

appointments constituting promotions to the administrative rank and entitling to special pensions. The amalgamation of the sanitary and vaccine departments will confer solidity and practical usefulness, which have been hitherto so conspicuously absent, on the former; but, when sanitation has now become an integral part of a medical education and of the business of a medical executive, we fail to see why a separate sanitary administration without a corresponding executive continues to be considered desirable or necessary. Our strong and reiterated opinion regarding the unification and consolidation of all health administration and agencies in India remains unaltered, and the experience which has been gained in India of a separate sanitary administration is not favourable to the efficiency or advantage of the system.

The casualty roll of the year has unfortunately been a heavy one. Of Surgeons-Major J. ELLIOT, M.D., E. J. GAYER and J. R. JOHNSON, we penned a few lines at the time by way of a brief record of their services and in expression of regret for their loss. Surgeon-Major A. M. VERCHERE died at Aden of disease contracted while accompanying his regiment, the 13th N. I., on its return from the Mediterranean. Mr. VERCHERE was a well-informed medical officer, fond of his profession, and attentive to the sick. He was an occasional contributor to these pages, and his writings always bore evidence of observation and reading. In Surgeon A. Wood, M. D., who died of laryngitis while on furlough, the Government of the North-Western Provinces lost an excellent civil surgeon and good jail officer. Surgeon E. B. MEREDITH, who died at Port Blair of typhoid fever, was a promising young officer. Several junior officers of the Madras Medical Service died at their posts, and some of the subordinates deputed to the Madras Presidency contracted fatal disease on that service. The epidemic of yellow fever now raging in the Southern states of America has given a new and striking illustration of the risk to which the lives of medical men are often exposed in the practice of their profession. To the credit of the profession be it added that its members never fail to brave these risks with courage and fidelity to their calling and duty.

And, now, a word for OURSELVES. To have piloted the *Indian Medical Gazette* through another, the 13th, year of its existence is a cause of no small satisfaction; but without the constant and unsolicited aid which we have received from brother officers and others, the task would have been impossible. To all who have contributed to these pages during the year we tender our most grateful thanks; but not without a lively and hopeful regard to future favours. We should wish the *Indian Medical Gazette* to be reckoned as a common interest and bond by the

whole medical fraternity in India. On analysing the list of contributors to our 13th volume, we find that 47 belong to the Bengal, 6 to the Madras, and 1 to the Bombay medical service; 17 to the Army Medical Department; 7 to the Uncovenanted Medical Service; 17 are Assistant-Surgeons; 6 members of the Subordinate Medical Department (apothecary class); 4 Hospital Assistants and native doctors, and 8 medical men not in any public service and non-medical gentlemen.

The most disappointing feature in this statement is the paucity of contributors from the Madras and Bombay Presidencies, and the most hopeful the increasing number of contributors belonging to services trained and educated in this country.

The circumstance of this journal being printed in Calcutta should not, we would urge, imply any special limitation of its subject matter and circulation to Bengal; and we would once again appeal for support, literary and material, to our brethren of the sister presidencies. Medical science and its cultivation and exposition should take no reck of geographical and administrative divisions. With this sentiment we heartily wish our readers, wherever they reside,

A HAPPY NEW YEAR.

A FEW OBSERVATIONS ON ENTERIC OR TYPHOID FEVER:

APROPOS OF A REPORT ON THE SAME IN RELATION TO BRITISH TROOPS IN THE MADRAS COMMAND. BY SURGEON-GENERAL C. A. GORDON, A. M. D., AND A LEADING-ARTICLE THEREON IN THE "INDIAN MEDICAL GAZETTE," OCTOBER 1ST, 1878.

(COMMUNICATED.)

