

# Some Observations on the Use of Salol in Typhoid Fever.

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So many drugs have of late years been put forward as more or less specifics in the treatment of Typhoid Fever, that it seems almost presumptuous to take up such a subject for an essay; and yet to accept the old theory that all drugs are useless to check, control, or cure this disease, and that in careful nursing and dieting alone is hope for the patient, seems to deliberately close the door on all advancement in this direction: believing, as I firmly do, that where there is a disease depending on a specific poison there will ultimately be found an antidote for that poison, and a means by which such antidote can be administered so as to act upon the poison within the system, neutralizing its ill effects and assisting its elimination; and in no class of diseases does this seem to be more possible than in those which are caused by the action of living organisms. I believe that the Physician of the future, with his more perfect knowledge of these living organisms, their life history, and the chemical poisons to which their life in the human tissues gives rise, and with a more perfect armament of drugs at his command, will be able to destroy such living organisms in the system, or at any rate to neutralize the chemical poisons which are produced by their existence; and that so Tuberculosis, Pneumonia, Typhoid Fever,



Cholera, Yellow fever, and the allied diseases, will one by one be conquered and brought under control, at least in their earlier stages. -

It was in May 1890 that I first obtained a specimen of Salol and began as far as possible, to study its properties with a view of using it in Cholera, should the opportunity arise. Of the reasons which led me to believe that it would be of little use in fully developed Cholera, it is needless to speak; but it soon occurred to me that it might prove a valuable drug in Enteric Fever, and early in June of the same year I began to use it in that disease. I was led to this by the following considerations of the probable and possible causes of Typhoid fever.

1st Theory. - That the disease is caused by certain bacilli developed in the stools of people already suffering from Typhoid, or as others assert, in decomposing animal organic matter, apart from the presence of typhoid evacuations. That these bacilli or their spores, gain access to the human intestine in food, or more commonly in drink, and thence pass into the mucous membrane, lymphatic glands, spleen, and blood, where they increase and multiply during the earlier stages of the disease, being especially numerous when the ulceration of Peyer's patches begins. In the later stages they gradually disappear and are replaced by other microbes.

These special microbes were first observed by Recklinghausen in 1871, but were first fully described by Eberth, and Klebs - Klebs found in Peyer's patches, mesenteric glands, larynx, and lungs, bacilli which he describes as follows: - Rods 0.0002 mm thick, and of various

lengths forming filaments up to 0.05 mm long. These bacilli form spores.

Eberth describes the bacilli as short bacilli rounded at their ends, and sometimes constricted in the middle. Some of them contain spores and they stain very freely with methyl violet. These bacilli Eberth considers as the real cause of the disease, as they are the only ones he found in the blood and internal organs. He found them in blood, spleen, liver, and sometimes in the kidneys and urine. Other observers have described micrococci in colonies, in the tissues of the intestinal mucous membrane, mesenteric glands, and spleen. Gaffky has made artificial cultures of Eberth's bacilli on gelatine and potato, and they produced endogenous spores at a temperature of 38°C. The inoculation experiments however, failed to produce true Typhoid fever in the animals experimented on. It must also be noted that these bacilli have not been found in much more than fifty per cent of the cases in which they were searched for, and possibly the special microbe of Typhoid has yet to be discovered. On the other hand Gaffky's experiments were probably performed, on animals whose livers were in perfect order. Had they been performed on young men with exhausted livers, the results might have been widely different.

2<sup>nd</sup> Theory:- Professor Aitkin, in his recent work on "Animal Alkaloids", suggests a theory of what may be called auto-infection, without the intervention of bacterial action. He points out that Gautier has shown that animal alkaloids are a necessary product of vital physiological processes,

and that a fifth part of our tissues live after the manner of ferments, that is they are unarabolous, or putrefactive as to their life. He also points out that many of these alkaloids are distinctly poisonous; that the liver is the chief organ concerned in the destruction and elimination of such poisonous alkaloids; that the liver being inactive from any cause, such poisons may find their way unchanged into the blood and system at large; that, in exhaustion from any cause, as starvation or over exertion, the liver is one of the first organs whose functions suffer; and he quotes Lauder Brunton to prove that the vital processes are much more readily arrested by the accumulation of waste products within the organs of the body, than by any want of nutriment of the organs themselves. He also quotes a case of exhaustion fever, cured by simple rest, which M. Chomel diagnosed in the first instance as Typhoid; and he makes the following statement: "It is in special circumstances that bacterial invasion may elicit from mucous membrane x x x They-bacteria- are unable to settle in a perfectly healthy body, and can only develop when the physico-chemical condition of the tissues is markedly altered to suit their requirements." Professor Aitkin also quotes experiments of Buchard to prove that Potmaines are present in diarrhoea, typhoid, and normal, stools; are absorbed from the bowel, and appear in the urine; and he asks is it possible that Typhoid fever is a stercoræmia?

