Lectures on the etiology of typhoid fever / by Robert King.

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LECTURE

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ETIOLOGY OF TYPHOID FEVER.

BY

ROBERT KING, M.B.,

PHYSICIAN TO THE MIDDLESEX HOSPITAL.

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Gentlemen,—I propose to address a few remarks to you to-day on the etiology of typhoid fever, directing your attention, by way of illustration, to the case of Emily B., now lying in No. 19 bed, Northumberland ward.

This young woman entered my service as nursemaid some two months ago, and undoubtedly contracted the disease in my own house. On Monday, May 19, she first complained of sick headache, but not so severe as to prevent her from discharging her duties; on the following day, however, diarrhœa set in. These symptoms—viz., headache, vomiting and purging, with a furred tongue, and loss of appetite—continued on and off all the week; nevertheless, she went about and got through her work with more or less help pretty much as usual, and no complaint was made to me until Thursday, May 22, when I was asked to prescribe for the diarrhœa. I ordered a little aromatic chalk mixture, and the next morning she said she was better, but on Saturday afternoon she was obliged to go to bed. I saw her in the evening, and found her shivering and complaining of severe pain in the lower part of the abdomen, which was increased by pressure. The tongue was thickly coated; the bowels had acted five or six times during the day, and she had also vomited. She had no appetite, but complained much of thirst and pain in the head. The skin was pungently hot, temperature 103·5°. The face was flushed and the eyes were suffused; the pulse was rapid and irregular.

There was no evidence of any uterine or ovarian disturb-

ance, and I was forced to admit the disagreeable probability of typhoid fever. The next morning (Sunday) the temperature was normal, and she was better in every respect, but of course remained in bed all day. Towards evening the temperature rose to 101°, and she did not feel so well. On Monday, May 26, I sent her on to the hospital, where she was admitted under the care of my friend Dr. Cayley, whose clinical assistant, Mr. McCausland, has kindly furnished me with the further notes of the case, which conclusively prove the correctness of the diagnosis.

The state on admission was as follows:—

Patient complains of headache, thirst, and abdominal pain. Her tongue is coated, except at the tip and sides; it is, moreover, somewhat dry and brown down the centre. Pulse 120; temperature 104°. There is some tenderness over the abdomen, with gurgling in the right iliac fossa, but no rose spots. The splenic dulness is much increased, extending upwards fully four inches from the lower margin of the ribs.

Apex-beat of heart is diffused, and impulse is heaving, the first sound being prolonged and roughened. There is a systolic bruit over the thyroid gland, which is somewhat enlarged, but there is no well-marked exophthalmos. Bowels acted shortly after admission; motion quite loose, and of a light yellow colour.

The subsequent progress of the case was chiefly remarkable on account of the singularly irregular fluctuations of the temperature, which ranged from below normal to 104°, and as often as not was higher in the morning than at night. The headache and vomiting were also unusually persistent, and during convalescence the fever assumed a distinctly intermittent type.

Suffice it to say that the diagnosis was never for a moment doubtful; that in its clinical details the case presented little or nothing particularly uncommon; that it was not marked by extreme severity; and that, in point of fact, its chief interest centres in the evidence it affords as to the source from whence the poison was obtained, and the light which is thus thrown on the etiology of the disease.

A generation or two ago the distinction between typhoid and typhus fever was unknown. Germs had yet to be evolved from the inmost recesses of the unborn brain, and the modes of conveyance of the poison were scarcely more fully understood than they were in the days of Hippocrates.

At the present time, however, a man need not have studied medicine in order to be able to tell you that typhoid fever is intimately associated in his mind, with bad drains, sewer-gas, and polluted drinking-water; indeed, in seeking for the cause of an outbreak of this fatal malady, these are amongst the first points to which we should naturally direct our attention.

