in the absence of air, as in a flask filled with Pasteur's fluid. But though thus closely resembling the butyric bacillus in appearance, there were some points of difference. One was their growth in gelatine. This was evidently a bad medium; only two out of several inoculations were successful, and, when successful, only a moistening, not liquefaction, took place. A second is the formation of spores in the rods when grown freely in air on the potato.

As previously mentioned, no butyric acid was formed in the Pasteur's fluid; and in a subsequent experiment, in which sterilised skim milk was, after inoculation, kept fermenting actively for twelve days, no butyric acid was detected by the smell and none was obtained on distillation, the acid developed being lactic acid only, yet the quantity of inflammable gas given off was very considerable. The gas was not analysed, but on ignition it burned with a pure blue flame, and in all probability consisted of H with CO_2 . Present notions of the butyric fermentation, by which the development of CO_2 and H are accounted for by the splitting up of two atoms of lactic acid ($\text{C}_3\text{H}_5\text{O}_3$) into one of butyric acid with the development of two atoms of CO_2 and four of H, would necessitate the presence of sufficient quantities of butyric acid to be easily detected by the smell and chemically isolated, yet both the milk and Pasteur's fluid contained practically nothing but lactic acid, though large quantities of gas had been evolved. Of course these differences may depend a good deal on the method and conditions of the fermentation. When potato paste (2 ounces in 8 ounces of water) was inoculated directly from the matter obtained from the stomach, inflammable gas formed freely. The total acidity reached 6.24 grammes of NaHO per litre, and of this 4.28 grammes were due to lactic acid, the rest to butyric, from which the lime salt and the acid itself were obtained pure.

Another curious point about the fermentation is the very highly acid medium in which it took place. For instance, vomited matter, the acidity of which was carefully tested and found to be equal to 3.4 grammes of NaHO per litre, fermented freely when kept at a suitable temperature. The quantity of free HCl in this was ascertained both by ordinary methods of analysis and by adding definite quantities of the filtered vomit to a standard methyl blue solution, and comparing the colour change produced with that of the same solution to which different quantities of diluted HCl had been added. It has been shown conclusively by Klemperer, as had been previously pointed out by Ewald and myself, that methyl violet is not affected by acid albumen, but only by free hydrochloric acid, and as no other agent existed in the gastric juice likely to produce any colour change, any difference produced may fairly be attributed to free HCl present. In both of these ways the free acid was estimated at between 2 and 3 per mille, that is, it exceeded that of normal gastric juice, yet the fermentation proceeded in the fluid with great activity until the acidity reached 4.33 grammes of NaHO per litre. Now butyric fermentation is said in the textbooks only to take place in a feebly acid or neutral medium, yet here we have a fermentation proceeding actively in a medium which is not only highly acid, but the acidity due to one of the most powerful antiseptic agents—free hydrochloric acid. Of course this may be accounted for to a great extent by the quantity of ferment present relatively to the quantity of acid. It is evident that a stomach containing immense numbers of bacteria may set up and carry on effectively a fermentation which under other conditions would be impossible; but there remains still here matter worthy of further investigation by others whose time and means are better fitted for it than mine. I am inclined to think from the results of my investigations that this bacillus, though in many points closely resembling the clostridium butyricum, is not identical with it. Milk mixed with equal parts of 0.4 per cent. of HCl and inoculated with 1 drachm of vomit fermented freely; but, on testing, the milk gave no reaction with methyl violet, showing that the acid was not free, but combined with the albumen of the milk. Potato paste, 2 ounces mixed with 6 ounces of 0.2 per cent. HCl solution, failed to ferment, probably owing to the quantity of albumen present being too small to give sufficient nutritive pabulum for the growth of the organism. When the acidity of some vomited matter which fermented freely was raised from 2.7 grammes of HCl per litre, by the addition of a solution of 0.4 HCl to a little higher stage, the fermentation was completely stopped, though the microscope showed that the fluid still swarmed with active bacilli.

These facts show that this bacillus can grow in gastric juice of normal acidity and even in hyperacid juice. When present in

very large quantity it is evident therefore that it is not the acid of the gastric juice which prevents its development in the stomach in normal digestion. Most probably, as with the common yeast, the reason of its non-development in the normal stomach is that the food is rapidly got rid of before fermentation has time to take place. When, however, owing to stricture of the pylorus, starchy and saccharine matters are constantly retained in the organ, fermentation becomes possible, though secretion be normal. The remedy is to remove the residual contents of the stomach at least once a day, and to wash out freely with antiseptic agents such as I have suggested, whilst carefully regulating the diet and restricting it to such substances as pass most easily out of the stomach, and are least liable to fermentation.

