portion of the same beer, with arsenic in such proportion as gave a large deposit in the heated tube, I added a very small quantity of a bi-sulphite of calcium, and got no deposit of arsenic on the heated tube, only sulphur appearing there. The arsenic was obtained from the flame at the end of the tube when received on porcelain. The difficulty in connexion with the 12 samples of Manchester beer is now explained. I had to do the samples in a day. I did take 12 working hours and knowing nothing of the presence of sulphite I found no arsenic. I may say that I had only about 11 ounces per sample, and in the small quantities which I used for experiments the trace of arsenic was probably lost in the preparation of the beer for the Marsh test. I may premise that I tried a series of experiments with the original beer. Any test to succeed must deal with not less than 100 cubic centimetres, and this should be charred *only to intumescence* with acid, then diluted and filtered, and the filtrate evaporated. I did this process in flasks, but found that the medical officer here was doing the process in porcelain basins which I now adopt. If one omits to evaporate the filtrate sulphurous acid may easily be present (carbonaceous matter and sulphuric acid) and spoil the experiment.

I am, Sirs, yours faithfully,

CHARLES ESTCOURT,

Public Analyst to the City of Manchester.

Manchester, Dec. 10th, 1900.

To the Editors of THE LANCET.

SIRS,—The obscure nature of chronic poisoning by small doses of arsenic has been shown in a startling manner by the wholesale beer-poisoning outbreak in Manchester. Does not the revelation point a moral as to the harm that may be done by administering small doses of arsenic in medicine over a prolonged period? That arsenic is an irritant to the kidneys, skin, mucous membranes, and all other excretory outlets to the body has long since been experimentally established. Yet the drug is given daily by medical men and its use is extended over long periods of time. In one case under the writer's care a patient had been treated with arsenic for years at various skin hospitals. He is suffering from chronic Bright's disease, which, in the absence of other assignable cause, may, not unreasonably, be attributed to the arsenic. It is a well-known fact that during a course of arsenic a patient's urine may become albuminous. So strongly have I felt upon this subject that personally I have practically ceased for years past from prescribing arsenic either in hospital or in private practice. It has been said somewhere that arsenic never yet cured a skin disease—a dictum with which most dermatologists will be inclined to agree—although it undoubtedly now and then appears to modify favourably some cutaneous troubles. The question arises, is it worth while for the sake of this remote possibility to expose a patient to the risk of grave mischief from the action of a virulent irritant poison upon excretory and other organs? Given in minute doses for a short time the drug certainly appears to exercise a beneficial effect in some cases of anæmia.

In my own case arsenic has a curious effect. I have on various occasions experimentally taken five minims of liquor arsenicalis and within 12 hours a fine punctate irritating rash appears on the back of the wrists. I have little doubt that arsenic is answerable for some at least of the cases that present themselves at the skin hospitals. Supposing a patient suffering from arsenical beer rash, so to speak, went to a hospital in Manchester there would have been no clue as to the nature of that eruption prior to the discovery of the poison at the breweries.

I am, Sirs, yours faithfully,

Bentinck-street, W., Dec. 4th, 1900.

DAVID WALSH.

PREVENTIVE INOCULATIONS AGAINST THE PLAGUE.

To the Editors of THE LANCET.

SIRS,—A course of three lectures on bubonic plague was recently delivered in London at the Examination Hall of the Royal Colleges of Physicians and Surgeons by Professor Calmette, Director of the Pasteur Institute at Lille.¹

In the third of these lectures Professor Calmette described a new method of preparing a prophylactic against plague,

the product being a substance which possesses (in accordance with Professor Calmette's statements) the advantages that its strength can be easily determined and that it may be kept for a considerable time without becoming contaminated and without losing any of its immunising properties. In this way he thinks that he has solved the problem of obtaining an easy and sure inoculation against the plague—a description which does not apply to Professor Haffkine's prophylactic.

Professor Calmette's method consists in filtering off the plague microbes from their culture media, drying them, and keeping the dried substance carefully in sealed bottles. This substance can be easily weighed and dissolved in water, and it is then ready to be used for inoculation.

In connexion with this subject I must advance the claim that a dried "vaccine" against the plague of definite strength and admitting of preservation was for the first time prepared by myself in collaboration with Dr. Galeotti, as is shown by papers published in different periodicals in 1897 and 1898.²

The method followed by us can be briefly described. When plague microbes are cultivated on large agar-agar plates they form a thin layer which is scraped off from the surface of the solid culture medium and treated with caustic potash. By the strong alkali the microbes are not only killed but are also completely disintegrated and dissolved. To the resulting solution, which is of a mucilaginous consistence, acetic acid is added till a white and flocculent precipitate is thrown down which changes on drying to a brown amorphous mass without losing its properties. This precipitate is a nucleoproteid free from the metabolic products of the plague bacteria and when used for immunisation it is dissolved in a solution of carbonate of soda. It is usually dried in vacuo and can afterwards be kept in sealed bottles and dissolved again in determinate proportions.

