On the epidemics of the Bengal and North-West Presidencies / by K. MacKinnon.

ON THE EPIDEMICS OF THE BENGAL AND NORTH-WEST PRESIDENCIES.

By K. Mackinnon, Esq., M. D.,

BENGAL MEDICAL SERVICE.

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"En aliud arcanum naturæ vita ipsa vix minus mirabile."

PESTILENTIAL EPEDEMICS (CHOLERA.)

Cholera.

The above euphonical latinity which has clung to my memory, since the long past years, when I first pored over the pages of the Conspectus is, I think, an appropriate heading to the subjects left for me to deal with in this third and last chapter on epidemics. The pestilence known by the name of Cholera. The malignant fevers which under the designations of Pali Plague and Mahamurree bear such a resemblance to the pestis of authors if they are not the identical disease, the epidemic fevers which in some jails have been so malignant and peculiar as to cause suspicion of contagion and the deadly visitations of disease of late described from the Banks of the Indus, these form the subject of this paper.

It is surely a hidden thing or mystery of nature that in 1817* an epidemic disease which if known before, had certainly not been seen lately appeared in Jessore, a district of Bengal—that from that time to the present it has never entirely disappeared with us—at times seizing a victim sporadically—at others lighting up into the blaze of a most deadly visitation—now in one, then in another locality—in one season—not in another—that it is obedient to no laws of appearance spread or conveyance which we can always

* My own belief has always been and has been published, that Cholera is described by Celsus Aretedeus and Sydenham, and see Dr. Bascomb on epidemic pestilences, page 21—describing the disease in A. D. 262.

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apply to it—that with equal obscurity as to what it is or how communicated, it has since the period above named ravaged various parts of Europe, Asia, Africa and America, treacherously and suddenly shewing itself—as inexplicably ceasing at once and altogether.

Since I last wrote upon Cholera, its recent epidemic visitations in Europe have engaged, for it the attention of the most scientific men in the profession. Advanced organic chemistry and the microscope have been applied to investigate its exciting cause, but we know not yet whether it be an actual material substance, or but a property—a modified one of course of the atmosphere by which we are surrounded. In the elaborate and interesting report of the College of Physicians in London published in 1854,* the view ultimately adopted as to the cause is that it is a matter increasing by some process whether chemical or organic in impure or damp air, and that although of course it is diffused with the air, it is also distributed and diffused by means of human intercourse.

Now although we must admit the association of the disease with or I might say its attachment to these conditions of damp and impure air, I speak as a local observer and say, that we may have the disease, and therefore its remote cause, without the presence of either. I have seen the disease prevail intensely when the dry hot winds were furiously blowing, and where in Barracks freely perflated by them and kept clean, there could be no actual impurity such as the senses or even chemistry could recognise, and most certainly no damp. When Troops are attacked on a line of march or in boats, the disease cannot always be associated with the dampness or impurity of the air. We find, said the Editor of the *Lancet*, that "it swept the surface of the Arabian desert where a drop of rain never falls and where a spring of water is the object of a journey of 100 leagues," the fact is not weakened by its antiquity.† It is a great salient fact with regard to Cholera, and it is one which I observed and acted upon at least 12 years ago, that the cause affects locality and some times a locality that is very limited in extent. To some it may seem unnecessary to dwell upon

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* If I may venture to characterise this report, I would say that is a learned and labored effort to apply laws to what no laws we yet know of are really applicable, every principle laid down is found to require numerous exceptions from observed facts.

† Lancet 1832.

this, but whatever the case may be now I know that but two years ago when I was officiating Secretary to the Medical Board, I found it little recognized even by the profession, and not at all by others to whom the health of large bodies of men is entrusted, that in change of place lies the best chance of escape from this formidable enemy. Cholera.

I was myself so well acquainted with facts, proving the advantage of such a measure that I suggested and received the sanction of the Medical Board to the issue of a Circular, pointing out the propriety of adopting this step on all emergent occasions. I shall probably bring to the mind of my readers that this is opposed to observations made in other parts of India, especially by Dr. Lorimer in his valuable Report from Madras.

The last conclusion aimed at in the Report I am considering, is in seeming contradiction to the Cholera law just noticed. If the cause clings to localities and even to confined ones, how can it be true also of it that it is said to be diffused and distributed by human intercourse. The answer is there may be local batteries for evolving it and moving wires for conveying it. This question of its carriage by human intercourse is to my mind as perplexing as any thing in the whole range of Medical enquiry. There is so much evidence for and against it.

In England there appeared to be clear proof that its appearance followed the arrival of ships from places where the disease prevailed, and that it first showed itself among persons holding intercourse with the crews of these vessels. In America the facts recorded appeared to show yet more clearly that the disease had been conveyed. It has been held as an argument for the spread of Cholera by human intercourse, that Regiments are attacked in marching through countries where the inhabitants are uninfected.

I cannot see the force of this illustration and how is it that it is in particular tracts of country that Troops become affected* and that experience has shown that after passing certain spots crossing a particular river, for instance the disease is sure to disappear: all this again points to the local origin and sojourn of the cause† and we have a stronger

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* It is very remarkable that Troops even in large bodies in the North West Provinces are very seldom attacked with Cholera on a line of march.

† The detachment of General Adams had 62 cases in one night, on pitching their camp near a village somewhere in Central India. In the Grand Army of the Marquis of Hastings in 1817, while thousands of men were dying daily,

Cholera. fact often observed in this country pointing to the same conclusion, namely, that the Cholera often breaks out suddenly among Soldiers on board of Boats or Steamers. Here we have no fresh renewed human intercourse. The troops may have embarked well and unaffected, they may move on through a country where there is no disease, suddenly Cholera bursts forth siezing many victims in a limited time. Let the Boats be deserted the disease will disappear almost to a certainty, although the amount of human intercourse continues as before. All this may some times happen with abundant space and ventilation removing the suspicion that the poison conveyed by or exported by individuals, is merely intensified by crowding and bad ventilation. These, in my opinion, only act by assisting the real cause for we may have crowding and want of ventilation to any extent without Cholera being an inevitable consequence. A very remarkable occurrence of some years past may be mentioned. A Regiment proceeding by Boats to Dacca was visited by a severe out break of Cholera. They landed and another Regiment took possession of the Boats for proceeding Up-country.

The disease left the Regiment which had landed and immediately assailed the other Corps. Surely there is proof here of local contamination and not of the disease being conveyed by human beings: all I intend to urge here is, that as we see Cholera epidemics in this Presidency, it is not by human intercourse much less as a contagion, that it prevails. However difficult it may be in theory to think that it can originate in more ways than one, it seems impossible to resist giving consideration to the array of facts, in proof of its being carried by means of human intercourse, which some of the Reports from Europe supply, nay more that as an exuded poison from the bodies of Cholera patients it has been conveyed on clothes and bedding to distant places there causing the disease. I would here beg to refer to Dr. Alison's paper on epidemics in the April No. of 1853, *British and Foreign Quarterly Review.*

But while it is thus admitted that there is much plausible ground for believing Cholera to spread by human intercourse, and it would seem in cold countries even by contagion and infection, such would by no means be my

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the disease stopped as if by magic, on the Army moving but a short way to high ground across the Chumbul. How are these facts explained by the theory of communication from one person to another.

conclusions from what I have myself seen and heard of the disease among us. The spread by the movement of human beings is certainly not a law it observes here at present. Who has seen it brought into Calcutta from sea, and is it not the ordinary truth regarding it that if it infests a ship going down the river, it will usually leave her when she gets to sea, the intercourse then closer than before. Of the communication of the disease by contact or from an infected person through the medium of a proximate atmosphere, I have seen no proof in my own experience and in doubting that it happens here, I am, I believe, expressing an all but universal belief. That those attending persons sick of Cholera are sometimes themselves attacked is of course true; but if this be the rare exception,* and not the rule, is it not more rational to account for the exception by supposing that the disease arose from the usual source. Cholera.