In conclusion, I should just like to point out how desirable it is that a fermentation test should be added to those already in use in investigating cases of stomach disease. Almost all the symptoms of dyspepsia, such as flatulency, acidity, etc., have been hitherto chiefly accounted for by fermentation, but it does not seem to have occurred to anyone how easy it is to put that to experimental proof. If the food after a stay of four hours be removed from the stomach by a tube, previously well washed or steeped in carbolic lotion, into a sterilised flask of suitable capacity, and the latter be connected by means of an india-rubber cork and bent glass tube with a bottle full of water or mercury standing in a basin, it is easy by keeping this on the top of an oven or in a warm chamber, to see whether any gas is evolved. The acidity of the fluid may be tested at the beginning of the experiment, and at the end to determine whether any increase has taken place. In this way the presence or absence of such ferments as give rise to flatulency or acidity may readily be determined. If no gas be developed in twelve hours, the absence of such a quantity of ferment as will produce flatulency in the stomach may be regarded as certain, and if the rise in acidity be slight, or none at all have taken place, fermentation acids as a cause of the symptoms may be safely excluded. It is much easier to prove the absence of gas-producing ferments in this manner than by gelatine cultivations, and such a simple test can be easily used in every doubtful case. It has the advantage over microscopical examinations of giving more easily appreciated and conclusive evidence. There is always a good deal of doubt about the presence of yeast in small quantity when it is mixed, as it usually is, in the contents of the stomach, with all kinds of cells derived from the mucous lining of the stomach and upper parts of the alimentary tract, and with the various remains of partially digested food. Twelve hours' cultivation will serve to develop yeast cells to such an extent as to make a mistake impossible, and the same may be said of micro-organisms. A fermentation test will also show conclusively whether these are gas producers or simply belong to the different innocent forms usually found in the stomach. I have often employed this method, but without taking up too much of your time, I can only here say that it is remarkable in how few cases any gas is evolved.

THE ETIOLOGY OF RHEUMATIC FEVER, AND AN EXPLANATION OF ITS RELATIONS TO OTHER DISEASES.

By B. N. DALTON, M.D. LOND.

THE consideration of certain cases of rheumatic fever which had been under my care led me, some months ago, to form the opinion that this disease might be caused by breathing air contaminated by the emanations from sewers and drains.

Until I read in the JOURNAL of September 14th the report of the recent discussion on the relations of tonsillitis to rheumatism, I was not aware that this opinion was held by anyone else, and the evidence which I could adduce not being adequate to conclusively establish its truth, I had intended to seek for further proof before bringing the matter forward.

The statement made by Dr. Haig-Brown, in the discussion referred to, that the annual percentage of cases of rheumatism amongst the inmates of a large institution had been reduced from 4 per cent. to 1 per cent. of all cases of illness by improvement in the drainage, was so strongly confirmatory of my view, which I found he also held, that I began to write the present paper. Whilst writing it I have obtained so much further evidence in

support of my view that there can, I think, be no doubt of its truth.

Though some few observers have, as I now find, recognised a connection between drain poison and rheumatic fever, it is not generally acknowledged. I have found no allusion made to such cause in any textbook, and, with few exceptions, none of the members of the profession to whom I have mentioned the subject had any idea that rheumatism might be so caused.

The knowledge of such a causal connection is of great practical importance, and seems to give the clue to the explanation of the relations of rheumatism, not only to tonsillitis, but to the other diseases also with which it is frequently associated.

My suspicion of the connection of sewer gas with rheumatic fever was first aroused by the simultaneous occurrence of this disease in two brothers. Their house was free from any serious drain defect, but they had only a few days before returned from seaside lodgings, where their mother had a slight attack of tonsillitis. As tonsillitis is so frequently caused by drain effluvia, there is some ground for thinking that very probably the lodging-house was in an insanitary condition.

Looking back, then, to other cases which had previously been under my care, I found that a considerable number of the patients had certainly been exposed to drain emanations.

