freely. He saw the child again at 12. It appeared quite well, but he had scarcely left the house when he was called back. The child suddenly fell in a convulsive fit and died. A small red spot was found just between the 5th and 6th vertebra. Death took place full 12 hours after the child was taken ill. This was the cause of death from scorpion sting of which the writer had heard.

He justly remarks that, when unhealthy persons are stung, very unpleasant complications are likely to ensue. This is observed especially in cases of viper bite at home; the discharges, especially from the stings, are liable to infection of the lymphatic tubes and tend to dangerous suppuration. A person of strumous constitution, who has been suffering from remittent fever caught in the Wynn Head, died in one of his fingers. He complained of an excruciating burning pain in the axilla. The glands were much enlarged and painful to the touch. The pain in the finger was soon relieved by the application of caustics, but the glands ran into suppuration and caused a very poor man great anxiety. The writer has found Ipecacuanha and Liquor Ammoniv very good remedies, but he mainly relies upon the application of lunar-caustics. He lived in a house "filled with scorpions," and many of his servants had been stung. Caustic was immediately applied and afforded immediate relief. He never knew it fail. Sometimes the precise seat of the puncture cannot be ascertained. The caustics should then be applied rather widely in the situation of the pain to the extent of vesication. By wiping the part with a clean handkerchief the puncture may usually be discovered by the evaporation of fluid at that point. This being touched with caustics, the patient will be immediately relieved.

Dr. Day tells me that he will always observe, in recent cases, that the skin immediately around the puncture acquires a fiery hue.

Jousset, in the Gazet. M. di Turin, details some interesting observations made by him on the poison of scorpions. It seems that there are three species of scorpions found in Southern Europe and Africa. 1. Oedogaster, a little scorpion of 2. The Scorpis Ocelatus, of a clear, yellow colour, is found in the south of France, in the country, and is very poisonous. 3. Scorpis egyptis is found in Africa, is 15 centimetres long, and the sting of the Scorpis is fatal to man. The Scorpis ocelatus has the sting at the end of the tail. The animal makes use of this to defend itself, and also to kill its prey. Even if it seems to have been mortally wounded, it still can safe defend itself by biting its tail. The poison of the Oedogaster is more violent than that of the Scorpis ocelatus.

The quantity of poison is very small in the Scorpis ocelatus, but the experiments report 1, 20, 30, 40, 50, and even 100 volts of the poison given in a large scorpion; but it is very active, since this suffices to kill a dog of medium size.

Dr. Jousset experimented with the poison on the tree-frog (Rana Ar- hopena) in order to discover what histological changes were affected by the poison. These changes occurred rapidly under the influence of the smallest quantities of the poison; death supervening without convulsions, and the frog all green at the moment of death, except a violet tingue, which was punctured became rigid. In five minutes, in the middle capillary vessels the frog is seen to be compressed, and some globules which had the appearance of being deformed and lengthened. As the force of the circulation slackens, the movements became more apathetic, and the frog all green on its back. In the frog punctured with the Scorpis ocelatus, it was seen that the frog was still alive after the punctured was filled with red globules pressed one against another and stationary. He draws the following conclusions: 1. The poison of the Scorpis Ocelatus acts directly upon the blood-globules, and seems only to act upon these. 2. In action there is an extensive decomposition of the globules; their substance is converted into globulins, and the lower portion is converted into free globulins. 3. When this property is lost, the globules become clumped to each other and to the healthy globules, so as to form small clumps which obstruct the mouth of the capillary vessels and the obstacles to the circulation. Thus this poison is seen to act in a chemical manner, not in a mere anaesthetic way.

Typhoid Fever - its nature, mode of spreading, and prevention.

By William Budd, M.D., F.R.S., London, 1873.

A treatise on the Continued Fevers of Great Britain By Charles Murciiison, M.D., LL.D., F.U.S., &c, &c, London: Longmans, Green and Co. 1873.

Dr. William Budd is a staunch and consistent contagionist. His views regarding the communicability of cholera, originally enunciated in 1854-55 and rigidly maintained ever since, are well known. In 1857, he published his opinions on the subject of typhoid fever, asserting that this disease is a "strictly self-propagating fever, (mainly) disseminated by the specific discharges from the sick." He has even carried his contagion doctrines so far as to apply them to phthisis, and tuberculosis. His arguments are not without force, and the general opinion of the medical world is that phthisis is also a zymotic disease of specific nature, similar to typhoid fever, scarlet fever, typhus, syphilis, &c.; that tubercle is a specific material contained in the eruptions of contagious fevers; that through this material the disease is "propagated from one person to another and disseminated through society."