It might have seemed to many or most of us that the existence of typhoid or enteric fever in India was beyond the region of doubt—the existence of the disease as a fact apart from all theory as to causation, as an entity to be proved at the bedside independent of the circumstances which had originated it. There are two books which may be said to be in the hands of every medical officer of the Indian and British Departments, who have entered in late years.—Parkes' *Hygiene* and Aitken's *Science and Practice of Medicine*. In the 4th edition of the former, page 635, is a table of "the causes of sickness and death among Europeans in India, copied from Dr. Bryden," and "although the table is for one year, it yet represents fairly the average sickness except as far as cholera is concerned." In it enteric fever figures as follows, in 1871, per 1000 of strength:—

Bengal Army	38 admissions.	171	deaths.
Madras Army	44	147	"
Bombay Army	23	148	"

In a foot note it is added,—“the immense mortality of enteric fever to cases admitted also shows that many cases were only diagnosed after death;” and the disease is noted as one from which the young soldier must be guarded against on his arrival in India. In the 6th

edition of Dr. Aitken's work, vol. 2, p. 1212, it is stated that "both typhus and typhoid are now known to occur in India."

In the report of Surgeon-General Gordon, we are asked to concur with him that 175 cases of disease returned by the medical officers of the British service in the Madras Presidency as enteric fever between 1871-77 were not cases of enteric fever at all, and "to conclude that the phenomena stated of late years to constitute specific pythogenic fever, are none other than such as were known by old and experienced medical officers in India as pertaining to fevers of the country,—endemic and climatorial"

To commence with, it is necessary to take exception to the use of such composite adjuncts as "specific pythogenic," which, taking the words in their present accepted meaning, are self-contradictory. For a disease to be specific in origin, is implied the existence of the special virus or poison (apart from all other circumstances) to originate it, and without which it could not be brought into existence; while to refer it in causation to such a complex element as "filth," implies a spontaneous origin apart from a special virus: the two are antagonistic. Dr. Aitken (who is brought forward as a standard authority in the Report), speaking on this very subject, says (6th ed., vol. 1, p. 576-7), "Each of these specific poisons (and, as we have already seen, they are numerous) thus multiplies in the same way and in the same remarkable medium, out of the same living organisms of the human frame; yet each of these several poisons sets up a series of changes which always issues in the reproduction of its own specific kind of disease, and no other. Small-pox gives rise to small-pox, scarlet fever to scarlet fever, measles to measles, and so on. Herein lies their *specificity*. Such being the doctrine attempted to be maintained in these pages, the theory of the spontaneous origin of enteric fever, or of any other specific disease, must be in the same relative position as when it seeks to explain by such a principle the propagation of plants and animals. These—namely, plants and animals—likewise at least two diseases—namely, syphilis and small-pox—are certainly now known to propagate only by the law of continuous succession, whatever may have been their primary source. But the hypothesis of spontaneous origin and indefinite propagation of enteric fever has assumed a definite form of expression in the doctrine which attempts to teach that enteric fever is often actually caused by the products of common putrefaction—a doctrine which has been cleverly embodied in the nomenclature of the subject by Dr. Murchison."....."The term 'pythogenic fever' or fever 'born of putrescence,' is the name by which Dr. Murchison at once designates enteric fever, and theoretically implies its origin. He has thus rashly committed science to a hypothesis of a highly doubtful nature." We have now Dr. Gordon going beyond this, and concluding that the cases returned as enteric are not so because it is not shown that "the local and specific conditions to originate the occurrence of specific pythogenic fever" were in existence; thus inverting the usual process of reasoning from facts to theory, and making the recognition of the disease dependent on the possibility of previously elucidating existing causes which, to say the least, have not yet received general acceptance, and which by some are regarded as highly problematical. Apply the same reasoning to small-pox, measles, &c., and where shall we be stranded? But we may at once say that we have sought in vain throughout the report for any qualifying designation to the cases returned as "typhoid," whether as "specific," "pythogenic" or otherwise; and to so characterise them, especially as in the "standard of comparison" a three-fold opinion at least as to origin is brought forward with which to test them and choose from, and then to deny that they are enteric at all because that the conditions of the theory of causation are not shown to be present (some dating back to 1871) is

much like putting up a man of straw for the sole pleasure of knocking him down again.