It would occupy too much space to give the evidence in detail, and I am obliged to be content with giving very briefly some of the most significant facts which my investigation and inquiries have brought to my notice; but I am sure that if further evidence is required it will be forthcoming from other general practitioners who have investigated the sanitary condition of their patients' dwellings. At least seven cases of rheumatic fever in my practice have occurred in houses in which drain defects existed. In two instances two cases occurred in the same house at the same time. One patient who had had several attacks in two different houses had in both houses been exposed to drain effluvia, which had produced other illness in other members of the family.

A young lady went to stay with an aunt and cousins in the country. After a few days she felt so ill that she returned home, and a few days later was laid up with rheumatic fever. The drains of the lodging-house were defective, and a servant and several of the cousins had sore throat, and one of them diphtheria.

Twenty years ago three of the residents in the dressers' rooms at Guy's went home ill with sore throat; this was in each case followed by rheumatic fever. The waste pipe of the lavatory basin attached to the rooms went straight to the drain. One of the three residents so affected was a former fellow student, and is now one of the surgeons of the hospital. He told me these facts, and said that he had always attributed his rheumatic fever to the emanations from this lavatory basin.

These facts seem sufficient to prove that rheumatic fever may be caused by exposure to the effluvia from drains. That there may be other media than sewage for the development of the poison, and other channels than the respiratory by which it may enter the system, are both probable; but seeing how frequently the air of houses is polluted by sewer gas it seems reasonable to suppose that in towns the poison is thus very frequently introduced into the system. Though a poison may be air-conveyed to the pharynx it may subsequently be carried into the alimentary canal and through its mucous membrane enter the tissues of the body. The poison of rheumatism to be thus conveyed by air must almost certainly be some form of microbe. Such a cause for rheumatic fever has been suggested by Dr. MacLagan and others. In the JOURNAL of June 25th, 1887, is a paper by Dr. Mantle, who found bacteria in the synovia and blood of rheumatic patients and cultivated them, but he did not prove their connection with rheumatism by inoculation.

The probably bacterial nature of the disease brings it into relationship with those diseases which are known, or on good grounds supposed, to be due to micro-organisms, and renders intelligible the points of family likeness which it displays to them. Much might be said on this subject, but it will suffice to allude to: 1. Its tendency, especially if untreated, to run a pretty definite course, lasting on an average about six weeks in adults and about two weeks or less in children. 2. The tendency to relapse, a tendency found in typhoid, and to a less extent in scarlatina and diphtheria. 3. The hyperpyrexia, which, occurring most frequently in rheumatism, occurs also in typhoid and septic peritonitis. 4. The affection of the same parts in rheumatism and pyæmia, and the sweats of these diseases. 5. The greater incidence of the disease on the young, and the comparative im-

munity of the old, in this resembling all the other members of the group. 6. Its greater prevalence at certain seasons, the like being observed with regard to typhoid, scarlatina, and others. 7. The marked hereditary tendency, as with tuberculosis, typhoid, and diphtheria. 8. The liability to repeated attacks, in this respect resembling most erysipelas, and to a less extent measles, diphtheria, and variola.

A recognition of the bacterial nature of the disease also throws light on the influence of those drugs which have been found most useful, namely, iodine, iron, quinine, salicin and salicylates, an influence recognised in other diseases of the same class, drugs beneficial to some extent but for the most part powerless actually to cure, or if curative, requiring a prolonged administration to prevent relapses.

That the poison of rheumatic fever is a special one renders it possible that this disease should coexist with others; that it may enter the system by inhalation of sewer gas renders probable its coexistence with others which may be contracted in the same way. Sewer gas contains many kinds of bacteria, some innocent, some pathogenic, and the person who breathes it may be affected by none, by one, or by more kinds of those present, according to his special susceptibility and according to the kinds, number, and relative proportions of the microbes which happen to be present.

As ordinary yeast is a variable mixture of several kinds of sugar-destroying fungi, which produces results varying with the proportion in which each kind is present, to such an extent that to procure uniform results in brewing it has been found desirable to grow on from a single cell in sterilised liquids the special yeast fungus required; so the emanation from a drain or sewer is a variable mixture of several kinds of disease-producing fungi, and leads to results varying according to the soil it meets with, and according to the kinds of organisms present and their relative proportions.

