lished after removal of the plugging stone was remarkable.

The absence of general symptoms is the usual course of events in the early days of mechanical suppression of the renal function. Cases have been reported that have lived for sixteen or twenty-two days before a fatal end. The symptoms described in these cases are contraction of the pupils, sleeplessness, and muscular twitching, none of which were observed in my case. It is quite possible that the impaction of a stone in the ureter of one side may cause reflex inhibition of secretion in the other kidney, and cases which seem to bear out this view have been noticed.

Another point of note is that notwithstanding the fairly active life of my patient, the mass of stone in the left kidney caused no irritation or suppuration.

An important question in such cases is, How long must operative treatment be delayed on the chance of permitting the stone to pass? I remember the late Dr. Moxon, in a clinical lecture on this subject, relating a case in which after fourteen days a calculus passed, and the urinary secretion became re-established; at the same time he threw out a hint that surgical proceedings might one day be taken for such a state of things.

In these cases the onset of dangerous symptoms, however long delayed, is nearly always sudden, and rapidly leads to a fatal termination.

There will probably be a greater tendency to postpone operative measures, if there are no clear indications as to which side should be the one attacked; but the amount of extra shock from operation on the wrong side may be neglected in many cases. It is of course difficult to determine accurately the absence of calculus far down the ureter without incising the ureter and passing down a probe, and this condition could be easily passed over on the supposition that the kidney first explored had its function reflexly inhibited. I do not know whether it would be easier to diagnose the presence and position of a stone in the middle and lower parts of the ureter from the abdomen, but some authors recommend this preliminary examination.

An operation has these advantages over expectant treatment, that the cause is removed, for the probability is that more than one stone will be present, and therefore a repetition of the trouble is prevented. Further, even if the block in the ureter be not removed, life may be preserved by the passage of the renal secretion of the wound.

Whether or no the delay of an operation as long as urgent symptoms are absent increases the mortality after operation must be left for the present undecided, as the cases on which to base an opinion are happily very rare, but it is certain that the retention of the products of urinary secretion in the blood cannot be without deleterious effect.

It is probable that the mortality after operation will always be high, because the condition is one which occurs in chronic gout, and therefore in debilitated subjects. Taking everything into consideration, I feel that the earlier after diagnosis an operation be performed, the better chance the patient will have of recovery.

DEMONSTRATION OF THE TYPHOID BACILLUS IN SUSPECTED DRINKING WATER BY PARIETTI'S METHOD.¹

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On March 30th I was asked by my friend and former hospital colleague, Dr. W. R. Morris, of Waterford, to undertake the examination of a sample of drinking water which he suspected of being concerned in a rather serious outbreak of enteric fever that had occurred in his practice. I sent him a sterilised bottle, and having received the sample, proceeded at once to make a series of plate cultures with quantities varying from 0.25 c.c. to 1 c.c. of the water, which looked pure, sparkling, and free from sediment. The nutrient sub-

stratum was the ordinary peptone gelatine, and the plates were kept at 22° C. Colonies developed with great rapidity, and proved to belong for the most part to rapidly liquefying putrefactive species. Amongst the non-liquefying colonies were several that presented a superficial resemblance to those of Eberth's bacillus, but microscopic examination failed to support their identity. Before it was possible to adequately examine the various slow-growing colonies, liquefaction had proceeded to such an extent as to destroy even the most dilute plates.

Under these circumstances recourse was had to the method proposed by Parietti.² Three tubes, each containing 10 c.c. of neutral broth, received respectively 3, 6, and 9 drops of Parietti's solution (5 g. phenol, 4 g. HCl, 100 c.c. aq. dest.) and 10 drops (a pipette delivering about 15 drops to the c.c. was used) of the water were introduced into each of the tubes which were set aside to develop at 37° C. The next day growth had occurred in the 3-drop and 6-drop tubes only, whilst the 9-drop tube remained clear. The two first-named were then plated out on gelatine at 22° C., and in forty-eight hours the "original" plates were thickly covered with uniform non-liquefying colonies, whilst the dilutions displayed most typical typhoid colonies, some of which gradually attained at 22° C. a diameter of 0.5 c.m. The wavy outline and translucent texture were very striking. A trace transferred to the hanging drop showed the organisms to be short thick bacilli with rounded ends and endowed with most remarkable powers of motility. With some difficulty I succeeded in getting the flagella stained, using the mordant prescribed by Loeffler,³ and 1 c.c. of alkali. They were very numerous, and scattered over the whole surface of the bacilli, which they greatly exceeded in length. They were also much curved. Transplanted on to potato, a typical slimy quasi-invisible growth developed at 22°; at 37° C. the result was not "typical," as a dirty-white opaque streak made its appearance along the middle of the half cylinder of potato (Globig's method), which was much discoloured. The same result was obtained by Kamén,⁴ and it is generally ascribed to the acid reaction of the potato. From my own experience in the present and other cases, I am inclined to think that culture at 37° C., by causing rapid discoloration of the potato, greatly predisposes to this atypical growth.