Beaumont's experiments show that poisonous substances are absorbed from the intestines in health, and are converted into innocuous combinations,

chiefly in the liver, before entering the general circulation. We know that abundance of Glycogen in the liver increases its power of destroying such poisons as are presented to it. This power diminishes as the Glycogen disappears, and what is true for the liver is probably true for other parts of the body.

Lauder Brunton holds that the poisonous products of the typhoid bacilli are excreted into the intestine, and reabsorbed from thence.

Now without adopting Professor Aitkins view of auto-infection in its entirety, his remark that bacteria are unable to develop in a perfectly healthy body, is worthy of serious consideration in relation to the above facts; for we know that the bowels are constantly full of bacteria and micrococci, and putrefying albuminous substances. We also know that a body of men may all drink water or milk polluted with the special typhoid poison directly traceable to a typhoid source, but only a comparatively small number of them will develop Typhoid fever. In those which escape the liver is properly performing its functions as head sweeper to the internal economy. The bile is the great disinfectant of the contents of the intestines, and prevents the too great accumulation of bacteria and poisonous alkaloids within the bowel. The liver further seizes upon such poisonous products of bacterial putrefaction as are absorbed from the bowel, and converts them into innocuous substances of such a nature that they are easily excreted by the kidneys; but if a person is subjected to excessive fatigue, or starvation, or both combined, what occurs? The liver ceases to act properly; Glycogen disappears both from the

liver and the other tissues of the body; there is deficiency of bile poured into the bowel; the poisonous products of putrefaction in the bowel increase, are absorbed, pass the liver into the general circulation, and still further depress the system. Also it is highly probable that in the systemic famine a sort of exhaust action is produced, and more absorption goes on from the intestines than is the case in well nourished or unexhausted animals. The whole system is poisoned and its vitality lowered; excretion is checked, and waste products from the system accumulate, producing still further depression and loss of general vitality, and still feebler action of the liver. The vitality of the cells of the mucous membrane of the intestine is lowered, and their physico-chemical condition is so markedly altered as to admit of the invasion into them of the typhoid bacilli, should such be present in the contents of the bowel. With this condition of things there will be furred tongue, headache, lassitude, fever, possibly some bronchial irritation, and may be, a little diarrhoea; and the case will be diagnosed as Simple Continued Fever, or as Typhoid, according to the particular view taken by the medical attendant; but rest and simple food will cure the disease in a few days. The case is one of exhaustion fever, and is probably in great measure a stercoroemia. If however, the special typhoid bacilli are present in the bowel while the above processes are taking place, they pass into the altered cells of the mucous membrane of the intestine, gain admission to the weakened system unchecked, and all the symptoms of true Typhoid rapidly develop. After increasing and multiplying for a season, these special bacilli

begin to decrease in number and finally disappear; but they leave a system loaded with its own waste products, and also with other forms of bacilli and micrococci. These waste products are no doubt stored to a great extent in the spleen and lymphatic glands, and are poured into the general circulation at intervals, producing the remittent and intermittent types of fever so commonly observed when the disease is subsiding. As Lauder Brunton puts it, the dust and refuse of the house are being carried periodically through the passages of the house to be thrown into the dust cart outside, and great dirt and inconvenience is caused in the house during the transit. Now it is perfectly conceivable, that, at this period, one or more of the special typhoid bacilli, or their spores, which have originally reached the spleen, and been stored up therein, may have preserved their vitality, and being given up by that organ, and excreted into the bowel, may there find in the putrefying albuminous matter of the faeces, a suitable nidus for their continued growth, development, and reproduction, and so the intestines again become filled with typhoid bacilli, which in turn penetrate the still weakened cells of the mucous membrane, and gain access to the system at large, producing a relapse. I have taken exhaustion from fatigue, and want of food, as examples of two causes which lessen the functional activity of the liver, and which are at the same time well known predisposing causes of Typhoid fever, but of course any cause which lessened the functional activity of the liver, such as confinement and bad air on board ship, the motion of the ship in many cases, mental depression