The volume whose title we have above quoted is a fuller development of the papers which Dr. Budd published in the Lancet and British Medical Journal from 1856 to 1860. The subject is more clearly and forcibly brought before the reader, the nature of this disease more clearly described, illustrations of its mode of prevalence and spread given in greater number and more fully narrated and discussed, and the arguments in favor of its contagiousness more completely marshalled and thoroughly stated. The volume, as it now stands, constitutes the most concise, clear and forcible exposition extant of the doctrine of which Dr. Budd is the most distinguished and successful representative of.
and advocate. His conclusions, as summed up by himself in the last chapter of the book, are as follows:

Typhoid fever is, in its essence, a contagious or self-propagating fever, and the donor of the great natural family of contagious fevers of which small-pox may be taken as the type.

The receptacle of the infected man is the soil in which the specific poison, which is the cause of the fever, breeds and multiplies. The receptacle of the infected body and the disturbance attendant on it constitute the fever.

1. This reproduction is the same in kind as that of which we have in small-pox or typhoid decomposition.

5. The disease of the intestine, which is its distinct anatomical mark, is the result of the diffusion of the poison and bears the same pathological relations to it in which the small-pox eruption bears to small-pox.

6. As might have been anticipated from this view the contagious man might be isolated by an act of cold, chilly, in the absence of charges from the diseased intestine.

The very result, sewers and the clog which, under existing sanitary arrangements, are the common receptacles of these discharges, are also the principal instruments in the transmission of the contagion; and consequently, an outbreak of typhoid fever, if it is cut off, chiefly, in the absence of charges from the diseased intestine.

8. If, once cut off by the intestine, this poison may communicate the fever to other persons in two principal ways,—either by contaminating the drinking water or by infecting the air.

As an inevitable consequence, the impalpable minuteness of the contagious unit, and the many invisible and untraceable ways in which it is transmitted, cases must be constantly occurring, exactly as in other contagious diseases, whose linear descent cannot be followed, and which spring up, therefore, under the semblance of spontaneous origin.

The circumstance of such cases obviously constitutes so proof whatever that this fever ever does arise spontaneously.

The individuality of the contagious attaching to the reproduction of the specific poison in the living body itself, as well as the facts relating to the geographical distribution, past and present, of the various contagious fevers, furnish the strongest evidence that such evidence can ever be, that none of these fevers originate spontaneously; but are propagated by the medium of a continuous succession.

12. By destroying the infective power of the intestinal discharges by strong chemicals, or otherwise, the spread of the fever may be entirely prevented. As the very fresh case as it arises, the disease in time may be finally extirpated.