When I came into possession of the house I now occupy, during the summer of 1872, the connexion which existed between the water-closets and the drinking-water cisterns was as follows: There were three closets in the house, that appropriated to the servants being located in the basement. The supply of water for this closet was derived directly from a cistern situated in the pantry behind the kitchen; and from this same cistern every drop of water used in the basement of the house was obtained, including the supply to the kitchen boiler. The waste-pipe of this cistern led directly into the soil-pipe of the closet above mentioned. Thus a double communication was established between the lowermost closet and the cistern which would naturally supply the water for drinking and cooking purposes. This admirably contrived and popular arrangement for impregnating water with sewer-gas I at once altered, by utterly abolishing the closet in question, and allowing the waste-pipe of the cistern to empty itself over a well-trapped grating. The kitchen cistern was thus completely cut off from all communication with the house-drain. It was, moreover, fitted with one of Lipscombe's cistern filters, so that all the water drawn from the kitchen tap, or passing into the kitchen boiler, was previously filtered through charcoal. A hand filter supplied daily from this source was placed just outside the dining-room door, and from this the drinking-water was supposed to be derived.

The second closet, situated on the ground floor, in rear of

the hall, requires no comment, being supplied by a small cistern of its own, altogether inaccessible to servants. The third closet occupies a place on the landing, midway between the second and third floors, and is supplied with water direct from a large cistern in the roof of the house; the waste-pipe of this cistern leads into the soil-pipe just below the trap of this closet, and thus forms a sort of ventilating shaft between the top of the soil-pipe and the cistern in question.

The day and night nurseries are on the third floor, and directly opposite the door of the latter room there is a housemaid's sink, with a tap supplied with water from the top cistern just mentioned. Seeing, then, that the same double communication exists between the upper cistern and closet, which *did* exist between the kitchen cistern and closet before I effected the alteration already described, it is no wonder that I have always regarded the water derived from the top of the house with grave suspicion, and the danger of using it for drinking purposes was most fully impressed upon every member of my household. Arrangements were also made for supplying the nursery with pure water from downstairs as often as it might be required. Until quite recently this system worked well, but in March last a new nurse entered my family, and there is conclusive evidence to show that the orders were disregarded. So far, then, all I have proved is, that of two available sources of drinking-water, one was cut off from every connexion with the house drains, and fitted with a charcoal-filter, while the other was in direct communication with the soil-pipe; and further, that this latter was nevertheless used by the nurse for drinking purposes. A *primâ facie* case is thus established, though without further testimony it does not necessarily follow that the typhoid poison was actually contained in the water derived from the upper cistern. By a singular coincidence, however, I am enabled to supply the strongest confirmatory evidence on this point, for, in consequence of having some lectures to deliver on water analysis, it so happened that during the months of April and May I several times examined the water supplied to my laboratory from the

very cistern in question, and, moreover, compared it with the water obtained from the kitchen cistern. The results are to my mind conclusive: for while the water derived from the latter source seldom contained more than ·01 milligrammes of free ammonia and ·06 of albuminoid ammonia per litre, and varied but little from this amount, the laboratory water obtained from the upper cistern yielded at times more than double the quantity of free ammonia, and occasionally as much as ·24 of albuminoid ammonia. Moreover, the proportions of both fluctuated widely. This variation may probably be accounted for by the fact that the cistern is completely emptied some days, while at another time scarcely any water is withdrawn from it, and time is thus afforded for the more complete absorption of the sewer-gas; moreover, it is highly probable that the pressure of this gas in the soil-pipe, and even the constitution and solubility of it, is very far from uniform.

As the possibility of direct exposure to contagion from another person affected with the disease may practically be excluded, while the probability of such exposure, if it had occurred, producing the disease is extremely small; and as the milk is above suspicion, and forms almost the entire food of the youngest child, who is in perfect health; as, moreover, there is the most positive proof that the water in the upper cistern is frequently, if not always, unfit for drinking purposes by reason of the large amount of albuminoid ammonia it has been found to contain, it seems to me that the evidence in favour of the typhoid poison being conveyed into the body of this patient by the water in question is simply irresistible, and it is further strengthened by the fact that she very rarely drank anything except water with her dinner, and was seen to fill her tea-kettle from the tap outside her door on more than one occasion. Of course, if she thought the water good enough for making tea, it is fair to suppose that she would not hesitate to use it for her dinner when the supply from downstairs might chance to run short.