or worry, would tend to have the same effect, and a very slight addition to these, in the shape of a little over exertion, exposure to heat, or abstinence from food for a few hours, will bring on an attack of "fever". The soldier on his arrival in India is exposed to all these influences, including a daily fast from 5-30 pm till 8-30 am, that is for sixteen consecutive hours, for however perfect the regimental coffee shop arrangements may be, they are of no service to the wretched draft on its way up from Bombay, to, say, Deutta. After leaving the train box, the men are often exposed to the heat during a fatiguing march in very unsuitable clothing. This is especially so in the case of drafts arriving in March and April, and joining their Battalion at a hill station.-

According to this view the first essential in the production of Typhoid fever, is failure of ~~function of~~ function of the liver, followed by a certain amount of stercoraemia, and accumulation of waste products within the system. This is perfectly curable, by remedies directed to the cause which produced the disease. In the one case simply rest and suitable food. In the other a blue pill and mild aperients; unless the second cause be present; this is the existence in the bowel of the specific bacillus of Typhoid.

The next stage, is a specific poisoning of the system by this bacillus or its products, accompanied by local lesions in the intestine; and the last stage, is a continued poisoning of the system, by its own waste products, arising from rapid waste of tissue, and defective excretion combined, and aggravated at pretty regular intervals by flushes of waste, thrown into the circulation from the spleen, to be eventually got rid of by the excretory

organs; and gradually tending to recovery as the excretory organs gain strength, and mastery, over the flood of waste matter presented to them. It will probably be objected; that this theory does not agree with the fact, that Typhoid has a more or less prolonged period of incubation; but it appears to agree well with the fact, that the incubation period is so remarkably irregular. Suppose that a healthy man swallows certain typhoid germs; there is no reason for believing that they will be passed per anum at his next motion. On the contrary, on reaching the lower part of the Ileum they have attained a suitable faecal nidus, and are in a suitable temperature, for their continued growth and development. What is more natural therefore than that they should grow, and develop, and infect successive supplies of faecal material, for a considerable period, probably for about a month. All this time if the liver is active, and the mucous membrane healthy, they are as innocuous as if they were in a glass bottle; but, at any period after their admission, should the functions of the liver fail, should there be exhaustion of the system, and lowered vitality of the intestinal mucous membrane, they will penetrate the morbidly altered cells of the latter, gain access to the circulation, and in a few days, or hours, the stage of invasion will be apparent. If this be true; it forces us to the

§ Note. - It must be noted; that the lower part of the Ileum, is the highest part of the intestine, which contains truly faecal matter, and is at the same time the lowest, from which much absorption takes place. It is therefore from this point, that a bacillus which requires a faecal nidus for its development, would naturally first invade the system.

conclusion, that a perfectly healthy man, may and often does, pass typhoid stools, and should there gain access to drinking water, one man's meat may become in the most literal sense, another man's poison. By typhoid stools, I do not mean those typical stools, which have not the slightest resemblance to really good peacocks; but merely, ordinary stools, which nevertheless contain typhoid germs in an active state. I hardly see how we can escape this conclusion, unless we are prepared to believe that only those persons who develop typhoid fever have swallowed typhoid poison, and that their comrades who have all drunk the same water, have yet in some mysterious manner avoided swallowing the typhoid bacilli contained in it.

My late friend and fellow student, Surgeon L. was almost constantly in my company, during the six weeks prior to his last illness at Abu Fatme. We lived at the same mess, ate the same rations, drank the same water, and shared our small stock of whisky, we walked, shot, bathed, and fished, in company, and worked at the same hospital. When not actually with me, his companions were known to me, and none of them developed Typhoid fever. We all drank Nile water, unfiltered, freely, and led active lives, exposed to the full force of the sun in the open, day after day. Surgeon L. was in hard condition, and excellent health and spirits, when one morning he started to walk to a hill which appeared to be some six miles away across the desert. He got back to Abu Fatme much exhausted by the heat and distance, which was nearly double what he had supposed. He had taken no food or water with him, none was obtainable

by the way, and he was parched with thirst. In company with Surgeon C. and myself, he drank some whiskey and water on his return: He had no appetite, and complained of headache and a feeling of depression and sickness. On the following day he had well marked fever, but without any of the characteristic symptoms of Typhoid. In three days his temperature became normal both morning and evening, and his tongue, which had been slightly furred, became clean; he however still felt listless, and languid, and a few days later, developed undoubted Typhoid fever, which terminated his career in about three weeks from the date of that fatal walk.

