incubator) he had observed normoblastic formation taking place from the already formed red blood corpuscles. He put forward the theory that the bone marrow should perhaps be considered rather as a locality for transformation during temporary stagnation of the blood corpuscles than as the seat of new formation of blood corpuscles.

BERLIN.

Locumtenents.—The Growth of Isolated Organic Tissues.—Central Heating.

SOME weeks ago a number of Berlin medical men held a meeting in order to regulate the question of substitutes for practitioners temporarily unavailable by their patients. It was brought about partly by a lack of suitable locumtenents, partly by the often exorbitant demands made by them, and partly by the disinclination of patients to be treated in the absence of their own doctor by a medical man unknown to them. At the meeting groups of about ten practitioners were formed, each practitioner binding himself, if chosen by the patient, to take the place of a colleague whom illness or some other reason may render unavailable. Fees will be regulated and fixed. The advantage of this arrangement for the patient is obvious, since it gives him a wide choice. At present the arrangement only applies to certain districts in the east of Berlin, but corresponding schemes are in view elsewhere.

Professor H. Braus (Heidelberg) recently gave a very interesting lecture at the "Urania," a Berlin institute where popular demonstrations relating to the progress of science often take place. The lecture was an exposition of the experiments of Carrel at the Rockefeller Institute, New York, who succeeded in making animal tissues grow when isolated from their connexions. On the present occasion the growth of nerves from brain tissue was illustrated. Having isolated a single ganglion cell from an embryo frog, he showed its growth on the screen. The rapidity of the growth amounted to 0.001 mm. per second; the length to 1 mm. For such experiments an institute has been planned in Berlin.

Nowadays it is exceptional in Berlin to build a house without central heating. Its advantage is great, but every now and then drawbacks from a hygienic point of view are urged. Dr. Wolff-Eisner has recently, in the *Deutsche medizinische Wochenschrift*, specified the disadvantages of this kind of heating. According to him, ill effects on health, especially catarrhs of the larynx, are caused by the very great dryness of overheated rooms. Besides this, the dust deposited on the pipes is partly organic in character and gets decomposed by heat; hence empyrheumatic products are mixed with the air of the room. As an answer to these statements, Professor Nussbaum has asserted, in the paper *Gesundheitsingenieur*, that these disadvantages can occur only when the central heating is faulty or improperly used. The air does not become unduly dry unless the heating is continued uninterruptedly, and this is not the case in properly managed central heating. He adds that steam-air heating, as distinct from hot-water heating, allows of easy and quick regulation of temperature. The *Verband deutscher Zentralheitungs-Industrieller* has also taken part in the discussion. It maintains that the modern system of central water-heating, when executed by a competent firm, is the most perfect heating system in hygienic and technical respects. As to the combustion of the dust on the hot pipes, this evil can be rationally prevented by not hiding the radiators in cases or grates, but, as is mostly done now, by leaving them exposed, and decorated in accordance with the general scheme of the room; then, like the furniture, they are accessible to duster and brush. If no dust accumulates, no dust can burn.

THE German Congress of Medicine will hold its thirtieth annual meeting at Wiesbaden in April, 1913 (15th to 18th), under the presidency of Professor Penzoldt of Erlangen. The principal subject proposed for discussion is the nature and treatment of fever. The discussion will be introduced by Drs. von Krehl of Heidelberg, and Hans H. Meyer of Vienna. At the request of the Organizing Committee, Professor Schittenhelm of Koenigsberg will deliver an address on the relations between anaphylaxis and fever.

Correspondence.

LORD LISTER'S EARLY CASE OF OPERATIVE TREATMENT OF FRACTURE.

SIR,—In connexion with the recent report on the Treatment of Fractures, and the address of Mr. Robert Jones, it may be of interest if I recall an early case of the operative treatment of fracture in the practice of the late Lord Lister shortly after his appointment to the chair of clinical surgery in Edinburgh, which made a vivid impression on my mind as a student.

A lad employed in a quarry in turning the handle of a winch to raise stone by a crane let go the handle, and as it rapidly revolved it struck him on the forearm. He received no medical treatment at the time, and many weeks afterwards presented himself at the Edinburgh Royal Infirmary with a useless arm. Lord Lister showed the case to his class, pointing out a badly united fracture of the middle of the shaft of the ulna, and a dislocation of the head of the radius. He expressed his intention of trying to break and reset the bone and to reduce the dislocation, pointed out the difficulty of effecting this owing to the lapse of time since the injury, and the necessity in the event of failure of cutting down upon and dividing the bone at the seat of fracture, and of excising the head of the radius if reduction were impossible after division of the ulna. After futile attempts to refracture the bone, this double operation was done under full antiseptic conditions, the wounds pursuing the normal aseptic course, the result being a perfectly useful arm. In those days, when some still flouted Lister's principles, his adherents regarded the case as a triumphant vindication of them.—I am, etc.,

December 14th. THEOBALD A. PALM, M.D.

