

is to be given of the diseases attributable to impure water, followed by an account of "germs" and the part they play in the causation of disease. Filters are to be dealt with, also sterilisation and the utilisation of chemicals for the routine purification of water. Arrangements are being made in the Southern Command by officers of the Royal Army Medical Corps to lecture at the more important centres of the Territorial Force.

#### THE SLEEPING SICKNESS COMMISSION.

Captain F. Percival Mackie, I.M.S., has been selected by the Government of India to join the Sleeping Sickness Commission which has recently left England for Uganda under the auspices of the Royal Society and under the direction of Colonel Sir David Bruce, R.A.M.C. Captain Mackie hands over his duties as assistant to the Director, Bombay Bacteriological Laboratory, and joins Sir David Bruce and his party at Mombasa.

#### ROYAL ARMY MEDICAL CORPS EXAMINATION.

An examination of candidates for not fewer than 30 commissions in the Royal Army Medical Corps will be held on Jan. 27th and following days. Application should be made to the Secretary, War Office, London, S.W., not later than Jan. 18th next, on which date the list will be closed. The presence of candidates will be required in London from Jan. 25th.

## Correspondence.

"Audi alteram partem."

### THE CONTAMINATION OF MILK.

To the Editor of THE LANCET.

SIR,—The question of the contamination of milk is one of immense public importance and is now beginning to receive the amount of attention which it deserves. As evidence of this I need only refer to the recent exhaustive report on the subject issued on behalf of the East and West Ridings of Yorkshire, to another important report by the medical officer of health of Chester to which you have devoted a leading article in THE LANCET of Oct. 24th, p. 1226, and also to a notice in the Times of Oct. 26th in reference to the action of the public health committee of the London County Council.

In the very elaborate work on "The Bacteriology of Milk" by Swithinbank and Newman (1903) they say "that there are two rises and two falls in the number of bacteria [in milk], the first being due to extraneous organisms, and the second to lactic acid organisms, "and they believe this" to be the almost universal rule in untreated 'natural' milk" (p. 135). At the first decline "the common extraneous bacteria die out, and that for the very simple reason that they cannot live in the presence of the new tide of acid-forming bacteria." This is said to occur after about 12 hours at temperatures between 60° and 98° F., whilst the second decline begins after six days at 60° and after 72 hours at 98°. As the decline progresses moulds (*Oidium lactis*, &c.) increase, so that ultimately these latter completely replace the bacteria. A similar statement is made by these authorities in reference to the "ripening of cream," concerning which they say (pp. 187-189), "In three or four days the number of organisms [bacteria] found in cream has abated, and eventually nothing remains but *Oidium lactis*, as in milk."

Seeing that these are the natural changes that occur in milk and cream one would expect to find very distinct references in reports on "the contamination of milk" to the presence and abundance therein of spores and conidia of moulds, these being bodies enormously larger than bacteria. Hitherto, however, I have been unable to find any specific statements on this subject, either in the Yorkshire report or even in the important work from which I have quoted, in which the inevitable appearance of moulds is so explicitly stated. This is to me all the more surprising because of certain observations of my own many times repeated during the last eight years. The first and simplest observations were of this nature. Fresh milk from one of the very best London companies was poured into a small one ounce sterilised beaker over which another was inverted. If left at a temperature of about 70° F. a thick layer of cream soon rises to the surface, and when minute portions of this are

examined by the microscope after about 33 hours though myriads of bacteria are to be seen no spores of mould can be detected; but in from 40 to 45 hours I have invariably found conidia of mould together with more or less mycelium infiltrating the layer of cream.

After studying the work of Swithinbank and Newman a few months since and reading the Yorkshire report I have repeated these observations, and carried them much further with the purest milk that I could obtain both from London and the country. The new observations have been of this nature. A shallow Petri dish containing some layers of blotting paper on which a microscope slip was placed was thoroughly sterilised (so that the blotting paper became scorched); and as soon as this had cooled a little some recently boiled distilled water was poured over the blotting paper, and a single drop of fresh milk was placed by means of a sterilised pipette on the centre of the microscope slip. The cover of the Petri dish, which had been thus briefly removed, was then replaced, arrangements having been made that in this damp chamber the cover should come into contact with the drop of milk, which was thus fixed between it and the microscope slip. This arrangement was made because it had been found that conidia of mould always tended to subside to the level of the slip. Therefore, when the cover of the Petri dish was removed for examination of the drop after some days the less important portion of the milk was carried away with it. The application of the cover glass to the slip could then be made and the result of the experiment studied.