This might perhaps be looked upon as supporting Professor Aikins view of auto infection, in which case, the exhaustion, would be the sole cause of the Typhoid fever. On the other hand it might be argued, that Surgeon L had swallowed Typhoid bacilli, which his constant companions Surgeon C and myself, escaped. Against the first view; I do not think that the theory of auto infection, can be upheld in its entirety, as a positive cause of Typhoid; and would point out, that the primary fever differed in its symptoms, and had subsided, before the Typhoid manifested itself. The second conjecture, my intimate acquaintance with Surgeon L., his habits, and surroundings, leads me to believe is extremely improbable, and my conviction is, that the intestines of all three of us contained Typhoid bacilli, at this time; that Surgeon L's liver, failed in function owing to the exhaustion, and that he in consequence suffered from poisoning by alkaloids of fecal origin, and by his own waste products; that he was recovering from this poisoning, and would have done so entirely, had it not been for the presence in his intestine, of the Typhoid bacilli, which were enabled to fasten on his system in its

temporarily unguarded state, and produce true Typhoid fever; also that had Surgeon C, or myself, been subjected to like failure of hepatic function, we should have probably developed the same disease.

Assuming this view to be correct, there is every reason to hope that we may, by thorough disinfection of the contents of the bowel during the premonitory stage of languor, headache, and malaise, prevent the absorption of putrifactive alkaloids, and so save the system, from that preliminary poisoning, which is needful in order to enable the Typhoid bacilli to fasten on the mucous membrane, and that should such bacilli actually be present they may be destroyed. It seems that on some such view as this, the practice of giving aperients in the initial stage of Typhoid is based. Unfortunately, it is very rare for patients to seek advice in this stage, and later, when actual lesions exist in the intestine, the effect of aperients is most injurious. I have myself seen several cases of severe haemorrhage, and one of perforation, follow the administration of purgatives, given under the impression, that because it was the first day on which the patients applied for advice, it was therefore the first day of the disease; when a little careful questioning, would have elicited the fact that the patients had suffered from languor, slight headache, want of appetite, and a general undefinable feeling of being unwell, for from ten days to a fortnight previous to their application for help. In fact I believe that this premonitory stage, in which alone aperients may be given with safety, is rarely seen by the Surgeon, and still more rarely recognised. In any case a suitable antiseptic, would be as effective as a purge, and far safer. If however this premonitory stage were the only one in which

intestinal disinfection was of use, it would be of little advantage to the practical physician, but assuming the theory of Dr. Lauder Brunton, that the poison is excreted into the bowel, and reabsorbed from thence, to be correct; there is every reason to hope, that a continued, and thorough, disinfection, of the contents of the intestine, will so lessen the intensity of the poison, as to reduce the disease to a comparatively mild type; will shorten its duration, and will guard against relapse. In any case where ulcers exist; it is obviously of advantage to have those ulcers bathed in aseptic products of digestion, rather than in a soup of putrefying albuminous substances. I have specially confined my-self to disinfection of the contents of the intestine, as our knowledge of the general disinfection of the system, is at present too vague and unsatisfactory to be of practical use.

Having satisfied oneself that a thorough continuous disinfection of the intestine is likely to be productive of good, the next questions which present themselves for consideration, are; First; To what extent can the contents of the bowel be rendered aseptic? And secondly; what are the most suitable antiseptics for the purpose? We can answer to the first of these questions; that probably the contents of the whole bowel, can be thoroughly disinfected, and rendered aseptic. The proportion of combined sulphates in the urine, is the measure of the amount of putrefactive products which are absorbed from the intestine; Baumann has caused these combined sulphates to disappear entirely from the urine of a dog, by clearing out, and disinfecting its intestine, with Calomel. It is doubtful if this could be done so completely, in the longer intestine of the human subject, by such a drug as Calomel

alone, without giving such a quantity as would be hurtful to the patient; but combined with other disinfectants, and with careful dieting, no doubt it may be accomplished. The problem is to find the best disinfectant, or combination of disinfectants for the purpose, and among these, calomel is certainly by no means to be discarded.