When Nature inoculates a nutrient medium a mixed growth is almost always the result. Immense pains have to be taken by the bacteriologist to secure a pure culture. Is it to be expected that the human body receiving a mixed inoculation should grow only one kind of organism? Is it not rather a matter for further inquiry that we so often get typical and uncomplicated cases in this class of disease? That some kinds of bacteria are antagonistic to one another may be the explanation, but that two or more may grow together without destroying one another is shown by the concurrence of diphtheria with scarlatina, with measles, and with typhoid; and of whooping-cough with measles. Such combinations are recognised. Why should we not then recognise the combination of rheumatism with other diseases in the same way, instead of seeking some causal connection with those diseases? As the rheumatic poison occurs in a medium that is frequently the vehicle for the conveyance of other disease-producing organisms, there is special reason for its frequent association with the diseases which they produce.

That rheumatism is an addition and not a part of the disease with which it occurs is to my mind a simple and satisfactory explanation of the relation which it bears to tonsillitis. Two poisons are absorbed at the same time; that which produces tonsillitis has a shorter period of incubation than the other, hence the rheumatism follows the throat affection. If the poison of both can exist in drain air, what more probable than their entry together?

In one of my old notebooks is the record of a case attended in 1875. It is headed "Typhoid Fever simulating at first Rheumatic Fever." The patient was a domestic servant, who, at the beginning of her illness, had so much swelling and tenderness of the joints that the nature of the case was at first doubtful. There were subsequently unmistakable symptoms of typhoid, which ran a normal course. I should explain such a case by saying that the patient had at the same time received the two poisons of typhoid and rheumatism; the latter, having a shorter incubation period, manifested itself first. Cases such as the above are, I believe, not very uncommon. If enteric fever be caused by drain air, we see again what probability there is of its association with rheumatism.

The Malta fever or Mediterranean fever, as described in the JOURNAL recently, has amongst its many phenomena marked rheumatic symptoms. It is by many attributed to the emanations from decomposing faecal matter. If it be so, may it not be a combination of rheumatism with other diseases, and thus specially deserve the name which has been suggested for it, "febris complicata?"

With regard to the relation of rheumatism to scarlatina I have some evidence that rheumatism occurs in patients living in houses with defective sanitary arrangements. The last case of scarlatinal rheumatism which I saw was in a house in which a defective soil pipe had been the cause of severe tonsillitis in two other adult members of the family. The patient had been specially exposed to drain emanations just before the scarlatina commenced. Two other cases of rheumatism with scarlatina occurred in houses in which defects in the drains were subsequently found. As sewer gas often penetrates bedrooms from the free communication that exists under floors and between the layers of lath and plaster partitions the poison may enter the system at any date after the scarlatina has developed itself.

Of the association of rheumatism with erysipelas I have met with no instance. Trousseau mentions it as sometimes occurring, and two such cases are reported by Dr. Downes in the JOURNAL of June 25th, 1887. As erysipelas occurs in connection with insanitary surroundings its association with rheumatism is quite intelligible.

How far the explanation of a mixture of poisons will hold with regard to the joint affections occurring in puerperal fever, pyæmia, gonorrhœal and other forms of urethritis, must be left for further inquiry, but I think it probable that in some of these the specific rheumatic poison may be at work. Defective drains may certainly cause puerperal fever, and the worst case of joint affection after urethritis which I have ever seen occurred in a house with gross defects in the drains.

The opinion held by some that there is a combination of rheumatism with gout—a true “rheumatic gout” is not incompatible with the views expressed as to the nature of rheumatism. The gouty constitution may modify the action of the poison and render the disease less acute. With regard to the possibility of various forms of chronic rheumatism being due to the same poison as rheumatic fever, we have good grounds for recognising it, seeing the great differences which occur in tuberculosis, which may manifest itself as an acute or very chronic disease. The changes in the body due to age no doubt play a very important part in modifying the action of the poison, but it is also likely that many individuals have acquired an immunity from the acute effects of the rheumatic poison by a similar process to that by which those constantly making *post-mortem* examinations acquired an immunity, the frequent absorption of poison in small doses rendering the body less vulnerable but not completely invulnerable.