The great difficulty in implicitly accepting so plausible and complete a theory of typhoid fever as this is, consists in the circumstance that it is the poison, which is the pivot round which the whole doctrine revolves, a hypothetical thing. Its existence has been inferred from the phenomena of the disease, and has not been positively demonstrated. Dr. Budd would contend that it has been positively demonstrated, and that the intestinal lesion is the mere mucous irritation; but in its absence, it constitutes it, the poison, if indeed it is not the poison; but this is precisely the where the doctrine rests on assumption, and is most destitute of proof. If this material contains or is the poison, every case or outbreak should, on the one hand, be traced back to a previous case, or in some way brought into relation with the specific material, without which the existence of the case or outbreak is, on the theory, impossible; and, on the other hand, we should be able to trace the poison, if not destroyed, from a previous case, and to others, which has been produced by it. There are numerous cases and outbreaks on record in which the anterior existence of the material in question has been undiscovered after the most searching inquiry; nor, in which its previous existence would not most appear to be, on the theory, impossible. There are other cases in which a source of the poison has been asserted to exist, which breaks down on more careful investigation. In these cases, our ignorance of the very existence of the poison, and of its manner of existence, is advanced as a reason why we should not too hastily deny the presence of an agent of which we know so little. The unseen being may travel or be conveyed in a thousand ways, of which we have no conception. On the other hand, cases are constantly occurring in which there is an enormous evolution of material, and, of course, no effort is made to destroy it, or to alter the composition of the specific matter, which ought to ensue, namely, the appearance of other cases, does not ensue. Here again our ignorance of the positive existence, nature and behaviour of the poison is used as an argument. We do not know under what conditions it is operated, and, if operated, what results are obtained. We do not know whether it is virulent, or whether, after operation, it will, and, if such the case, we have no right to argue that its development and dissemination should necessarily imply its ability to act injuriously; or perhaps, even if the ability exists, the opportunity may be wanting. The theory of the specific matter, considered as a specific poison, is due to a specific cause, few will deny, but that the specific material is generated, as Dr. Budd assumes, cannot, by any means, be accepted as certain until it has been positively demonstrated, and that certainty has not been as yet. The author's theory derives, however, no weight from a consideration of possible alternatives. If the specific material is not generated in the organism as Dr. Budd supposes, and disseminated as he supposes, it must be derived from some other material or under some other conditions; now, as far as our powers of investigation, whatever material or conditions we assume, we can show that these exist without being associated or followed with, or the appearance of typhoid fever. In some instances we can trace the relation between such cases as outbreaks of disease and typhoid fever, in those where we fail to do so, we can show that the alleged or apparent causes of the disease, are elsewhere existent and inoperative, we seem to go a long way towards establishing the contagion doctrine. This is in fact Dr. Budd's position; and, in the present state of knowledge, it would be impossible to deny its force. He contends, and with much reason, that towns are not so favorable a field for studying the circumstances of origin and spread of the disease as villages and isolated hamlets. He also advances as a strong point the non-existence of the disease in Australia, and where 'pyrogenic' conditions must be as rife as they are in England. Still, the theory must be accepted cum grano salis until the poison is positively demonstrated as a physical substance and its origin, nature, changes and final destruction investigated and described. Dr. Murchison is the principal champion of the theory which would ascribe typhoid fever to some product of sewage decomposition. His views are clearly and temperately stated in his treatise on typhoid fevers. He admits the occasional origin of the disease by communication from the sick to the healthy; but he cites other facts which show that the outbreak is, could not be traced, and where the fever arose without any apparent possibility of its existence. He argues that the mode of prevalence of the disease indicates its endemic rather than its contagious origin, and asserts that it is always found to be associated with decomposition, and the 'pyrogenic' condition. So firmly is he convinced of its origin from putrid sewage that he has invented for it the name "pyrogenic" (putrescence-begotten). He combats Dr. Budd's arguments in detail. He contends that persons are constantly exposed to the influence of typhoid excreta without giving an attack of typhoid fever. This argument is met by the assertion that it needs certain conditions to liberate the poison from these stools and render them operative for evil. He furthers arguments that typhoid stools are extremely prone to decomposition, and that any poison contained in them would be destroyed before issue from the body. This statement cuts both ways and is mere surmise. He asserts that "enteric fever is constantly appearing when decomposing sewage is present, but where every effort fails to trace the presence of typhoid stools." This is a question of evidence, and Dr. Murchison brings, we must allow, some strong evidence in support of his position. Even in cases where a sudden outbreak, depending on contaminated water, has occurred, it is not always possible to trace a specific occasion, and when the immunity of workers in drains and sewers from typhoid fever is not so complete as has been made to appear, and, even if it were so, the disease is caused by obstructed drains, and those men may acquire an immunity. Besides, their immunity is extraordinary on the other hand, cases, after all, in either case, certain conditions may be necessary to develop the disease. It has been argued that the disease is in some cases independent of organic impurities; but the existence of these is not always appreciable by the senses, nor even by chemistry, and they may exist even when not recognised. The success of measures employed to destroy the poison may support either theory. There is an analogy between the mode of prevalence of typhoid fever and that of malarious fevers. Experiments on lower animals do not as yet warrant any conclusions as to the etiology of typhoid fever.

Such is a very brief outline of Dr. Murchison's argument. His conclusions regarding the etiology of the disease are summed up by himself as follows:

"Enteric fever is either an endemic disease or the epidemics are circumferential."

2. "It is most prevalent in autumn and after hot weather."

3. "It is independent of overflows, and, as such be communicated by the sick who are not, and, in other cases, given off from the body in a virulent form, but is developed by the decomposition of the excreta after discharge."

4. "Consequently, an outbreak of enteric fever implies poisoning of air, drinking water or other ingesta with decomposition matter."

When we find two such men as Budd and Murchison holding such different views regarding the etiology of typhoid fever, as...
the extracts above cited represent, we can but draw one conclusion, and that is that further investigation is necessary; and it becomes all wise men, to be cautious in asserting what the poison is or is not, or laying down what it does or does not, until the existence of the poison is demonstrated. Meantime, if this measure of knowledge is denied us, we can but investigate the circumstances under which it has been alleged to exist; and improve the data on which our inferential conclusions rest. Above all things, we are not in a position to dogmatise. Attention has of late years been drawn to typhoid fever in India. Of its existence there can be little doubt; regarding its mode of origin and propagation we are profoundly ignorant. We are not aware of any single case in which the circumstances of origin of the disease have been investigated in such a manner as to warrant any positive conclusion.

