified the medical world by the alleged discovery of a peculiar fungus found in the substance and on the surface of the mucous membrane over and around Peyer's patches, and subsequently in the veins and lymphatics, in cases of enteric fever. As this fungus preceded the intestinal lesions it was naturally inferred that it stood in a causal relation to the latter. It was seemingly a very significant coincidence that this fungus was identified with the *crenothrix polyspora* which Prof. Cohn had found in well-water in a certain district in Breslau famous for the frequent occurrence of enteric fever among its inhabitants. Here then, to all appearance, we had at length a positive demonstration of the germ theory as applicable to enteric fever, and a satisfactory identification of the exact species of the typhoid fungus. The same eminent pathologist had previously described, in minute detail, the structure and life history of a microphite occurring in sheep-pox, to which he gave the name of *oidium variolæ*. Now the appearances thus interpreted by Dr. Klein have been shown by Dr. Creighton to be nothing more than products of the coagulation of albumenoid matter caused by the preservative liquid (chromic acid and spirits). This is candidly conceded by Dr. Klein himself, who, in a note published in the *Proceedings of the Royal Society* for June 15, 1876, wholly abandons his first position. This incident naturally enough casts a shade of suspicion over his interpretation of the appearances found in the cases of enteric fever, where the same coagulating reagents were employed.

Nevertheless, the doctrine of a *contagium vivum* in some form must be held to be true, though no ocular demonstration of the infective agent has been given, nay even though such demonstration should be forever impossible by reason of the ultra-microscopical minuteness of the particles. The analogies which suggest the hypothesis are numerous and so significant as almost absolutely to exclude a negative conclusion. It is, of course, unnecessary to recite in this presence the facts and arguments which, as stated by Liebermeister, Burdon Sanderson, Lionel Beale, and summarized by our associate, Dr. J. R. Black, are familiar to the members of this Section; but I repeat, that, while they do not suffice to indicate the precise characters of the infective organisms, they leave little or no room to doubt their real existence and active operation in the development of fevers. Dr. Beale has stated some apparently plausible grounds of objection to the doctrine of the fungus-like nature of the contagium, and has elaborated, with extraordinary industry and talent, his own theory of bioplasm particles as the germs of disease; while Dr. Burdon-Sanderson, in a recent paper to which I have already alluded, in comparing contagium to the unformed ferments, and denying, hypothetically, or rather with hesitation and reserve, that it possesses any morphological characters, seems on the point of abandoning the entire doctrine of a living contagium, since, according to the commonly accepted definition of life, viz., that it is organism in action, it would be a contradiction in terms to speak of life where there is no organism. Nevertheless he proceeds to argue that such a ferment cannot be a chemical body, a mere immediate principle, because when it acts on other bodies it maintains its own molecular integrity. "On the whole, it resembles an organism much more than it resembles a chemical body, for its characteristic behavior is such as, if it had structure, would prove it to be living. What is more characteristic of living protoplasm than that, while maintaining its own integrity, it alters the surrounding medium?" In applying these views to the much disputed question of the generation of bacteria, he contends that the objections to the doctrine of heterogenesis are as fundamental as ever, and that while the orthodox biologist need not now hold that every bacterium must have been born of another bacterium, he must yet hold that every bacterium must have been born of something which emanated from another bacterium, that something not being assumed to be endowed with

structure in the morphological or anatomical sense, but only in the molecular or chemical sense. Not only does it fail to exhibit characters which can be appreciated by the microscope, but it is capable of resisting destructive influences, particularly those of high temperature, which are absolutely fatal to the bacteria themselves. He concludes that "so far as the morphologist is concerned, germs have quietly gone out of court and given place to things which are ultra-microscopical—to molecular aggregates—of which all that we can say is, what we have already said about the ferments—that they occupy the border-land between living and non-living things." It will not escape notice that, though this statement asserts the non existence of structure in the morphological sense, it yet claims for contagium the potential characters of life. And after all, the negative proposition may have no other basis than the necessary limits of microscopical demonstration. Dr. Beale, using objectives of $\frac{1}{50}$ of an inch, magnifying from 2800 to 5000 diameters, was enabled to demonstrate the presence of formed particles not exceeding $\frac{1}{100000}$ of an inch in fluids apparently perfectly limpid when viewed under lower powers. (*Disease Germs; their Nature and Origin.* By Lionel Beale, M.D. Second edition, p. 75.) I hold, accordingly, that the theory of a *contagium vivum* has suffered no damage from these recent utterances of Dr. Burdon-Sanderson.

But as already intimated, the special object of this paper is not to speculate on the nature of the infectious material, but to investigate the conditions, especially the external conditions, under which enteric fever is most readily developed. Such conditions are often, nay, perhaps, commonly called *causes* of the disease, and with many persons this has led to no little confusion of thought as to the etiology of enteric fever. Conceiving that a definite effect cannot own more than a single efficient cause, there is a tendency to ascribe the development of the fever to an exclusive set of external conditions. Dr. Parkes, criticizing the theory of Budd, with reference to its exclusive pretensions, remarked that "his mind was of that order which refuses to admit two different origins for the same specific agent, and the undeniable facts of contagion completely override what seems to him the much looser evidence of independent origin." Nor is the ambiguity entirely removed by applying the term "assisting causes" to the influences which favor the development of the specific agent. The latter alone is the cause in the true sense of the

term, inasmuch as it is the one thing whose presence in the organism is absolutely essential to the manifestation of the disease. We shall presently inquire whether this infective agent ever originates spontaneously, but whether this be possible or not, whether it ever has a purely spontaneous origin by heterogenesis, or is always derived from progenitors of the same species, it may still be equally true in either case that some such infectious material is the cause, and the only cause, of certain fevers. But while I freely concede this unity of causation, I maintain that the external conditions which are more or less influential in determining the development of the infective agent, may be numerous and diverse.

Such a conclusion seems fairly warranted by the evidence collected by the late Dr. Derby in his paper on the "Causes of Typhoid Fever as it occurs in Massachusetts," published in Second Annual Report of the State Board of Health, January, 1871. Among the medical men of Virginia the conviction of the soundness of this conclusion is so strong that they are unable to withhold the expression of their surprise at the dogmatic assertion of the exclusive views of recent British authorities. I may be permitted to cite brief extracts from communications received from a few correspondents as representing the general sentiment of the profession in Virginia, where enteric fever has prevailed very largely, both endemically and epidemically.

Prof. L. S. Joynes, Secretary of the State Board of Health, says:—

"I may say in distinct terms, that I have seen no sufficient reason to justify me in accepting the one-sided view of its ætiology which seems now to be so generally adopted abroad, or at least in England. I cannot repress a feeling of astonishment on reading in a book like Bristowe's new treatise on Practice, the broad statement that 'it has been proved, apparently beyond all cavil, that enteric fever is *par excellence* the fever of fecal decomposition; that it occurs only among those who are exposed to the influence of defective drains or foul and overflowing cesspools,' etc. Such an exclusive opinion is, perhaps, natural enough in a great city; but when typhoid fever is studied in rural districts the evidence in its support is found to be sadly defective; and I dare say you will get few affirmative responses from the physicians of Virginia. I have no doubt that you will be furnished with a large amount of valuable evidence on this question which will enable you to

make a contribution to its literature which will tend greatly to repress the prevailing disposition to look at the subject only on one side. This tendency to exclusiveness appears to be innate and ineradicable. At least it has more or less cramped and enslaved the professional mind from the first dawnings of medical science to the present day, and few, even among the leaders, appear to be able to rise above it."

Dr. Harvey Black, now superintendent of the Eastern Lunatic Asylum of Virginia, but for many years a practitioner of medicine in Montgomery County, with a large experience in connection with enteric fever, says:—

"That decomposed excrements, especially when taken in water that has been contaminated, are potent agents in preparing the system to receive the typhoid virus, whatever it may be, I have but little doubt; while I am as fully impressed with the belief that the disease as frequently results without these excrementitious substances having any agency in its production, an opinion based upon the observation of cases where all the circumstances and surroundings precluded the idea that the patients had been exposed to any such influences."

Dr. R. T. Lemmon, of Campbell County, whose communication, based on an unusually large experience, will be again referred to, says:—

"I am disposed to believe that excremental matters contaminating the water or air will originate the disease, but I am also satisfied that there are other causes which have as yet eluded the investigations of scientists."

A striking illustration of this tendency on the part of British authorities to trace the origin of enteric fever to a simple factor is furnished by a recent paper of Dr. Stewart, of Barnsley, originally published in the *British Medical Journal* of March 10th, and copied into *Public Health* of London for March 16th, and the *Medical Record* of New York of May 5th, under the caption, "A New Theory of the Origin of Typhoid Fever." This gentleman, having traced an outbreak of the fever embracing seven or eight cases to the decomposition of blood from a slaughter-house, and assuming that the blame must be laid upon a single factor equally ubiquitous with the disease, sets about to inquire what this one factor present in decomposed blood and decomposed excrements might be. He arrives at the conclusion that the serum of the

blood is this essential factor, typhoid being particularly apt to prevail in the fall months after the privies have received the serous evacuations of patients with summer diarrhoea. "I do not propose," he says, "to enter into the discussion as to whether this disease can have an abiogenetic origin, although the facts upon which my theory is founded appear to favor that doctrine. Whether the fever arises only from the specific typhoid germs, which (according to the advocates of this theory) have a nearly omnipresent existence, and have the property of preserving their vitality in a dormant condition for many years, ready to spring into active and vigorous life when introduced into a proper nidus for their development; or whether the poison is manufactured from the ordinary germs existing in all the putrefactive processes which take place in certain animal fluids, or is elaborated by some subtle chemical change in the properties of the substance itself, does not signify so much to those who have to deal practically with the disease so long as we can put our finger upon the factor, element or pabulum, without which these forces would be rendered permanently impotent."

Now I have to observe that on evidence precisely similar to that on which Dr. Stewart traced some of his cases to decomposed blood, which happened to have an element in common with the discharges of serous diarrhoea, it will be shown that numerous cases may be traced to conditions in connection with which no such common factor can be assigned except by a gratuitous and violent assumption. Moreover, the effect is not coextensive with the alleged cause, and the negative evidence is particularly strong as against so exclusive a theory.

In order to ascertain the grounds for the opinions which prevail in Virginia, I addressed to the members of the State Medical Society, a circular letter, soliciting their views on the subject, and a report of the observations upon which their views are based. It is not necessary to reproduce the whole of that letter, which included the following questions:—

1. *Can you recall any cases of typhoid fever originating under such circumstances as to exclude the probability of the importation of the disease from some other locality? If so, state the facts.*

The replies were: Yes, 58; No, 10; Doubtful, 1.

Statements of facts to justify affirmative answers will be cited further on.

2. *What conveniences exist in such places for the deposit and final disposal of excrements?*

The answers to this question are so varied as to defy all attempts at a definite classification. In a majority of the cases the arrangements consist of privies without pits. In many such privies the excrements being under cover, and yet exposed to the winds, become desiccated and but little offensive. They are removed periodically to a compost heap, or scattered upon soil in process of cultivation. In other cases there are pits generally walled with brick on the sides, but not at the bottom, so that the liquefied contents are constantly soaking into the soil. In a few cases the water closet system is in effective operation, the soil pipes opening into sewers, which have their outlets in some small running stream. In a great many cases there are absolutely no special conveniences, the male members of a family resorting to the woods, the females using chamber vessels, which are emptied into a brook if one be close at hand, or into the garden, or stable yard.