Assuming, then, that this water was the medium through which the poison was conveyed, from what source was the

poison itself derived? The idea that it might have been supplied from without, through the company's mains, is of course utterly untenable, as in that case a regular outbreak of typhoid would have resulted; besides which, the analyses already mentioned point conclusively to contamination subsequent to delivery. With such evidence no reasonable person can doubt that it was furnished through the connexion existing between the cistern and the soil-pipe. The question, therefore, which we have to consider is, How did it get into the soil-pipe? At first sight perhaps this might seem to admit of a ready answer. The numerous advocates of the germ theory would probably tell you that typhoid germs were constantly present in the main sewers of all our large towns, and that notwithstanding their well-known propensity to become absorbed or dissolved by water, and the equally well-known fact that the flow of sewage is from the house-drain into the main sewer, and cannot be the other way, they nevertheless continually succeeded in eluding both water and traps, and, overcoming every obstacle which human ingenuity could devise, effected an entrance into our dwellings and so gained access to our bodies. Now, the main sewers in London are many of them of large size, and men are daily employed in them for the purposes of cleansing and repairing; nevertheless, the health of these men is as a rule good, and there is certainly no evidence to show that they are more subject to typhoid fever than those whose lot is cast in pleasanter places.

Writing on this subject, Dr. Parkes says—"The air in many sewers in London is not very impure. The analyses of Letheby and Miller have shown that generally the amount of CO_2 is very little in excess of that in the external air, and that there is hardly a trace of H_2S or fœtid organic effluvia. The air of house-drains is often, in fact, more impure than that of the main sewers." Again the same authority remarks—"An inquiry lately conducted into the health of the sewer-men of London did not detect any excess of disease among them, and I was informed that in Liverpool also the sewer-men have good health. The workmen employed at the various sewage outfalls, and who, though not

in the sewers, breathe the effluvia arising from the settling-tanks, do not find it an unhealthy occupation." A single fact such as this is worth a great deal of theory, since it proves most conclusively one of two things—either the so-called germs of typhoid fever do not exist to any great extent in the main sewers, or else they exist in that portion of the sewer-gas which is soluble in water, and so become absorbed by the fluid contents of the drain; hence, as the men do not drink the sewer-water, they do not contract the disease. At all events it is pretty clear that the typhoid poison does not exist in any large amount in the *air* of the main sewer; and the theory which supposes that it does so, and, moreover, finds its way thence into the house-drains, and thence again into the drinking-water, and so produces the disease, is, I think, in the highest degree improbable: for why was it not absorbed in the first instance by the water in the main sewer? Is the sewer-water already saturated? If so, the amount of poison must be very great, and the comparative immunity of the sewer-men becomes simply inexplicable. Moreover, the contents of the main sewer are naturally more advanced in decomposition than those of the house-drains, and it is well known that decomposing organic matter gives off a much larger amount of gas than similar matter already decomposed; add to which, the house-drain, in nine cases out of ten, is less freely ventilated than the main sewer, and, being so much smaller, it might naturally be expected that the pressure of gas would be far greater in the soil-pipe than in the sewer, and this pressure would be vastly increased if any obstruction existed whereby the discharge from the house-drain into the sewer was impeded.