Whatever that source may be there is abundant proof to show that much depends upon the susceptibility of the recipient. During outbreaks in Jails and Regiments, we almost invariably see the Medical Officers and attendants entirely escape. On a late occasion of the disease prevailing, and that for a very unusual time in one of Her Majesty's Regiments, the Lady of the Commanding Officer with a fearless devotion, and Christian charity which was beyond all praise and worthy of Miss Nightingale herself, was for weeks in constant attendance upon the sick, without being herself attacked. In fact the attendants upon Cholera patients being attacked is the very rare exception not the rule, and it is well that this can be stated with confidence. Upon what the susceptibility depends it is yet impossible to say. It is not upon mere debility or low vital power, for the disease often strikes down the strong and spares the weak inhabiting the same house. It would seem that there must be some necessary pabulum for the cause to work upon, not as in the case of a zymotic poison to cause a ferment and then a re-production of itself, but to act directly in vitiating or destroying vital actions. Some times the patient is destroyed as by electric fluid, and in all cases the functions of nutrition, assimilation and excretion are more or less affected not by increased action but by loss of power. The danger of

* So in China, I am informed by my valued friend, Dr. Grant, who served in the expedition to that country, that in an Hospital Ship to which numbers of Cholera patients were brought from the shore, there was not a case of the disease among the crew or the Hospital attendants.

Cholera. Cholera may thus be said to be manifested not by the violence of morbid actions but by the diminution or cessation of natural action. With respect to diseases having specific poisons for their cause it is known that their character as epidemics varies much in prevalence and severity in different years. Of Cholera, this is also true, but it is further to be observed of it here, that since its first appearance it has undergone a general change of character, which renders it difficut to conclude that its cause can be one actual specific and unchanging poison. It is now more collapsive in character and far more rapid in its progress than when I first came to India. The death by Cholera which is now considered the consequence of retained urea was, I think, much less common than now: some will say it was less observed, but I think a reference to authors will bear out my assertion. It may, I think, be stated that Cholera is with us more of an endemic disease than elsewhere, and that we can trace its connexion more with the variations of the season. There are some months of the year when we may be sure to see or hear of some cases of it, other months in which we may usually observe its total absence.*

The advent of the hot weather almost always brings the disease with it in a greater or less degree, and as this change varies according to latitude, so does the time of the appearance of the disease; again at the time when the cold season is commencing, is the next period at which the disease is most common either in a sporadic or ordinary epidemic form. I think it also correct to say that severe epidemic invasions are less obedient to this law of appearing at the change of season.

Since the above was written, and indeed at this very time there have been illustrations of the positions I have laid down above. In Calcutta this year the hot season set in unusually early, and in March Cholera made its appearance but not to any alarming extent, and in April and May it had gradually disappeared. The rains set in pretty generally in the end of May all over the country, and though this as a common rule, leads to the disappearance of Cholera, there has been during June and July a fearful visitation of the disease at Agra, a place not often obnoxious to it. Since that time it has moved westward and is wofully fatal at Lahore. What is taking place also illustrates two other peculiarities regarding the spread of Cholera. Its movement

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* See Tables appended.

from East to West* and the uncertainty of its appearance as regards places. Calcutta is at the present time free of the disease yet the local observer need not be reminded where lowness of site, dampness, and impurity most prevail. Cholera.

Not many days ago a letter appeared in the *Delhi Gazette*, from Dr. Balfour, the Civil Surgeon of Delhi, congratulating the inhabitants on the absence of the epidemic then so fatal at Agra. I remarked that by-and-bye he might have to make a different statement, and accordingly in a very few days, after the date of his first letter, appeared a second letter from Dr. Balfour, announcing the advent of an invasion of Cholera in Delhi, the first which has appeared since the year 1845, while in 1853 Benares, Allahabad and Cawnpore were awfully scourged. In Europe sea-ports as a matter of course ships do arrive from affected ports; human intercourse is caught at as the cause of appearance, although none even there deny the spontaneous origin of some outbreaks of the disease. There will be some to say that to Delhi it has been conveyed by human intercourse, and no doubt the believers in this manner of diffusion will find what they will consider proof in the undoubted fact that travellers daily arrive at Delhi from Agra, but so they do at Cawnpore and other places lying East of Agra. Then why does not the disease travel† as commonly in that direction also. As already mentioned, the appearance of the disease in America was traced upon strong proof to its actual conveyance by ships from infected ports in Europe. Without the means of examining facts in detail and therefore not attempting absolutely to deny the evidence adduced, I venture to observe that the arrival of ships being a constant occurrence and in some of them there being occasionally Cholera cases, it is easy and natural to seize upon this prominent fact, and to draw the inference that Cholera spreads by human intercourse.

I am not aware that there is any proof of the disease ever having come to Europe from America, and I believe that none deny the appearance of the disease on some occasions independent of the movements of human beings. To return to the point of how capricious the disease is in affecting places, and it may perhaps be added in attacking particular

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* A late writer upon yellow fever speaks of explosive waves of epidemic influence moving from East to West.

† I say as commonly, because there are exceptions to what I think a general Rule.

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classes of society, endless instances may be adduced to show that though never entirely absent in the large towns of Hindostan during the hot dry months, it is only occasionally that speaking of any particular place, it pays a formidable visit, such as to attract public attention and dismay. In the Returns from Jails and Regiments, we have abundant proof of this but yet an examination of the same documents will show that this fell destroyer has some choice localities for his occasional visits.

There seems to me much proof in what we observe here that the disease does not spread by human intercourse. It suddenly invades a Barrack where there has been no fresh intercourse with people coming from other places. If we might suppose that the first man or two affected had caught it from contact with natives, how shall we account for its continuing to attack numbers of men in the Barracks, when we know that a man as soon as he is attacked is sent into hospital usually at a considerable distance. Then how again shall we explain that while intercourse goes on as before or even more closely, a change of place often checks the disease immediately.*

I could adduce no more remarkable instance of this than in a fact lately furnished to me by Mr. J. Sutherland, Surgeon of the 8th N. I. The disease had prevailed in the Regiment while proceeding in Boats from Cawnpore to Benares, destroying in all 42 men out of a strength of 1,115. At Benares it was determined to remove the men from the Boats, pitching Tents for them on an adjacent eligible spot. I now quote from Dr. Sutherland's Report.—"On the same evening Tents were pitched and the whole of the men of the Corps had left their Boats. During that day 4 cases of Cholera were admitted into hospital, and on the following day there were 11 admissions, on the day after, *viz.*, the 14th only 5, and on the 15th 1 after which the disease disappeared from among the Troops, but continued to prevail for several days later among the women and children of the Regiment, who were crowded in great numbers in Boats of their own." The above is an extract from a Report by Dr. Sutherland, which I hope may be published in the Annals, for besides the fact of which I make use, there is much besides which is interesting; including some useful and practical hints for prevention and one

* An instance has lately occurred of a ship that left Calcutta with a healthy cargo of Coolies having been severely invaded on her voyage to the Mauritius.

more expression of disbelief that on this occasion at any rate there was any spread by contagion.

There is one fact stated by Dr. Sutherland which illustrates well, how the social habits and movements of men are apt to misguide us to the conclusion that the disease is conveyed by or on their persons.

The first case in the 8th N. I. was in the person of a Recruit, who had visited Cawnpore where the disease was prevailing at the time. If this man had come into a town or standing Camp, had died of the disease, and it afterwards had shown itself in others the proof of conveyance would be thought clear, but in the present instance the next cases were in another Boat four days after the death of the Recruit. Each Company had a separate Boat, no Boat escaped. From this general prevalence of the disease, Dr. Sutherland infers that the morbific poison that produced the disease originated from the assemblage of all the following conditions mentioned in his Report, "high temperature," "animal effluvia from over-crowding," aided by the exhalations from the banks of a river rapidly subsiding, and long fasting and privations.

Dr. Sutherland then puts the question. "Does Cholera originate from the above conditions independent of the infusion of the specific poison? Mere crowding, impure air and bad ventilation will not cause the disease under ordinary circumstances, may not the only element deficient be emanations, such as the swampy banks of the Ganges produce at that season?"

My answer is, we may have all these conditions without Cholera, and we may have the disease in the absence of them all. Dr. Carpenter, in his work on the principles of human physiology, well explains how atmospheric impurities act only as pre-disposing causes to specific poison, obviously by preventing eliminative purification of the blood, possibly by increasing the production of the poison.

Admitting that as regards other poisons acknowledged to spread by human intercourse and contagion, the proof of their actual material presence in the atmosphere is only inferential, yet it is also true (of most of them at any rate) that their materiality is proved by their re-production of themselves in the human body and in respect to these poisons, Small-pox, Measles, Scarlatina, &c., it is not a decided question whether they ever have at the present time a spontaneous origin. If they have, I suppose it will be conceded that the instances are rare and that generations

Cholera. of the poisons are the result of vital actions in the human body itself: all writers appear to agree in thinking that the Cholera poison, or I prefer to call it the Cholera cause, has often a spontaneous origin independent at any rate of human vital actions, and here therefore is wanting one point of identity of character or resemblance between it and the other diseases which spread by human intercourse, be that intercourse a mere vehicle of communication or a conveyance by the individual of a poison exhaling from himself.