Finally, I applied the fermentation test as recommended by Theobald Smith, of Washington,⁵ using Southall's ureometer tubes, and 1.5 per cent. solution of glucose and lactose in neutral broth. For comparison I employed a culture of bacillus coli isolated three months ago from the spleen and mesenteric glands of a case of atypical bowel infection indistinguishable clinically from typhoid. The result was peculiar. At the end of twenty-four hours the fluid had been replaced by gas for about an inch of the two coli tubes (glucose and lactose). No evolution of gas whatever had taken place in the glucose tube that had been inoculated with the water organism, but the lactose tube showed about a quarter of an inch of gas. How to read this result as yet I do not know, for Smith does not seem to regard it as possible that an organism which could not split glucose should yet be able to split lactose. In order to exclude the possibility of air having accidentally got into the tube, I allowed the gas to escape, and carefully replaced the tube in the incubator. Next day about the same amount had collected.

Summing up the results obtained, the balance of evidence would seem to be in favour of regarding this bacillus as that of Eberth. Motility, characters, and distribution of flagella, and growth on potato at 22° C., correspond with the classical characters of the typhoid bacillus. But even should the divergence appear to some sufficient to justify separation as a distinct variety in the biological sense, still it seems to me an important matter to demonstrate in drinking water an organism which if not the *B. typhosus* of Eberth is so closely related thereto that we may suppose it with much probability to have a similar origin. It might, perhaps, be regarded as either a colon-bacillus that has undergone extreme modification in the typhoid direction on the supposi-

¹ Read before the Section of State Medicine of the Royal Academy of Medicine in Ireland, April 20th, 1894.

² *Rivista d'Igiene e Sanità pubblica*, 1890, ref. by Kamén in *Centralbl. f. Bakt. u. Parasitenk.*, Bd. xi, 1892, p. 33.

³ *Centralbl. f. Bakt. u. Parasitenk.*, Bd. vii, 1890, p. 625.

⁴ *Loc. cit.*

⁵ *Centralbl. f. Bakt. u. Parasitenk.*, vii, 1890, p. 502.

tion that the views of the Lyons school are correct, or as a typhoid bacillus *sensu stricto*, the characters of which have undergone alteration as the result of a changed environment. That environment can and does alter the biological characters of micro-organisms is indubitable, and this very case affords a striking example of the fact. It will have been noted that the 9-drop Parietti tube remained sterile, although inoculated with typhoid-containing water. A trace of the slightly turbid contents of the 6-drop tube was introduced, and the tube set aside for twenty-four hours at 37°. The next day it was found to be turbid throughout, and plating out yielded a result identical with that above described in the case of the other tubes. Organisms of the colon group are highly flexible in this way. In connection with other water analyses in which they were present, I have pushed the dosage of acid and phenol (as Parietti's solution) to more than 1 c.c. in 10 of the broth, and still obtained growth, but only by gradual acclimatisation. Other organisms may sometimes share in this tolerance; for example, a 15-drop Parietti tube inoculated with sewage proved after incubation at 37° to contain only an enormous coccus, the biology of which I followed no further. For the above reason the requirements of those who insist upon absolute biological and morphological coincidence between the typhoid bacillus of Eberth, described as it has been from the study of cultures made direct from the bodies of typhoid patients, and the typhoid bacillus after many generations spent in such widely different surroundings as the contents of the privy or water pipe, seem to me based upon fallacy.

Dr. Morris has been good enough to furnish me with the following details about the outbreak: "The village contains ten houses with a population of about fifty. The houses are surrounded in each case by farm out-offices, with the inevitable manure-heap in the yard. The pump is surrounded with manure-heaps on every side. During the autumn and early winter it was dry. At Christmas it began to work again, and in order to facilitate matters, it was the custom to procure a bucketful of slush from the nearest pool and pour it down the pump. The first cases occurred during the second week in January, and nearly simultaneously in three houses. There were 18 cases in all, 2 of which died. I had 12 in my practice without a death. My cases were all connected with two houses. In one the entire family—father, mother, son, three daughters were all attacked; in the other a man, his wife and sister, two old women (aged about 70), and a little girl who came to nurse. Eight cases I would count as mild, namely, the two old women and the six young people (aged from 10 to 20). The others I would class as fairly severe, and in each case the severity was due to pneumonia. The pneumonia was right-sided in each case and the lung was long in resolving. Typhoid spots in 9 cases; constipation in 4; no hæmorrhage. Violent early delirium in one case. The diarrhoea generally lasted ten days. Marked temporary deafness in 9 cases..... The two old women and the little girl referred to went to their homes in different villages. I have not heard of any case occurring since then in their locality. I wonder if the typhoid bacillus is contained in butter..... All these parties were making butter and using the water."