3. *What is the source of water supply? If wells are used, what is their proximity to privies, cesspools, stables, pig-sties, etc.?*

In most of the rural districts of Virginia, the water supply is obtained from springs, and generally is of unexceptionable quality, but the instances are also quite numerous in which the supply is obtained from wells in suspicious proximity to privies, stables, pig-sties, etc., and sometimes receiving the surface drainage from kitchen yards.

4. *Have you observed any connection between the prevalence of typhoid and the saturation of the soil with organic impurities, though the water supply might be adequately secured from the possibility of contamination?*

The replies to this question were: Yes, 17; No, 27; some of which will require specifications to be given presently.

5. *Have you observed any connection between the prevalence of typhoid and a low level of the water in the wells?*

Seventeen (17) correspondents answer affirmatively. Thirty (30) had not noticed the state of the wells, and two (2) reported that the water was unusually high.

6. *Have you observed anything like an antagonism between typhoid and malarial fevers, or do you know of any well-observed facts, which*

seem to show that they prevail separately and alternately in given localities?

To this question there are sixteen (16) more or less positive answers in the affirmative. Thirty-five (35) correspondents answer No. Twenty (20) ventilate the controverted relations of typho-malarial fevers, some affirming the existence of the typical enteric element, and, therefore, denying the alleged antagonism, while others earnestly repudiate the doctrine of a hybrid product, and regard the fever in question as nothing more than an adynamic form of ordinary malarial fever. The correspondents who constitute this latter class range themselves on different sides of the question under consideration, most of them conceding the fact of the alleged antagonism, some by express statement to that effect, others only by implication resting upon their assertion of the extreme rarity of typhoid in malarial regions.

7. Do you know the history of any outbreaks of typhoid fever presenting the features of a wide-spreading epidemic?

The replies are: Yes, 14; some of which, to be presently cited, seem to me to be conclusive. No, 41.

Each correspondent was requested to state his own views as to the causation of enteric fever, which elicited the expression of a great variety of opinions touching the secondary causes, the writers in most instances avowing their belief in the existence of a specific germ as the essential cause of the infection, though, as will be presently seen, a very large majority consider that the infectious agent may be developed *de novo*.

CONTAGION.—As many as eighteen correspondents express a conviction of the contagiousness of enteric fever, but in most cases no distinction is made between pure contagion or the direct transmission of the morbid agent from person to person, and that indirect transmission which Liebermeister terms miasmatic-contagion, where the morbid agent is developed outside of the body, but requires as an antecedent condition that an infected person should furnish specific germs. This is an important distinction, and should be borne in mind in order to a proper interpretation of facts, which may otherwise present an ambiguous appearance. The oft-cited report of Dr. Austin Flint of the outbreak in the village of North Boston, in 1843, is so far from proving the disease to be contagious in the limited sense just defined, that the details given are much more consistent with the

idea of some change in the infectious agent having taken place subsequent to its separation from the patient's body. In other words, the actual facts would seem to show that the influence was that of miasmatic-contagion, rather than simple contagion. The first case following the imported one was twenty-three days after the arrival of the stranger, and there was reason to believe that the poison was received by this and other parties subsequently infected, through the medium of drinking water obtained from a well within three or four rods of the privy, in which the contents were allowed to accumulate, and in which the dejections of the sick stranger who imported the disease had been certainly thrown. This is the view now taken by Dr. Flint himself, who, in a paper published in the first volume of the Transactions of the American Public Health Association, "On the Relations of Water to the Propagation of Fever," recites the history of the outbreak in question, and refers the secondary cases to the contamination of the water brought about in the manner stated.

A few, however, of my correspondents, adverting to the distinction in question, have avowed their belief in the occasional propagation of enteric fever by direct contagion. In most of these cases there was no post-mortem verification of the diagnosis, but as the primary cases originated in rural districts where, it may be safely assumed, there had been no antecedent exposure to the contagion of typhus fever, there is every reason to believe that the diagnosis of typhoid was correct.

Dr. Fred. Horner, Jr., of Fauquier County, reports the following on the authority of Dr. Chilton, an experienced practitioner of Warrenton:—

"The corpse of a young lady who died after a brief illness with symptoms of typhoid fever, in a neighboring county, was brought to the residence of her grandfather near Warrenton to be buried. The remains were kept in the house over night preparatory to interment the following day. The coffin was opened by request of an aunt. The latter kissed the remains, near which she lingered for some time. She became ill in a few days with all the symptoms of typhoid, though previously in excellent health, and died two weeks after commencement of her illness. A brother of the deceased and several other members of the family took the disease, but recovered."

In a later communication Dr. Horner, after having obtained further details from Dr. Chilton, states that another aunt of the

deceased who had nursed the latter during her illness in Essex County, and had accompanied the corpse to the paternal home in Fauquier, was seized with the fever a few days after her arrival, but recovered. If it could be proved that the other lady derived the infectious agent from the living sister rather than from the corpse of her niece, the fact would be yet more significant as an evidence of direct contagion, since it might be considered that after death the body might furnish a *nidus* not materially different from the usual external vehicles of the infection.

Our associate, Dr. L. D. Waterman, of Indianapolis, Ind., while accepting the theory of "a contagium from enteric discharges modified in some unknown way after evacuation," is yet disposed to concede that the fever may sometimes be propagated by direct contagion. He reports the following case:—

"Suddenly, without ascertainable cause, the servant girl of a noted Methodist minister was attacked in this city July, 1874, and then successively his child 5 years old, himself, his son about 11 years, and his wife. His daughter aged 14 alone escaped, and she was sent away during the prevalence of the sickness. A privy of the usual out-house kind, a cesspool, twenty feet from the well, in filthy condition of decaying fulness, were the only discoverable factors. There is one fact in connection with the local outbreak in this family that is of some significance. His residence was the west half of a double brick building. The other half was occupied by a family who used water from the same well, but had no attack of fever. They also used the same privy vault, but by a partitioned off building. They also had a cesspool separate. It is hard to believe that either privy or well was the source of the disease, after the escape of the latter family. I cannot absolutely prove, but believe, that this disease was directly contagious, and owing to the periods of successive attack, was communicated from one to the other, not from water or privy."

Dr. M. G. Ellzey, in a very interesting and instructive communication, expresses the belief that the fever, after originating in some unknown way, may be communicated from person to person. He says:—

"Two of my sisters were seized about the same time by sharp attacks of typhoid (typical enteric cases) in midwinter, living in a most healthy and remote country home, snowed up and not having visited even a neighbor for six weeks, nor received a

visit or held any communication with persons off the place. There were no other cases at that time in the family, on the farm, or within ten miles of the place, had not been for six weeks before, nor was there one within six weeks afterwards. Water was used from a spring situated in such a manner that no possible drainage from any foul situation could reach it without reversing the natural law that water runs down hill. Every circumstance was closely and repeatedly scrutinized, and after reflected upon by me, without my ever finding out a possible cause for these cases. I admit that their nearly simultaneous occurrence was strongly suggestive of a common origin, and with this on my mind I searched most minutely into every circumstance connected with the surroundings of these ladies, with complete knowledge of the entire premises, and all the domestic arrangements of the family. These are the only cases that have occurred on that place in the past thirty years. For a period of fifteen years, with about sixty souls, white and black, dwelling there, no physician paid a professional visit. It is not unlikely, I think, that the second case, attacked within three days of the first, nevertheless received the disease from companionship with her sister, the period of incubation in this case being very short. I have seen many cases where the disease in this typical form has very evidently been communicated by companionship with the sick. In view of all the facts I have reached, in my own mind, the conclusion that the first of these ladies generated within her own system the virus producing the fever, and communicated it to her sister by their intimate association."

The following cases reported by Dr. G. W. Semple of Hampton are, some of them at least, strongly suggestive of direct contagion. In the first case the patient having been exposed, while suffering with the prodromic symptoms, to a cold rain storm about the middle of August, 1850, by which he contracted a circumscribed pneumonia, the existence of the enteric fever was at first overlooked, but subsequently recognized, suspicion being first excited by the disastrous effects of depletive treatment. Under a change of treatment convalescence eventually set in, but before it had made much progress a subsequent accidental exposure to a cold draught from the bursting in of the window of his room at night during an equinoctial storm caused his death. The narrative of Dr. Semple continues as follows:—

"The next son, 11 years old, was taken fourteen days after his brother first sickened, and after an attack with like symptoms became convalescent in five weeks. At this date having now no fever and only slight looseness of the bowels, I deemed it safe to remove him. Had him under my own inspection well bathed, put on him clean new clothes made for him by his mistress at my house, put him on a new bed with new bedding into a spring wagon, took him to my house and had him lodged in his aunt's, my cook's, room. His aunt's daughter spent much time with him, and in a few weeks was attacked with typhoid which lasted several weeks. It was impossible to keep her many cousins belonging to my sister-in-law, hired in Hampton, from visiting her, and each one who did had typhoid fever. Each was taken from the place where hired to my kitchen (*in Virginia this term is often applied to the entire house of which one of the rooms is appropriated to cooking uses, the others being dormitories for servants*) for treatment, and no other cases were traced to them. Many of these were mulattoes, on all of whom was seen the typhoid eruption. The mother of the first two patients, about the 1st December, went to live at a place where until then there had been no case of the fever, and in a few days thereafter her next son had the fever. The mother being cook, her sister was put to nurse the boy, and her children by frequent visits to her contracted the fever, and from them it extended first to the children of another sister, mother of a large family, and from them to all the young negroes above the age to commence the second dentition, thirty-eight in number, and last of all to one of the sons of the master of the family, who, contrary to the injunctions of his father and mother, frequently went to his nurse's house where her children were sick. This little boy was kept during his illness in a large well ventilated room in the second story of the mansion, the other members of the family occupying only the first, and the fever did not attack any other member of the white family, but it did not expend itself (among the blacks) until about 1st May, 1851. In July it made its appearance on the adjoining farm, Mr. Lowry's, commencing with a negro girl of about 20 years, and by October had attacked all the young negroes on the farm, twenty-seven in number. No white person except the youngest daughter of the proprietor had the fever, a young lady who had been most indefatigable in administering medicines and food to the sick.

"I had no idea how the disease had originated at Mr. Boncewell's, where the first mentioned case occurred. His house, as well as that in which the patient lived, was scrupulously neat and clean. The water was the best in the country, with no source of contamination near, the pig-sty, the stable, and the barnyard being at a considerable distance on a declivity from the house and well, and the night soil, soapsuds, and slops were regularly emptied upon the bank of stable manure near the stable. The fever however had evidently started from that as a focus, and had extended by infection to those in my kitchen, from them to others in Hampton, and from the same focus to my brother's-in-law. But I could not trace it thence to Mr. Lowry's, as the negroes all denied that there had been any communication. There was no local source of origin for the disease, no stable, pig-sties or cess-pool near the residence, and the excrements, etc., were all emptied into the salt river. The father of the girl who was first to have the fever on this farm, had in his house a mattress said to have been got from the beach near Old Point in January, 1850, which had been thrown overboard from a German emigrant ship wrecked there, and the man said that the father of the first mentioned patient had got one of the same mattresses before the fever occurred in his family. This the latter denied, however, and, as the mattress had been used in the family for so long a time before the fever appeared, I paid little attention to it at the time, but I am now inclined to think that the statement of the former was true, and that the latter denied it for fear of the ridicule of his master. There was no other source from which these two patients could have contracted the fever, except from these fomites. . . .

