In an ammoniacal solution the band γ disappeared, and the other two were not only feebler, but had slightly changed their positions: α at $\lambda = 564$ (middle); β at $\lambda = 535$ (middle). There was also, as before, a slight absorption in violet. (Fig. 2.) The urine gave none of the reactions of urobilin.

By oxidation, that is, on being carefully heated with fuming nitric acid, the urine became of an almost black colour, and at the same time gave off small bubbles of a colourless gas (probably CO_2). On carrying the oxidation yet a step further it became of a paler red colour, which gradually passed into a rose, then to an orange, and lastly it returned to the yellow colour of normal urine, and while in this condition it gave the reaction of urobilin, and that, too, very markedly.

By reduction with powdered zinc and sulphuric acid (nascent hydrogen) the red urine became yellow—almost colourless—but reassumed its original red colour on oxidation.

The red pigmentary matter was precipitated from the urine by basic lead acetate as a salmon-coloured deposit; and on the excess of lead being removed by filtration, the filtrate had a red colour, and yielded, on spectroscopic examination three absorption bands: α somewhat feeble at $\lambda = 578$; β more pronounced, but narrow at $\lambda = 539$; γ wider and pronounced at $\lambda = 487$ (Fig. 3.)

On being reduced by means of zinc and sulphuric acid, the above red solution gave a pale yellow, almost colourless, liquid, which, on being filtered and freed from the zinc yielded, with basic lead acetate, an orange precipitate. This precipitate, on being in its turn oxidised with fuming nitric acid gave a garnet-red colour, and showed the same absorption bands as the above spoken of red filtrate from the basic acetate of lead (Fig. 3). On still further oxidation the garnet-red precipitate became almost black, and on the oxidation being carried still a stage further, it became of an orange and then of a yellow colour, and gave, as in the case of the urine before precipitation, the reactions of urobilin.

The original colouring matter in the urine, when precipitated by ammonium sulphate, was of a brownish black hue, and was soluble in water as well as in alcohol (80 per cent.), but insoluble in ether, chloroform, and oil of turpentine. A watery solution of this precipitate gave only one broad absorption band, at $\lambda = 497$. (Fig. 4), while the filtrate from it (which was of a garnet-red colour) gave in addition a very broad band at $\lambda = 499$ (middle).

As will have been noticed, the two cases narrated bear so striking resemblance to each other, both as regards their symptoms and modes of termination, as well as regards the clinical condition of their urines (in as far as their respective examinations went) that one is forced to look upon them as having a common pathology. And the question naturally arises: How can the symptoms be explained and what is their probable pathogenesis? Although it is no doubt to be regretted that no necropsy was obtainable in either case, the loss is perhaps in reality insignificant, for as the physical signs manifested were, for all practical purposes, little better than *nil*, a *post-mortem* examination would probably have revealed no tangible or characteristic structural lesions. Be that as it may, I think I shall be pardoned if I attempt to explain the cause of the symptoms by the facts learned from the analyses of the urines, which, as far as they go, are sufficiently explicit.

To begin with, I think it will not be considered too much if I say that the symptoms in both of the cases closely resemble those met with in what is called auto-intoxication (self-poisoning). For as pointed out there was in each of them a notable absence of appreciable physical signs, associated with well-marked and even grave nerve disturbances—to wit, insomnia and excessive prostration, disordered intellect, and coma, ending in sudden collapse and death. At the same time, in both cases, ischuria was present in a well-marked form, along with decided chemical alterations in the colouring matters of the urine, of a character which unmistakably pointed to a faulty oxidation. For instead of the urine containing normal colouring matter it contained (in the case specially examined) two abnormal pigments. One was a red substance, precipitable by basic lead acetate and ammonium sulphate, possessing characteristic absorption bands, this pigment being by oxidation convertible into urobilin, and by de-oxidation transformed into an almost colourless yellow substance, likewise precipitable by basic lead acetate. Again, from this almost colourless precipitate a red substance could, by oxidation, be obtained, which, by still further oxidation, was in its turn equally convertible into urobilin.

Besides this, as was previously mentioned, the urine contained a

chromogen,² which, like the precipitate just spoken of, was by oxidation transformable into a red colouring matter, which on a still further oxidation was also changed into urobilin.

These transformations can be graphically represented as follows, if we let U represent the urine, C chromogen, and R the red colouring matter.