If Cholera poison be a human product it ought it seems to me to show as much and frequent evidence of spread by contagion or infection as by conveyance. If again it is also generated independently of the human organism, it is an addition to our difficulty that it has two sources of origin; and again if Cholera does spread by human intercourse and contagion anywhere the same ought to be true of it, in all countries where it appears; yet strange to say on this side of India where there has been more opportunity of observing than anywhere else, we are all but unanimous against these means of diffusion. Different views as I have already hinted at, prevail and appear to be gaining ground in the Madras Presidency. The explanation would appear to lie in the difficulty of such questions generally. I have already hinted at some of the sources of error and would add here that however strong the evidence on the side of human conveyance and contagion or infection if you will, there is, as is true in regard to some other diseases, yellow fever for instance, very strong testimony on the other side of the question. We all know the old saying of there being two sides to every story, which is but another way of saying how uncertain a thing is human testimony, and how when we wish to come at a conclusion we shall find evidence to support it. Judging from my own experience, after a long and extensive observation of twenty-nine years,* I find it impossible to believe that the cause of Cholera is anything but a poison or property in the atmosphere, which is generated in certain localities, sometimes of limited, and at other times of greater extent. Damp and impurity are not one or both the thing itself, but appear either to concentrate it, or by retarding the vital actions of assimilation and disintegration,

* When there is so much conflicting opinion and so much evidence on both sides of a question, it is surely natural that one who has had long experience, and who has never ceased to feel interested on the subject, should fall back upon the result of his own observation.

to leave the organism with less power of resistance. That an atmosphere pregnant with Cholera poison to some, can be breathed with impunity by others, there is abundant proof of. Cholera.

The disbelief in Cholera contagion or its spread by human intercourse, being far from a fashionable doctrine at present in the Medical world, I feel called upon to shew that even in Europe my view of the question has had, and has yet, some authority to support it.

Dr. W. Fergusson, "If contagion do exist at all, it must be the weakest in its powers of diffusion, and the safest to approach, of any that has ever yet been known amongst diseases. Amateur Physicians from the Continent and from every part of the United Kingdom, eager and keen for Cholera, and more numerous than the patients themselves, beset and surrounded the sick in Sunderland with all the fearless, self-exposing zeal of the Missionary character. Yet no one could contrive, even in the foulest dens of that sea-port, to produce the disease in his own person, or to carry it in his saturated clothing to the healthier part of the town where he himself had his lodging."

"Dr. John MacIntosh stated that he and all his assistants, as well as the attendants in the Cholera Hospital, escaped, although constantly handling the patients, examining their discharges, &c."

"Where it has been proved again to them that in the open but crowded hospitals of Warsaw, under the most embarrassing circumstances of warfare and disease out of a hundred Medical men with their assistants and attendants frequenting the sick wards of Cholera, not one took the disease, that for the sake of proving its nature they even went so far as to clothe themselves with the vestments of the dying, to sleep in the beds of the recently dead, and to inoculate themselves in every way with the blood and fluids, of the worst cases, without in a single instance producing Cholera Morbus."

In the *Calcutta Review* I find the following extract from a Report of the Board of Health, furnished, I think, by Dr. Bedford, and included in an excellent article by him on Indian Epidemics.

The first case that occurred in the town of Leith in 1848, took place in the same house, and within a few feet of the very spot from whence the epidemic of 1832 commenced its course. On its re-appearance in the town of Pollock Shaws it snatched its first victim from the same room and the very bed in which it broke out in 1832.

Cholera. "This return to its former haunts has been observed in several other places and the experience abroad has been similar."

Do facts like these point to a spread by human intercourse, or to what I have here and elsewhere so much insisted upon, local contamination. To adduce more recent opinion, I find in the very last number of the *British and Foreign Review*, April 1856, which I have only had the opportunity of seeing after much of the foregoing was written, a paper by Mr. G. H. Greenhow, on the history and origin of Cholera, in which my own views are strongly and ably supported. This author appears to doubt the importation of the disease from other countries, and has offered proof from the writings of old authors that Cholera is not the new disease, many suppose. I had myself recorded a similar conviction eight years ago, but I have not questioned nor do I yet dispute the fact of a fresh and severe appearance of the disease in this country in 1817. Nor that since that time it gradually moved westward, *i. e.*, that is, as far as the appearance of the disease is concerned; once appearing its attacks have been renewed and one might venture to predict that it will gradually establish itself as an endemic of European climates.

But leaving the point of foreign introduction out of the question, this paper contains many facts and opinions which militate against the views of diffusion by human intercourse and contagion: I conclude my notice of it with the following quotation given from Dr. Barton, an American writer:

"New Orleans was in no condition to localize it. Whilst an epidemic state of the atmosphere exists over the whole country, the disease will only be developed where there exists also in more or less intensity the localizing conditions of filth, stagnant air, moisture, &c." It might perhaps have been added that the state of the atmosphere mentioned but not particularized, may cause the disease without the localizing condition, if existing to a high degree, or if it meets with a ready recipient.

It is in this way I account for sporadic cases, for I believe sporadic and epidemic Cholera to be identical. Of course, I do not mean bilious Cholera.

In the Library of Medicine, Vol. IV., Practical Medicine, page 107 and following pages, many striking examples, old and recent, will be found in proof that no amount of contact, nor handling of the sick, no examination of the dead and the discharges, even in very confined space, will cause the disease, and

on the point of communication by intercourse I extract from this work the following :—"During the summer of the same year 1832, we observed the epidemic at Plymouth where Cholera prevailed to a great extent, there also the parts of the town inhabited by the wealthier classes were *almost* exempt from the disease, and though continual intercourse with the country people was kept up, it did not spread to the rural villages adjacent. "Dr. Albers in his Report of Cholera at Moscow says—During the epidemic, it is certain that about 40,000 inhabitants quitted Moscow, of whom a large proportion never performed quarantine, notwithstanding this fact, no case is on record of Cholera having been transferred from Moscow to other places." Cholera.

I may add to this that if the disease had happened to appear in any place to which people from Moscow had fled, the fact would immediately have been seized upon as a proof of spread by intercourse, notwithstanding the truth known now and long since that the disease sets all quarantine at defiance. At this very time our Indian ships are accused of conveying the disease to Mauritius, albeit subjected to close quarantine; As the Editor of the *Englishman* wisely suggested not long ago—let the authorities get rid of their local abominations, and Cholera wont trouble them much* whencesoever it may come or whatever be its origin.†

Since writing the preceding pages, the work of Dr. C. Morehead of Bombay, has reached India, and it is with no small satisfaction I find that the opinions I have thus expressed are supported by one usually so candid and dispassionate in his judgment, and who has had so wide a field for observation, but I think Tables which are to be appended will show that in this part of India the disease is most prevalent in certain months, and that these months vary in different parts of the country, Dr. M. expresses a different opinion.

It is further with much gratification I conclude this part of my subject by extracting a part of a letter from Dr. H. A. Bruce, a valuable servant of the Government, whose zealous and humane exertions I had some opportunity of witnessing during part of the time he alludes to. This

* But it may appear sometimes in defiance of conservative measures as the sequel of this paper will show.

† More recently still there has been an example in the same Island of the mischiefs arising from erroneous views on the subject: a ship was subjected to quarantine, and death and privation followed although Cholera was actually raging in the Island at the time, and although there had been no cases of the disease on board of the ship for some time before.

Cholera. observer appears to me to draw the fair and logical distinction between actual sequences and coincidences and surely the ultimate conclusion of one who reasons, so fairly, after great opportunities merits much consideration.

C—— touches doubtingly on its contagious nature and perhaps he is right. For we no sooner mention the subject than we are abroad in a sea of contradiction.