The antiseptic which we require, must be so far non-poisonous, as to be capable of being given in considerable quantity, without injury to the system at large.

It must be unirritating to the bowel.

Its depressing action on the heart, if any, must be slight, and easily counteracted by stimulants.

It must have no irritant effect on the lungs, or kidneys.

It must not act so as to check the digestion of milk in the stomach.

It must not be given in such a soluble form, as to be readily absorbed from the stomach and upper part of the intestine. The ideal antiseptic indeed, would be insoluble in gastric juice, and capable of only slow solution, by the bile, pancreatic juice, or intestinal secretions, during its passage along the small intestine.

The first means of securing this asepticity of the intestinal contents, which occurs to us, is a judicious selection, and careful regulation, of diet, and very much can be effected by this means alone.

The next is the natural bile, and no doubt could we stimulate the hepatic functions much good would result, both from disinfection of the contents of the bowel, and by restoring the power of the liver, to convert the poisons absorbed therefrom into harmless substances, before they reach the general circulation. In the very earliest stage we may do this and often cut short the

disease; unfortunately in fully developed Typhoid, I know no means of effectually rousing the liver to a full sense of its duties, all the usual hepatic stimulants fail, and most of them are inadmissible, owing to their irritant action on the bowel.

Of the chemical disinfectants, there is a large number to choose from, of which I have only had personal experience of six; namely, Turpentine, Iodine, Carbolic acid, Calomel, Pure Terpine, and Salol. The action of Quinine, I have watched in the practice of others; chiefly when the disease was called "Remittent fever"; and never saw any effects, which would induce me to try it, while there are manifest disadvantages attending its use.  $\beta$  Naphthol, said to be a very insoluble drug, occupied a good deal of my attention at one time, but from its retarding action on digestion, it would appear that it is readily soluble in the stomach at any rate, and indeed it appears to have been fairly tried without success; as might have been expected, since the two properties mentioned above, ready solubility in the stomach, and power of retarding digestion, should be enough to cause the rejection of <sup>an</sup> ~~readily~~ antiseptic drug, in Enteric fever.

I used Turpentine freely in 1884-85-86, in all cases in which there was no renal complication, and though the results as a whole, failed to satisfy me, I still consider it to be a most valuable drug in certain conditions, as pneumonia, tympanitis, hæmorrhage, and extreme debility with congested lungs. As a disinfectant, it probably fails, through being absorbed too rapidly, to reach the lower part of the bowel.

My next experiments were made with Iodine and Carbolic Acid combined, a mixture which was highly commended, by in the Retrospect of Medicine 1881.

My colleague Surgeon Lendrum had used it with apparently excellent results, in a series of about fifty cases, and I was induced to try it: I used his formula  $\left\{ \begin{array}{l} \text{Ti}^{\text{ii}} \text{ ʒss} \text{ m}^{\text{ij}} \\ \text{Cin}^{\text{ss}} \text{ ʒss} \text{ ʒss} \text{ ʒss} \\ \text{aq}^{\text{ij}} \text{ ʒss} \end{array} \right\}$  every 3-4 hours, and made a careful record of a long series of cases. On the whole, the results were disappointing. Certainly I had a lower death rate than those of my colleagues who were using similar care and dietetic treatment without antiseptics, and the drugs seemed to act favourably in those cases which were brought under their influence early, and in those attended with marked diarrhoea, but I could not see that much benefit was obtained from their use in those severe cases which only came under observation and treatment when the disease was fully developed. Cases only too common in an Indian hospital. Probably as with turpentine, the greater part of the drugs was absorbed, before reaching that part of the intestine, in which alone they could be of much use. At that time the estimation of the amount of putrefactive absorption, by the proportion of aromatic sulphates in the urine, was unknown to me.

My next series of experiments was made with Calomel, and here the results were distinctly favourable. In all four cases, tongues cleaned, pulses steadied, and temperatures fell, in a regular manner that was highly satisfactory, and at the same time, the Calomel never seemed to cause, either diarrhoea, or constitutional symptoms. I have never seen a case of salivation, produced by the use of Calomel in Typhoid fever, though I have given from 3-4 grains daily, for as much as twenty days in some instances. The favourable influence of the drug appeared to be due to a general stimulating effect on the excretory organs, (though its influence on the hepatic secretion was hardly perceptible) as much, as on its

direct antiseptic action on the contents of the bowel, and for the former reason, I still use it in a modified degree, in conjunction with Salol. The dose used, was one grain of Calomel, combined with Extract of Hyoscyamus, and Extract of Toraxacum, given in pill, every five or six hours.