"All the facts of my own observation indicate very strongly that typhoid fever is a specific, infectious, exanthematous fever, having but one source of origin *from the person of the sick*. And I incline to the opinion that such is its only source, notwithstanding all my reading on the subject. I have seen some cases of a second attack of smallpox and many of measles, but never one of typhoid fever."

Dr. W. A. Thom, of Eastville, Northampton Co., reports that his region, a very malarious one, is remarkably exempt from typhoid fever, and says: "In a practice of thirty-five years I can recall but one epidemic of pure typhoid fever, and that was brought here by a young lady who had been at school in Alexandria. From her it spread through three families, evidently by conta-

gion, as it passed successively through those who had been engaged in nursing her."

Specifications given by other correspondents in evidence of contagion admit of another interpretation. They make no distinction between contagion and infection. Indeed, in most cases it is obvious that the writers merely meant to affirm that the fever is miasmatic-contagious, the communication requiring an intermedium other than the air between the infecting and the infected persons, involving too some change in the infectious material.

It must be admitted that evidence, such as has been cited, is far from being conclusive. Viewed by itself it suggests the idea of contagion as a possible, or even not improbable, solution of the problem, but viewed in connection with the overwhelming amount of negative evidence, the argument is shorn of much of its strength. Physicians and nurses, who attend enteric fever patients, are not more frequently attacked than other persons who have never seen such cases. "Up to the year 1865," says Liebermeister, "I have never seen, in the hospitals which I visited (Greifswald, Berlin, Tübingen) a single hospital patient, physician, or nurse attacked with typhoid fever, although such cases are placed in the general wards. Other observers have had the same experience. According to Murchison, during a period of fourteen and a half years in the London Fever Hospital, 2506 patients with typhoid fever were treated, and during that time only eight originated in the hospital." Now contrast this with the results of the unequivocal contagiousness of typhus fever. In Ireland, according to the same authority, in the year 1847, no less than 500 medical men—about one-fifth of the entire number—suffered from typhus, and of these 127 died. "In the Crimean War, at the height of the epidemic among the French, out of 840 attendants in twelve hospitals, 603 were taken sick during a period of 57 days; more than 80 surgeons died of the disease in the course of the campaign."

It was this very epidemic in 1847 to which Stokes, in his Lectures on Fever, refers as furnishing proof of the contagiousness of fevers in generals, as he denies the specific distinction between typhus and typhoid.

Thirteen correspondents by express statement, and very many more by implication, maintain the non-contagiousness of typhoid, on grounds similar to that taken by Murchison and Liebermeister.

ter, while most of the number concede its infectious nature and admit that the infection may, in some cases, be remotely derived from human living beings.

Dr. Rob. B. Tunstall, of Norfolk, says:—

“Were I to form an opinion based upon my own experience, I should exclude contagion as a mode of propagating typhoid. I have had it in my own family five times, and on each occasion confined to a single case. Three of these originated out of the city and two here.”

Dr. Wm. Selden, of the same city, speaks yet more emphatically to the same effect.

“In an experience of nearly fifty years, I cannot recall a single case where the disease was imported from any other locality. During this long period I have seen a few cases of typhoid fever every year, never more than ten or twelve in any one year, and all of them were manifestly of local origin. My father’s experience, with which I am so familiar as almost to regard it as my own, was to the same effect. The disease prevailed here more or less every year, even in the last century under the name of typhus mitior, or nervous fever, the name typhoid not being then in use. I have never met with a single instance where the disease appeared to be due to contagion. I have never used any precaution against contagion other than such cleanliness and ventilation as were required for the sick, and yet I can recall but three instances where more than one case occurred in the same house in the same season. In one instance two brothers were attacked simultaneously; and in the other two instances both cases were exposed to the same cause of disease, namely, a foul cellar. My practice has been, for the most part, in the better class of society, where strict attention was paid to cleanliness, especially to the changing of bed-clothes and the prompt removal of all excrementitious matter.

“I am well satisfied that in our city to which my observation has been limited, typhoid fever has never exhibited any sign of contagion, although the evidence of its being occasionally at least propagated from person to person in Europe, seems irresistible.

“My opinions are based upon very long, if not very extensive, observation, and although chiefly negative may be of some interest as opposing the tendency which in the present day is constantly increasing, to regard almost all diseases as contagious. I

have alluded to the opinions of my father, and would like to mention, as a proof of his accurate clinical observation, that he was in the habit of 'feeding fevers' before Graves was born. One of the first lessons in practice that he ever taught me was, that you could not cut short a continued fever by active treatment, and that the best chance of getting the patient safely through it was to sustain him from a very early period by suitable nourishment and wine. One of my earliest recollections is being fed, during an attack of typhoid fever when I was five years old, with Naples biscuit (dry sponge cake) dipped in good old Madeira."

Dr. Rob. T. Coleman, Professor in the Medical College of Virginia, at Richmond, bears a similar but somewhat qualified testimony. He says:—

"I can recall no case where the evidence was at all conclusive that the disease was imported from some other locality. On the contrary, I have known, during the war, in a large number of instances, individuals affected with typhoid fever to be removed to points more or less remote from the localities where it was contracted and scattered broadcast in farm-houses; and although no rigid restrictions were imposed as to their intercourse with the members of the various households that ministered to them, yet I cannot now recall a case originating unmistakably from the presence of these patients in the families that entertained them. While this is true in my own observation, it is proper to state that there is reason to believe that in a few cases within the experience of others the disease was contracted by contagion.

"I will mention, as typical of a large class, the case of my brother Lewis, who, while engaged in nursing a number of students with typhoid fever at Concord Academy, where he was aiding my uncle Frederick in teaching, contracted the disease and in its incipency rode in a carriage about twenty-five or thirty miles to my grandfather's. Here he was extremely ill; at one time it was thought that his death was inevitable from the excessive hemorrhage from the bowels. Yet no other case occurred although there were about seventy-five or eighty people on the plantation, a number of whom were variously employed about him as nurses, general attendants, etc. etc. My conclusion, then, is, that although this disease has many features analogous to the manifestations in the exanthemata, the *taches rosées* and the inflammation of Peyer's glands being its local expression, corresponding to the eruption

in measles, scarlet fever, etc., and although, like them, one attack usually guarantees immunity from subsequent ones, yet that as a rule typhoid fever is *very mildly contagious*, and that we have to look for some endemic or epidemic cause to account for its extensive prevalence."

Doubtless the "contagion" in such cases is usually mild, as it requires a concurrence of conditions and is indirect, the poison having to undergo a change after its liberation from the diseased symptom, which change may be wholly prevented or its extent be greatly diminished by proper sanitary precautions. In the exceptional cases, if there be such, in which the disease has appeared to have been propagated by direct contagion, a plausible though purely conjectural solution of the anomaly may be found in the assumed fact that, in the advanced stages of the disease, when the intestinal glands are in the state of ulceration, and the intestinal excretions are retained by opiate or other astringent treatment, the disease germs may find a suitable nidus for their further development before their discharge from the bowels, and thus supersede the necessity for the intervention of external pythogenic influences.

While, therefore, the evidence of direct contagion in the case of typical enteric fever is inconclusive and at best is confined to a few exceptional cases, the proof of indirect contagion by means of air or water contaminated by the dejections of fever patients is such as to leave no room for doubt. It is only the narrow exclusiveness of Dr. Budd's theory—its denial of other modes of propagating the poison—which has subjected it to a damaging criticism.

If it were necessary at this date to furnish proof of the fact that a case of typhoid imported into a previously healthy locality may, under favoring conditions, produce a local outbreak of indefinite extent, numerous conclusive observations might be cited from the mass of correspondence in my possession. But inasmuch as this point is seldom contested, and as my space is limited, I will cite a single instance which may be taken as a sample of many others. Dr. Z. I. Walker, of Brownsburg reports that:—

"During the summer of 1859, there were thirteen cases of typhoid fever at Mr. Bratton's, in Bath County. His son John was a medical student of the University of Virginia. He became sick with typhoid fever at the University, went home and died; twelve other cases in the family followed rapidly. Persons who

visited the family contracted the disease. There were no cases in that portion of the county. There never was a case in the family before."

Indeed, as already stated, as many as ten of my correspondents, most of whom declare that their experience has been large, have had little or no difficulty in tracing every outbreak that has come under their observation to a clearly ascertained importation. *Per contra*, fifty-eight (58) correspondents assert that they have observed cases which originated under such circumstances as absolutely to exclude the probability of importation of the disease. These gentlemen accept, therefore, the doctrine of the *de novo* development of the fever; a doctrine which is not without prominent advocates even in Great Britain, where the theory of Budd is so popular. Thus Dr. Murchison's theory of a pythogenic origin of the enteric poison has many advocates, and Sir Wm. Jenner in discussing this question in 1875 said, "My own prejudices are greatly in favor of the specific origin of this in common with all the contagious diseases. I have long advocated their specific differences, and that each has its specific cause, *i. e.*, a cause which is unable to produce any other specific disease. If each could in every case be referred to emanations from those previously sick of the same disease; it would tend greatly to strengthen the general views I have long held and advocated. I am prejudiced, therefore, in favor of this opinion. I hope it is true; but then I must say that at this moment the weight of evidence and argument is rather on the other side." He suggested as the best mode of settling this question "*to thoroughly scrutinize every isolated case that occurs in out-of-the-way country places.*" In accordance with this suggestion Dr. Cornelius Fox investigated twenty-seven isolated cases, only three of which were *clearly* traceable to pre-existing cases of this disease. Excluding also sixteen others respecting which there was a trace of doubt, there remained "eight decidedly untraceable cases, four of which apparently originated from excremental pollution of water, and the remainder from excremental pollution of air." He cited also the particulars of an outbreak of diarrhœa, of only a few days' duration in most of the cases, which occurred in a school at Croydon, under charge of Dr. Carpenter, due to the contamination of a well by simple sewage, where there had been no antecedent case of fever." And yet, though most of the boys were affected with

simple diarrhoea, three had typhoid fever with distinctly marked rose-colored spots and of twenty-one days' duration.

On this point the testimony of most of the fifty-eight correspondents who gave an affirmative answer to the first question of the circular is very emphatic. A few examples may be given:—

Dr. C. Duffey, Newbern, N. C. "Yes. A number of cases in which after most careful inquiry the probability of importation was excluded."

Dr. C. J. O'Hagan, Greenville, N. C. "Nearly all the cases which I can recall since the war, seemed to originate spontaneously. No importations could be traced. Last spring (1876) in a family residing in the county, one of the children, a school-boy of twelve years' was attacked while at school near his home with typhoid. None of his school-mates had the disease. It attacked every member of the family but two. No local cause could be discovered."

Dr. S. Putnam, Montpelier, Vermont. "According to my observation, during a somewhat extensive practice for thirty years or more, typhoid fever has more generally occurred as an epidemic from August to November, particularly in dry seasons, after a succession of yellow, smoky days, *without storm, winds, or electrical display*, the beds of streams and ponds having become dry, or half dry and stenchy, and vegetation parched or shrivelled. Under these circumstances, constituting, as I conceive, an epidemic influence, typhoid fever has often soon appeared, more frequently in families residing on the banks of streams or ponds, also, often in the farm-houses scattered over the hills, here and there, without any possibility of tracing the importation of the disease, or of its spread from house to house by contagion."

Dr. L. B. Edwards, Richmond. "A farmer who lived eight miles from Lynchburg, near the base of the Tobacco Row Mountains, had not for months before his attack been where there was sickness. For several weeks just prior to his attack he had been engaged personally in clearing and plowing some 'new grounds' on the side of the mountain. There was nothing in the history of the case that indicated an importation of the disease."