What I have invariably found has been this: that in four or five days with a temperature of 70° to 65° F. thousands of torula-like bodies or conidia were to be found among the milk globules and swarms of bacteria. It seems perfectly clear that the milk has to be rendered acid by the swarms of lactic bacteria before the moulds can make their appearance. But we may well ask, Whence come they? Can we believe that the air is so full of the spores or conidia of *Oidium lactis* or other common moulds that in every single drop of the purest and most carefully preserved milks supplied to the public these moulds can be made to make their appearance?

My observations are thoroughly in accord with the statements of Swithinbank and Newman as to the natural changes occurring in milk and cream. Yet the bacteriologists tell us nothing about the prevalence of the conidia of moulds in milk, and although they are so much larger than bacteria they seem almost to elude their observation. I searched carefully for information on this subject in the Yorkshire report and the only definite statement I could find is that made on p. 56, in which Dr. Thomas Orr says in reference to the examination of sediments obtained by the centrifuge from various specimens of milk: "Bacteria were always present and sometimes yeast cells." Surely bacteriologists ought to tell us something more about the invariable appearance of moulds in milk.

I am, Sir, yours faithfully,

H. CHARLTON BASTIAN.

The Athenæum, Pall Mall, S.W., Oct. 26th, 1908.

### ON CHRONIC MORPHINISM AND ITS TREATMENT.

To the Editor of THE LANCET.

SIR,—In THE LANCET of August 15th, p. 439, there appeared what may be termed a "causerie" on the Opium Habit by Sir Dyce Duckworth, characterised, as Professor A. Gamgee in his subsequent paper in THE LANCET of Sept. 12th, p. 794, remarks, by much grace of style, but which, taken as an exposition of the question by a medical teacher, shows conclusively how little the subject is understood in England.

"Sir Dyce Duckworth," says Professor Gamgee, "attempts no analysis of the essential causes, leaves absolutely untouched the nature and the clinical characters of the multiple cravings ..... which are rendered evident by the circulatory, respiratory, digestive, metabolic, and nervous phenomena. .... He ignores the methods by which these distressing symptoms may be combated ..... and he throws the whole weight of his authority" in favour of what Professor Gamgee calls the "extremely vicious" English system, a system which to recommend at the present day, when the morphia habit can be cured without the infliction of suffering, is, as Mr. J. Q. Donald remarks, "almost criminal," and shows either the most unpardonable ignorance or the most surprising inhumanity.

In his subsequent paper Professor Gamgee has corrected Sir Dyce Duckworth's omissions and errors and has given a most excellent *résumé* of what is known to those who have given special attention to the subject. And I can myself endorse all the more fully nearly every word that he says, in that there is scarcely a line in which he has not been anticipated by myself and which is not simply a repetition of what I have myself written. As far back as 1888 in the *Encephale*, in my "Cure of the Morphia Habit" of 1890 and 1901, in the "Medical Annual of 1894," and in your own columns in 1901, I have repeatedly insisted on the same points as Professor Gamgee—the multiple nature of the craving, the three great indications of treatment, and the three great means of relief, identically the same as those now proposed by Professor Gamgee and which I have even called my "therapeutic triad."