From these facts it is seen that the abnormal colouring matter is nothing else than a precursory state of the normal urinary pigment, being, in fact, merely a lesser oxidised stage of it. Consequently there must have been a default of oxidation; and if this occurred in the pigment of the urine, we may infer that there equally existed a deficit in the oxidation of the other substances throughout the body. The auto-intoxication then one might suppose would be produced by a default of oxidation—more especially in those substances which originate in the body. As to the cause of this defective oxidation, it would be injudicious in the present state of our knowledge to attempt to theorise. It is, however, I think, by no means too much to expect that when more cases of a similar nature have been met with and carefully investigated, a reasonable explanation of the cause of this faulty oxidation will be found.

THE VALUE OF HYDRONAPHTHOL AS AN ANTISEPTIC.

Presented to the Section of Medicine at the Annual Meeting of the British Medical Association at Birmingham, July, 1890.

By THOMAS H. BRYCE, M.A., M.B.

(From the Bacteriological Laboratory, Western Infirmary, Glasgow.)

Hydronaphthol has been before the profession for some time, but, so far as I am aware, its value as an antiseptic has been tested by only one series of published experiments detailed by Dr. Edington, of Edinburgh, in a paper published in the BRITISH MEDICAL JOURNAL of last year. It is said to be non-poisonous, non-corrosive, and non-irritant, and, moreover, a powerful antiseptic agent. I had the opportunity of seeing it tried as a clinical antiseptic by Dr. Renton of this city, and his disappointment with the results led me to investigate its claims. The fact that the following experiments seem to place an agent which certainly has powerful antiseptic qualities on a surer, if on a narrower basis, is my excuse for laying the result before the profession.

Hydronaphthol is one of the naphthol series, being a derivative of β -naphthol, and occurs as a whitish powder of fine flaky crystals. The preparation I used was that of Messrs. Seabury and Johnston, sold by the chemists as hydronaphthol powder. The powder is freely soluble in alcohol, but is, unfortunately, very insoluble in water. It is not wholly soluble in 2,000 parts of cold water. In hot water (above 70°C .) it is much more soluble, but after cooling there is a considerable deposit.

Preliminary experiments conducted in the manner afterwards detailed with a saturated solution, and also with a boiled and subsequently filtered solution (that is, strongest solutions which could be got in water) proved that each, after one hour's exposure, failed to kill *staphylococcus pyogenes aureus*, though the boiled solution was effective after twenty-four hours. It therefore became necessary to obtain a solution stronger than could be obtained in water alone, and to this I attributed the clinical failure of the antiseptic.

I then tried mixing a spirituous solution with water, but the hydronaphthol was at once thrown out when the solution was stronger than 1 to 20 of spirit, while the precipitation occurred more slowly when a solution of 1 to 40 was mixed with water. Glycerine, however, if added to the spirituous solution in excess,

² Of a slight yellow colour, probably due to another colouring matter in the urine.

prevented this precipitation in water, and I therefore made up a standard solution, thus:

Hydonaphthol	...	...	1 part	} 1 part
Rectified spirit	...	...	10 parts	
Glycerine	...	...	9 parts	

making a brown solution of 1 to 100; and with this I was able to get a clear and permanent solution of any strength.¹

My experiments were designed to test, first, the germicidal, and secondly, the inhibitory power of hydonaphthol. I used a single organism in all the experiments—*staphylococcus pyogenes aureus*—which I selected both for its ready recognition by its distinctive colour and its frequent occurrence. As a preliminary, I demonstrated by plate cultivations that the *staphylococcus* existed in unimpaired vitality in boiled water four days after inoculation.

My first series was conducted as follows: solutions of the antiseptic were made up in sterilised flasks with water which had been boiled for fifteen minutes on three successive days. This sterile solution was then inoculated with the organism, and after the required time had passed, two or three drops of the inoculated solution were transferred by a sterilised pipette or a sterilised platinum wire twisted into a little coil, to a quantity of melted gelatine in a large tube, which was then solidified in a thin film on the interior of the tube by rolling under a tap of cold running water. The tubes were set aside for a week, and the results noted.

The fallacy of contamination was avoided, first, by keeping the gelatine to be used for some time to prove that it was sterile; secondly, by carefully sterilising all vessels and implements; and thirdly, by using sterilised water for making up the solutions. I thus made sure I was dealing with *aureus* alone, and any chance contamination from the air when the tubes were open I could easily discount by the distinctive colour of the colonies of the *staphylococcus*.

Results were controlled in every experiment by a number of (control) solutions treated in the same way, and made up with sterilised water containing rectified spirit and glycerine in quantities corresponding to the hydonaphthol solutions.