Dr. W. H. Bramblett, Newburn, Va. "I can recall a number of cases occurring under such circumstances as to exclude the probability of importation. In a thinly settled country, as that to which my practice is most restricted, there are many isolated neighborhoods cut off by mountains and streams from other por-

tions of the county. The families composing these communities never have a visitor from a distance; they rarely go out of their own neighborhood and never out of the county, in fact occupying a position so cut off from communication with the rest of the world that the appearance of the disease in their midst would appear irreconcilable with the modern theory of its mode of propagation. Some of these neighborhoods I have had under observation for ten years, and it is here that typhoid fever often seems to have its beginning, and can be traced to none of the ordinary sources of contagion agreeably to the modern theory. The same community is never visited on the year following an outbreak, and new outbreaks occur in communities which have not been invaded for ten years, or never before at all. I propose to report many of these cases in full in a paper which I have in hand to be communicated, when finished, to the *Virginia Medical Monthly*."

Dr. W. H. Macon, Hanover Co., Va. "Mrs. E., living in the county of New Kent, had typhoid fever for thirty or forty days, convalesced, relapsed, and died. She had not been anywhere to contract the disease; had not left her farm, certainly not the neighborhood. No other case occurred. This case must have originated there, if I may use the word, spontaneously, and several other cases similar to this might be mentioned."

Dr. A. W. Fontaine, New Canton, Va. "I have repeatedly seen isolated cases of the fever in different families at the same time, and at different times, which gave rise to no suspicion of importation."

Dr. W. B. Conway, Blacksburg, Va. "I had here in my practice among the citizens of this town (none of them students of the college) fifteen cases last year, and two now under treatment which could not be traced to importation. They originated here from local causes. These were, first, two heavy floods in rapid succession that swept through the valleys on each side of the town and left the grounds adjacent to the streams covered with vegetable and fecal matter; the fecal matter being washed from many privies as well as other places of deposit. All this matter was exposed to the hot August sun and the exhalations from it were carried by the breeze, generally blowing up the valley, over those parts of the town where this fever was developed. The second cause co-operating with the first, was the filthy condition of privies and premises generally, as well as inattention to personal cleanliness."

Dr. C. S. Morton, Appomatox Co., Va. "The majority of the cases I have treated seemed generally to originate spontaneously, but were generally followed by a number of cases in the same family, and in some instances were clearly communicated to friends from a distance coming to visit the patient. Privies are used chiefly by the female members of the white family. But I have known many cases to arise where there were no privies and where it was natural to suppose that there was not any accumulation of the excrements near the dwelling sufficient to contaminate the atmosphere appreciably. I am told that typhoid fever was a great scourge fifty or sixty years ago when there were no privies used in the country as a general thing. The increased use of privies at the present day has not been attended by any increase of the disease, but rather a marked decline. The water supply is obtained almost exclusively from springs."

Dr. S. W. Dickinson, Louisa Co., Va. "I am attending now a negro girl with typhoid, who, so far as I can learn, has had no chance to contract the disease from any other person; but her brother died with typhoid in the same house about ten months before she was taken with the disease. This is the only case I can recall where anything was known personally about the chances of contracting the disease."

Dr. W. H. Daughtrey, Southampton Co., Va. "I do not remember having met in my practice with any cases of typhoid which I thought were imported, but invariably attributed their origin to some local cause. My practice has been in the country where privies are generally located sixty or seventy yards from the dwelling. The excrements are not generally removed therefrom. The water supply is derived chiefly from wells. I have never, that I remember, attended a case of typhoid where the well was less than thirty or forty yards from the privy, stable, or pig-sties."

Dr. R. L. Sandford, Westmoreland Co., Va. "Twenty years ago I treated a case of typhoid which could not be traced to importation. It was the only case in the family, and no other existed in ten miles of the place. Since then I have had many cases similar. No special arrangements for the deposit of excrements. The water supply in this section (tide-water) is ample, but mostly from wells, which are generally remote from privies, stables, pig-sties."

Dr. H. Black, formerly of Blacksburg, Va. "In the winter

and spring of 1875, I treated five cases of typhoid in the family of a farmer near Blacksburg. There was no typhoid disease prevailing in the neighborhood at the time, and there was no circumstances that caused me to believe that it was imported from any other locality. These patients were visited by the members of two other families living respectively a mile and two miles distant and in opposite directions. In each family two cases occurred. In two of these, and in all of the five cases, there was an unusual degree of irritation of the bronchia of a spasmodic nature. In June following, and six miles from the above-named residence, a broad body of table-land and some ridges intervening, and the watercourses running in opposite directions, a young man, the son of a farmer, was attacked with typhoid fever, and died at the end of five weeks from hemorrhage of the bowels. The father was attacked in about three weeks after the son, and died after an illness of between four and five weeks. Another son who lived a mile off, and who had been nursing his father and brother, was taken sick, went home, and barely recovered after an illness of many weeks. In all but the last case, undue bronchial irritation existed. At this place, as at the former, the excrements were deposited about the out-houses. Moreover, the yard at this place, but not at the former, was filthy from being run over by hogs, dogs, and fowls. The pig-sty was close by, and there was a damp dirty cellar beneath the house. The water was from a weak spring, and was slaty to the taste. The spring was protected from the surface drainage, which, however, flowed around it as it came from the yard on the hill-side above. No case of typhoid had been known in the family before.

“Had I access to my books I might, no doubt, recall many cases where there was not the slightest reason to suppose that the disease was imported from other localities, an experience which I think will accord with that of most physicians who have treated it in the more sparsely settled and especially mountainous sections of the State.”

Dr. Bedford Brown, Alexandria, Va. “I have many reasons to believe that typhoid fever may be generated *within* the system spontaneously. In my own experience cases have originated on high isolated points where the locality was sterile, the drainage most perfect, the water pure and limpid, gushing out from rocky mountain sides. In one of these instances, the building was new, made of wood, airy and comfortable, supported on pillars two or

three feet high, underneath which the pure mountain breezes had free access, and the situation was on a high mountain point without the vestige of a local cause in the form of privy, sty, cesspool, inclosure, decaying animal or vegetable matter. Yet in this building were two well-marked cases of malignant enteric fever following in rapid succession. Neither of these victims had left the premises for weeks previously. . . .

"Thus apart from all contagious and infectious influences, there may exist such a combination of circumstances as want, exposure, hardship, mental depression, and it may be others, as to generate enteric fever. . . .

"I furthermore am impressed with the conviction that typhoid fever, as it exists and prevails in different localities, is due to a difference in its etiological factors. . . .

"I have never been converted yet to the theory of Louis and his followers that typhoid fever was strictly a specific disease arising from infection or contagion, from the mere fact of the existence of a fever which usually runs a definite course towards restoration, with depressed vital action and ulceration of Peyer's glands and diarrhœa. Pneumonitis might be entitled to the claim of specific character on the same principles."

Dr. W. S. Stoakley, Northampton Co., Va. "The cases of typhoid fever which have prevailed here exclude all probability of importation, and appear to originate from causes entirely local, or from some peculiarity of the atmosphere in antagonism to that which produces malarial fevers. The abundance of space in the country makes it very easy to get rid of excrements, and it is very rare that there is impure air from such causes, unless it arises from compost heaps with which earth is mixed, or from the field where it is spread broadcast. We see this type of fever prevailing when every thing like manure is gathered up from the premises (even woodpile refuse) and carted out upon the fields and mixed with earth."

Dr. Alban S. Payne, Fauquier Co., Va. "I remember as far back as the winter of 1834, that a gentleman of wealth, living four miles west of Warrenton, proposed to emigrate with his negroes, during the coming spring, to Mississippi. In the winter months a strange malady broke out amongst his negroes, and by the following spring he had lost thirty by death. This was typhoid fever. There was no sickness anywhere else of a serious character in a hundred miles of this plantation. It was a very

dry winter, and the springs on the plantation were never of the best. In all other respects the topography was good. Privies are seldom used by these hardy mountain people, and excrementitious matter is generally dried in the sun or carried off into watercourses by the rains."

Dr. J. S. Apperson, Smythe County, Va. "To none of a group of cases of typhoid fever which occurred in my practice between the 8th of April and 12th of October have I been able to assign any satisfactory cause. They seemed to be sporadic in the nature of their origin, and gave rise to no other cases of a similar kind. Three lived in an elevated region, a mile or two back from the section where the disease was most prevalent, but had no communication with those there affected. The remaining case seven or eight miles distant, and did not infect others."

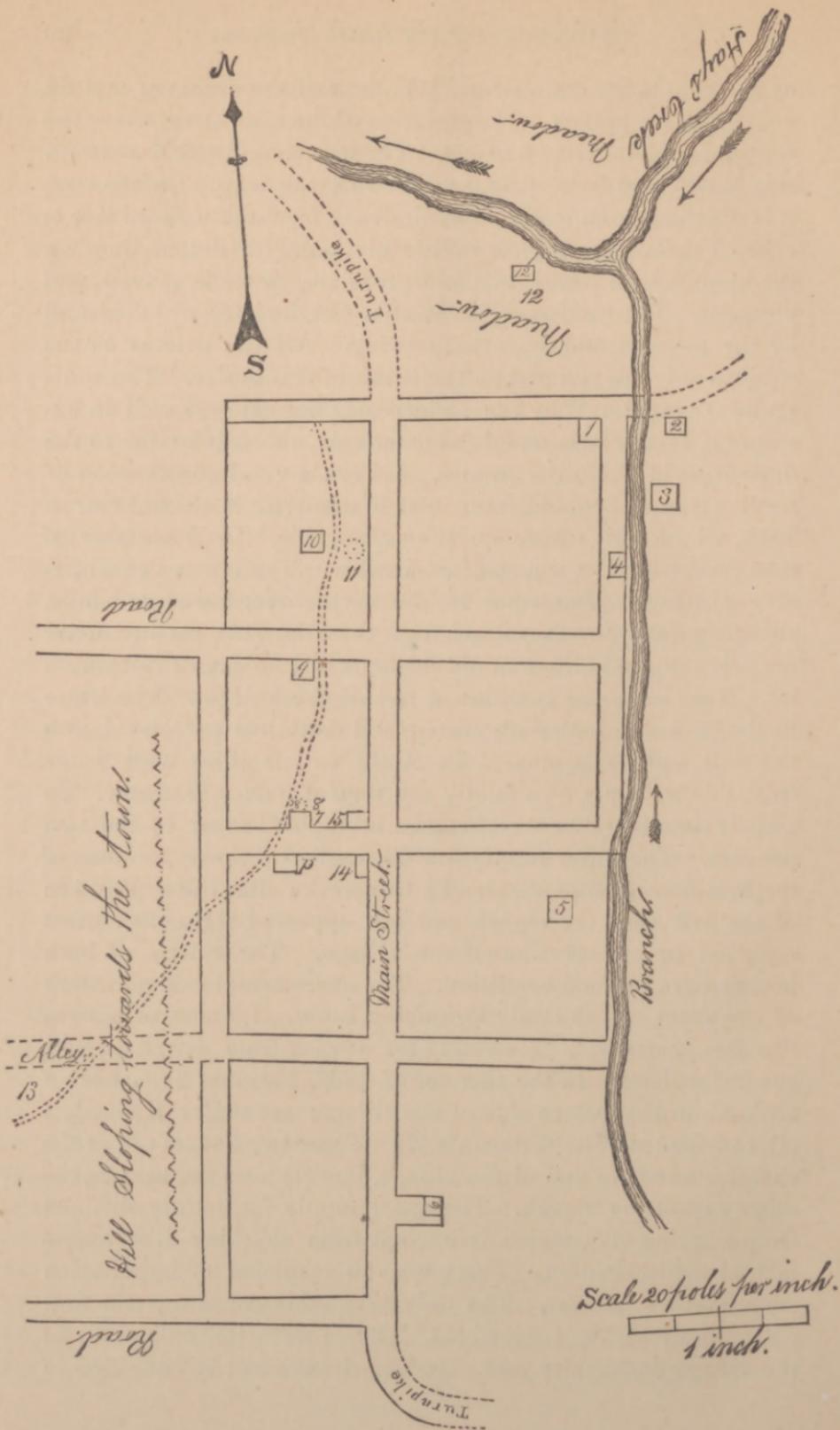
Dr. Benjamin Lee, Philadelphia, Pa. "The following facts in regard to an epidemic of considerable proportions which occurred in the town of Wilmington, Delaware, in the spring, summer, and autumn of the year 1848, may be of interest. Although I cannot say with positiveness that the possibility of importation was excluded, yet the general opinion of the physicians of the place was that it originated there *de novo*, in consequence of a large, deep, and wide excavation of the principal street, left open for a considerable time, undertaken for the purpose of laying new water mains. The disease prevailed more extensively, and was more fatal in the immediate neighborhood of this excavation than elsewhere. Excrements were deposited in cesspools, and removed by scavengers. Water supplied by pumping from Brandywine Creek into a reservoir may have suffered some contamination from manufactories higher up the stream, but had never produced typhoid before."