No man can resist the evidence that has been collected *pro.* but I am decidedly *con.*

"It most decidedly is not a contagious disease in the com-
"mon acceptation of the word; the fact of the Artillery get-
"ting it last year after the 70th going to their end of the sta-
"tion, is just one of those facts that a contagonist would very
"ligitimately lay hold of, but do you suppose that there was
"not constant intercourse between those men before the
"latter were moved? I'll be bound to say that there were
"men of the 70th every day in the Artillery barracks and
"every night too before the latter Corps was moved, and *vice*
"*versa,* such at least has always been the case hitherto and we
"have no reason to suppose it was otherwise at the time we
"speak of, still there is no doubt of the fact and it will rest
"with you to give that fact its due weight, but the facts with-
"in my own experience of its *non*-contagious nature are too
"strong to admit of my opinion being changed, here are
"one or two of them—In 1848, I had Cholera in the Fu-
"sileers from May till September, during the whole of that
"time I may say the hospital was never free of some cases,
"and at times it was crowded with them as you know. Well,
"my rule was never to have the Cholera ward without one of
"the subordinate Medical establishment in it, they were re-
"lieved every four hours, and so were the ward coolies some-
"times; the whole establishment may have been said to live in
"the wards, the coolies for hours together never left the beds
"of the patients, the Medical Officers did nothing but admi-
"nister to their wants, and yet not ONE man, European, half
"caste, or Native ever showed the least symptom of Cholera.
"I took most particular care to have them mustered and
"looked at—but there was not in that year even a case of
"bowel-complaint among them. Now recollect that I had
"often not less than 100 men thus exposed and all this time
"their fathers, mothers and brothers were dying daily in the
"bazars. I used to give these men leave daily to go to bury
"or burn their relations, but not one of them even showed a
"symptom of the disease though they lived in the head-
"quarters of it. In 1849 the result was exactly the same.

" Not one man of the Hospital establishment was attacked· Cholera.
" I have seen cases I will allow that many people would have
" said proved contagion. I have seen a mother come into
" Hospital with a child, the child die, and the mother return
" from its funeral in confirmed Cholera. I have seen that
" same mother die within 24 hours after her child, and her
" husband who attended her and followed her to the grave
" within another 24 hours."

" I have seen a man return from his comrade's funeral after
" he had attended him in Hospital and seen him die of the
" disease; but there was always great mental depression and
" distress in those cases. Mental depression predisposes as
" much to the disease as any thing I know.* So confirmed
" was I of this fact that I latterly objected to let any soldier
" be attended by his comrade in Hospital. I always made them
" select his attendant from men who knew nothing and cared
" as little about him. It was a curious fact too, that the dis-
" ease did not seem to attack the attendant till the excitement
" was over. It was always after the death and funeral of the
" person they were anxious about. If I had my notes by me,
" I could give you several examples of this. This predispos-
" ing effect of mental depression struck me as long ago as
" April 1833. Cholera then broke out in the Jail Hospital at
" Bancoorah; and during the few visits I was able to pay
" with Dr. Cheek to the Hospital, I found that almost all those
" attacked and all the fatal cases, were among some Cole pri-
" soners who had been caught and brought away from their
" houses during the insurrection there, and who had been
" remarked as especially melancholy and disspirited during
" their confinement.

" I was, immediately after sent to Diamond Harbour,
" where an inundation had swept away whole villages, and
" where Cholera was raging. Lord Bentinck had caused a
" dole of rice to be served out gratis every day to the suffer-
" ers and, in fact, to all comers; and the result was that the
" poor, even from the vicinity of Calcutta, were collected
" there; but the deaths had all been among those who had
" lost their homes, or whose local interests were affected."

Some may consider that this question of contagion and diffusion by the intercourse of men is not one of much practical importance; but it appears to me that when we

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* In the present season both at Lahore and in Fort William there have been painful examples of the effect of fear, thus justifying the apprehension that the belief in contagion is gaining ground.

Cholera.

reflect upon the consequences which might follow to society if the views to which I oppose myself, became matter of popular belief, a different conclusion will be arrived at. Some very high authorities in the profession advocate the doctrine of contagion and diffusion by human intercoure, than which if true, there could be nothing more calculated to add to the horror and misery caused by a pestilence in itself sufficiently appalling without these things being true of it. Is it because these theoretical views however ingeniously reasoned are opposed to the experience of ordinary every-day observers that so little practical evil has yet resulted from their promulgation.

It may go to strengthen the arguments adduced above against one theory of the manner in which Cholera is diffused, that another and more recent belief which I proceed to notice has its advocates equally confident in the correctness of their views and also offering proofs to corroborate them.

It will be conjectured that I allude to the theory, that the disease spreads by means of the alvine discharges. I find that there are two ways in which this is supposed to happen—1st, by means of the discharges percolating into drinking-water, wells, pumps, &c.; and 2nd, by poison from the alvine discharges floating in the atmosphere, or attaching itself to bedding, clothing, &c. In the 2nd instance, I suppose it is usually believed the Cholera poison finds admission by the lungs.

In this country we have, as I think, ample proof by reason of particular social and conventional habits, that we may have epidemic Cholera in its most intense forms; without either of the above means of communication being operative. On a line of march, for instance troops encamp at places, the wells of which are often situated miles away from all human habitations, and where certainly no alvine discharges could have gained admission; yet at certain seasons, and in some localities, many cases will occur during the few hours the troops are halted there. They move on and the disease may continue, though they drink from a new, and we may conclude, an untainted source, and where the air must be equally free from the same kind of impurity, or it may cease, as it often does, on a change of locality.

If exposure to or contact with Cholera discharges was a source of danger; we might expect the attendants, and in an especial degree, the sweepers in the hospitals to be liable to attack; but this, as far as I know, has never been noticed in regard to the sweeper caste during the 40 years Cholera

has been known in India: yet upon this degraded race, and upon it alone, devolves the duty of removing Cholera discharges, not only in public hospitals but in private dwellings, and as regards bedding and clothing, they are washed in this country by one caste alone, who might therefore be expected to suffer markedly, but this has not been observed. Cholera.

During the late outbreak (a fearful one too) in H. M.'s Ship *Britannia* in the Black Sea, how can we suppose that the drinking-water or the alvine discharges acting in any other way, could have caused the disease. The drinking-water is kept in tanks, and our knowledge of the discipline and cleanliness observed in a Man-of-War, forbid the supposition that alvine discharges could be left to do mischief, if indeed they ever possess such a power.

I shall now adduce an instance to show how from preconceived theories we draw conclusions from circumstances and facts, that on closer investigation admit of more rational explanation. In 1853, H. M.'s 70th Regiment, then stationed at Cawnpore, experienced an awful outbreak of Cholera. It so happened that in the previous year a fresh disposition of the privies had taken place, and forgetful of or overlooking the fact that the disease had on many previous occasions attacked Regiments in the same barracks. some Medical Officers at the station, attracted by the plausibility of the new theory, immediately rushed to the conclusion that the wells being contaminated by the privies, must have caused the disease. It seems to me that the following remarks by Dr. Bruce quite disprove the views taken by others. "I have read C.'s letter and return it? He gives too much force to the supposed impregnation of the well-water last year at Cawnpore. The rear of the Barracks is known to be a honey-comb of those filthy holes filled with the soil from the privies; but the following circumstances must be considered before we admit the evil effects to the extent he does. First, the filth is never removed to them till it is solid. Water is never found there till far below their level, for they are shallow, now as they are situated on a particularly dry bank, and as percolation pre-supposes a considerable quantity of water present this water must come from the surface, but it so happens that the very seasons when the percolation ought to be the greatest, are those in which there is the least sickness, and certainly, so far as my experience goes the least Cholera, that is, when the rains are abundant."

Cholera.

The alvine discharges are supposed to cause Cholera either by the direct action of the poisonous ingredient upon the intestinal canal or by its mal-influence upon the blood. I have seen the detail of some very disgusting experiments by a Mr. Lindsay, executed with a view of causing Cholera in the lower animals by means of their being forced to swallow human Cholera excretions. I cannot now refer to them, but my impression at the time was, that there was not clear evidence of the disease produced being actually Cholera, but the consequence of starvation added to the injesta I have spoken of. But in a Lecture I have before me, written by a Mr. Pairman,* delivered at the Biggar Atheneum, in Scotland in March last I find it very dogmatically stated to be a now-ascertained fact that Cholera is caused by fungi or Cholera seeds, which multiplying themselves act upon the intestinal lining, finding their way there either by means of drinking water or through the medium of the atmosphere; in the latter case also acting locally and not by lung absorption. The generation of the Cholera fungi or seeds is said to depend upon certain atmospheric conditions, connected with damp, defective electricity, and absence of ozone. These views are perhaps in the right direction, but we require more proof before we can believe that the poison of Cholera has merely a local action, or that the multiplication of the fungi or seeds could be going on while the violent expulsive commotion is taking place at the same time. It is difficult further to understand that these fungi should be in a state of activity in water, otherwise pure, as well as in foul almost dry drains and ditches, and in the intestines. In an epidemic such as Dr. Bruce described to me from Cawnpore where the fatality was so fearful, we might surely expect that if poisoned water was the source of mischief, there would be some cases where the invasion of the disease had immediately followed the potation of what with the poor soldier is not a very favorite beverage. Dr. Bruce writes—"I believe I am within the mark in stating that 100 men† died before a single case was saved, I have never known any thing so deadly." It is difficult to believe that water so poisoned as to cause such havoc as this, can disclose nothing to the chemist, or the microscopist, yet I have not seen that anything very positive has been asserted on either head. Dr.