The immunising properties of this "vaccine" were fully demonstrated by numerous experiments made in the Pathological Laboratory at Florence and in the Parel Laboratory at Bombay at different times from 1897 to 1900. These experiments were performed on rats, rabbits, guinea-pigs, monkeys, &c. As regards the human subject, it was tried with good results in Florence, in Bombay, and in Australia, applications for it being made by the medical men of several towns of that country on the occurrence of the recent plague epidemics. For the present it is prepared in the Vaccine Institute in Berne under Professor Tavel's direction.

With this "vaccine" it is very easy to obtain a high degree of immunity in the human subject without any of those ill-effects which follow inoculations made with other prophylactics—ill-effects due to the impossibility of standardising the liquid and to the presence of other substances which are pathogenic without being in any way immunising. By our method we isolate the only substance—the nucleoproteid—by which immunity is produced and nothing but this substance is inoculated in proper doses, well dissolved in water and therefore in the best conditions for rapid absorption. On the contrary, by Professor Calmette's method not only the active nucleoproteid but other substances composing the bodies of the bacilli are introduced into the organism and their solution and diffusion in the tissues are more difficult and slow.

Therefore if Professor Calmette's claim of having obtained a better "vaccine" than Haffkine's is justified we can claim to have produced the best prophylactic of any since our "vaccine" has not only the advantages shown by Professor Calmette's but also the other ones which have been mentioned above.

I am, Sirs, yours faithfully,

Florence, Nov. 30th, 1900.

PROFESSOR A. LUSTIG.

KIRKES' PHYSIOLOGY AND AMERICAN PIRACY.

To the Editors of THE LANCET.

SIRS,—Some months ago Mr. John Murray, publisher and proprietor of Kirkes' Physiology, drew attention by letters to the press to the fact that an unauthorised edition of a book bearing the same name was being published and sold in America. It may interest your readers, particularly those who have had to grapple with questions of international copyright, to know that this reprehensible proceeding still continues. In fact, a fresh edition of the unauthorised

¹ THE LANCET, Nov. 10th, p. 1366; Nov. 17th, p. 1454; Nov. 24th, p. 1522.

² Deutsche Medicinische Wochenschrift, 1897, Nos. 15-19; Brit. Med. Jour., vol. i., 1897, p. 1027, and No. 2041, 1900.

version has just appeared. In a recent number of the *Boston Medical and Surgical Journal* the following note is appended to reviews of the two rival editions:—

It seems strange to the uninitiated on reading the above reviews to find two books, both called "Kirkes' Handbook of Physiology"; the reader will naturally stop and wonder which is the book. To explain this peculiar anomaly, by giving a short history of "Kirkes' Physiology," this note is written. About the year 1850 Dr. Kirkes, with the aid of the late Sir James Paget, produced in England "Kirkes' Handbook." This book, in vulgar parlance, "filled a long-felt want." It passed through many editions until at last Kirkes died; then, after passing through various minor changes, the editorship of the volume was assumed by Mr. Baker and Dr. Harris, and by them the book was kept up to date, Mr. Murray, the publisher, owning the book. In this country the book was reproduced by Lea Brothers and Co., and by them sold to Messrs. Wm. Wood and Co. (What they sold is a mystery, as they had no rights to sell.) This firm never had permission to use either the late Mr. Baker's or Dr. Harris's name in connexion with their publication. Still the publication and the sale went merrily on. Over four years ago Dr. Harris and Mr. Baker resigned their offices as editors. Mr. Baker is now dead and Professor Halliburton assumed sole control, producing the present English publication or Blakiston's American edition. Although Mr. Baker and Dr. Harris have had no connexion with the book for over four years Messrs. Wm. Wood and Co. still produce a "Kirkes" claimed to be edited by these men and revised for American consumption by Warren Coleman and Dr. Charles L. Dana. In Wood's edition under review several illustrations can be seen (those on blood crystals) which have been lifted bodily from Halliburton's edition and no acknowledgment made. There is also a remarkable similarity between the two editions in parts of the text. Perhaps this may be explained by the theory of telepathy. Of the two editions I understand that Blakiston's is the only edition published in this country with the owner's permission and from which the editor, Professor Halliburton, receives any remuneration. However, the present unfortunate state of international copyright law places no legal obstacles in the way of Messrs. Wood and Co.'s producing the book as they have done, but there is no doubt that fair-minded people will be shocked to find that in the noble profession of medicine men are found who assist in such questionable proceedings, even after personal protests not only from Mr. Murray, the owner, but also from Professor Halliburton. To sum up the whole matter, it simply means that Blakiston's is the authorised edition, while Wood's is not. *O tempora, O mores!!*

This statement is substantially correct. The figures of blood crystals which are specially mentioned as having been "lifted" were in the handbook before I assumed control. But other figures which have been added during my editorship have been pilfered. So that the ethics of the case remain unaltered. The editor of the *Boston Journal* is to be thanked for the determined stand he has made in the matter in the interests of international honesty.

I am, Sirs, yours faithfully,

King's College, London, Dec. 6th, 1900. W. D. HALLIBURTON.

"SUGGESTIONS FOR THE PRACTICAL TREATMENT OF TYPHOID FEVER."

To the Editors of THE LANCET.