Dr. W. G. Rogers, Charlottesville, Va. "The first cases of typhoid fever that I saw occurred in Northern Albemarle, at Mr. D.'s farm. His dwelling was upon a flat, about two hundred yards from Buck Mountain Creek. He owned about seventy-five negroes, whose cabins were upon the same flat, which was a little more elevated than the spring, and from fifty to one hundred yards from it. As there were many negro children, and each family raised one or more hogs, the premises were very filthy, and so located that the rains, doubtless, washed the filth into the spring used by both whites and blacks. There were forty-five or fifty cases of typhoid among the negroes, and three

or four of the whites also had it. The most of these were sick at the same time, and *as there were no cases in the neighborhood, and it could not be traced to importation or contagion*, it was believed that the water used for drinking was poisoned by fecal matter washed into the spring, whilst the air was rendered impure by emanations from the same, and that these probably acted as exciting causes of the outbreak of the disease, which was of a very malignant form, and proved very fatal. But that this could not have been the only cause is proved by the fact that the same filthy state of the premises had existed for at least a quarter of a century. The cabins had long been old and dilapidated, and yet this was the first outbreak there, or in that neighborhood. Though this supposed exciting cause had thus existed there for years, the fevers that had hitherto prevailed were the bilious remittent, and more rarely the intermittent, whilst the diseases generally were highly sthenic, requiring active depletion. One or two free general bleedings, which would now kill a patient with pneumonia, then cured the disease. Upon the advent of typhoid fever all diseases became asthenic, requiring a tonic and stimulating treatment. There must, therefore, have been a general atmospheric cause that did not before exist, which was necessary to render the exciting causes of typhoid effective, and gave an asthenic character to other diseases."

Dr. Z. J. Walker, Rockbridge Co., Va., whose account of a local outbreak of typhoid fever in the contiguous county of Bath, arising from an imported case, has been given above, reports several cases occurring in his own county, where there was absolutely no ground to suspect importation. "Brownsburg is a small village on the turnpike leading from Staunton to Lexington. The situation is a dry one, no marshes in ten miles, the country is rolling, drainage good. The village occupies a plateau, bounded on the west by a range of hills; on the east there is a rapid descent to a branch which skirts that side of the village. This branch is parallel to the main street, and about seventy-five yards from it. There are but two rows of houses, one on each side of said street. The population is about two hundred. I have resided here as a practitioner for eighteen years. During this period I have treated in this village about thirty-five or forty cases of typhoid fever, *all of which, except five cases, occurred on the western side of the main street*. Not a single house on that side escaped. Only two houses on the eastern side were affected. The turnpike

on the map is the main street. On the east the drainage rapidly goes off to the branch. The elevation of the main street above the branch is about thirty-five feet. The water supply for the eastern side is obtained from *springs* marked on the map. On the west it is obtained from *wells*. The drainage from the western side is effected through a shallow ravine (marked with dotted lines on the map), which passes in close proximity to wells, privies, and pig-pens. This ravine drains an alley (marked 13), which is used by the negroes and boys as a privy. All the privies on the western side are situated on the brink of this ravine. The wells are also near it. Why the eastern side has enjoyed such an exemption, I have been unable to determine, unless it be due to the difference in the water supply. I have a vivid recollection of eight cases of typhoid fever, which occurred in the autumn of 1865, all of which were on the *western* side. In September of that year, we had a wet spell of weather. There was a considerable overflow. The water in the ravine overflowed its banks, sweeping down the excrement from the alley (13), passing under my privy (P), and filling an old ice-house (7) on James Withrow's lot. This ice-house is within a rod of the well (8). The water in the ice-house soon disappeared, and doubtless percolated into the well and contaminated it. Only two families used water from this well, my own family and that of James Withrow. In a short time there were eight cases of typhoid fever in the two families. The water supply was changed after the appearance of the first four or five cases. In two weeks after the appearance of the first case the eighth and last appeared. No other cases occurred in any portion of the village. The cellars of both houses were in good condition. The excremental contamination of the water was the only assignable cause. I, alone, of my entire family escaped. I suffered for a week from diarrhœa, and general malaise. In the summer of 1866, I treated five cases of typhoid on the eastern side of the village; one at Mrs. Kennedy's (1), and four at Wm. Withrow's (2). These two houses are at the extreme northern end of the village, one (1) near the branch, the other east of the branch. The water supply for both is obtained from a spring (12), which is exempt from anything like excremental contamination. There was no suspicion of importation from another quarter. The only assignable cause was the foul emanation from the tanyard (4). These were the only cases in the village during the year. Indeed, I have had but one case in



the village since that date. There is not much travel through Brownsburg. There is no hotel in the place, and travellers do not stop with us. None of the persons attacked had been away from home. In March, 1875, I had three cases at a house on Walker's Creek, near the base of the North Mountain. This is a thickly settled portion of the county. The three cases occurred within a period of a week. There was no reason to suspect importation. The family, consisting of seven persons, had moved into the house about six weeks before the outbreak. The house is on a little bluff of about twelve or fifteen feet elevation. The spring issues from beneath the bluff. The privy is behind the house on flat ground, from which the water during a rainy season does not readily run off, but sinks into the soil. I suspected excremental contamination, and ordered that the water supply be changed. Before the occurrence of these cases there had been a heavy rainfall."¹

Dr. C. O'Hagan, Greenville, N. C. "I have known a single severe case to occur in a family where no importation had been possible; where the hygienic conditions were good, and where in a large family no other member suffered, and where it was impossible to attribute it to fecal contamination of either air or water."

Dr. John Wilson, U. S. N. "During my term of service (1872 to 1875) at the Chelsea Hospital, we had an occasional case of fever from the Marine Barracks. The men thus affected had been under punishment by confinement in prison; room situated in the basement. The disease could hardly have been imported, as the prisoners were less abroad than their companions. It was not probably communicated by the water, as the prisoners used the same kind of water as the other men. It could not have been

¹ EXPLANATION OF MAP OF BROWNSBURG.

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|---|--------------------------------|--|
| No. 1. Mrs. Kennedy's. | No. 5. Spring. | No. 8. Well, just back of the ice-house. |
| " 2. Wm. Withrow's. | " 6. " | " 9. Pig-pen. |
| " 3. Tanyard. | P. Privy. | " 10. " |
| " 4. Spring. | No. 7. Old ice-house. | |
| " 11. Well, about 2 poles from pig-pen. The dotted lines (----) in the western part of the town indicate the natural drainage of the high country immediately west of the town (ravine).
The courses of main and parallel streets are N. 25½° W.
The cross streets run N. 64° W.
The length of the village is about 90 poles.
The width of the village is about 43 poles. | | |
| No. 12. Spring (in meadow) used by Mrs. Kennedy and Wm. Withrow. | | |
| No. 13. Alley, used as a privy by the town boys and negroes. | | |
| No. 14. My house. | No. 15. James Withrow's house. | |

by the general atmosphere to which all were about equally exposed. The floor of the prison rooms is considerably lower than the surface of the adjoining street, and on the opposite side of the street is a hill, occupied by dwellings of the poorer sort, so that probably the soil about the rooms is in some degree infected with decomposing human excreta and domestic filth."

Dr. F. B. Watkins, Richmond; formerly practised in Prince Edward Co. "I can recall many cases which originated under such circumstances as to exclude the idea of importation. These cases originated at home. The majority were negroes living in *old log cabins*. So certain was I that the fever originated in certain cabins that I ordered them to be *burnt*, not simply evacuated. I doubt not a vast amount of evidence could be arrayed to establish the convictions of physicians and of owners of slaves, that certain houses were the *fons et origo* of the fever. And the family being removed and the houses burnt the progress of the disease stayed."

Dr. T. B. Greer, Franklin Co., Va. "I have witnessed two epidemics of typhoid fever and have the facts of another, which prevailed almost in the same locality, and were confined to a space of fifteen miles north and south, and about ten east and west, lying just under the Blue Ridge, where the disease originated spontaneously. The last of these epidemics occurred in the fall of 1876. The first occurred in 1842 before I entered the profession. Ever since that time there has been more of the disease in that locality than in any other part of this or the adjoining counties. This location is on the slope of the Blue Ridge, the finest watered and the best drained locality in the State. Gradually undulating, without wells, cesspools, or decomposing organic matter except forest vegetable matter, this locality is also thinly settled and all excrementitious matters are effectually and promptly washed off. The soil, of necessity and in fact, is less saturated with organic impurities than other parts of the county where the disease prevails only sporadically. It is my opinion that here, at least, the disease is attributable to local telluric influences which are yet enveloped in obscurity, and to which the investigations of the profession will eventually turn to find the first cause. There were over one hundred cases last fall in this locality. I did not see a single case outside of it, and only heard of one. I have noticed for several years that this same locality has been remarkable for the prevalence of diphtheria and

erysipelas. I do not think that there are half a dozen wells in that district, and many of the cases occurred in neat, newly settled places. In other respects it is remarkable for pure water, healthy atmosphere, and the comparative absence of all other diseases."

Other citations will be given in connection with the assignment of special conditions. For the present let the foregoing be taken as a sample of the affirmative conclusions of fifty-eight correspondents as to the possibility of the spontaneous development of typhoid fever. It will not have escaped notice that in some of the cases the possibility of importation does not appear to have been wholly excluded by the facts as reported, but in most of the others the evidence viewed by itself seems irresistible. A strict logic requires, however, that all the objections to such a conclusion should be duly considered, and some of these are sufficiently cogent. Stokes's argument founded on the comparatively rare occurrence of typhoid in the filthy cabins of the Irish peasantry and analogous facts occasionally observed throughout this country are not easily reconciled with Murchison's theory. Here is a case in point:—

Dr. John A. Graham, Lexington, Va., writes: "There is a large spring at the lower end of the town of Lexington. The ravine, or hollow, running up from the spring is occupied with negroes and others in shanties and tenement houses, with hog-pens and privies never clean. Now, if it can be proved that the filth from the ravine communicates with the channel of the spring, what a splendid place for typhoid fever we have here. The water is used by all the people in that neighborhood for drinking, cooking, etc. Some years ago a dyeing establishment was started on the ravine, among the shanties, say seventy-five yards above the spring. The dye water thrown out from the back of the house, passed through the soil, and came out at the spring coloring the water. I think the town authorities had the dyeing establishment removed. The privies and the hog-pens remain. I have watched that locality for years with an eye to typhoid fever, but not a single case has occurred."