* See volume 17 of Ranking's Half-yearly Abstract.

† This was in H. M's. 70th Foot in 1853, at Cawnpore.

Snow compares the action of the poison which he supposes to exist in water to the "continuity of molecular changes by which combustion, putrefaction, fermentation and the various processes in organized beings are kept up."* Cholera.

Turning to the view that Cholera poison in the shape of alvine discharges causes the disease through the medium of the atmosphere, or by attaching itself to clothing, linen, &c., one would expect that invasions would show a gradual increase in the number of persons attacked: in other words that as there was more production of the poison so would the number of cases gradually increase; now this is not by any means what usually takes place. It is far more common to observe that the cases are more numerous and fatal at the onset of an epidemic and gradually diminish in frequency and intensity.

The almost entire absence of Cholera at high altitudes is another argument against the theory under notice, for we know that the disease may prevail in all ranges of temperature. The practical deductions which have been drawn from this theory of the disease, appear to me highly dangerous, and are certainly opposed to the great bulk of Medical opinion and experience. It is thought well to encourage the purging, because the poison is thus eliminated. It seems impossible that any one can believe this who has read an account of the very wonderful effect of saline injections into the veins. It is true that in rare instances death from Cholera may happen without much discharge, but it would appear undeniable, that in the generality of cases the symptoms in part depend upon the thickened state of the blood.†

From all that has been said, I shall now presume that I have shown that it is neither true of Cholera, that it is contagious or spreads by human intercourse, or that it is in any way communicable by means of the alvine discharges, and next proceed to consider what is most probable as to its real cause, for positively we are not yet in a position to speak.

The minute relations existing between the atmosphere and organization, are yet little understood. We know that oxygen is essential to animal life, but we are not able to explain why epidemics depending upon the same poison, differ so greatly in their prevalence and mortality in one season as compared with another. In addition to the specific

* A vague theory vaguely expressed.

† If we yield the point that no good arises from checking the purging in the early stage of Cholera there is nothing left, but despair, for after a time all medicine is useless.

Cholera. poison, there must be some positive or negative property in the atmosphere, which causes this difference: neither are we able to explain upon what it depends that vegetable life is so differently, and I might say so capriciously affected, in different seasons one year an abundant harvest, another an absolute blight and death of some forms of vegetation. In the same way it has occurred to me that Cholera may depend upon something which prevents the atmospheric air (the oxygen, perhaps, in particular) from performing its usual offices in the lungs and in all the organs. We know that less oxygen is absorbed, less carbonic acid eliminated, less heat produced. With the exception of the vomiting and purging, these negative facts constitute the disease for the non-secretion of bile and urine follows in the train of non-oxygenation.

It seems to me even possible to account for the vomiting and purging as a consequence of the impeded function in the lungs, and elsewhere. The quantity of fluid contained in the system, remains at the outset of the disease unchanged. The lungs are partially collapsed as well as the tissue of the liver and kidney, for it is a law that upon the performance of function does the activity of the circulation in an organ depend; so many floodgates being closed, an outlet is wanted and is found on the surface of the intestines. The exosmose may depend upon the effect of non-oxygenation on the nervous system, there are other signs of nervous depression.

It does not appear a very irrational conjecture that a peculiar electrical* condition of the air is necessary for securing the chemical changes which occur first in the lungs and subsequently in the various textures and organs of the body, and it does seem possible that if electricity has any thing to do with these processes, some derangement in the equilibrium of this subtle, obscure and little understood agent (as far at any rate as it is connected with vital actions,) may be the cause of Cholera.

The other alternative, as I have placed the question, is to suppose, that the cause of Cholera is a locally generated and locally acting poison, known merely by its effects, and most capricious in its appearance in its intensity and in its period of duration in places where it shews itself.

One theory is, that the disease consists in the non-per-

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* "Electricity performs many important offices, it is active in directing the operations connected with the vitality of both animal and vegetable organization."

formance of healthy functions and gradual death in consequence : the other that there is an ingested poison. Cholera.

Perhaps the presence of a certain amount of ozone is necessary to effect the changes on the blood and tissues. In my last paper I quoted Dr. Clemens, who spoke of ozone as a powerful vital stimulus.

The former of these theories appears to me best to explain the great differences in the intensity of different epidemics, and perhaps also it will best account for what appears to be an undoubted fact, namely, that the cause of Cholera has on all ordinary occasions no existence at a certain elevation, which may be stated at 6,000 feet, nay more that parties who have been exposed, and even had symptoms of the disease at lower levels, will, on ascending to the elevation I have named, show no more sign. I must again quote from Dr. Bruce, and now what I think will be "considered of very great interest. In connexion with this matter of change of air, you may give a little attention to the effect of altitude in checking the progress of the disease. In 1845, when it raged universally over the North-West Provinces, we had it very fatal in the Dhoon and some in my Battalion, the type was a very severe one and a large proportion died of those attacked both at Deyrah and Rajpore, the former is about 2,000 feet above the sea, and the latter perhaps 7 or 800 feet higher, but though there were 1,000 men going daily from Rajpore to Mussoorie, and back again, I could only trace one case of death at Mussoorie, that was in a cooly, who brought it up with him and died in the Bazar. Now, this to say ; the least of it is remarkable, the coolies died in large numbers at Rajpore, and some of those who were daily going back and forward, must have necessarily brought the seeds of the disease up the hill with them, but they did not seem to be able to germinate at the elevation of 6 and 7,000 feet. Not an inhabitant of Mussoorie or Landour was attacked, and not a man in the Depôt. Again, in 1852, the disease visited the Dhoon in its most deadly type, but it was unable to establish a footing at Mussoorie or Landour ; something of the same kind occurred at Subathoo in 1845. The 1st Fusiliers suffered dreadfully there, but I believe it was not known at Simlah or Kussowlie :*

* I find that Dr. Bruce makes a mistake in exempting Kussowlie in 1845. In September and October of that year H. M. 9th Regiment lost 38 men there. The general fact that an altitude of 6,000 to 7,000 feet is a safe guard must yet be held true of which Dr. Bruce says well "what an argument for having our European Troops in the Hills."

Cholera. Subathoo is 4,200, Kussowlie 6,500, and Simlah 7,000 feet."

"One question I have not answered, I am of opinion that Cholera neither requires damp nor organic decomposition to render it noxious, but it requires both to produce it in its most virulent form. Its existence is sometimes universal*, but its great fatality always local."

Colonel Mountain died of Cholera at Simlah, but I know of no record of epidemic Cholera there, or at equal elevation.

The Tables which are appended to this paper, show in the series of ten years from 1844 to 1854 the percentage of deaths in the Jails of Bengal and Behar to have been 1½ per cent; less than ½ per cent. in those of the North-West, and in the Jails of the Punjaub during the 7 years of our possession a mere fraction. Again, they show that while during this whole period there has been no Jail in Bengal, which has entirely escaped the disease: there are six Jails in the North-West which have not had a case of Cholera. Allyghur, Boolundshuhur, Bijnore, Paneeput, Simlah and Beawar. In the Punjaub the difference is much more striking. There are 15 Jails, which have never yet been visited.