On first using Salol, my belief was, that owing to its somewhat insoluble character, it was likely to pass the stomach at least, unabsorbed, and that in its further progress, it would be gradually broken up by the pancreatic fluid, and carbolic acid be set free in the bowel, at a level, at which its powers, would be likely to be expended in preventing bacterial putrefaction of the contents, instead of being rapidly absorbed into the system, as crude Carbolic Acid. That this was so, to a certain extent at any rate; I satisfied myself, by proving, that the appearance of carbolic acid in the urine, was produced by Salol much more rapidly in healthy animals fed exclusively on milk, than in those fed on a quantity of meat. In fact when the animal had been fed exclusively on a large quantity of raw meat, so as to produce considerable intestinal disturbance with very offensive stools, it was very difficult to get the characteristic colour of carbolic acid in the urine at all. The possible weakening action of the drug on the function of the heart; I tested as far as possible on my own person, and satisfied myself that it was so slight, that a fairly large quantity, might be given with perfect impunity. I am aware, how rough, and unsatisfactory, these experiments necessarily were, and have always regretted, that I have never had at my command the apparatus, requisite to test the amount of putrefaction of the contents of the intestine, at any given

time, by a quantitative analysis of the free, and combined, sulphates in the urine.

To patients suffering from Typhoid fever, I commenced giving  $2\frac{1}{2}$  grains of salol every two hours, or about thirty grains a day. Later, in the earlier stages of the disease, when the heart was acting strongly, as much as 5 grains every two hours, has been given with good result, but owing to the longer periods of continued sleep enjoyed by these patients, the total quantity, given in the twenty four hours, rarely exceeded forty grains. In every case I found marked improvement follow the use of the drug. The temperature invariably came down; not with profuse sweating, and a sudden fall, to rise again in a few hours, as is the case with Antipyerin, and Antifebrin; but with a steady decline, accompanied by a gradually quieting pulse, a moistening and cleaning tongue, and a gradual relief of bowel distention, and tympanitis. In most cases, the decline in temperature was regular and rapid, and the case proceeded at once to convalescence, without a check. In others, the temperature declined steadily, or was held in check for a time, the tongue cleaned to a certain extent, and the pulse improved, but there remained a strong tendency to retrogression, immediately the administration of the drug was stopped.

Examples of these two classes of cases were afforded by two young officers, belonging to the same regiment, who were both under my care at the same time, in the Station Hospital at Mareilly. 2<sup>d</sup> Lt. W-, gained his regiment, suffering from well marked Typhoid of about nine days duration. He was taken into hospital at once, and put on Salol; he was slightly delirious, and his temperature was  $104^{\circ}4'$ . On the following evening it was  $104^{\circ}6'$ . From this it steadily declined, day, by day,

The delirium passed off: The tongue cleaned: and convalescence proceeded without a check, to perfect recovery. Within a few days of the admission of L: W: 2: L: B-, was admitted in the very earliest stage of the disease, and was treated with Salol from the first. The case proceeded in a very mild way; the evening temperature rarely rising to  $102\frac{1}{2}$ , and never higher. On the thirteenth day, the temperature was normal, morning and evening, and the tongue was moist, and nearly clean; on the fourteenth day, the temperature remained normal. At this period, I handed over the case, in order to take five days leave of absence, and before leaving, I stopped the Salol, under the impression that it was no longer needed. My successor continued the treatment in all other respects, but on my return, I was astonished to find, that L: B's temperature, was ranging, between  $104\frac{1}{2}$ , and  $104\frac{6}{10}$ ; that he was delirious; with a dry brown tongue; sores, on lips and teeth; considerable tympanitis; and commencing congestion of both lungs. The M.D. in charge of the hospital, considered his case most serious, and almost hopeless, and had telegraphed, for his brother to come to him at once. There was no suspicion that any error of diet had been committed. The nurses, were trained Sisters; the orderlies were well trained men; and both L: B. and his friends, were fully alive to the danger which would be incurred, by any indiscretion in diet, on his part; besides which; I have L: B's solemn assurance, that he took nothing but what was allowed in his diet. The administration of Salol was resumed; the temperature gave way; very slowly at first, but afterwards more rapidly, and in five days from its readministration, the evening temperature was again only  $102\frac{1}{2}$ , and the tongue was moist and cleaning rapidly. From this date the case