This is not an isolated case. Many other similar facts are cited by other correspondents, and it is well known that Dr. Budd and others who advocate his theory of continuous development cite numerous and well-authenticated instances in which pythogenic influences failed during indefinitely long periods of time to de-

velop fever until a case imported *ab extra* was brought in to initiate the zymosis. Now, I say that such facts as these cannot be without significance. They are not to be summarily dismissed on the ground that positive proof in a few cases outweighs any amount of negative evidence. If purely pythogenic influences suffice of themselves to generate the disease they should produce this result with some degree of uniformity and not exceptionally only. What then is the true significance of the facts cited by the advocates of a *de novo* development? Why simply this, that the disease may appear under favorable conditions where importation by human intercourse or by fomites has been pretty certainly excluded. The unknown factor which, in addition to the pythogenic influence, may be necessary for the development of the disease, may be specific germs conveyed otherwise than by human intercourse, or exhumed from the soil where they may have been deposited at some long anterior period, being preserved meanwhile in a state of dormant vitality, or possibly multiplying and undergoing developmental changes differing from those which take place after their introduction into the living body, or finally, the disease germs may, as Lionel Beale supposes, be formed in the system under suitable internal and external conditions. Thus, according to any of these suppositions, either of which may be legitimately held as a provisional hypothesis, the apparently spontaneous development of typhoid fever neither requires the abandonment of the doctrine of a *contagium vivum*, nor drives us to the alternative of accepting Bastian's doctrine of heterogenesis. On the contrary, the supposition of a wide atmospheric diffusion of disease germs serves to explain and harmonize the otherwise irreconcilable observations of Budd and Murchison.

The limits of the vitality of disease germs, and of their capacity of diffusion through the atmosphere in clouds of definite dimensions, have not been ascertained, but it is in accordance with analogy to believe that they may resist destructive influences which would be fatal at a more advanced stage of development, and be capable of transportation to great distances in a desiccated state without losing their capacity for development. It has recently been ascertained that grains of wheat, and even of Indian corn (a semi-tropical plant), left several years ago by Arctic explorers in a region where the temperature scarcely, if ever, rises above 50° below zero, have yet retained their power of germination.

Limiting thus the import of the phrase "*spontaneous*" or "*de novo*" development of typhoid fever, we may now inquire what external conditions seem to be potential in determining this development. The views of several correspondents on this question have been given in connection with the citation of their opinions as to the general fact, but we may now proceed to indicate *seriatim* the varied conditions which, on evidence more or less satisfactory, may be considered to exert an influence in determining the prevalence of typhoid fever. The limits of my space will compel me to withhold a large part of the evidence in my possession with reference to the several heads.

(1) Typhoid fever may be developed by excremental filth, undergoing moist decomposition with the result of contaminating the local atmosphere, and especially the water supply as derived from wells, springs, rivers, or rivulets, into which the excremental matters have been introduced by surface or subsoil drainage.

This proposition is so well established on evidence familiar to all students of sanitary science, that I consider it unnecessary to enlarge this paper with further proof, but will simply say that strong confirmation is afforded by observations reported in communications from Dr. E. M. Hunt, Chairman of this Section, Dr. Denison, of Denver, Colorado, Dr. Brandeis, of Louisville, Ky., Dr. H. B. Baker, of Lansing, Michigan (Contributions to the study of the cause of typhoid fever in 4th Annual Report of the Secretary of the State Board of Health of Michigan), Dr. Putnam, of Montpelier, Vt., and Drs. Ellzey, Fautleroy, G. W. Briggs, Macon, Basham, Baskerville, Cunningham, Madison, S. P. Christian, and others of Virginia.

(2) Several correspondents refer to reported outbreaks of typhoid fever, in England and in this country, which were traced beyond the possibility of mistake to drinking milk which had become contaminated by the use of foul water in washing the milk cans, but none of these cases fell under their own observation.

(3) The influence of vegetable decomposition in determining the development of typhoid fever is maintained by a number of correspondents, as—

Dr. R. L. Sandford, Westmoreland Co., Va. "In conversation with a physician of this county some years ago, he related a circumstance which strengthened my opinion in favor of vegetable decomposition as a cause of typhoid fever. One member after

another of a family was seized with typhoid, and all the first cases proved fatal. Upon examination he found in the basement of the house a quantity of turnips and cabbages in a state of decomposition. He immediately had them all removed, and the place cleansed. The cases then on hand recovered rapidly, and there were no more afterwards."

Dr. R. W. Martin, Pittsylvania Co., Va. "Six cases of typhoid occurred eight miles north of Chatham on the line of the Virginia Midland R.R., when this road was in process of construction. The locality had several years before been a favorite spot for the development of typhoid. These cases were undoubtedly genuine typhoid. The subjects were citizens of different counties, and were persons of respectability. None of them had resided in the vicinity less than three months. The location was an eminence overlooking a large mill-pond to the west. The water from this pond supplied two mills, and consequently during the day ran down very low, leaving a large surface of mud mixed with decaying vegetable matter exposed to the sun's rays, and surrounded by a large area of saturated soil."

Dr. W. B. Gray, Richmond, Va. "About two miles north of Bowlesville, in Fluvanna, there was a millpond, and for two miles north of that I had numerous cases of typhoid fever. Not far from the head (back) waters of this pond there was a chalybeate spring, which was visited by two young ladies living a mile east of the pond. They resorted to the spring a little before sunset. Both of them had typhoid fever, while no other member of the family had it this year, though about one hundred persons, white and colored, lived upon the premises. (In 1855, it prevailed extensively in this and in the neighboring families.)

. . . . From the best observation of which I have been capable during a practice of twenty-five years, including twenty years in the country and five in this city, I am satisfied that *vegetable decomposition* is, oftener than animal, responsible for typhoid. Whenever I see a prolonged spell of dry weather followed by heavy rains, in midsummer or early autumn, experience has taught me to expect typhoid or other form of fever."

He states the somewhat remarkable fact, ascertained by inquiries made on the spot by himself and Dr. C. W. Brock, that the butchers of Richmond, who for the most part live in a particular quarter on the outskirts of the city, seldom if ever have

a case of typhoid in their families or on their premises where the slaughtering of animals for the market is carried on.

Dr. C. C. Conway, Rapidan, Va. "The house in which a young man, æt. 19, resided at this place, was inundated during the great flood of 1870, and after the water subsided there was a heavy deposit of organic matter in the yard and house. This was scraped together in large heaps and left to lie exposed to the hot September sun. In about eight days he had slight premonitory symptoms. Subsequently the fever was fully developed, and he died about three weeks thereafter."

Dr. Baker's "Contributions to the Study of the Cause of Typhoid Fever," referred to on a preceding page, contain a notice of an outbreak of typhoid fever in the township of Cannon, Kent County, Michigan. The cases occurred in the immediate vicinity of Malone Lake, which had been drained and lowered six feet during the summer, exposing about sixty acres of muck land formerly covered by water. Dr. Chamberlin, the health officer, gave it as his opinion that the cause of the outbreak was the effluvia from decaying vegetable matter.

(4) Several correspondents make a distinction between vegetable decomposition as commonly exhibited and the decay of *dry timber*, ascribing to the latter a far more potent influence in the production of typhoid fever. Dr. R. T. Coleman, of Richmond, if not the originator, is the most earnest and zealous advocate of this doctrine. He states the grounds of his conviction, as follows:—

"A large number of slave owners of my acquaintance have been struck with a terrible outbreak of typhoid fever in their old decaying negro cabins, so malignant at times that new cabins were built and the old ones burned before the disease could be stamped out. I recollect hearing my former colleague, Prof. Beverly Wellford (President of the Amer. Med. Association in 1853), mention a very striking illustration of the above. While the occupants of the old cabins had typhoid fever those in the newer cabins escaped. They drank the same water and were fed from the same kitchen. Nor was the soil around the old houses any more saturated with organic impurities, for, under our old Virginia system of farming, all accumulations of filth were regularly scraped up, and hauled off to enrich the land. There was nothing but the decaying cabins to account for the disease. So

thought the owners, so thought their doctors, and they acted on this hypothesis with success.

"There was in this city, just before I settled here, a wagon-yard which had become excavated by the friction of the wheels and the trampling of the horses. The owner was a builder, and in a dull season he concluded, in order to give occupation to his hands and increase his revenue, to build on this lot. It being inconvenient to haul earth to fill up the excavation he had shavings brought from his shop, and these mixed with the soil served as the foundation of the house and its surroundings. Of some twenty people who lived in that house in the following five years, six had typhoid fever caused as they and I believed by the decaying shavings.

"When the war came on I went as a surgeon to Western Virginia in the Lee and Loring campaign. When our little army reached Valley Mountain, in Randolph County, two regiments occupied all the 'cleared land,' while the rest of the command had to camp in an adjoining forest. In this so-called cleared land the trees had been 'belted,' and had for years been slowly rotting. Many of them in various stages of decay bestrewed the ground. We were at Valley Mountain about two months, and in this time 80 per cent. of one of the regiments on this cleared land, and about 25 per cent. of the other, had typhoid fever, while of the regiments camped in the forest not more than 5 per cent. were attacked. What caused the difference? Decaying timber, I say—there is no other explanation. In physique, morale, equipment, the 21st Virginia regiment was certainly equal to any of the others. We had substantially the same water, the same rations, and the fever commenced its ravages before there was time for soil contamination.

"May not this mode of clearing (by belting trees and leaving them to rot standing) account for the fact that the mountains of Virginia, which in my boyhood were the great fountains of health and strength, have become the *nidus*, the most congenial home of typhoid fever and its congeners?"

Similar views founded on similar observations are expressed by quite a number of correspondents, but with especial emphasis by Dr. Charles Duffey, of Newbern, N. C., and by Dr. A. G. Tebault, Dr. C. W. P. Brock, Dr. C. R. Cullen, Dr. R. L. Madison, and Dr. F. B. Watkins, of Virginia. The two last-named gentlemen refer to the expedient, often resorted to in local outbreaks

on farms in Virginia, of burning the old cabins before the progress of the fever could be checked. There seems to be little or no room to doubt the existence of a morbid influence derived from the old cabins, but that the factor in question is decaying timber is not, I think, equally certain, although the collateral evidence given by Dr. Coleman renders his conclusion not improbable. But in a few cases of this kind, which have fallen under my own observation, another and very potential factor was discovered in a mass of sludge, which had been formed under a very badly jointed floor by the drippings of slops, and possibly, to some extent, of the excreta of children. In these cases there was thus soil contamination of the worst sort.

(5) The influence of a soil saturated with organic impurities in determining the development of enteric fever is affirmed by seventeen correspondents. Facts stated in this connection by several of these correspondents have been already cited in the preceding part of this paper.

Dr. S. W. Dickinson, of Louisa Co., Va., has, like myself, noticed the influence of this factor in connection with old negro cabins. He says:—

“Typhoid fever was very common here prior to the emancipation of slaves, when they were often crowded in ill-ventilated houses that had stood for years in the same place with the most favorable chances for saturation of the soil with organic matters, but since the negroes live in the woods in great measure, and in new log houses on new sites, that or other causes have diminished the frequency of the disease.”