These facts may be considered to show that in the ordinary course of things there is more Cholera in Bengal than in the North-West, but while I have been engaged with this paper, melancholy proof has arisen that there is exception to this rule, and perhaps to warrant the conclusion that where the disease is most endemic, its epidemic vesitations are least severe. In June and July, Agra as I have mentioned already, was awfully visited: Cholera subsequently appeared at Alyghur, Gwalior, Meerut, Futtyghur, &c., but its most fearful infliction has been at Lahore where, sad to tell, it has carried off more than 25 per cent. of the Artillery force; Medical Officers and subordinates: Ladies, Women and Children also falling victims. On this occasion, speaking of locality, it has been wide spread for the Fort of Lahore and the Cantonments of Annurkallee and Meean Meer, embracing an area of miles, have all come under the swoop of the pestilence. Those who assign outbreaks of Cholera to cognizable meteorological elements during its presence, such as temperature, the state of the air; with regard to moisture, weight, prevailing winds, &c., will not find much to assist them in the melancholy history of this year,

† Universal must be understood in a comparative sense.

but it bears out an inference, I ventured to draw many years ago, that uncommon or unusual conditions of the weather bring this and other diseases. At Agra the weather is described as having been cloudy, hot and sultry, without rain, but with no particularly high range of the Thermometer. At Meerut while the disease was raging, rain was falling heavily with, I think, high wind. At Lahore, the rains were falling heavier while the disease prevailed than they have done for years, and upon the whole there has not been so much rain in the North-West Provinces, especially in the Punjaub for many years past, and there has been a very unusual prevalenee of Easterly wind. The absence of violent thunder storms has been remarkable every where. At Ferozepore where the 70th Regiment have again been severely visited, I observe it stated in the *Delhi Gazette*, there have not been such heavy rains for eleven years, when H. M.'s 62nd there lost a great many men from Cholera. In Calcutta where the rains began in the end of May, and the weather continued rainy throughout June, July and August, with much less sunshine than in ordinary years, there was much prevalence of Fever with occasional cases of Cholera among the natives and in Ships lying in the river, but it has been since the weather cleared up and after some days of bright sunshine, that the disease broke out in the Queen's Regiment in Fort William. Part of the wing of the Regiment was moved to Chinsurah with the best effect. Cholera.

Those again who consider that filth, confined space, and bad ventilation, are always essential to even the most malignant outbreaks of Cholera, must get over the difficulty, if they can, that the Barracks at Meean Meer are clean, capacious and well ventilated, and that while in them death was so relentlessly seizing its victims, in the neighbouring city so notorious for filth, narrow streets and dense population, there was little if any of the disease. It afterwards appeared, but that does not affect the argument.*

The remaining remarks I have to make upon the Cholera epidemics of this country, shall have reference to the means of prevention. It is painful to observe that overlooking the truth how little treatment avails after seizure, the great resource of change of locality is not resorted to, sometimes

* At Umballah in 1845 the European Troops that escaped best were living in stables. In Fort William this year the men in the New Model Barrack were attacked although it had not half its complement of occupants on both these occasions, and in all places the Sepoys have had trifling mortality though their lines can scarcely be called cleaner or better ventilated than European Barracks.

Cholera.

on account of the consequent discomfort to the troops, and at other times as I have known because sufficient attendance cannot be secured. This does indeed seem very grave infatuation. We know that there is a poison around almost as deadly as the bite of a snake and far less amenable to treatment: experience teaches that by change of place we may escape from it.

What reason then is there in giving consideration to mere discomfort, or to what so seldom affects the issue as attendance and Medical treatment. This is indeed to attach importance in the wrong direction, to weigh in the balance mere trifles in comparison with the weighty consideration that valuable lives are being daily and hourly sacrificed, which might and would probably be saved. You ask me, says Dr. Bruce, in his letter to me so often quoted, what I think of removal. "Its effects on my Regiment were "magical on both occasions that I tried it. In 1848, we had "had the disease for four months raging in the Regiment, "and it raged equally in the town and bazars. We moved "out, I think, on the 1st of September, three or four incipient "cases were taken from the Barracks, some showed themselves on the road and the rest within 48 hours of our going "out, but we had no case after that. We were out, I think, "for 10 days, and were driven in by rain, and though we "found the disease equally bad in the town, on our return, "not a single case occurred afterwards in the Regiment. In "1849, we moved out in July, the disease was prevalent in "the villages close to us, Colkadeo for one and Puttealah, I "think was the name of another, on the day we moved out, "and for two days after it, men came in with Cholera symptoms, several with cold tongue, purging, &c., but not one "fatal case, they were checked by one dose, and after the "second day there was not a symptom of the disease among "the men. We were out a month on that occasion, and returned, as before, to find the disease still in the town; but "strange to say, not one case occurred in the Regiment from "the day we went into Camp."

To these instances many might be added, but it is true that the move has not always the desired effect. Under such circumstances I ask, emphatically, what is the wisest course to pursue; to fly to a greater distance yet from the destroyer, or to go back into his jaws. If we go back to a locality where the disease is prevalent, we shall not often be so fortunate as Dr. Bruce was, on the two occasions he mentions. The movement should be away from tainted

Cholera.

places, consideration of discomforts should not deter us. The courage which can brave the presence for months of a deadly epidemic is more admirable than that which carries the soldier up the breach and to the mouth of cannon; but it seems to me that when it is possible he should be spared the trial. In extreme cases it seems that on the doctrine of chances we might, by separating Regiments into Wings or Companies, and moving them in different directions gain something.

Without pretending to think that the views I have suggested on the cause of Cholera have been proved, or are very original, I think there is sufficient to suggest the trial of such means of purification as might lead to the destruction of the poison or improve the diseased condition of the atmosphere. It has not been found to any extent successful to cause the inhalation of oxygen by persons affected with Cholera, but that does not prove that the free generation of the gas and its diffusion in the air of affected places might not prove a means of prevention. We know that in many instances the tainted spaces are limited in extent, in one particular Barrack for instance, and in such cases to experiment would not be difficult. It appears to be a fact almost established that the absence of ozone and the presence of disease go together. The presence of this substance is easily detected in the atmosphere, and the ozonometer is so simple an instrument that it might easily be placed in each Hospital and Barrack. We have no formula for procuring ozone. In fact this substance has never been separately produced, but we know that electric sparks taken through perfectly dry oxygen, bring part of the oxygen into the state of ozone.*

This coupled with one of the few things which appears to be ascertained of the mysterious agent producing Cholera, namely, that crashing thunder storms cause its disappearance, would seem suggestive. Cholera has appeared a second time in Fort William in this month. On this last occasion, on the occurrence of a violent thunder storm it ceased at once. I would observe lastly, that since though not yet tangible, we know that the cause of Cholera is a positive or negative quality of the atmosphere existing only at times and often

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* The properties by which the allotropic oxygen is characterized are such as indicate a greatly increased powers for combination.

It is evident that allotropic oxygen (ozone) is necessary to the preservation of animal life in perfection. Pereira's Materia Medica.

Cholera.

confined to limited spaces, we may hope some day to discover something that will prove its antidote, and in the mean time general principles may teach us that we should try by perflation, ventilation, conbustion and ablution to destroy, expel or diffuse it, I venture to suggest heavy salvos of Artillery close to Barracks and may even propose the discharge of individual pieces of ordnance into the interior of affected Barracks.

It is not from perflation such as the ordinary force of the wind produces, that I would expect much benefit, but from artificial means.—Such as would excite violent disturbance and commotion in the atmosphere.

Since the above was written, I see a letter from Dr. Knight, of Byjnore, in the *Delhi Gazette*, advocating the slow combustion of damp gunpowder. He mentions that after a severe general action in Poland, Cholera disappeared. The fact being undoubted, however we may explain it, that persons well fed, well housed and in short enjoying the comforts of civilized life, are liable to Cholera, in a less degree than those differently situated, teaches us what general principles of prevention ought to be followed. Temperate but generous living, the avoidance of exposure and fatigue, personal cleanliness, changing the dress after being heated, attention to ventilation, and when possible to the choice of locality, perhaps the use of tonics.

There are times as this season has unhappily illustrated when no class of the community escapes. Whether this is owing to a wider diffusion of the cause or a higher intensity of its action, we are unable positively to say, I lean to the latter opinion because on all occasions of the disease showing itself among troops and prisoners, officers and medical men constantly frequent localities where the cause is at work, but they escape on ordinary occasions.

While engaged with this paper, I wrote to Dr. Rumley, in charge of the Horse Artillery at Lahore. He has kindly replied to the queries I put to him. "No proof to his mind of contagion.—The wells distant from the privies, and the disease stopped while the same water was being used, Space ample, Ventilation if any thing, too free, movement of disease from East to West. First appeared after days of heavy rain and South-East wind. Our removal did some good." The next move was back into the disease.

Before I proceed with this paper, I wish to notice a statement made by Dr. Morehead, at page 329, vol. 1st of his work, lately published on the diseases of India, having

reference to an opinion expressed by me in the first paper of this series. He says—"The opinion entertained by Dr. Mackinnon that the natives of India receive small-pox in a milder form than those of other countries is not supported by Hospital Returns or clinical experience. Cholera.