Negative results were thus confirmed, and the possibility of the spirit and glycerine in the solutions alone being antiseptic guarded against; again, the possible inhibition of the growth of the organism in the gelatine by the drops of hydonaphthol solution transferred to it was discounted by remelting a tube, which showed a negative result, reinoculating with the organism from a pure culture, and rolling as before. If the organism grew the negative result was thus confirmed. Further, as the reaction between antiseptic and organism must be modified not only by the strength of the solution of the antiseptic, but by the dose of the organism and the length of time during which it is exposed to its action, the experiments were arranged to meet these three factors.

Having first defined 1 to 500, by experiment, as the limit of the power of the antiseptic over a large dose of the organism acting through ten minutes, I made up solutions from 1-400 to 1-2000; and then conducted the experiment as I have described. An attempt was made to estimate approximately the number of organisms in each drop of the solution transferred to the gelatine, by a plate cultivation, but as the colonies which developed were innumerable, the rough method was adapted of adding the inoculating fluid in such quantity as rendered the solution turbid in Series A, slightly milky in Series B, and in Series C such a small dose was added that the solution remained clear. Detailed, the experiments were as follows:

Series A.—Three experiments, the third controlled by making agar-agar plate cultivations after twenty-four hours, as well as rolled gelatine cultures. Large dose: Solutions, one ounce rendered turbid.

Result, 1 to 500 efficient in 15 minutes.

" " 600 " 1 hour.

" " 800 " 24 hours.

Series B.—One experiment. Solutions, one ounce rendered slightly milky.

Result, 1 to 700 efficient after $\frac{1}{2}$ hour and 1 hour.

" " 1,000 " 24 hours.

Series C.—Three experiments. Solutions one ounce clear.

Result, 1 to 800 efficient in $\frac{1}{2}$ hour.

" " 1,500 " 24 hours.

¹ For comparison with other results it should be noted that my solutions were made up by volume as in ordinary practice, with the exception of the initial solution in spirit.

In all the experiments two or three solutions between the one which showed complete sterilisation, and the one which developed a growth equal to that in the control tubes showed an impaired growth.

Series D.—Following the method adopted by Mr. Maylard in his experiments with iodoform, published in the *Annals of Surgery* for January of this year—pus containing organisms was used as the inoculating fluid. There were three experiments of this class.

In No. 1 the pus was obtained from a septic empyema; in No. 2 from a septic chronic abscess, to which, as it contained very few organisms, *staphylococcus aureus* was added and the tube left in the incubator for two days, the original yielding, at the end of that time, numerous colonies of *aureus* and many white colonies; in No. 3 the pus came from a case of acute suppurative peritonitis, and yielded, on plate cultivation, innumerable colonies of *aureus* and some *staphylococcus pyogenes albus*.

The pus was added to about 14 to 16 times its bulk of solution, the tube was well shaken, and, at the end of fifteen minutes, one hour, and twenty-four hours, plate cultivations were made, with one from the original for comparison in each case. As will be seen from the table, the antiseptic took longer to act. Even

Abstract of Experiments.

Hydonaphthol solutions. One-third ounce of				A. Large dose. Exposure.			B. Medium dose. Exposure.			C. Small dose. Exposure.			D. Pus. Exposure.			E. Gelatine + hydonaphthol.
				15 min.	1 hour.	24 hours.	$\frac{1}{2}$ hour.	1 hour.	24 hours.	$\frac{1}{2}$ hour.	1 hour.	24 hours.	15 min.	1 hour.	24 hours.	
1 to 400	...	a	a	a		a	a	a	a	c	c	a	b	a		
1 to 500	...	a	a	a	a	a	a	a	a	c	c	c	c	a		
1 to 600	...	a	a	a	a	a	a	a	a	c	c	c	c	a		
1 to 700	...	a	a	a	a	a	a	a	a	c	c	c	c	a		
1 to 800	...	c	b	a	c	b	a	a	a	c	c	c	c	a		
1 to 900	...									c	c	c	c	a		
1 to 1,000	...		b	b		c	a		b	c	c	c	c	a		
1 to 1,100	...		c	c					b	c	c	c	c	a		
1 to 1,200	...		c	c	b				b	c	c	c	c	a		
1 to 1,300	...		c	c		c	b		b	c	c	c	c	a		
1 to 1,400	...		c	c					b	c	c	c	c	a		
1 to 1,500	...		c	c					b	c	c	c	c	a		
1 to 1,600	...		c	c		c	c		b	c	c	c	c	a		
1 to 1,800	...		c	c					b	c	c	c	c	a		
1 to 2,000	...			c		c	c		b	c	c	c	c	a		
1 to 2,300	...			c					b	c	c	c	c	a		
1 to 2,500	...								b	c	c	c	c	a		
1 to 2,800	...								b	c	c	c	c	a		
1 to 3,000	...								b	c	c	c	c	a		

a, sterile; b, impaired growth; c, profuse as controls; d, contamination from air.

after twenty-four hours 1 to 600 failed to affect the growth of the organisms, while 1 to 400 failed after an hour's exposure.