Dr. R. C. Bowles, of Columbia, Fluvanna Co., Va., also refers to a similar exemption of that section of the State which has been observed since the war, but, not admitting any improvement in the houses of the negroes, he excludes local causes altogether.

“Since the war, or for a period of ten years, if there has been a case of well-marked enteric fever in this township I have not seen or heard of it. How to account for this exemption I do not know. Among the negro population the disease was especially fatal. To recover was the exception, though, as slaves, they were better fed, better clad, and better housed than now. So far as I can say, from personal observation of the disease, it was not due to any local contamination of air or water, but was truly epidemic.”

Dr. A. M. Fauntleroy, of Staunton, Va., refers to an outbreak

of typhoid fever during the summer of 1876, which was due, he thinks, to the saturation of the soil with organic matters, although in this case the wells also were probably more or less contaminated.

"Goshen stands upon ground which forms a *natural basin*, readily receiving through the porous soil the drainage of the surroundings. The soil was saturated with organic impurities and other filth. *Prima facie* one might be led to ascribe the outbreak to the soil impurities alone, but from the lack of all measures to protect the wells, I am strongly inclined to the opinion that it was due to the impregnation of the water supply with the specific poison deposited in the latrines. The water level in the wells was below that of previous years, because the rainfall during the year had been less. The distance from the well to the latrine is about *fifty feet*."

(6) Of the seventeen (17) correspondents who have observed a connection between the prevalence of typhoid and a low level of the ground water, only one or two enter into any particulars, the others simply affirming the fact of the coincidence. Where particulars were stated they commonly referred to a very foul state of the water in the wells, and by implication the contaminated water was charged with the production of the fever. Whether the surface or subsoil from which the ground water, previously high, has receded, is more potential in developing fever than a wet soil, cannot be decided from the evidence in my possession.

(7) That the spread of typhoid fever may be connected with some undefined *telluric* influence not necessarily dependent on soil-saturation is asserted by several correspondents, who cite some very significant facts in justification of this opinion. I refer particularly to the remarkable testimony of Dr. T. B. Greer, already cited, respecting the extraordinary prevalence of typhoid fever in a part of Franklin County, Virginia, where the usually assigned causes would seem to be absolutely excluded.

I have already given the testimony of Dr. Benjamin Lee, of Philadelphia, with reference to an outbreak of fever in Wilmington, Delaware, apparently caused by a deep excavation in some of the streets. Similarly Dr. F. D. Cunningham, of Richmond, states that in 1873 there was a decided increase of cases of typhoid fever in that city concurrently with extensive excavations in the streets.

Dr. R. S. Payne, of Lynchburg, Va., in an extremely interesting

communication, recalls the changes produced in the medical topography of that city and its surroundings by the construction of the James River and Kanawha Canal, which was commenced in 1831.

“The low grounds between the hills and the river banks were watered by springs from the hills, and as the river bank was higher than the foot of the hill the low grounds were necessarily swampy in character, and until the grounds were drained by the canal we had ague and fever along its line every summer and fall. While the canal was in progress of construction, the laborers engaged in this work, as well as those upon the farms for twelve or fourteen miles below Lynchburg, suffered severely with congestive chills. If the patient did not die during the third or fourth chill, the case generally became complicated with severe diarrhoea, the fever assumed a continued type, frequently marked with tympanitis and delirium. After the newly lifted earth had been exposed to the winter's frost and a summer's sun the miasmatic features were greatly abated, and the fever now generally assumed very distinctly the characteristic features of typhoid. Still, on the same farms, and sometimes in the same families, you would see during this epidemic a well-marked case of chill and fever, but if not arrested within five or six days, and especially if the patient took an aperient, however mild, a diarrhoea would set in, the intermission would be less and less marked, and ultimately the fever take a typhoid character. This peculiarity was confined to the population residing on the river. About two and a half or three miles from the river typhoid fever broke out on a plantation with a large family of negroes, and many died. There was no malarious complication here. The only chill I saw was produced by myself in sponging a patient with cold water, with a view of reducing the high temperature. A chill came on by the time I had sponged the face, neck, and one arm, and the patient died in less than two days.”

(8) The relations between typhoid and malarial fevers constitute a very important subject of investigation, in regard to which various and diverse opinions are entertained.

A considerable majority of my correspondents who notice this subject at all, adhere to the common opinion that these two distinct types of fever have each its distinct specific cause, and both may prevail at the same time in the same place. A small proportion of this class of correspondents believe further, that the

two diseases may coexist in the same individual as a hybrid, constituting one of the varieties of the so-called typho-malarial fever, the other varieties representing hybrids between malarial fever and typhus, or between malarial and relapsing fever. For what appears to be very conclusive clinical and pathologico-anatomical evidence of the correctness of this view, I must refer to Dr. Woodward's paper on Typho-Malarial Fever, read in the Section on Medicine of the International Medical Congress. Nevertheless such a position is earnestly controverted by many Southern physicians, whose residence in malarious regions gives them opportunities for observation on a large scale. Many of these observers contend that the so called typho-malarial fevers are simply malarial fevers with adynamic tendencies. A similar opinion has been recently pronounced by Dr. Loomis, of New York, who ascribes the adynamia to pythogenic influences, which cannot, he thinks, cause enteric fever in the absence of a specific germ.

Another class of correspondents report observations which lend a certain degree of plausibility to the doctrine of an alleged antagonism between typhoid and malarial fevers. That this antagonism is not absolute is conclusively shown by the occasional coexistence of the two diseases side by side; and, as I believe, as a hybrid in the same subject. Nevertheless, that enteric fever is far less common in malarious regions than elsewhere, and that its frequency increases in these very regions during periods in which there is a suspension of malarious influence, are, in my judgment, well-established facts.

The late Dr. Elisha Bartlett quotes approvingly the following passage from the London *Lancet* of March, 1847, as the conclusions to which M. Boudin had then recently arrived. "Those localities in which the producing cause of endemic intermittents thoroughly modifies the constitution are remarkable for the infrequency of phthisis and typhoid fever. The localities in which phthisis and typhoid are particularly prevalent are remarkable for the infrequency of intermittent fevers contracted on the spot. The drying up of a marsh, or its conversion into a lake, diminishes or prevents intermittent fevers, but seems to dispose the organism to a new series of diseases, in which phthisis and typhoid fever according to the climate are particularly prominent. After a residence in a thoroughly marshy locality, an individual enjoys an immunity from typhoid fever, the degree and duration of

which are in direct proportion to the length and degree of the exposure."

Even Dr. Woodward, while maintaining that the unknown causes of the periodical fevers and of typhoid exist side by side, and often coexist in the same person, calls attention to the fact, which he justly characterizes as very important, "that intermittent and remittent fevers often disappear more or less completely from neighborhoods in which they have long prevailed and are replaced by typhoid fever."

At first view this may seem easily intelligible. A newly settled and badly drained region in a suitable climate has all the conditions to generate malaria, and by the time the beneficial effects of drainage and tillage are felt, the contagium arising from human beings will be sufficiently diffused to give rise to endemics of typhoid fever. Dr. Stormont, of Topeka, says, "We have no typhoid in Kansas—why I don't know. Has not this been true of all the Western States in the first few years of their settlement? It was certainly true of Illinois."

But Dr. Woodward goes on to say: "But let me next observe that the change thus effected is not always a permanent one. Often in individual years the intermittents and remittents reappear in epidemic-wise in regions such as I have described, and then the typhoids vanish for a time, to return once more when the temporary prevalence of the periodical fevers comes to an end." My correspondence supplies abundant and emphatic evidence of the truth of this proposition, as will appear from the following extracts of letters:—

Dr. Charles Duffey, Newbern, N. C. "The fact that we so rarely see typhoid in our malarious regions, notwithstanding the constant existence of acknowledged predisposed causes would prove this (namely, the alleged antagonism). The year 1874 presented a larger number of typhoid cases than I have seen for years, and yet we had unusual immunity from malaria. In 1872 the conditions were exactly reversed."

Dr. S. W. Dickinson, Louisa Co., Va. "Prior to 1873, typhoid was the prevailing fever of this section, and malarial fevers were quite unknown. In 1872-3-4, malarial fevers became *very common*, and we still have some few cases every summer. During this time typhoid has been almost extinct, *except when originating from imported cases*. The sentiment of other physicians questioned on this point is that there is an antagonism between typhoid and

malarial fevers, and I think my observations on that point warrant such a belief."

Dr. R. T. Lemmon, of Campbell Co., Va. "I have had frequent occasion to mark an apparent antagonism between typhoid and malarial fevers. In the year 1845 this section of the State was fearfully scourged by a typhoid epidemic. The backbone or ridges suffered more severely, while the malarial region on the banks of the Staunton River and some large and old mill-ponds escaped entirely, the usual amount of ague prevailing there. The line of demarcation was very apparent. Subsequently I have seen cases of typhoid within the malarial region, but no cases of intermittent at the same time. I have noticed another fact, that the cases of typhoid occurring in malarial districts were milder."

Dr. F. B. Watkins, Richmond, Va. "So extremely rare were cases of typhoid fever in my field of practice in the valley of the James, in Goochland County, where malarial fevers were positively epidemic for some years (I saw 52 cases in one season in one family of 80 persons), that when I went to the county of Prince Edward and encountered my first cases of what everybody called "the fever," I felt great and serious embarrassment, and often invoked the aid of professional brethren before maturing my diagnosis. This was about 20 years ago, in 1857. I can truly say that I had seen no such cases on James River. I saw a medical friend to-day from James River, and found that his experience coincided with my own. I have conversed with several physicians, who, like myself, have settled here (in Richmond) since 1865, and they all concur in the correctness of my observation that there is a seeming antagonism between typhoid and malarial fevers."

Dr. J. C. Green, of Danville, Va. "Since the war there have been but few cases of typhoid fever anywhere within the range of my practice, and indeed, since 1869, I have not seen a single case, nor has a single one occurred within the town limits or immediate neighborhood. This is the more remarkable, when the rapid increase of population has almost transformed a village into a populous city, with all its sewerage necessities, while no efforts have been made to provide sewers, or in any way to remove the enormous accumulation of excrement and decaying organic matter, which are permitted to contaminate air, earth, and water. But while enjoying complete exemption from typhoid fever, we are scourged with malarial fevers. Within the last

year there have been quite a number of cases of typho malarial fever, which have been confounded with typhoid, but to any one familiar with both diseases, the cases have been sufficiently easy to distinguish, and warrant me in affirming that we have not had a single case of enteric fever since 1869. I am well aware that the facts I have stated are directly opposed to the ordinarily accepted etiology of typhoid fever, but 'facts are stubborn chieils, and winna be driven,' and while we have the ideal conditions for typhoid outbreaks, we enjoy the most perfect immunity. In this same region, typhoid fever had previously existed for a number of years, first appearing in 1845, when it supplanted malarial fevers, to be itself supplanted by them at a later period. Elder practitioners, who witnessed the disappearance of bilious remittent fevers, graphically describe the appearance in 1845 of the new enemy, and the ill success which attended their first efforts to treat it."

Dr. R. W. Martin, of the same county, gives a strongly confirmatory testimony. Similar opinions, on similar grounds, are expressed by Dr. Brandeis, of Louisville, Kentucky, and by Drs. Fontaine, R. W. Nelson, Stoakley, Flournoy, S. P. Christian, Powell, Z. B. Herndon, Adkins, Barham, and others, of Virginia. *Per contra*, a more or less decided negative opinion is expressed by a much larger number of correspondents, who consider that the existence of the two classes of fever side by side, in the same locality, at the same time, refutes the idea of the supposed antagonism between them. Unquestionably, this fact does disprove the theory of an absolute antagonism, but, nevertheless, there remains the curious fact that in regions permanently malarious, cases of enteric fever are very rare, relatively to the non-malarious regions of the State, the difference being simply enormous, and that in less malarious regions, where periodic fevers occasionally prevail, there is often observed an alternation between their prevalence and that of typhoid.