Now, it so happened that during the summer of 1875, the parish authorities—actuated, no doubt, by the best of motives, but with more of zeal than discretion—insisted upon providing every house-drain in the street with a ponderous cast-iron trap. This was fitted, at the unfortunate householder's expense, at the point of junction between his drain and the main sewer, and undoubtedly affords a melancholy example of the folly of extending in practice the excellent theoretical

principle that every man should consume his own smoke; for the trap in question consisted of a square iron bottomless box, let into the brickwork in such a manner that the lid would open outwards from the house-drain into the sewer, and, the contents of the former having passed, would close again by its own weight—which, by the way, was no trifle. The benevolent idea was to keep the foul air of the main sewer out of the house-drain: the practical result was to prevent the fouler atmosphere of the house-drain from escaping into the sewer. At all events, such was the case with regard to my own house, as the closets—particularly the upper one, which hitherto had been perfectly free from any offensive smell—henceforth required the frequent use of carbolic acid. The only possible effect of such a trap must be to retard the evacuation of the house-drain, and allow time for decomposition to take place therein more fully than it otherwise would do, while an accumulation of filth was collecting at the door, and waiting until reinforcements of sufficient weight arrived to effect an entrance into the main sewer. For the first few hours after it was put down this door might have worked, perhaps, with tolerable ease, but it is impossible to imagine that it could have been in its place for a week without being well-nigh set fast with rust. Be this as it may, it is quite certain that it either failed most signally in shutting out the typhoid poison, or else that poison must have been generated *de novo* on the premises, for there had been nothing like typhoid fever in the house for certainly more than twenty years, and no direct communication exists between my house-drains and those of my neighbours. The advocates of the germ theory would, of course, insist that my parish trap must have got propped open, and that some of their pet parasites, declining to become absorbed either by the water in the main sewer or by that in my house-drain, contrived, with a malevolent sagacity out of all proportion to their size, to find their fœtid way up the soil-pipe, and so through the waste-pipe into the cistern; but if so, why did they wait until they were compelled to dispute their right of entrance with a massive cast-iron trap, when they could have come in without let or hindrance

any time these twenty years past? How was it that, for time too long to tell, both cisterns communicated directly with the soil-pipe, and yet none of these mischievous microzymes ever thought of invasion before? I cannot but regard the theory of *contagium vivum* as highly dangerous in the hygienic point of view, since it practically excludes the possibility of diseases such as typhoid fever being generated *de novo*, for the only alternative would compel us to admit the unphilosophical doctrine of heterogenesis. Let it be once admitted that such poison can be produced by fæcal decomposition, and that such decomposition can occur within our own dwellings; and let it be moreover acknowledged that, just as in the case of the septic poison, decomposing matter is deadly, while that already putrid is rather offensive to our senses than toxically dangerous, and we shall hear much more of house-drains, and far less of sewer-gas. I do not wish, however, to convey to you the impression that I consider healthy fæcal matter capable of evolving the typhoid poison during the process of its decomposition. Were it so, I fear the disease, unhappily prevalent as it is, would be far more common. Probably you are all aware that healthy stools, like normal urine, are non-albuminous, while in certain diseased conditions either the one or the other may contain albumen. The evacuations of patients suffering from intestinal inflammation or ulceration—such, for example, as we meet with in typhoid fever or tubercular disease—are always albuminous; and possibly it is by the splitting up of this highly complex compound that the poison is generated. What may be the nature of the influence which determines the character of such decomposition, it is, of course, impossible to say; for, in the first place, the albuminoid group is a large one, and the distinctions between the various substances thus classed together are very imperfectly understood. We know, however, not only that animal and vegetable albumen differ, but that numerous modifications of this compound are met with in each sub-kingdom of nature. The albumen of egg differs from that of serum in its behaviour with reagents, and it would scarcely be too much to assert that almost every tissue of our bodies has an albumen

of its own; besides which, even the same kind of albumen may undergo more than one species of decay. There is no difficulty, therefore, in accounting for the multiplicity or specificity of poisons if they are derived from such a source. Now, if we consider the special peculiarities of the fever-poisons, we shall be struck at once by their extreme potency, by the exceeding minuteness of the lethal dose in each case. In vain may we look through our list of known poisons for anything to equal it, but it is most nearly approximated by the more active members of the alkaloidal group; nor does the resemblance end here.