The fact that a stronger solution, with longer exposure, was required to kill the organisms in pus is interesting, apart from the question of hydonaphthol. A drop of the solution in Series A showed, on cover-glass preparations, a greater number of organisms than a drop of pus iii, so that it is not wholly a matter of the dose of the organisms.

Series E consisted of one experiment; 10 cubic centimetres of nutrient gelatine was carefully measured into 22 tubes, and then sterilised; while still fluid hydonaphthol solution was added to 18 of these, in such proportions that as nearly as possible they represented solutions of 1-500 to 1-5000. To 3 rectified spirit and glycerine was added to serve as control tubes and one was retained pure. The tubes were then sloped, and when the gelatine had solidified *staphylococcus pyogenes aureus* was sown on the surface of each. The organism grew luxuriantly on all above 1-2500, but on all below that strength the hydonaphthol inhibited its growth.

Thus in hydonaphthol we have a powerful antiseptic agent. It cannot, however, be trusted in simple aqueous solution, owing to its insolubility in water within the limits of its germicidal power. Some method, such as I used, must be employed to retain it in solution, and when that is done in 1 to 400 we have a powerful and reliable antiseptic. Its insolubility in water is, however, its great disadvantage, and a cheap and reliable method of preparing an

efficient solution must be devised before it can have any extensive application in surgical practice.

In concluding, I must thank Mr. A. E. Maylard, both for the opportunity of conducting this investigation, the interest he took in the experiments, and the scrutiny he kindly gave to many of my results.

ENDEMIC FEVERS AT BERMUDA.

By CHRISTOPHER HARVEY,
Staff-Surgeon Royal Navy.

[Communicated by THE DIRECTOR-GENERAL OF THE MEDICAL DEPARTMENT OF THE NAVY.]

THE following notes, made when I was a surgeon at the Royal Naval Hospital at Bermuda from 1876 to 1879, having never been published, may be of interest at the present time, now that public attention has been directed to that place by the circumstance of the Guards being sent there.

The islands consist almost entirely of dead and decayed coral rag, which is so porous that rain water can seldom lodge, surface evaporation is small, and the general aspect of the soil is dry. There are no running streams of water in the islands; the people rely upon rain water collected in tanks, or upon well water, which is usually brackish. There are numerous marshes or "ponds" as they are called in local language, many of them in direct communication with the sea. These swamps are frequently fringed by mangrove trees, among whose thick and tangled roots are vegetable matters undergoing chemical change.

These appear to be all the local materials necessary for the formation of miasmata and the propagation of those febrile diseases characterised by intermissions and remissions; but so far from these swampy districts being considered noxious, there is a prevailing opinion in the colony that those persons who reside in close proximity to them actually enjoy better health and live to a greater age than those who live at a distance from them or on higher ground. Analysis of the deposits in these swamps reveals a considerable proportion of tannates and a comparative absence of animal matter, which, with the fact that most of the trees and plants are non-deciduous, aided by the known purifying action of salt tidal water, must prevent excessive decomposition, and may not unreasonably be accepted as the causes of their negative qualities.

The absence of any authentic records of the causes of deaths in the islands prior to 1867 (from which period returns are available) gives to the reports of the Royal Naval Hospital a peculiar interest, as they continue there so far back as 1811 without a single break, before which date the diseases of Bermuda are known only by tradition. The state of the Royal Naval Hospital, as long as it continued to receive the sick from troops stationed throughout the islands, afforded a good indication of the general health of the colony, this advantage being greatly enhanced by the addition in 1824 of sick convicts, which class of patients continued to be admitted within its walls until 1848, whilst a portion of the troops enjoyed that privilege down to so recent a date as 1875.

Periodic Fevers.—Remittent fever, during the first half of the present century, was locally believed to be endemic in Bermuda, and was considered to be the only form of local fever prevailing there. It was thought to be the same as the common bilious remittent of tropical climates, and liable to become epidemic by sufficiently exciting causes. Many persons believe that its aggravated form constituted yellow fever, and that they were but types of the same disease. Beginning in the summer as a mild remittent, it became severe in proportion to the advance of the season, the increase of temperature and excess of rainfall over the average, and declined with the advent of cold weather. So marked was this that the first case rarely occurred until June, and few were heard of after the middle of December.