SIRS,—In Dr. W. Ewart's interesting paper on the treatment of typhoid fever published in THE LANCET of Dec. 8th he refers to the beneficial effects resulting from the administration of 20 minims of the liquor hydrargyri perchloridi with 15 or 20 minims of the tinctura ferri perchloridi every six hours—a plan recommended some years ago by Mr. W. B. Wedgwood of King's Lynn. Whilst fully endorsing Dr. Ewart's statements I nevertheless think it well to point out that the administration of the perchloride of iron should generally be restricted to those cases of typhoid fever in which diarrhoea is a prominent symptom. If there is a tendency to constipation other remedies may be used more advantageously.

Both the perchloride of mercury and the perchloride of iron act as germicides. That this is the case with mercury is well known. After being absorbed into the system it is afterwards largely eliminated by the mucous membrane of the intestines, both large and small, and so comes in direct contact with diseased surfaces. The perchloride of iron is also a germicide. It readily parts with some of its chlorine and so may be regarded more as a preparation of chlorine than of iron. To show that this is the case it is only necessary, as Professor Binz¹ has pointed out, to add a drop of the tincture or solution of perchloride of iron to a solution of potassium iodide mixed with a little mucilage of starch when a deep blue colour is produced, precisely as if pure chlorine water had been added. Tincture of guaiacum, the well-known test for nascent oxygen, also acquires a blue colour on the addition of a drop of the iron solution. The beneficial effect of the remedy is therefore, in my opinion, due chiefly to the

chlorine which it readily gives up, and its action consequently must be exerted chiefly, if not entirely, on the contents of the stomach and duodenum. When there is a tendency to constipation I think that there is some risk after the administration of iron of increasing both the constipation and the pyrexia. In such cases a solution of euchlorine made from potassium chlorate and strong hydrochloric acid may be used as a drink from time to time instead of the perchloride of iron. The administration of euchlorine water, too, is also to be recommended in preference to iron for those individuals (and the number is not small) whose stomachs will not tolerate ordinary doses of the latter remedy.

Furthermore, the question arises whether calomel is not a better remedy in typhoid fever than corrosive sublimate. The former reaches the lower part of the small intestines before it is converted into the latter, and so acts more directly and specially upon the lower part of the ileum, where, as we know, the morbid changes are generally most marked.

In 1893 I discussed in THE LANCET² the action, not only of calomel in typhoid fever, but also of other remedies, such as the subnitrate of bismuth, tannic acid, oil of turpentine, &c., all belonging to the germicide class, and to that paper I would refer those who are interested in the subject. As I there pointed out, these are all old remedies—germicide remedies—their use, though empirical, suggested and adopted as a result of the keen clinical observation and experience of the physicians of a bygone age. The scientific investigations of the present day are only teaching us now how to employ these remedies with greater precision.

I am, Sirs, yours faithfully,

P. W. LATHAM,

Consulting Physician to Addenbrooke's Hospital,
Cambridge, Dec. 8th, 1900. Cambridge.

"THE TREATMENT OF SPRAINS AND OF SOME FRACTURES."

To the Editors of THE LANCET.

SIRS,—The address by Mr. Tubby on this subject is of much interest. As you have on two former occasions¹ been good enough to insert in THE LANCET communications by me relating to the treatment of recent sprains and dislocations by movement I would ask your permission to recur to the subject. After a pretty extensive experience of treatment by pressure by cotton-wool and bandage and also by strapping, especially at the time when Mr. Cotterill first published his book, which at present I am sorry I cannot lay hands on, I have a very decided preference for the movement treatment. Mr. Tubby says that there are two methods of treatment, one of which is by "rapid forcible movement immediately after a sprain." Then again, he says, referring to the treatment by mobility from the first: "It is one which can only be employed in very slight cases and even then with some misgiving, for in certain patients the symptoms instead of rapidly clearing away become more pronounced and permanent thickening and disability of the joint remain."

Is it necessary to use rapid forcible movement? My opinion is that forcible movement is generally employed (as a rule successfully) by the bone-setter after a patient has been under treatment by immobility for some time. A recently sprained joint is extremely painful and has at first to be handled very tenderly. Only very slight friction and squeezing can be borne. As the pressure is gradually increased the patient is encouraged to move his joint till flexion and extension are as complete as possible. No force is used. In the case of the ankle on attempting to walk the expression of surprise on the patient's face denotes that there is a considerable difference in the state of his joint. I am not speaking from the experience of a few cases, for I have employed movement in the treatment of recent sprains and dislocations for a good many years. I have used it for sprains of every degree of severity. Impairment of the usefulness of a joint may remain after a sprain, no matter what kind of treatment has been employed. I cannot remember

¹ Binz's Pharmacology: Sydenham Society's Translation, vol. ii., p. 46.

² Some Remarks on the Pathology and Treatment of Typhoid Fever, by P. W. Latham, M.D. Cantab., F.R.C.P. Lond., and Arthur C. Latham, B.A., Balliol College, Oxford, THE LANCET, March 4th and 11th, 1893.

¹ THE LANCET, Feb. 2nd, 1898 (p. 465), and June 16th, 1900 (p. 1756).