Most of those who controvert the first branch of this statement, and affirm the frequent prevalence of typhoid, assume that all the typho-malarial cases are pure typhoid slightly modified in some of its manifestations by coexisting malaria, whereas, this is true only of a part, and perhaps but a small part of the cases, the others being, some of them, hybrids between malarial fever and other forms of continued fever, and some simply malarial fevers with adynamic complications.

We have not sufficient data on which to form a conclusion as to the explanation of these facts. It might be conjectured that the *contagium* of periodic fevers requires a saturated soil for its effective development, while that of typhoid when derived directly from the earth, which sometimes would appear to be the case, finds more favorable conditions in a comparatively desiccated state of the soil, if not for its own germination, at least for its liberation and passage into the atmosphere. Very many of my correspondents advert to the fact that typhoid has prevailed more extensively in seasons of drought generally succeeding previous heavy rainfalls, and a similar observation is made by Dr. Bourdon, Médecin de l'Hôpital de la Charité, at Paris, who gives, in the April number of the London Lancet of this year, "a sketch of the recent epidemic of typhoid fever" in that city. He states that in the months antecedent to the outbreak, the rainfall was below the average proper to this period of the year, and that the exacerbation of the disease distinctly coincided with the dryness and the heat. He concludes that the lowering of the level of the water in the sub-soil exposed a whole series of putrid foci, and that the high temperature was well adapted to induce fermentation, and at the same time to favor the diffusion of the effluvia produced, by causing an unusual amount of evaporation.

(9) The English theories of the causation of typhoid fever exclude the idea of an epidemic influence in the old Sydenham use of that term as opposed to a purely local influence. In a loose way, this term is often of late applied to outbreaks manifestly due to local causes, provided their sphere of operation be somewhat extended. It is believed by many, however, that the infective principle of enteric fever may occasionally spread by atmospheric currents in a definite direction, and invade successively various points along the line of its transit, quite irrespective of human intercourse.

Of 56 correspondents who advert to this question, 41 state that they have never known enteric fever to prevail otherwise than from local causes, while 15 are well satisfied that it does occasionally exhibit the characteristics of a true epidemic. Many of the former have had so large an experience that their negative testimony should have great weight, unless overruled by undeniable and unmistakable positive observations. Such observations are, however, not lacking. The late Dr. Elisha Bartlett, who was particularly careful not to confound the typical typhoid

with any other form of adynamic fever, stated that it often prevails over wide ranges of country, so generally and so extensively as to assume an epidemic character, and that in this epidemic form it becomes migratory in its character, wandering about the country, attacking one neighborhood this year, another the next, and so on. Similar statements are made by several correspondents. I cite a few specifications.

Dr. R. T. Lemmon, of Campbell Co., Va., who has been already mentioned as having had an unusually extensive experience in the study of this disease says: "I have witnessed an outbreak of typhoid fever which had all the characteristics of a wide-spreading epidemic; no local cause could have had anything to do with its existence, and its propagation seemed entirely independent of a personal medium. No condition of things exerted any influence in securing exemption or mitigating its virulence, *except malarial surroundings*. Very few families living within the bounds of my practice escaped, and of the families invaded by it, the proportion of individuals attacked was 50 to 80 per cent. of the whole, and in some instances every one had it. It came from the west, and its progress, though rapid, could be distinctly traced eastwardly."

Dr. Lemmon adds that he has been practising medicine for thirty-eight years, has seen more or less typhoid every year, appearing several times in the same families, sometimes after an interval of twenty years, but he has never known one to suffer a second attack.

Dr. F. B. Watkins, of Richmond, states that "in the years 1860, '61, and '62, typhoid fever exhibited the features of a wide-spreading epidemic in the counties of Prince Edward and Charlotte."

Dr. W. G. Rogers, of Albemarle Co., says: "In 1847-8-9, typhoid fever prevailed extensively throughout the larger portion of Albemarle and adjoining counties, and, it is believed, in the greater portion of the Piedmont region in Virginia. It continued to prevail in less degree for several years, and again increased about the years 1856-7-8. It has prevailed more or less down to the present time, and its epidemic influence still renders asthenic nearly all other diseases."

Dr. R. W. Nelson, now of Charlottesville, but for many years a practitioner of medicine in the counties of Hanover, Goochland, and Powhatan, a little above the level of tide-water, adverting

to the alternation of typhoid and malarial fevers, so distinctly marked as to induce him to believe that there is some sort of antagonism, mentions that on the subsidence of periodic fevers, after the fall of 1849 until 1866, typhoid frequently prevailed, not usually in circumscribed localities, but epidemically.

Dr. J. H. Griffin, of Salem, Roanoke Co., states that "typhoid or enteric fever first appeared in our region of country at Jonesborough, Tenn., in 1841 or 1842, where it was reported to have been extremely fatal. It slowly travelled in this direction, stopping at certain localities, devastating whole families and neighborhoods, then skipping over a country, sometimes miles in extent, it reached this place in the summer of 1844."

In a later communication, Dr. Griffin referring to some outbreaks in succeeding years, admitted that they might be possibly due to local causes, but contended that no such explanation would apply to the outbreak of 1844, which he regarded then, and still regards, as a true epidemic. In the course of the following year (1845) this epidemic, proceeding eastwardly, crossed the Blue Ridge and invaded the counties of Campbell, Pittsylvania, etc., as already stated in the letters of Dr. R. T. Lemmon and Dr. J. C. Green, relating to the antagonism of malarial and typhoid fevers. A similar statement is made by Dr. Martin, of Pittsylvania C. H., on the authority of his father, Dr. Chesley Martin, who remembers that the epidemic was preceded by one of intermittent fever.

It thus appears, by the concurrent testimony of several competent observers, that a fever having the clinical characters of the typical enteric fever prevailed over large areas of country in different parts of Upper and Middle Virginia, at different times in the interval between 1844 and 1866, generally succeeding a like prevalence of periodic fevers, which, in a very great degree, subsided on the appearance of the typhoid affection. I have not a shadow of doubt as to the accuracy of these statements, and while I would do nothing to discourage any efforts to stamp out this formidable disease, I am constrained to say that the conditions of its development in the rural districts of Virginia are too numerous and varied, to permit us to hope to accomplish such an end by exclusive attention to the removal of the products of either animal or vegetable decomposition. There are yet many unsolved mysteries with regard to the conditions of the development and spread of this fever, and it is to be regretted that the exclusive ascription of its generation to the influence of putres-

cent sewage contaminating the air we breathe or the water we drink should be permitted to divert the attention of competent observers from other less easily assigned, but possibly equally influential agencies.

In this inquiry into the causes of enteric fever, the specific fixedness of type has been assumed. The clinical and anatomical grounds for this conclusion, as set forth by Louis, Gerhard, and Sir Wm. Jenner, have, indeed commanded very general acceptance, but the doctrine which asserts the possibility of the occasional blending of types of fever has some earnest advocates. Omitting any special reference to papers heretofore published, I cite the greater part of a communication from Dr. S. Putnam, of Montpelier, Vermont. This gentleman, having a copy of my circular, which had been forwarded by Dr. Bowditch, president of the Association, furnished a very interesting statement of facts derived from a large experience in connection with the typhoid fevers of New England. He says:—

“In reply to Dr. Cabell, who inquires for the *facts* which have led me to believe in the frequent origin *de novo* of typhus or typhoid fever, I can only state, that, according to my observation, during a somewhat extensive country practice for thirty years and more, typhoid fever has more generally occurred as an epidemic from August to November, particularly in dry seasons, after a succession of *yellow, smoky days, without storm, winds, or electrical display*, the beds of streams and ponds having become dry or half dry, and stenchy, and vegetation parched or shrivelled. Under these circumstances, constituting, as I conceive, an epidemic influence, typhoid fever has often soon appeared, more frequently in families residing on the banks of streams or ponds, but often also in the farm-houses scattered over the hills here and there, without any possibility usually of tracing the importation of the disease or its spread from house to house by contagion.

“Under these circumstances in the fall of 1865, I saw thirty-seven cases and made notes of them. In regard to etiology, I briefly quote from my report of the epidemic of that year as published in the Transactions of the Vermont Medical Society for 1866. ‘The fever began to manifest itself in this vicinity in August, and cases continued to occur, mostly in September and October, but a few in November. The atmosphere in this region, according to common observation, was remarkable during

the summer of 1865 for heat, dryness, and the absence of electrical phenomena. Certain localities and families though wide apart suffered most, two or three or sometimes four or five being sick in the same house. About most of the dwellings in which the fever occurred, the sanitary regulations were objectionable, many of the houses being old and not as favorably situated and constructed for ventilation, cleanliness, and health as those of modern date.'

"We do not say that these insalubrious conditions were the absolute cause of fever, for no such cause exists, we conceive, independently of the varied states of predisposition on the part of the individual. We simply claim that the conditions may be such without and within the human body, that the chemistry of nature may and does at times evoke poison or ferment capable, under certain conditions of the body, of effecting changes in it of a more or less specific character, and that according to our experience the conditions without the body tending to predispose to or excite the manifestation of fever are often as above described.

In September, 1859, away in the forest, with only a few acres, cleared about the log hut, cow-house, and pig-pen, I attended a young man thirteen days with well-marked symptoms of typhus fever. As he began to recover, his mother gradually came down and continued long sick with equally well-marked symptoms of enteric fever. There could be no question of the identity of the diseases in either case. They were decidedly characteristic cases, and there can be no more doubt that they originated *de novo* then and there."

After citing numerous cases of typhus and of typhoid dysentery occurring nearly or quite simultaneously with cases of enteric fever, Dr. Putnam refers approvingly to Stokes' doctrine of the correlation and convertibility of zymotic diseases, and quotes the remark contained in a paper of Dr. Henry Kennedy on the "Intermingling of the Types of Fever," read before the Medical Section of the British Medical Association in 1867, that "the types of fever are much more mixed and intermingled than is commonly supposed or the books teach."

In conclusion, it appears that further and careful observations are needed to establish on indisputable evidence several of the opinions which have been expressed in this paper respecting the etiology of enteric fever. In regard to many of the cases

reported by several correspondents, it does not appear that the accuracy of the diagnosis was placed beyond suspicion, and this circumstance very materially detracts from the value of the conclusions based upon them. It is desirable, therefore, that isolated cases, occurring in out-of-the-way country places, from which the suspicion of direct importation of the disease can be readily excluded, should be thoroughly scrutinized as to the grounds of a positive diagnosis, and then as to all the conditions both internal and external under which the disease may have been developed. Especially does it appear necessary to notice the meteorological conditions, which may concur in point of time with the development of wide-spread epidemics of fever, and the relations of such epidemics with the appearance or with the suspension of the prevalence of other miasmatic diseases. Dr. Henry B. Baker, Secretary of Board of Health of Michigan, has already published, in the recent Annual Registration Reports for that State, some very interesting and valuable tables exhibiting the number of deaths from different diseases in connection with "the Meteorological Conditions existing at the same time." Such observations will doubtless eventually lead to important discoveries touching some of the conditions of the spread of enteric and other fevers epidemic-wise.