proceeded slowly, but steadily, towards recovery, and there was no further relapse. Surgeon Clement M. S., was so much struck by this case, that he asked me to give him some Salol for a case of his then in hospital, with a very high temperature, which was entirely unaffected by Antipyrin, and Antifebrin, both of which drugs had failed to reduce it, even temporarily. This man was delirious, with a rapid, weak, and at times fluttering, pulse, a dry, brown, tongue, and rapid accumulation of scales, on lips, and teeth; he had obstinate vomiting, considerable tympanitis, and congestion of the bases of both lungs. Surgeon Clement was afraid to use the cold bath, on account of the cardiac weakness. On the morning of the day following that, on which the administration of Salol was begun; the temperature showed a well marked remission, and the evening temperature, was  $0.3^{\circ}$  lower, than that of the previous one. Inglewin, was then given to check the vomiting. From this time forward, the temperature steadily declined, day, by day, and the patient ultimately made a good recovery, without relapse. These three cases, I think sufficiently indicate the general tenor, of the consecutive series, of twenty seven unselected cases, in which I have used Salol. The temperature in all, fell regularly and slowly; The tongue grew moist and cleaned; Tympanitis was reduced; There was marked absence of the remittent temperature towards the close of the case, and marked freedom from relapse.

If Salol is given from the earliest stages, I think the cold bath will rarely be needed, but it should be given, if the temperature at any time exceeds  $102^{\circ}$ . If the disease only comes under observation when fully developed, the cold bath is a valuable adjunct to

to Salol, as long as the temperature remains above 102°. No case, of either haemorrhage, or perforation, has occurred among the twenty seven, in which I have given Salol. Of course I make no deduction from the statistical evidence, of such a small number of cases, but it seems reasonable to infer, that by protecting the ulcers from the very irritant products of albuminous putrefaction, and placing them under the most favourable conditions for rapid healing, we shall best guard against the occurrence of these two, "unavoidable" Causes of mortality.

In my later cases, where no renal disease was present, I have combined Calomel with the Salol, giving the Calomel in half grain doses, two, or three, times a day, and think the combination of value, in stimulating the excretory functions of the different glands. If there be diarrhoea, derangement of the stomach, vomiting, or great weakness of the digestive power, Influxin, in doses of 2-6 grains is administered, every 2, 3, or 4, hours, according to the nature of the case. If diarrhoea continue, Salol and Calomel are omitted, for a short time, and one m of Carbolic acid in half an ounce of water, is substituted. In all cases of diarrhoea, the lower bowel is at once cleared, by a fairly large enema of equal, containing Castor oil ℥ss, Tincture of Cassia ℥j, Tincture of Opium mʒ; and if much tympanitis be present, a drachm of Turpentine, is added. Throughout the case, the large intestine is evacuated by a similar enema, as often as required to prevent the accumulation of putrefying faecal matter therein. Should great tympanitis exist, a few doses of Turpentine, or pure Terpine, are also given by the mouth, and large linseed poultices,

are applied over the whole abdomen. In hæmorrhage; all other drugs are stopped, and Turpentine, is given freely by the mouth; with occasional hypodermic injections of Ergotine, if the bleeding is profuse. I have however, had no trouble with the two latter complications, since beginning the use of Sedol. Symptoms of commencing pulmonary troubles are constantly watched for, and treated as they arise.

With regard to diet; my invariable practice is to give three to four pints of pure milk, mixed with half the quantity of freshly made barley water, daily, throughout the whole of the acute stage. Should particles of undigested milk appear in the stools, the proportion of barley water is increased, and if needful a little inulin is given. Ice to suck, and a moderate quantity of pure water, at a ~~moder~~ suitable temperature, are allowed, should the patient crave for them.

Should diarrhoea come on, a small quantity of raw meat juice, is a valuable aid to medicinal treatment.