Intermittent Fever (Ague) has never been known to arise spontaneously in the islands; moreover, persons who have contracted malarial fever elsewhere often resort to Bermuda as a restorative. It is not a little striking that those who declare remittent fever to be endemic, also admit that the allied fever intermittent never occurs.

These fevers have no place in the modern returns of sickness in the islands, while the number of cases of enteric fever returned has vastly increased, and this word enteric (or typhoid) has now absorbed all or nearly all those fevers with varied names that were

formerly described. The records of the Naval Hospital indisputably prove that remittent fever does not occur in the islands. The first case on the books was in 1832; no other cases appear until 1846, and those recorded since were all received from vessels after their arrival from the West Indies, etc. Of intermittent fever, 93 cases only were treated between 1811 and 1879, not one of which was contracted in the islands.

Continued Fevers.—The continued fevers of Bermuda, excluding yellow fever and dengue, which are acknowledged exotics, have frequently been the subject of discussion, and until recent years the different forms had not been sufficiently defined to enable any practical deduction as to their nature to be drawn. There can be no doubt that fevers have always, more or less, prevailed at Bermuda. Historical records show that towards the end of the last century, at least as far back as 1780, when no Imperial Government establishments existed in the islands, a fever occurred every year during the summer and autumn months, which was termed typhus fever, a name it continued to bear until the early part of the present century, when it became known among the local practitioners as remittent fever, while at the Naval Hospital, even as early as 1812 it was recognised as a continued fever, and was returned as such with occasionally typhus. Thus in the returns of the Naval Hospital five cases of typhus fever are shown in the thirty-three years from 1811 to 1844 and none of enteric or typhoid during the same period, while no fewer than 872 cases of continued fever are entered under the various designations of fever, febricula, synochus and mixed fever. That a large proportion of these were cases of enteric fever is proved by reference to the records of the *post-mortem* examination made at the time. Thus in 1825 two cases of "mixed fever partaking of the nature of synochus and typhus, were characterised by the discharge of blood from the intestines." In 1844, on dissection of a man who died from "synochus, the ileum so far down as the ileo-cæcal valve presented one chain of extensive and deep excavations." In 1847 the cases revealed "ulceration of the cæcum," and again in 1848 two cases of synochus showed ulceration of the ileum, and in 1850 a case displayed several congested patches of the intestines, and these lesions were constantly observed until the year 1858, when five cases entered as continued fever are described in the remarks by Dr. Rees as typhoid, and this is the first distinct reference to the disease as such in the records to the hospital. In the following year a case died in which "Peyer's patches" were ulcerated. From that date to the present time typhoid or enteric fever frequently occurs in the returns. In 1862 six cases were entered as such from the ships of war, and since that year the disease has always been returned under its distinctive appellation.

In the year 1846 fever of a mild type prevailed generally throughout the islands during the summer and autumn months, causing 85 entries into the Naval Hospital from the naval, military, and convict establishments, with 3 deaths. The disease was returned as remittent fever, but no *post-mortem* notes could be found. Again, in 1849, many cases described as aggravated remittent by the local practitioners, appeared among the native inhabitants, and although it spread among them to such an extent as to amount to an epidemic, it did not extend to strangers—a fact of significance, as the latter belonged to the Government establishments, whose water supply was from a different source to the natives, and who were probably not allowed to mix with them during the progress of the fever. No account is given of its mortality, but the death-rate among the civil population rose from 15.8 per 1,000 of the year before to 21.2, falling to 12.3 the following year. No cases were treated at the Naval Hospital.

These examples are doubtless instances of localised epidemics of enteric fever, which in the olden days were described as remittent (as explained under that subject), but which now are classified correctly as a result of improved pathological and clinical knowledge; for we cannot doubt, from the evidence of the *post-mortem* examinations at the Naval Hospital, of the existence of typhoid in the colony as far back at least as 1825; the autumnal fever of Bermuda was in reality enteric. It must not, however, be overlooked that general practitioners in a place like Bermuda rarely have the opportunity of examining the bodies of persons dying from obvious diseases, while the naval and military medical officers nearly always hold a *post-mortem* examination on patients dying in hospital, and so would naturally be the first to discover the true nature of the fever in question. It is now a recognised fact that enteric fever manifests itself to some extent every summer and autumn among the civil population, sometimes even assuming the proportions of a localised epidemic.