I will ask your permission to contrast as briefly as possible Professor Gamgee's propositions with those to be found in the different contributions of mine just mentioned. Rightly recognising that the understanding of the morphia habit depends upon a proper comprehension of the nature of the craving, Professor Gamgee insists upon the *multiple causes* of this condition—its nervous, circulatory, respiratory, digestive, and metabolic elements. I pointed out as far back as 1887 that the craving was not to be looked upon as a pathological entity but that it could be split up into numerous "factors" which are the different physiological perturbations caused by the want of the accustomed stimulant. It is true that I have never seen primary respiratory trouble. This is nearly always of cardiac causation and therefore classable under circulatory causes, and with the latter I have insisted upon all the other "causes of craving" mentioned by Professor Gamgee—the digestive, the nervous, and the metabolic—and these constitute my "factors of craving." It is the recognition of the true nature of these "multiple causes," or "factors of craving," that enables us to decide what are the indications of treatment. Professor Gamgee finds that from these "multiple causes" three principal "groups of symptoms" may be distinguished and that these require for their relief three kinds of therapeutic intervention. Professor Gamgee's "groups of symptoms" are (1) the nervous symptoms; (2) the circulatory and respiratory symptoms; and (3) the gastro-intestinal symptoms, for which he respectively advises (a) very hot baths, (b) heart tonics (i.e., digitalis and strychnine), and (c) Vichy water. These three "*groups of symptoms*" are identical with my three great "*indications of treatment*": (1) nervous irritability, (2) cardiac disturbance and failure, and (3) stomach difficulty, the only difference being that I have never seen "gastritis," for which Professor Gamgee recommends the Vichy water, but simply functional trouble, perversion of secretion, chiefly hyperacidity, which fact has been confirmed by Hitzig and Erlenmeyer. If Professor Gamgee has not written the word "gastritis" hastily and really means that this condition occurs in his patients I can only attribute it to his very mistaken practice of substituting morphia by the mouth for the hypodermic injections and of keeping his patients in bed. I would point out that Mr. Donald appears to be of the same opinion. He tells us that he has not found symptoms of gastro-intestinal catarrh to be common and has had no difficulty in treating such symptoms *except when the patient has been in the habit of taking the morphia by the mouth*. Professor Gamgee's three "lines of treatment" are, again, identical with what I have called "my therapeutic triad," which consists of (1) the Turkish bath, or when this is not obtainable the hot-air bath, followed by hot and tepid douches; (2) heart tonics (digitalis and sparteine); and (3) bicarbonate of soda, especially in the form of Vichy water.

In two details only there is a slight difference between Professor Gamgee's three lines of treatment and my therapeutic triad. The first concerns the nature of the hot baths, Professor Gamgee recommending an ordinary very hot bath, whereas I much prefer the Turkish bath or hot douches. There is not the slightest doubt about the relief experienced by the morphia habitué in an ordinary very hot bath (at a temperature of from 39° to 40° C.), *whilst it lasts*, but prolonged baths of this kind are depressing, especially to the heart, and often cause increased craving afterwards. Within the last few days I have had an example of this, a bath at 40° C. taken at 10 P.M. instead of the douche, which was out of order, leaving the patient in a state of

restlessness and agitation, particularly felt in the feet and hands, which were swollen and puffy, clearly showing the general atonic condition. This was further shown by the presence in the urine voided in the night of ten centigrammes per litre of albumin. An extra dose of morphine would have set things right, but the patient had fortitude enough to do without it, and after a sleepless and miserable night things righted themselves in the morning. The Turkish or hot-air bath is, on the contrary, both sedative and tonic, and irritability being the expression of weakness it is evident that a toni-sedative application must be more efficacious than a purely sedative one. When a properly appointed Turkish bath is not available an ordinary hot-air bath, ending with a tepid douche as cool as can be borne, which enhances the tonic effect, is a good substitute, but best of all are repeated hot douches at from 43° to 45° C. of short duration, and these should be *brisées* for the body but directed on the feet with full strength. The percussion thus produced leaves the whole organism stinging with *bien-être* and straightened out to a degree that is almost incredible. I have seen patients crawl to the douche at the hour when their dose of morphia was due, tired out and full of pain, with the craving which would soon become unbearable coming on, and often so prostrate that it was necessary to help them to undress. A few minutes later, strengthened and soothed, they have been able to dress briskly and have walked away full of energy, all desire for morphia temporarily suppressed, the allowed dose being deferred for some hours. This I can affirm not only from many hundreds of observations but also from personal experience in my own weaning. Another detail in which Professor Gamgee differs from me is the recommendation of washing out the stomach with Vichy water instead of simply giving it as a drink. Professor Gamgee says that it is because his patients suffer from *gastritis*, which is due no doubt to the substitution of morphia *per os* for hypodermic injections. My own plan is to give the Vichy water as a drink, because there is not only hyperacidity of the stomach but also an acid condition of the system generally, which washing out the stomach would not reach.