*** The case recalled by Dr. Palm is, no doubt, that which formed the subject of Lister's paper, "On a Case illustrating the Present Aspect of the Antiseptic System in Surgery," published in the BRITISH MEDICAL JOURNAL, 1871, vol. i, p. 30, and reprinted in *The Collected Papers*, vol. ii, p. 165 (Oxford: The Clarendon Press, 1909). Any surgeon who will study these volumes will find not only that Lister in his practice anticipated methods which are only now becoming usual, but also that he discussed the problems arising with so much foresight and insight that he left little concerning principles for later elucidation.

TREATMENT OF INEBRIETY.

SIR,—May I ask you to give me space for a few remarks on Dr. Francis Hare's book on *Alcoholism*, and the review of it in the JOURNAL of December 7th, p. 1612? Dr. Hare's book is the first real attempt since the great pioneer work of the late Dr. Norman Kerr to deal adequately with the causes and treatment of alcohol inebriety. Dr. Hare has thrown much light on the causes of alcoholism not found in any existing book on the subject, and done much to show the necessity for a better understanding of the inebriate by the profession and by the public. The remarks on the causation of delirium tremens and alcoholic epilepsy and his suggested preventive treatment I am pleased fully to endorse, and hope that they will find their place in any future textbook dealing with these conditions.

I am disappointed to find that, so far from clearing up the vexed question whether inebriety as we meet it in medical practice is a vice or a disease, Dr. Hare in his book and you in your review of it give out once more that, "though alcoholism may amount to a disease, in a very large proportion of cases it is only an exaggerated self-indulgence." I am well aware that in making this statement Dr. Hare is merely corroborating the findings of the last Parliamentary Committee. That this conclusion is not the only one possible after many years' experience of inebriety and of inebriates is evident, however, from the following statement made by Dr. R. W. Branthwaite, late Medical Superintendent of the Dalrymple Home, Rickmansworth, and now His Majesty's Inspector under the Inebriates Acts, before the Twelfth International Alcoholic Congress. He said:

I do not believe any drunkard out of the eight thousand or so I have known has voluntarily and of intention made himself so.

On the contrary, I am convinced that *all who possessed sufficiently developed mental equilibrium to appreciate the seriousness of their condition*, have urgently and honestly desired and striven to lead a sober life, and failed in a struggle against *a defect or weakness* the magnitude of which a normally constituted individual is utterly incapable of fully realizing. (The italics are mine.)

Dr. Branthwaite (I think at the same Congress) has given the following definition of an inebriate:

> An inebriate is an individual who may or may not desire to live soberly, but in any case cannot, unless and until some change takes place in his mental state.

If we are to accept this statement of opinion and this definition, it seems to me that we must accept the fact that pathological inebriety (we are not concerned here with occasional self-indulgent drunkenness) is neither a vice nor a disease, but a symptom of psycho-neurotic or psycho-physical disease or defect. My own experience of nearly ten years' treatment of alcoholic and other forms of inebriety leads me to accept Dr. Branthwaite's statement and definition, and to regard inebriety not as a vice or a disease, but as a symptom of psycho-neurotic or psycho-physical defect or disease, curable or incurable in proportion to the amount of disease or defect which is present and responsible for the inebriety. The less defect or disease present the more curable is the inebriety, and vice versa, the actual degree of inebriety being of less importance. I think that such a view of the nature of inebriety need in no way inculcate the doctrine that inebriety is a disease over which the inebriate has no control, and so destroy the greatest incentive to amendment, while it removes from his mind the feeling that he is held altogether responsible for behaviour which he has earnestly striven against but failed again and again to amend. He should be taught that with his submission to and co-operation in treatment his condition is curable, and that the enormous difficulties in the way of self-cure are fully recognized and understood.

Your review calls attention to the need shown by Hare for looking for a physical cause for inebriety. Experience also shows the at least equal importance of looking for psychic causes, which are generally not so apparent or easy to discover. Nothing in Dr. Hare's book leads me to alter an opinion arrived at some years ago—that it is to psycho-therapeutic treatment *in its widest sense*, with the use of drugs as adjuvants and not in any shape or form as specifics, that we must look for a cure of pathological inebriety, alcoholic and otherwise.—I am, etc.,

J. W. ASTLEY COOPER.

Ghyllwoods, near Cockermouth, Dec. 10th.

AN INTRACELLULAR PARASITE DEVELOPING INTO SPIROCHAETES.