In man's body there exists an organ specially contrived for the purpose of eliminating an organic alkaloid. I need scarcely tell you that organ is the kidney, and the alkaloid is urea. Now, in case of disease—and, what is more remarkable, even in health, when an exclusively albuminous diet is persisted with—what we find is that the organ which normally eliminates the alkaloid, abnormally eliminates albumen; and as the quantity of the latter increases, that of the former diminishes. It is therefore probable that the alkaloidal body urea is produced by the normal decomposition of albumen, and hence, if the raw material is exported, the manufactured article cannot be supplied at the same rate.

If we turn now to the chemical properties of the organic alkaloids, and compare them with those of albumen, the resemblance is most striking. In the first place, albumen is by no means particular whether it plays the part of acid, or base, or both; and the same is true of some of the organic alkaloids. Strychnia, for example, is one of the most powerful of the vegetable bases; but it nevertheless combines readily with many of the metallic salts, forming compounds of sparing solubility, just as albumen does. Moreover, the metallic salts which most completely precipitate albumen—such as mercuric chloride, for example—form also the most insoluble compounds with strychnia.

But we have several group tests for the alkaloids, solutions which yield precipitates with almost every one of these bodies. The most noteworthy are Sonnenschein's reagent, or the

acidulated solution of sodic phosphomolybdate, which is a perfect precipitant of albumen. This solution will precipitate all the organic alkaloids with the exception of urea. Bouchardat's reagent, which consists of a solution of iodine in iodide of potassium, is another group test precipitating nearly all the alkaloids, and likewise a very complete precipitant of albumen. Precisely the same may be said of tannogallic acid; while carbazotic acid and mercuric chloride are each of them efficacious in precipitating not only albumen, but also many members of the alkaloidal group. Furthermore, the precipitates produced by carbazotic acid have in each case a distinct tendency to assume the crystalline form. Nor is the coagulation of albumen by heat an anomaly without a parallel in the alkaloidal world, for if cold water be saturated with aconitia and then filtered, we have a most undoubted alkaloid which exhibits this remarkable feature, for on boiling some of the clear filtered solution in a test-tube, we find that it becomes milky from the precipitation of a portion of the alkaloid.

When distilled with alkaline permanganate, both albumen and the alkaloids yield ammonia, while, according to Professor Odling, the putrid material which is frequently found in sewers is closely allied to the compound ammonias, and occupies a place midway between methylamine and ethylamine. I do not mean to infer from these facts that albumen actually is an alkaloid, but I think it is very likely that some of the products of its decomposition in disease may be of an alkaloidal nature, more poisonous than aconitia, and possibly, in some instances, more volatile than nicotine. In connexion with this subject it is interesting to observe that the hypothesis of alkaloidal fever-poisons is to some extent supported by clinical experience, for one of the vegetable alkaloids, viz., morphia, has been frequently known to occasion a form of intermittent fever nearly allied to ague. Dr. Levinstein of Berlin has published several such cases, and the subject is also mentioned by the late Dr. Murchison in a clinical lecture published in the *Lancet* of May 10 last. Moreover, the volatile vegetable alkaloids as well as ammonia, and the typhoid poison can all exist in air, can