Where, however, I differ *in toto* with Professor Gamgee is concerning the conditions under which the treatment should be applied. Professor Gamgee, following many other writers, recommends the patient to be kept in bed and morphia to be given by the mouth instead of by hypodermic injection. I never give the morphia by the mouth and I have never met with the slightest symptom of gastritis. Keeping morphine *habitués* in bed only aggravates the monotony of the suppression, whereas the great object is to amuse and distract the patient and make time pass as rapidly as possible. I used to give rectal injections as soon as the patient had decreased hypodermically as far as is possible without discomfort, but since the introduction of dionine and hyoscine I usually give these *at this time*. If dionine is given as a substitute at once, as in the German plan of decreasing the morphia progressively and increasing the dionine in inverse ratio, by the time the morphine is suppressed there will be a certain addiction for dionine and difficulty may be experienced in giving it up. My plan concerning the administration of dionine is founded upon the fact that when morphia is suppressed suddenly there is the most unbearable suffering for five or six days and then recovery. Hence I give dionine when I have reduced the morphia as far as possible for the same length of time that the patient would otherwise have suffered—i.e., for five or six days *in full doses*. It must then be stopped in four more days. Hyoscine has been chiefly used in America as a "knock-out" cure, much vaunted by some, but strongly condemned by Crothers, with whom I agree to a great extent. I have, however, found it extremely useful in moderate doses towards the end of the cure, but there is no succedaneum that is suitable for all cases.

My present plan is the application of my three factors of treatment with progressive decrease of the strength of the hypodermic injections to a vanishing point, the constantly reducing dose being given always in the same quantity of salt water in a 0.7 per cent. solution. If plain water is used the injections, when the solution gets below a strength of 1 in 50, become gradually more and more painful, which in itself is not a disadvantage as it renders them less desirable. But as watery solutions of morphia become still weaker they cause inflammatory induration which may lead to abscesses.

whereas with morphia in salt solution there is neither pain nor inflammation. Rectal injections are given only at the last moment. Some patients prefer to be kept ignorant of their rate of progress, and I accede to their wishes, but I never use constraint or compulsion. The ideal way is for the patient to decrease every day by his own initiative, the *ensemble* of the treatment preventing any discomfort that might stand in the way. This constitutes a real re-education of the will which is the best guarantee against future relapse.

I may conclude by saying that with this treatment, so persistently advocated by me, unheeded by the profession but now shown to be absolutely scientific, and as it has been called "physiological," by Professor Gamgee, supplemented by the means I have alluded to, the morphia habit is absolutely curable. The profession should know that on the lines laid down in the contributions to THE LANCET by Professor Gamgee and myself, the morphia habit is, *in those who really intend to give it up*, a very easy and simple thing to cure. It is therefore the bounden duty of medical men who undertake the treatment of morphia patients to learn how to treat such cases, inasmuch as it is through medical ignorance and carelessness that the habit is first formed, and by medical incompetence in treatment that the patient is led to believe his case is hopeless and that that chronic discouragement is brought about which is the chief element of difficulty.

Montreux, Oct. 1st, 1908.

OSCAR JENNINGS.

## DIVISION OF THE AUDITORY NERVE FOR PAINFUL TINNITUS.

To the Editor of THE LANCET.

SIR,—In reference to Mr. C. A. Ballance's case of division of the auditory nerve for painful tinnitus published in THE LANCET of Oct. 10th it would be interesting if he would provide your readers with a detailed account of the operation. Personally I should also like to hear from Mr. Ballance why he did not open and destroy the cochlea in preference to submitting the patient to the more serious cranial operation. The result in the case published by me in your issue of Sept. 19th has been so excellent (the patient having had no return of the tinnitus since his operation) that I should hesitate to proceed to division of the nerve before destruction of the cochlea had been tried. I note that Mr. Ballance's patient was free from tinnitus on May 24th, four months after the operation.

I am, Sir, yours faithfully,

Harley-street, W., Oct. 22nd, 1908.

MACLEOD YEARSLEY.

To the Editor of THE LANCET.

SIR,—I was much interested in reading in your issue of Oct. 10th an account of an operation under the above heading by Mr. Charles A. Ballance. In his remarks Mr. Ballance says: "Attempts to divide the auditory nerve have been previously made, but I am not aware that the operation I have just described was employed in any of them."