SIR,—May I be permitted to make a few remarks on Mr. E. H. Ross's paper in your issue of December 14th?

In January of this year, prompted by clinical and pathological observation, I inquired into the life-history of the organism of syphilis. The lymphatic gland suggested itself to me as the most likely place for the other phases to occur, and I accordingly looked for and found them there. Early in September of this year Mr. E. H. Ross, who had come to the Lock Hospital for some syphilitic material, was informed by my house-surgeon, Mr. Moolgavkar, that I had been working at the subject for some months. On September 14th Mr. Ross came to see me at the Lock Hospital; wishing to give him the same facilities for research as I had myself, from that day I placed all my material unreservedly at his disposal, and asked my house-surgeon to give him every assistance possible, and, moreover, I presented him with five sections in which practically the whole life-cycle can be followed out. Surely it is customary to acknowledge the source from which material is obtained when one's work is entirely dependent thereon.

One would imagine that the jelly method was the only means by which the life-cycle of the organism of syphilis could be worked out. For showing up the intracellular phases it is on the whole preferable to the *in vivo* method of staining with borax methylene blue, but cannot compare with it in the case of the extracellular phases. With the borax methylene blue the organisms remain alive for hours, allowing fertilization to be studied with ease, as I have shown, which is by no means the case with the jelly. The extracellular phases are the most important from a diagnostic point of view, as they are more commonly seen than the intracellular in latent and tertiary syphilis, where difficulty in diagnosis is most apt to arise.

Mr. E. H. Ross's article is stated to have been received on October 11th, and in the article the spirochaetal coil is mentioned and figured, yet in Mr. H. C. Ross's letter to the *Lancet*, written October 15th, it is stated that there is no coil in the human parasite.—I am, etc.,

Wimpole Street, W., Dec. 16th. J. E. R. McDONAGH.

THE ETIOLOGY OF BERI-BERI.

SIR,—With regard to the interesting note on the etiology of beri-beri by H. G. Browning, on p. 69 of your issue of July 13th, 1912, I should like to submit the following. Briefly, Dr. Browning states that the native crew of a ship were divided into the deck crew and the engine-room crew, both eating from the same general stock of provisions. That beri-beri developed among the engine-room crew but not among the deck crew, and that the only difference between the two crews was the fact that the deck crew lived in dry quarters while the engine-room crew lived in wet quarters. He draws the conclusion that this points to dampness playing a large if not the largest part in the causation of beri-beri.

In view of the fact that such communications serve to throw doubt upon the theory that beri-beri is caused by a deficiency in the diet—a theory which I regard as a demonstrated fact—I believe some comment may serve a useful purpose.

In the first place, the facts are not given in sufficient detail to enable any definite conclusions to be drawn. It may well have been that because of some difference in taste, caste, or religion, the food actually eaten by these two crews may have differed, although they were drawn from the same supply.

However, even granting that the two crews ate the identical articles of food in the same amounts, this fact does not necessarily militate against the deficiency theory, since the conclusion that dampness played a part in the etiology rests upon the unproved assumption that the two crews differed in no other particular than the respective dampness or dryness of their quarters. They also differed in the kind and amount of exercise taken, the amount of fresh air breathed, and possibly in many other respects.

Vedder and Clark have shown in the *Philippine Journal of Science*, Section B, vol. vii, No. 5, 1912, that the nerve cells in the cord of fowls that had developed polyneuritis as the result of a polished rice diet showed anatomic changes closely simulating those found in the nerve cells of animals that have been greatly fatigued. Moreover, typical degeneration was found in the fibres of the brain and cord, in addition to the usual changes found in the peripheral nerves. It seems, from these observations, that the neuritis preventing substance, or vitamine, is a food-stuff or building stone essential for the normal metabolism of nervous tissue. If this is true, the case reported by Dr. Browning may be explained very simply in accordance with the dietary hypothesis as follows: The men in the engine-room crew performed heavy manual labour as stokers under the adverse circumstances of excessive heat, etc. The deck crew did not perform so much work. Under these circumstances the metabolism of the nervous tissue of the engine-room crew would be much more active than in the case of the deck crew, and a much larger supply of this essential vitamine would therefore be required by the former. The same diet which might contain sufficient vitamine to maintain the deck crew in health might be utterly inadequate for the engine-room crew.

This explanation is not stated as a fact, but as a distinct possibility, and as showing that Browning's deduction that dampness played a part in the etiology of beri-beri in this instance does not necessarily follow from his observed facts.—I am, etc.,

EDWARD B. VEDDER,
Captain, Medical Corps, U. S. Army.

Board for the Study of Tropical Diseases,
United States Army,
Division Hospital, Manila, P.I.,
October 16th.