Turning now to the leading article, there is a sentence which certainly challenges criticism. At page 277 we find,—“It may be a matter of scientific interest to differentiate and define the type of fevers, but the point of real moment is to know to what causes they are due.” To this it may be replied,—how can we enter into an elucidation of the causes unless we are first certain of the subject to be enquired into? We must have some substantial basis to work upon, some entity placed beyond doubt, before we can theorize as to its origin; fevers defined in type and differentiated from each other in order to raise their etiology on a scientific basis and beyond mere guess work. And if we have it not, what is the worth of sanitary rules, what the groundwork for their existence? Is not the very want of precision the present opprobrium of medicine as a science, and an obstacle to sanitary advance? Take for example "common continued fever" as seen in India or elsewhere, will any one assert that the cases classed under it have a common type due to a common cause? Will any one tell us the cause or causes to which these cases are due, and put them in a form to meet general acceptance with satisfactory evidence to support his theory? Is it not rather nearer facts to say, that under this heading we have different types of disease with probably different causations, and that of the causes we know very little which will stand the test of scrutiny? It is essential that we should be agreed as to the disease before we theorize as to cause, that its types and modifications be clearly pointed out; that we should proceed from facts to deducible inferences, and not that the existence of a disease be made dependent on our ability to find out ascribed causes. The present subject excellently illustrates the point.

What then is enteric or typhoid fever?—The highest general authority is the nomenclature of disease drawn up by the Royal College of Physicians, London, and medical officers in Government employ are obliged to conform to it in their returns. At p. 5 we find,—“Enteric Fever, *Synonym*, Typhoid Fever, *Definition*: A continued fever, characterised by the presence of rose coloured spots, chiefly on the abdomen and a tendency to diarrhoea, with specific lesion of the bowels.” And if we turn to the text books, take for example Aitken, p. 539, 6th edition, the definition of the disease there given is but an extension of the previous one; but in neither is there any reference to causation, it is not necessary for diagnosis. Here then are certain symptoms enumerated, the concurrence of which indicate the disease. That individual peculiarities modify these symptoms is certain, but that need not detain us now. The question is,—is such a disease met with in India? Surely such a question of facts, apart from all theories of causation, ought not to remain in doubt. The Sanitary Commission of India, the Army Medical Department, return it as in existence; individual observers in India (Bryden, the pages of the *Indian Medical Gazette*, &c.) attest its presence; and the executive British medical officers in the Madras Presidency during 1877, and previous years, assert its existence, the which is denied by Dr. Gordon on the grounds above stated.

In this report we have an abstract of 147 cases of asserted enteric fever, made apparently by Dr. Gordon himself from the case books, and ranging between 1871-7; sanitary reports on stations made in 1878; and a tabulated return of the cases arranged under given headings "transmitted to the medical officers holding charges." Now on this point—the existence of the disease—we have the abstracts and the tabulated returns to judge from. In a few instances it is acknowledged by the medical officer that the disease returned as enteric fever was not so, and it must be allowed that many of the abstracts (some confessed so by Dr. Gordon) are too meagre to carry with them any conclusion whatever—for