May I point out that I had previously successfully performed the operation exactly in the way described by Mr. Ballance? We differ only in the means used to displace the cerebellum. The patient was a man, aged 42 years, under the care of Dr. A. P. Beddard, upon whose suggestion I undertook the operation. The operation was done in two stages on March 13th and 24th, 1904, and was completely successful. Mr. Richard Lake mentions my case in his instructive paper "L'Etat Actuel de nos Connaissances au Point de Vue des Interventions Opératoires dans le Vertige et les Bourdonnements d'Oreilles" (*Archives Internationales de Laryngologie*, 1904), and it is also referred to in that excellent work by Mr. P. Macleod Yearsley, "A Text-book of Diseases of the Ear."—I am, Sir, yours faithfully,

Harley-street, W., Oct. 26th, 1908.

DONALD ARMOUR.

## NOTES ON A CASE OF ACUTE GOITRE.

To the Editor of THE LANCET.

SIR,—In reference to the case reported by Dr. S. J. O. Dickins in THE LANCET of Oct. 24th, these fatal cases of goitre by sudden increased pressure on the trachea are more common in Cumberland than they seem to be in Sussex, and we are constantly alive to the possibility of such occurring

anywhere in this district. Intubation of the trachea or section of the isthmus is equally futile, and the only plan likely to succeed is liberation of the windpipe from pressure by immediate removal of half of the enlarged thyroid gland. I have tried tracheotomy years ago, also division of the isthmus, and am confident that the removal of the pressure is the only course to adopt. The soft trachea is so squeezed, compressed, and altered in shape that a very slight increase of pressure upon it will induce suffocation. When the trachea is hard, as it becomes with age, there is no fear of compression or suffocation.

Of course, any ordinary goitre will waste under the use of large doses of iodide of potassium, but it is not always safe to wait for this gradual method of absorption. In the *International Clinics* for 1896, Vol. IV., fifth series, p. 242, I contributed a paper upon the Operative Treatment of Goitre, and included in the remarks are some cases similar in their symptoms to the case Dr. Dickins has described.

I am, Sir, yours faithfully,

Carlisle, Oct. 26th, 1908.

H. A. LEDIARD.

## THE RELATION OF THE PHARMACIST TO THE PHYSICIAN.

To the Editor of THE LANCET.

SIR,—I was much interested by reading your annotation under the above title in your issue of August 22nd, p. 576. It deals with the same subject and almost from the same point of view as I did in one or two letters (on Copyright of Prescriptions) that I wrote to you ten years ago. According to Dr. C. S. N. Hallberg, whose article in the July *Bulletin of the American Pharmaceutical Association* you quote, a prescription is designed only for a particular person, for a particular purpose, at a particular time; it is the utterance of the prescriber, who alone should direct and control its employment. This is the opinion I have always maintained, and I believe it to be quite reasonable and right. It is a physician's duty, in giving a prescription, to direct for how long it is to be employed; if he does not do so he might be held to be guilty of negligence. It is a dangerous abuse for the prescription to be used by other people or even by the same patient at another time without skilled advice. There are very few prescriptions at all that could be left with entire safety to the discretion of the patient.

For the last 12 months or so I have adopted a method which seems to overcome the difficulty, and perhaps some of your readers might be glad to adopt it also. It is the custom in this country for medical men to write their prescriptions on slips of paper, on which are printed their names, addresses, and hours of consultation. On each such slip I have had printed also, near the top, in red ink, the words, "Not to be repeated after ..... days." The date is written immediately above. The scheme was approved by the Medical Defence Association of this State, and I understand that some other medical men have now resorted to it and have found it work well. I have had no trouble with patients. Most of those to whom I have explained it appreciate it as extra care and attention. Nor have any druggists, so far as I know, infringed the prohibition.

I am, Sir, yours faithfully,

F. LUCAS BENHAM, M.D., M.R.C.P. Lond.

Exeter, South Australia, Sept. 23rd, 1908.

## THE CAUSE AND PREVENTION OF DENTAL CARIES.

To the Editor of THE LANCET.

SIR,—If Dr. O. Clayton Jones had read the original communication referred to by Dr. Harry Campbell I doubt if he would have said that "every theory of dieting has been tried on the children of this generation without effect." For in that communication I wrote: "Now that we know the rôle of the foodstuffs in the etiology of caries it is possible for us to show how the disease may be prevented, and although it has been said that people would not adopt the method we advocate it is a fact that already many people have adopted it and when it has been commenced at a sufficiently early age the results have so far been absolutely perfect." The latter part of the above sentence refers to ten children who were brought up according to the method advocated, and every one of these children has perfect teeth. This may not appear to