all be absorbed by water, and can all be expelled from their aqueous solution by prolonged boiling. Nor is this all, for animal charcoal, which possesses such extraordinary power of disinfecting and sweetening the atmosphere of a sick-room by absorbing and oxidising putrid organic effluvia, is also capable of absorbing an almost unlimited quantity of gaseous ammonia, while many of the vegetable alkaloids, if mixed with it in sufficient quantity, are wholly irrecoverable by chemical means. With regard to other disinfectants and so-called germicides, it will generally be found that their antiseptic power is in direct proportion to their albuminoid affinities; and the fact that meat may be preserved from putrefaction for some time by the agency of carbolic acid, salicylic acid, thymol, and a host of other substances, admits of a ready explanation when we consider that the antiseptics in question form with albumen very insoluble, and therefore comparatively stable, compounds. Perhaps the weightiest argument which can be adduced in favour of the theory that the specific fever-poisons consist of living particles or germs, is based upon the undoubted multiplication of these poisons in the system of the infected person; but I think that even this fact admits of a different explanation: for if the poison consist of some product of albuminoid decomposition, gifted with the power of affecting those particular elements of tissue from which it was derived, it is only natural to suppose that the process thus started, possibly in some one constituent of a single cell, would be liable to spread to similar constituents of adjoining cells, in much the same manner as the ordinary inflammation of a serous membrane, for example, is known to spread. The combined products of the decomposition of the mass of tissue thus finally involved would of course supply abundant material for an enormous increase on the amount of poison originally employed. Thus the multiplication of the poison may possibly represent the waste of tissue instead of marking the extent and progress of bacterial growth. This is well illustrated in the case of specific inflammatory diseases, such as gonorrhœa, for example. A very minute portion of the gonorrhœal pus is sufficient to occasion the disease; and no

one, I suppose, would dispute the fact that the poison is multiplied a thousand-fold.

Again, with regard to the period of incubation of the specific fevers, which is supposed by many to represent the time occupied by germ development, it must be borne in mind that *living* bodies do not putrefy, neither do the *living* cells of which those bodies are composed, and as in somatic death, so also in molecular death, the process of disintegration is a work of time, it may be therefore that the premonitory symptoms of these diseases set in when molecular death has proceeded to such an extent as to interfere with the functions of the affected part. If so, it would be natural to expect variation in point of time within certain limits; and clinical experience teaches that this does actually occur.

If, then, we can divest ourselves of the notion that the typhoid poison is a living entity propagating its kind by one or other of the various methods peculiar to vitalised bodies, I think there is no great difficulty in assuming the possibility of its arising *de novo*. So far as we are aware, the principal morbid change which takes place in the body of a person affected with this disease consists in a species of inflammatory affection of the intestinal mucous membrane, specially attacking the glandular structures known as Peyer's patches. Moreover, the evidence is conclusive that the evacuations from this diseased surface contain the great bulk of the poison generated in the body of the patient. I have already told you that I do not believe that the typhoid poison can result from the decomposition of healthy stools; but when those stools cease to be healthy, and become albuminous, I think it is by no means improbable that such may be the case.

Now, it so happens that, for three or four years past, one of the inmates of my house has been affected with chronic mucous diarrhœa, and I have fully satisfied myself that the fæcal evacuations of this individual contain albumen. After carefully considering the evidence which I have thus briefly suggested, rather than laid before you, I am inclined to think that it is not only possible, but even probable, firstly, that the poison of typhoid fever is a soluble, and perhaps volatile body, formed on the ammonia type by the decomposi-

tion of albuminous matter derived from inflamed intestine—that it may be, in fact, a sort of animal alkaloid; and, secondly, that in the case to which I have specially directed your attention to-day, the poison was most likely generated *de novo* in my own house-drain, during the putrefaction of the albuminous stools already mentioned. I think also that, owing in a great measure to the obstruction existing at the point of junction of this drain with the main sewer, the gaseous products of the decomposition were prevented from escaping into the sewer, and, ascending the soil-pipe, were absorbed by the water, which, as I have already indicated, was undoubtedly the medium through which the poison was conveyed. To those among you who may hereafter be engaged in country practice, opportunities will probably be afforded of investigating this interesting and important subject, apart from the sources of fallacy which a sewered town presents; and to such I would say, if no case of typhoid fever has been previously known in the district for a long time past, and if it be quite impossible, as it often is, to trace any direct or indirect communication with an infected person, or assign any cause for the appearance of the disease, it may not be altogether unprofitable, to seek for evidence of some perhaps trivial enteritic affection, producing albuminous stools.

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