General inferences from statistical data, more particularly when the diagnosis is not always certain, may guide, but they certainly do not demonstrate. The only way of arriving at a knowledge of the circumstances of origin of the disease is to investigate them in detail. This has not yet been done in India. The matter is one of extremely delicacy and difficulty, but that is no reason why it should not be tried. Recently, at Dr. Murchison's suggestion, some copies of his work have been distributed in India; and medical schools and the offices of Deputy Surgeons-General in the presidency have been furnished with a copy. Medical officers may thus obtain access to the latest information on the subject. The researches of Buhl and Pettenkofer have proved that the cause of the fever, whatever that may be, is subject to the operation of some definite and appreciable conditions. If we cannot demonstrate the poison itself, we can, at least, discover some of the conditions under which it exerts its influence; and to do this would be a decided advance. It seems also quite clear, from whatever point of view we regard the subject, that the practice of sanitation and the disinfection of the stools offer good hopes of limiting the extent of the prevalence of the disease. We close these remarks by impressing most strongly on our readers the necessity of adopting these practical measures, whether they incline to the contagious, pythogenic, or any other theory.

Selections.

EXTRACTS FROM A REPORT ON LEPROSY AND YAWS IN THE WEST INDIES.

By Gavin Milroy, M.D.

(Continued from page 52.)

Treatment of Leprosy—Dr. Beaupreth's Views and System.

The rational treatment of a chronic disease must mainly rest on the knowledge of the causes or external agencies which promote or excite its development. One of my interrogatories was accordingly framed to obtain as exact information on this subject as possible, more especially in those cases where the disease appeared to have originated de novo, i.e., independently of hereditary predisposition. On no point of my inquiry has the concurrence of opinion been more conspicuous and emphatic. There may be differences of opinion on other topics; on this there is next to none. All agree that whatever tends to lower the vital energies, and to deprive the condition of the blood must be regarded as one of the chief favoring causes of the disease. Prominent among these causes is the ordinary food, in respect of its quality as well as its quantity. In looking over the replies of the respondents now before me, there is a continual recurrence of such expressions as these:—Bad food and destitution; want of proper food; starvation and the use of bad food; insufficiency and faulty quality of food; unwholesome and inimurious food; bad and insufficient alimentation; non-nitrogenised food, a diet restricted to a few articles possessing low nutritive properties; poverty and unwholesome food; insufficiency of food both as to quantity and nutritive quality. If we descend to particulars, and seek to make out the special articles or items in the food on which the disease and insufficiency of the same mainly depend, we cannot but be struck with the constant reference to the diet consisting, as respects the supply of animal food, almost entirely of salted fish (often in a putrescent state) and of salted pork and beef to the nearly total omission of fresh meat. Some of the respondents directly specify the absence of fresh animal food as one of the chief causes, while others plainly indicate the same opinion without expressly naming the defect in question. Together with the unwholesomeness of the diet of the labouring classes, the want of a sufficiently abundant supply of good vegetable food is repeatedly mentioned in the replies. Such want in climates where nature is ever so prolific is of course mainly due to the idleness and apathy of the people. But defective diet is not the only promoting cause of the malady. That the injurious influence of malaria, and, indeed, of whatever serves to lower the vital energies, or to impoverish the blood, may serve to aggravate, if not to produce, this tendency to the development of the leprous distemper, is very generally recognised. The not unfrequent occurrence of cases of the disease in the well-fed and well-conditioned classes clearly indicates the operation of other influences. Hitherto, this pathological subject has not been scrutinised by resident medical men with that care which its great importance demands. The following remarks are only to be viewed as mere hints and suggestions for consideration. It is extremely difficult even to conjecture the relative frequency and severity of the disease. The well-to-middle and upper classes (who studiously shield all reference to the subject), so as to enable us to compare this frequency with that existing in the lower strata of society. That the proportion is but small is rendered probable by the absence of the disease among the former are, in some places, far from being rare. In Trinidad, I was told that the disease is not uncommon in respectable Creole families, more especially among those of Spanish and of French extraction, in and around Port of Spain; and the neglect of the medical men in Jamaica have no doubt that the Jewish residents, who are mostly natives, and almost all in good circumstances, are decidedly less afflicted than others in the same condition of life. Dr. Bowring, referring to the Jewish families, alludes to the large quantity of salt fish, also salted and knapped fish, and preserved fish roe, consumed in their ordinary diet. It is to be remarked that, among the well-conditioned classes of all races throughout the West Indian Colonies, much greater amount of fresh fish is generally used than in Europe, often to the supersession of fresh meats entirely on some days of the week. Dr. D. says:—The condition marriage of certain Jewish families in and in, tends to favor its development. Dr. Philippo also alludes to marriages of consanguinity as a probable predisposing factor, and suggests, that a great deal towards the prevention of the disease would be the avoidance of such marriages.