or against the diagnosis made at the bedside, or the alternative diagnosis offered in the report. But, putting these on one side, it is very difficult to understand from the abstracts presented to us, under what other heading than enteric fever 32 at least of these cases could with any propriety be placed; while 7 of them correspond to "standards and types" (text book standards selected by Dr. Gordon) "applied conscientiously and perhaps severely" by one "evidently imbued with a strong spirit of scepticism" (*J. M. Gazette*, 1878, page 278). It is quite possible that errors in diagnosis have crept into the returns made by executive medical officers; on the other hand it is clear that similar errors are existent in the alternative diagnoses of the report, for example:—"A fever of 40 days' duration in hospital" considered "febricula" (No. 25, page 28); a fever of 31 days in hospital with stools described as "typhoid" in the abstract, also "febricula"; a case of "fever, diarrhoea and abdominal pain, and disease in a few days clearly recognized as enteric," "rose coloured eruption," "blood passing from bowels at end of 4th week" and dying from coma preceded by convulsions after 36 days in hospital, is thus remarked on; "from the history as given in the case-book, the case is more like one of ardent fever terminating in heat apoplexy than any thing else" (14, page 44) Apart from errors in diagnosis by some, it is the height of improbability, nay impossibility, for so many medical men, over a period of six years, all to have mistaken enteric fever; a most culpable ignorance (to say the least) must be general in the department to allow of even an approximation to such a state of affairs.

Limiting ourselves to the seven cases whose abstracts stand the test of rigid scrutiny and comparison, we cannot resist the conclusion that we have here corroborative evidence that typhoid or enteric fever (clinically and pathologically similar to the European disease) is one of the diseases met with in India.

How then stands the causation?—Parkes, 4th ed., Hygiene, p. 454, writes:—"External cause.—A poison of animal origin; one mode of propagation is by the intestinal discharges of persons sick of the disease; other modes of origin and transmission are not disproved." And at p. 123 it is stated that "almost all the events which have been recorded in connection with the origin of typhoid fever" are explained by "the doctrine that a specific cause is necessary for its production." Aitken, 6th ed., vol. I, p. 577, writes:—"The history of enteric fever, whose leading features have been described in the previous pages, is wholly inexplicable upon the 'pythogenic theory' of Dr. Murchison. On the contrary it is emphatically the history of a specific disease generating a specific poison, and propagating itself by it." This also is the opinion of Dr. Budd. This *specific* theory requires the introduction of a case into a community where the disease is not, to allow of its existence and spread in that community; it excludes all other theories of propagation; it asserts that the disease may be stamped out. On the other hand we have the "pythogenic theory" which regards the disease as dependent on the introduction into the body of the elements arising from putrefactive processes; hence the requisites are, filth in a state of decomposition, and a recipient human body. It denies the necessity of a specific virus—an individual suffering from the disease—for originating the disease in a healthy community, it asserts a spontaneous origin. It is needless to say, that if this be the true theory the disease cannot be stamped out, but will spring up whenever and wherever unsanitary conditions exist and human beings are exposed to these. A third theory is given in the Report of the State Board of Health of Massachusetts,—"that it is putrefaction of animal and vegetable substances, *under cover*, which gives typhoid;" this clearly is an extension of the Murchison theory as to the noxious elements, a limitation of it to certain necessary conditions. A fourth theory

is,—that the present limits of asserted causation of the disease must be extended, but that the disease once produced may be propagated to others; this, the latest, is also American. Here then is a range of causation which cannot be termed exclusive; there is one undoubted fact—that typhoid fever is communicable by the intestinal discharges of one sick of the disease.

Do the Indian cases conform to any one or all of these, or must we still further extend the etiology circle? It is needless to show that any theory to stand its ground must embrace all the known circumstances of each case.