Lastly, I wish to call attention to the explanation which a recognition of the specific nature of the disease and of its common mode of propagation affords of the otherwise unexplained facts given by the British Medical Collective Investigation Committee in their report.¹ The subjects of hereditary predisposition, age, time of year, have been already alluded to, and there remain for special consideration the influence of occupation, of exposure to wet and cold, and of special states of the weather. A table in the report gives out of 655 cases—of which details were sent to the Committee—71 domestic servants, 63 school children, and 46 married women. These together make up more than one-fourth of the whole number. These persons would be much indoors, more constantly in a polluted air due to defective drains; and the domestic servants, specially exposed to effluvia from broken drains under the house, untrapped sink pipes, and from defective soil pipes, from which air is forced when slops are thrown down a closet. Married women, amongst the lower classes, fulfil the duties and are thereby exposed to the same extra risks as servants. Children are by their age rendered specially susceptible. The Committee noted the remarkable number of cases occurring amongst servants, but only suggested that inordinate consumption of meat and beer, and want of exercise, probably rendered them more liable than other classes. Exposure to wet, cold, or fatigue, or a combination of these, is stated in the report to have been a previously existing condition in 430 of the cases. As predisposing causes there can be no doubt that they play an important part. Such causes, by depressing the vital powers, render the body more vulnerable and capable of being acted on by what at another time would have no effect. This is so well known with regard to other diseases that one readily recognises it as a powerful predisposing influence in the case of rheumatism. Fowls, frogs, and fish are under ordinary conditions insusceptible of inoculation with anthrax. Pasteur succeeded in inoculating a hen by administering to her a prolonged cold footbath.² Others have successfully in-

oculated frogs and fish by raising the temperature of their surrounding medium.³ In both these cases, although alteration in the temperature of the blood may have had some influence, the vital processes must have been modified by the abnormal conditions.

The lowering of the vital resistance by the occurrence of another disease may also render an individual more easily affected by the rheumatic poison.

Conditions of weather may act indirectly in causing rheumatism. In cold and wet weather a house is shut up more closely, and the air in it being rendered warmer and less dense than the outside air there is greater suction at any existing leak in soil pipe or drain; hence in such weather there is greater chance of “catching” rheumatic fever. Atmospheric influences come into play in other ways. A certain range of temperature must be specially favourable to the micro-organisms concerned, and a moist still atmosphere would render their stay in and around a dwelling more prolonged than in windy weather. Dr. Haig-Brown states that he has observed outbreaks of tonsillitis and rheumatism after close, still, damp weather.

If the views expressed in this paper be correct, there will be the satisfaction of knowing that rheumatism, the treatment of which even now is more or less unsatisfactory, is to a great extent a preventable disease, the prevalence of which will be to a great extent lessened in the future by the progress of sanitary science. Meanwhile the occurrence of rheumatism, whether well-marked as “rheumatic fever” or manifesting itself only by the so-called “growing pains” of children, or even as a continued fever without joint affection, as is sometimes the case, calls for as thorough and careful an examination of the drainage of the patient’s residence as does the occurrence of typhoid, diphtheria, or any other pythogenic disease.

REPORT ON AN OUTBREAK OF DIPHTHERITIC TONSILLITIS AT ETON COLLEGE IN SEPTEMBER, 1889.

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Soon after the commencement of the Michaelmas term there occurred at Eton College an outbreak of tonsillitis, which could apparently be accounted for by the exceptionally damp, muggy weather which then prevailed, and which was causing the disease in so many parts of the country. Although, however, after a time the weather improved, cases still occurred in the school in increasing numbers, and assumed a more serious and diphtheritic type.

As the drainage of the town and the water supply are known to be as perfect as it is possible to be, it became necessary to endeavour to discover some other cause for the epidemic. Accordingly I turned my attention to the milk supply of the College, and learned that there were no less than fourteen distinct sources from which it was delivered. I found that one farmer supplied entirely six of the houses in which cases of tonsillitis had occurred, and one in which there had been no case; he also supplied half the milk to two other houses, in one of which there had been no case. None of these houses had anything in common excepting the milk supply. There was only one other case in the whole school, in a house not supplied with milk from the above mentioned farm; this was a case of simple tonsillitis, and had probably an independent origin. The above facts seemed strongly to indicate, therefore, that milk taken from a certain farm was the cause of mischief. It was suggested that I should visit this farm, which I did on October 19th, being assisted in every way by the owner. All the cows (eighteen in number) as far as I could discover were perfectly healthy, and no sores could be detected on any parts of their bodies. The sheds were clean, large, and well ventilated. One new cow only had been recently purchased, and she came from a neighbouring parish in which no disease was known to exist. She had a calf three days old when bought, but I was assured that her milk had not been used until the proper time had elapsed. The dairy itself and all pails and cans were clean and in good order, and there was no illness amongst the dairymen employed on the farm or in their families. Water for all purposes was taken from a surface well in the yard, but some distance from

¹ JOURNAL, February 25th, 1888.

² Louis Pasteur: *his Life and Works*, pp. 209, 210.

Crookshank's *Bacteriology*, p. 288.