The reply to Dr. Morris's very natural question would appear on *a priori* grounds to be decidedly in the affirmative. An experimental investigation of the matter has been published by Laser,⁶ who found that typhoid bacilli preserved their vitality in butter for six days.

In the above observations I have avoided, as far as possible, going into the extensive Continental literature of this subject. I have chiefly aimed at giving a narrative of facts, and have been prompted to do so by the important rôle which may, on the strength of such cases as this, be justly claimed for the bacteriological analysis of drinking water. The accumulation of such results may shortly begin to influence public opinion to so great an extent that chemical results will in no distant future be regarded by public boards as insufficient unless accompanied by the information obtained as the result of careful bacteriological analysis. Every credit is due to practitioners who, alive like Dr. Morris to modern scientific acquisitions, are instrumental in calling attention to such cases as that which I have just reported.

⁶ *Zeitschr. f. Hygiene*, Bd. x, 513.

THE EFFECTS OF COMPRESSION OF THE COMMON CAROTID ARTERY.

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SCHIFF¹ recorded many years ago that the results of temporary occlusion of one common carotid artery were formication and tingling over the opposite side of the body, followed by anæsthesia if the compression was maintained for a longer time. The symptoms began with sensations in the face on the same side, and darkening of vision of the eye on the same side. From this observation, Schiff argued in favour of the crossing of the sensory tracts. In one experiment on himself, Schiff was seized with twitchings of the opposite hand; and in one other experiment in the presence of a doctor, he fell to the opposite anæsthetic side, and was seized with convulsions of the opposite extremities. Schiff notes that the sensations grew most intense in the opposite hand. He attempted to compress both carotids on himself, but his arms fell powerless. In some individuals no result was obtainable, and this was due to free anastomosis, as shown by the continuation of weak pulsation in the external maxillary artery after compression of the carotid.

Mr. Spencer and Professor Horsley,² in a recent most interesting research on the control of hæmorrhage from the middle cerebral artery by compression of the common carotid, found that, on applying the pressure in monkeys, "all pulsation of the middle cerebral immediately ceased." The cortex became paler, and the excitability to electrical stimulation of the so-called motor areas was lost. The compression had no effect in diminishing the blood supply of any other part of the cortex than that supplied by the middle cerebral. Two hours after ligature of the common carotid in the monkey, the excitability and the circulation were again fully established through the anastomosis of the circle of Willis. These results depended on the anatomical arrangement of the middle cerebral, which is a direct continuation of the line of the internal carotid.

Neither in these experiments nor in the surgical cases on record of ligature of the carotid artery has there been any manifestation of spasm or motor discharge resulting from the sudden cortical anæmia. It is very possible that this may be due to the lessened cortical excitability produced by the anæsthetics employed. Temporary paralysis³ with aphonia and anæsthesia has been observed in surgical cases of ligature several times.

On coming across the observation of Schiff several months back, one of us (H.) tried the experiment of compressing the left common carotid, and at once obtained the sensations of formication and tingling spreading down the right side of the body and accompanied with feelings of vertigo. On January 5th, 1894, H. again tried the experiment, and was astounded by the immediate result of half a dozen clonic spasms of the right hand, which at the time lay upon the arm of a chair. The hand felt passively moved as if by some external agent, and consciousness of the spasms arose only from the peripheral sensations produced by the movement. The experiment was followed by vertigo and nausea lasting for some minutes.

On January 12th H. repeated the experiment with the purpose of observing the effects more closely. On compressing the left carotid against the spine, an undefinable sensation, which may be called numbness and pricking, arose in the left eye; this was followed by a distinct and rapid march of formication and numbness down the right arm, right leg, and then up the left leg. At this point the compression was withdrawn; the sensations, however, grew in intensity in the right hand, drew the attention to that

¹ Schiff, *Lehrbuch d. Physiol.*, 1858-59, p. 108.

² "The Control of Hæmorrhage from the Middle Cerebral Artery and its Branches by Compression of the Common Carotid," *BRITISH MEDICAL JOURNAL*, vol. i, 1889, p. 457.

³ A complete history of these cases is to be found in the above paper of Spencer and Horsley.