The subject being one of interest and importance. I cannot feel satisfied to have it disposed of on the mere *ipse dixit* of however high an authority. I gave what I considered very strong figured proof from Hospital Returns in support of my opinion, I showed that during a period of six years from an average strength of European Soldiers of 18,795, the percentage of deaths to admissions by small-pox was 28·38, while in the native army averaging in the same period 1,12,952 men, the ratio of deaths to admissions by eruptive diseases which could only include a mere fraction of varicella was only 7·79 per cent. The same returns showed that although all European soldiers are supposed to be protected by vaccination, their deaths to strenth in the six years amounted to ·37 per cent., while in the native army recruited from districts, where neither vaccination or inoculation are practiced, nor vaccination after enlistment, the mortality by small-pox is only ·27 to strength in the same series of years.

But it was not on these grounds alone that I based my opinion, I was acquainted with several instances where European children took severe small-pox from matter taken from the arms of native children who appeared only to have vaccine and I instanced one most interesting occurrence (see pages 9 and 10 of my 1st paper, No. 3 of this Journal) where after a successful production of disease from a cow it continued in a long succession of cases in natives to appear in so mild a form that it was considered to be cow-pox, yet in the very first three European children punctured with this virus, the result was small-pox with one death.

If we consider the Hospital Returns and Clinical experience upon which Dr. Morehead opposes my opinions, it seems to me they can hardly be held sufficient in themselves, even if I had not adduced a much larger array of figures on the other side of the question. My views applied chiefly to the Hindoos, and agricultural population, who form the great mass of the people. Let us hear Dr. Morehead :—"The admissions from small-pox were with four exceptions confined to Mussulmans and Portuguese, many of the former were sailors and probably strangers in Bombay." Now,

Malignant Fevers.

as far as we can draw conclusions from the numerical data furnished by Dr. Morehead himself, 261 cases : it would appear that at Bombay the disease prevails most among Mussulmen and Portuguese and these by no means feeders on vegetables.

MALIGNANT FEVERS (PALI PLAGUE AND MAHAMURREE.)

Except for the reason that I had intended to make this series of papers a sort of Record of the epidemics which prevail or have recently done so, in these Provinces, I might now bring this paper to a conclusion, for I can have little to say that is novel or interesting on the subject of Pali Plague and Mahumurree and even less on the Fevers lately described from the Banks of the Indus by Drs. Lyell, Farquhar and Maxwell.

Dr. Morehead in his late work has disposed of the Pali Plague and Mahamurree as an Adynamic-Fever infectious in character, observed at Pali and elsewhere.

It may be necessary to inform those who are new to the study of Indian disease, that the Pali disease was pronounced by a number of Medical Officers who had the opportunity of seeing it to be identical with the plague of Egypt, and they gave excellent reasons for arriving at the conclusion, I may notice in particular the reports of Drs. Irvine and Keir. Out of seven Medical Officers who saw the disease, there was only one who did not think it the plague, and certainly its symptoms and its fatality impress me with the same belief. It is 20 years since the occurrence in Marwar, I believe that local circumstances continue what they were, that there is as much filth, crowding, and imperfect ventilation, and yet we have not since heard of this fatal Fever; a seeming proof of foreign introduction such as used to convey the plague on occasions to London, Marseilles, &c., when there was local pabulum for its spread. In Kumaon again the disease appears to exist as an Endemic as in Egypt; there too the recent reporters Drs. Pearson and Francis believe the Mahamurree identical with plague, and that is my own conviction.

But if the point be conceded that the Pali disease was purely of local origin and if not plague, it was certainly a most deadly pestilence,* it is most difficult to explain that

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* From 80,000 to 1,00,000 was computed to be the mortality.

Malignant Fevers.

while all local cognizable conditions remain the same, there has been no return of the disease for the long period of 20 years. In Kumaon on the other hand, the disease between the description of which and that of Pali, I can trace no difference, appears to have been pretty constantly present since 1826, but I am happy to hear from Dr. Pearson, that the sanitary measures have been eminently successful. "Mahamurree having practically ceased to exist," as no doubt would happen in Egypt itself, if the same measures were pursued, and perhaps they have been, since it appears that for many years past, the plague has not shown itself in the Levant or even in Egypt. It would seem from what I have stated of the Pali epidemic that something was there required besides crowding, filth, and bad ventilation, to cause the invasion of such a fatal disease, and the perusal of the reports written at the time will inform us, that the state of the weather was very unusual.

FEVERS WITH YELLOWNESS OF SKIN.

But a more striking instance of a general epidemic influence acting beyond, and indeed independent of mere local contaminatons is to be found in an earlier period of the history of our epidemics.

In 1816, the usual rains were exceedingly scanty all over the country, but in the last fortnight of September, rain fell so heavily as to inundate the whole country.

In the language of Dr. Jameson, who wrote of the epidemic I am alluding to, "it was not to be expected that so great a deviation from the common course of nature should prove wholesome to the human constitution."

"Disease exhibited itself in the shape of a bilious Remittent of a violent inflammatory type, accompanied like the Yellow Fever of the West Indies, with suffusion of the skin." "The width of its range precluded the possibility of its being referred to any cause purely local, and that it was not kept up by infection alone was proved by the indiscriminate nature of its attacks. It seized equally Europeans and Natives, and as readily entered the open and spacious house of the Officers and Civilians as the crowded barrack of the Soldiers or the low filthy hovel of the Natives." I may remark here, that this fact of general diffusion may be held to go for or against contagion, according to the general views of the reader upon that subject.

Malignant Fevers.

It was certainly not true of the next recorded epidemic in all other respects so similar, but occurring at a different season of the year, that it equally attacked all classes and ranks, for the Yellow Fever, which prevailed in the upper Dooab, Rohilcund and Delhi Districts in 1836,* was strictly confined to prisoners in Jails, and poorer classes in native villages, while the disease of 1816, which spread from Dinapore to Delhi, but was most fatal at Cawnpore, made no such distinction ; it destroyed, besides thousands of natives,† four hundred men in two European Corps, and a number of Officers; a degree of mortality of which Dr. Jameson was then able to say that it exceeded any thing then on record in the Medical Annals of Bengal, and as regards Fever this is true still.

It is an interesting subject of enquiry whether such fatal Fevers as these‡ and we may include the epidemics of the same kind described from the Yusufzaee country near the Indus, can depend upon an identical poison with our ordinary epidemic Fevers often prevailing to a very great extent but with a very small rate of mortality, and this even at times in the same locality. Unlike these last, the fatal types more usually, however, prevail where there are favoring local conditions, but to this rule I have given one prominent exception.§ If it be true, as so confidently stated by Drs. Farquhar and Lyell that these Fevers spread by contagion, this of itself is to my mind indicative of a different origin than pure marsh poison, and the symptoms and the treatment seem to lead to the same conclusion. Death occurs often as early as the third day and usually on the 7th, there is not so clear and defined a remission as in miasmatic Fevers, the yellow tinge of skin is almost universal, the tongue is coated with a dark fur, there are hæmorrhages, and diarrhœa appears to be far more prevalent than in pure marsh Fever. Quinine often specific in the one and useful, however severe, appears to do no good in the other.

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* This epidemic prevailed in the hot weather and rains.

† In four months August, September, October and November.

‡ From the middle of January to the end of June 8,500 deaths out of a population of about 55,000.

§ I mean that with respect to this Cawnpore epidemic we have not the same evidence of impurity, crowding and bad ventilation as was true of the epidemics in the Jails and villages of Rohilkund, and of the Derajat, on the right Bank of Indus, but it is a fact that at this time there were four European Regiments in the Cawnpore Cantonments, while at present there is only accommodation for two, but it has been proved here and in oher countries that pure miasma Fever may prevail in the absence of all cognizable impurity and not only this, but in the West Indies the deadly Yellow Fever.

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Dr. Maxwell on the other hand appears not to consider the Fever he saw at Dera Ghazee Khan contagious, and he speaks of its being an Intermittent when the disease first showed itself. The whole question of whether even the worst malarious Fevers ever become contagious, and if so how it happens, is of great difficulty: I lean to the belief that it is usually places, not men, that give out the poison, as regards Fomites do they convey effluvia from the sick or are the local impurities carried by them.