Should there be great debility in the later stages, raw meat juice is given, with, or without, brandy, in quantities carefully regulated to the requirements of the individual case, and if not overdone it is of great value. Alcoholic stimulants, are given ~~carefully~~ when the condition of the pulse and tongue indicate the necessity for them, but rather earlier, and more freely, than would be necessary in the same class of cases, in England. ~~They~~ are always given, if the lungs show signs of commencing congestion. Aerated waters, are never allowed. Egg flip, is avoided as far as possible, but is given; made with ammonia, or Brandy, in cases of pneumonia, where the bowel symptoms, are subordinate to the pulmonary ones. The above dietary, is strictly carried out until the temperature

has been normal for three or four days, and the liver shows signs of returning activity; when a pint of good beef tea, (not merely a watery extract of the salts of the meat), is allowed. If it be thickened with a little wheaten flour, the beef tea is rendered, both more digestible, and more nutritious. If there is constipation, as there usually is at this period, a little strained oatmeal gruel, is allowed, and is usually very grateful to the patient. No further change is made (except to chicken tea, or mutton broth, should the patient prefer either of them to the beef tea), until the temperature has continued to be strictly normal, for fourteen complete days, when a little bread, without crust, is allowed with the broth. If this be tolerated for two days, a little bread and milk is added. In two more days, a custard pudding is added; the quantity of milk and barley water is decreased, and the quantity of broth is slightly increased, next day, three more ounces of bread, without crust, and with a scrape of butter, is permitted. In two more days, a couple of lightly boiled eggs, of which the yolks only must be eaten, are added, and the bread, which has now reached a total of eight ounces a day, is given in any form the patient pleases. At this stage, a small piece of the breast of a carefully selected chicken, or preferably of a snipe, if in season, is given, well roasted, with a little gravy and bread, and a trustworthy attendant is instructed to see, that it is eaten slowly, and very mouthful well chewed. Minced meats, are an abomination, as they are spooned down by the hungry patient unchewed, and unmixed with saliva, and set up gastric irritation, diarrhoea, and not improbably a relapse. After this; the stops are decreased, and solids

are increased pretty rapidly, till a full ordinary diet is reached. No mention of fish, has been made in the above dietary, because in India, they are often hard to get, are always full of bones, and when both these difficulties have been overcome, are not worth eating.

Before closing this essay, I would wish to draw attention to one point about the action of Salal, which has been already touched upon; and that is, the curious absence of the remittent, and intermittent, types of temperature, which so commonly precede convalescence in cases in which it is not used. My hypothesis is, that the proteid waste which escaped elimination as uric acid during the stress of the disease, was stored up in the Spleen; just as according to Dr. Haig's theory, uric acid is stored in the Spleen in Gout; and that during convalescence, when the blood has been to a great extent freed from the products of tissue waste, which have previously been circulating in it; this proteid waste is poured periodically into the circulation, by the now contracting spleen; giving rise by its oxidation, to the periodic rises of temperature; and being eliminated from the blood, in the form of uric acid. This is the more probable, as if the spleen be carefully watched throughout the period of remittent, and intermittent, temperatures, it will be found to be rapidly diminishing in size. Now it is quite possible, that Salal acts on this proteid waste stored within the spleen, in much the same manner, as Salicylic acid, acts on the uric acid stored therein in Gout, assisting in its conversion into a harmless product, which passes into the circulating blood; is converted into uric

acid; and eliminated by the kidneys; without giving rise to any of those unpleasant symptoms, which attend the oxidation of the crude material.

It was my original intention, to have supplemented these notes on the use of Salol, by a large series of cases, accompanied by temperature charts; and I had collected twenty seven consecutive cases in which Salol was given. For more than eight months I have delayed the completion of this essay, in order to collect a sufficient number to be of value, but have only had three "suspicious" cases of continued fever during the whole of that time. Therefore as incomplete statistics, may be made to prove anything, or nothing, and are worse than valueless: I have thought it better to only quote three cases, which are fairly typical of the remainder, and to state in general terms, the advantages, which I believe I have found from the use of Salol. Since writing the above, I see that Dr. Deyardin Beaumetz; advocates the use of Salol in Typhoid fever, but I have not been able to obtain any record of his cases. In conclusion; I must apologize for many deficiencies in this essay, but in up-country stations in India, there is a dearth of standard medical literature; Modern works of reference are hard to obtain; and the apparatus necessary for independent enquiry is wanting. §.

### §. Note.

With the exception of a few months in the winter 1885-86, I have been absent from England; in the Soudan, Upper Burma, and in up-country stations in India; since August 1884: and this must be my excuse for venturing to put forward any claim to the degree of my university on the strength of such a thesis as the above.

I certify that this thesis is entirely my own composition  
and that it is in my own handwriting, and that I have  
been for more than five years in actual practice.

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16<sup>th</sup> Nov - 1891