An answer to this question of a satisfactory nature has not hitherto been forthcoming, nor does the report of the disease in the Madras Command throw much light on the subject. These cases range from 1871 to 1877. The abstracts give no clue to any real or probable or possible causation. The sanitary reports on the barracks, &c., bear date none earlier than March 1878, and although the attention of the medical officers then doing duty with the corps was directed to the subject of causation, yet "it will be observed that only at a very small number of the places mentioned is an allusion made in regard to the existence of recognized causes of enteric fever, and that where such allusions occur, they are expressed in terms so problematic as to indicate the undecided views held by medical officers while writing about them" (p. 101 of Report). In the tabulation of cases in the appendix, columns for "particulars in regard to exposure or otherwise to emanations of animal or vegetable origin"—"other apparent cause of attack, infectious or otherwise"—"any other particulars likely to elucidate the etiology of the disease in individual cases,"—are given, yet the replies convey but little information; apparently because none was forthcoming. "No history," "not traceable to exposure," "no evidence," "nothing of a definite character," "not stated," "none traceable," "exposed only to effluvia from latrines in barracks," "unknown,"—such are the staple replies; on the positive or rather inclining to the positive, side of the subject we have:—"possible infection" 3, "supposed infection" 2, "traced to exposure" 1, "following attendance on his brother ill of the disease" 1, "unsatisfactory state of latrines" 1. It is clear that as a basis of causation these cases are next to worthless, but, when it is remembered that a considerable period of time (years in many instances) had elapsed between the case and this enquiry, it is not to be wondered at that the answers are so often negative. If any good is to come out of an enquiry into the subject it must start from a recognition of the disease at the bedside, and a careful scrutiny into the previous surroundings of the individual made without delay and by competent enquirers. The scattered nature of these cases (sporadic) tends in itself to show that the causation was not to be found in the barracks but in a something to which the individual alone was exposed apart from the company or corps of which he formed a component; had it been otherwise, localized outbreaks would have been the result, arguing from the ascertained facts of the disease in Europe. The soldier has a wide range of wandering, through bazaars, native haunts, adjacent villages, and is consequently exposed to many sources of disease; and it is impossible to overlook this important fact in endeavouring to trace the exciting cause of such a disease as enteric fever.

How do the ascribed causes of the disease in Europe stand as possible factors of the disease in India?

Specific factor.—Is there a case in existence to propagate its kind? The answer surely must be in the affirmative for India generally, and if we want late information of the Madras Presidency, we can find it in the Sanitary Commissioner's report for March—June 1878. At page 94 for March and April it is stated that "3 cases of undoubted enteric fever have been admitted in the hospital (Madras) during the past month, two of Eurasians who are recovering, and one of a native who died, and in whom the diagnosis was confirmed by a

post-mortem." And at page 105, "the occurrence of cases of typhoid fever in the compound of the 'Seven Wells' is reported, under conditions very similar to what is found in Europe." At page 135 of the report for May—June, enteric fever is reported as existent in the Lawrence Asylum, Ootacamund, "at the present time," and mention is made of an outbreak also there in 1876-77.

Animal putrefaction.—The existence of this factor cannot again be doubted. To speak of no other smells, the presence of human excrement can hardly be avoided; the native squats wherever he may happen to be. If effluvia from decomposing human ordure can originate the disease, we have undoubtedly a potent and common cause in our midst; while if the disease be present in the native community, then considering the certainty of possible propagation through the medium of the intestinal discharges, the probability of its transmission by air and water contamination is very strong indeed.

Vegetable putrefaction.—Here again no doubt of its existence can arise, whatever doubt we may have as to its power in originating this disease.

What then is the summary?—(1) that enteric fever, similar clinically and pathologically to that of Europe, does exist in India. (2) That the causes ascribed to the disease in Europe are also present in India. (3) That the evidence necessary to connect these in India is not forthcoming, and that the etiology of the Indian disease is a work for future enquirers.

Review.

REPORT ON THE EPIDEMIC OF CHOLERA OF 1875-76 IN THE CENTRAL PROVINCES.

BY SURGEON-MAJOR S. C. TOWNSEND, F.C.U.,

Sanitary Commissioner, Central Provinces.

THIRD NOTICE.

As we have already shown, Dr. Townsend has altogether failed to prove his statement that human intercourse governs the epidemic diffusion of cholera, or that it even plays any important part in determining that diffusion. We pass on to examine his 4th conclusion: "That the infective matter of cholera may multiply or increase in water containing sewage or other animal organic matter in a state of decomposition."

This conclusion and also his next conclusion, as we shall find presently, virtually admit that the theory which would account for cholera by a germ manufactured within the human organism and transferred from person to person is patently inconsistent with actual facts. As grounds for this conclusion we are referred to the facts that cholera exhibits a manifest preference for the beds of streams, and that in many instances the rapidity with which cases follow one another in local outbreaks is so great as to be incompatible with a belief that they can have been the result of the progressive transplantation of material from person to person. We quite agree with Dr. Townsend in regarding these phenomena as utterly opposed to his previous attempts at generalizations, but we fail to see that either of them warrants the conclusion at which he now arrives. The fact that cholera exhibits a preference for the beds of streams is one which has long been recognised, indeed it was the extent to which this occurred in the great Bavarian epidemic of 1854 which formed the starting point for the soil-theory of the origin of the disease. Dr. Townsend, it is true, maintains that his observations regarding the occurrence of cholera in localities situated along the course of streams show that the specific cause of the disease was

conveyed by the water, and here he enters upon new ground. A careful study of the maps accompanying the Report, and illustrating the dates of the appearance of cholera in villages on or close to streams, does not, however, favour the idea that the diffusion of the disease was due to the spread of any specific material by means of water—unless, indeed, it be assumed that such a material can travel with equal facility against and with the course of the stream. The series of maps forms the most prominent feature in the Report. They are intended to demonstrate the truth of this water theory, but the most careful examination of them fails to find any evidence in its favour. They are valuable chiefly because of the very plain facts recorded in them, but these facts are as directly opposed as they well could be to the doctrines which have been set forth in the Report.

The assertion that the suddenness with which outbreaks of cholera frequently manifest themselves is a proof that the cause of the disease multiplies in water, is somewhat startling. The phenomenon may certainly be regarded as opposed to a belief in the propagation of the disease by means of contagion, but it is illogical to affirm that it is in itself conclusive evidence of the existence of a morbid agent in water, or of its multiplication there. All that it really indicates, when taken apart from preconceived opinion, is the probability that in these cases the diffusion of the disease is due to some condition of locality and not to personal contagion.

The next conclusion which we are asked to accept is:—"That the infective material of cholera when introduced into the human organism will not produce the symptoms which constitute the disease unless a certain state of system suitable for its action in this manner has been previously induced." In other words, the evidence that cholera is a contagious disease utterly breaks down when confronted with the phenomena of prevalence and the influence of human intercourse on these phenomena is entirely subordinate to conditions of locality. The conclusion when thus formulated is one with which few who are acquainted with the subject, will be disposed to disagree. When we find that only 2 deaths from cholera were registered in the Berars in 1874 and over 22,000 in 1875, we do not require anything further to show us that human intercourse plays a very subordinate part in determining the prevalence of the disease. Festivals, pilgrimages and marriage feasts go on steadily year by year, and cholera is always present in some parts of the Peninsula, but its annual prevalence varies to an extent which is utterly inexplicable on the theory that it is due to a poison multiplied and reproduced within the human organism and transferred from person to person. We have a great array of facts against such a belief, and none affording any real evidence in its favour. Evidence in favour of the influence of human intercourse is not necessarily evidence of the development of the morbid agent within the human organism. People in going from one place to another convey many materials beyond their own persons, and the facts regarding the diffusion of cholera which appear to manifest the influence of human intercourse are much more in accordance with a belief that the cause of the disease while developed outside the human organism may be transferred from place to place by human agency, than with one which maintains that the disease is due to a contagium developed and multiplied within the organism. As, however, Dr. Townsend is satisfied that the cause of cholera is a product of the human organism, we need not be surprised that he should invoke predisposition as a *deus ex machina* to help him out of his difficulties. Had his first conclusion any substantial basis, it would be hard to find any other explanation for the manifestly subordinate influence of human intercourse, but, in the absence of such a basis, the phenomena can only be regarded as adverse to the theory, and certainly not as affording grounds for another con-