If we accept the conclusions, and they appear undeniable from the illustrations I have adduced in my last paper from this and other countries, that marsh miasm, and the Fever it occasions are things apart from vegetable smells and these or animal impurities, we must probably come to the belief that these Malignant Fevers associated as they are with filth, overcrowding and bad ventilation, are not purely miasmatic if miasmatic at all, and that therefore even if we had not the evidence of those who have observed them, of their being contagious, we might have concluded they would become so especially in the higher latitudes, and where the monsoons scarcely prevail. But the contagious character of these Fevers is a point which I must myself consider as not yet decided. It may be that having miasm as their cause their severity depends upon the vitiated condition of the atmosphere. The arguments and the cases given by Drs. Farquhar and Lyell to prove contagion are all to be explained by the facts, that in all affected villages local sources of disease existed, and that the individuals whose cases are given had visited these localities. The Hindoos living in the same villages are admitted to have escaped; their persons and houses were cleaner. Further, if we consider the extreme prevalence of Fever in our Indian Jails, and how little there is in their interior, or in the country immediately surrounding them, to be called marshy ground or to generate malaria strictly so called, we may perhaps think that the ordinary Fevers of our Jails depend upon animal impurity, the consequence of crowding, bad ventilation, poor diet and mental depression acting in combination with atmospheric vicissitudes, but that these causes are not concentrated enough on ordinary occasions to render them contagious. But it seems to me possible, nay probable, from what we see here, that marsh poison may act in combination with animal and vegetable filth, and so produce these Malignant Fevers of which I am treating. Certain it is that they differ in

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many essential characters from the Fevers caused by pure miasma. They have not either an identity with the typhus of cold countries, and in fact assimilate more to the plague than to any other disease, for I cannot think that they are identical with real yellow Fever.

The yellowness of skin often shows itself, I believe, in true plague, and is a very constant appearance in these Fevers. Swelling in the glands is described as an occasional symptom in the Rohilkund Fevers of 1836, and also in the more recent epidemic of the Eusufzaee country.* Hœmorrhage prevailed in both, and their mortality gave that proof of a pestilence.

Dr. Lyell compares this Fever to the relapsive Fevers of Edinburgh, as described by Dr. Rose Cormac. Dr. Jenner, who describes the relapsive Fever of Europe, tells us it is by no means a dangerous disease, nor can I trace in his description any close resemblance to this fatal disease, although they certainly have the character in common that the sick are subject to relapses, a character pointing to the suspicion of marsh poison having something to do in the matter. The yellowness of skin I find mentioned as a symptom of relapsive Fever occasionally occurring.

Whether the Yellow Fever of the West Indies is the same disease with ordinary Remittent Fever, is a point not yet decided, but there can be no doubt that a Fever of that country which has as prominent features, yellowness of skin, and the black vomit, confines its ravages to the newly arrived European—the negros and the creole inhabitants escaping; so says Dr. W. Fergusson under the head of Yellow Fever in his admirable notes, but if he be correct in his statement that this is Yellow Fever and the only Yellow Fever, how shall we account for the outbreaks of what is called Yellow Fever in new Orleans and other cities of America and also in Spain. It appears to me that though the disease may show some difference of symptoms,† in those lately arriving from Europe it is not likely that there are two kinds of Yellow Fever known in the Western Continent, although we have the difficulty that Europeans arriving in no part of India suffer from a like disease. If our Yellow Fevers be identical

* Dr. Maxwell shows that this Fever prevailed in the whole country called the Dejerat and confines itself to the Right Bank of the Indus.

† These differences would appear to be the violence and fatality of the gastric symptoms the copiousness of the black vomit, and the truly wonderful absence of head symptoms up to the approach of death, see pages 146 and 147 of Dr. Furgusson's notes published in 1846.

with any Yellow Fever of the Western Continent, which I do not believe, it must be with one which I suppose to be endemic in new Orleans and other places, and there occasionally affects epidemically also the permanent inhabitants. I confess the difficulty I have felt in deciding in my own mind the real character of these Indian Yellow Fevers. Even if the point of their contagious character had been settled, most readers would not think that the question had been then disposed of. To my mind it would prove that human effluvia or exhalations were in operation, for although I believe that low marshy situations will influence the severity of contagious diseases. I do not believe that a real marsh Fever is ever contagious, and by contagion I do not mean contact, but the conveyance of infection on the person or by Fomites. Malignant Fevers.

LAND SCURVY.

If time had admitted of it there is one other epidemic of this country of which I should have written at some length, although not coming within the category of a pestilence. As far as my knowledge goes in no other country has it been observed that land scurvy shows itself in an epidemic form. But it has done so here, and particularly in Rajpootanah, and as I think also in Scinde. It was a knowledge of this which induced me when a Member of a Committee directed by Government to report upon the probable causes of scurvy among the troops which came from England in the end of 1853, to give it as my opinion that the appearance of the disease on board of so many ships, many of which vessels had on previous voyages brought troops in perfect health to Bengal, that the prevalence of scurvy depended upon an epidemic constitution of the atmosphere in that particular season. The presence of scurvy on board of ships is usually assigned to the continued use of salt provisions; there was evidence before us that the voyages had not been of inordinate length, and that the men had been freely supplied with fresh hermetically sealed meat and with potatoes prepared in the same manner. In fact the salt meat theory as entirely true, must be abandoned; for the Rajpootanah epidemic was among Regiments of Native Infantry, where no animal food is consumed, and I am inclined to believe that epidemic scurvy may be independent of every kind of diet, for these sepoys in Rajpootanah used the same

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grain with the neighbouring villagers, who had no scurvy among them. In Scinde and even at Lahore a great prevalence of scurvy has been seen with no changes in diet to account for it, and this is true also of the hills as pointed out by Dr. Grant in his paper on hill Diarrhœa. The disease is also endemic in the Jails of the Saugor and Nurbuddah Territories. The whole question is very mysterious, for while in the hills and on the voyage round the Cape in the year I have spoken of, unusual damp might be held to influence the presence of the disease, in Rajpootanah, on the other hand when the epidemic prevailed in 1835, the season had been very unusually dry.*

The more I consider the question, the more do I believe that there is something beyond the mere quantity of nitrogen and oxygen usually detected in the atmosphere, besides its temperature, amount of vapour, weight, &c., to study and to elicit discovery from, before we shall arrive at the true explanation of the absence and prevalence of epidemics. Let those who think differently, tell me what has been done yet by means of chemistry, and our present metereological instruments. "But it is now admitted, says a late writer in the *Bombay Times*, that mere matters of pressure, temperature, and humidity, with which we have hitherto almost exclusively concerned ourselves, form a very small proportion indeed of the elements of climate either as affecting the weather or the health of living creatures."

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* And strange to say at Rajpootanah the disease was confined to the sepoys.

No. I.—Bengal.

Table shewing the deaths by Cholera in the Bengal Jails for ten years, and the number of deaths in each month.

Years.	Strength.	No. of deaths in January.	No. of deaths in February.	No. of deaths in March.	No. of deaths in April.	No. of deaths in May.	No. of deaths in June.	No. of deaths in July.	No. of deaths in August.	No. of deaths in September.	No. of deaths in October.	No. of deaths in November.	No. of deaths in December.	Total.
1844, ...	24894	8	12	48	84	56	134	36	12	5	5	9	3	412
1845, ...	24929	4	21	54	110	58	12	22	30	34	33	42	24	444
1846, ...	24771	2	4	12	15	13	14	16	6	4	15	45	38	174
1847, ...	23916	9	6	23	33	42	73	54	16	9	13	14	5	297
1848, ...	22678	1	6	5	39	50	31	11	12	7	9	9	18	198
1849, ...	22895	9	1	3	28	24	124	69	20	13	25	12	20	348
1850, ...	20884	41	4	43	57	26	9	3	19	2	3	19	14	240
1851, ...	21810	3	2	19	103	45	30	63	7	1	6	10	17	306
1852, ...	21895	10	44	65	147	65	31	4	10	9	31	37	47	500
1853, ...	21495	23	15	38	25	96	104	167	37	9	7	14	15	550
Total, ...	230167	110	115	310	641	475	562	445	169	93	147	211	201	3469

Average strength, ... 23016
Annual Average number of deaths, ... 346
Annual Ratio per Cent. of deaths to strength, ... 1·50

No. II.—North-West.

Table shewing the deaths by Cholera in the North-West Jails for ten years, and the number of deaths in each month.

Years.	Strength.	No. of deaths in January.	No. of deaths in February.	No. of deaths in March.	No. of deaths in April.	No. of deaths in May.	No. of deaths in June.	No. of deaths in July.	No. of deaths in August.	No. of deaths in September.	No. of deaths in October.	No. of deaths in November.	No. of deaths in December.	Total.
1844, ...	23764	0	1	2	1	4	1	4	2	5	2	4	1	27
1845, ...	23292	1	1	3	1	4	14	19	60	33	31	24	0	191
1846, ...	23226	0	1	1	3	7	12	6	1	24	22	24	0	101
1847, ...	24201	0	1	1	4	7	3	7	11	11	2	5	2	54
1848, ...	24195	1	1	2	3	3	3	19	1	5	11	1	2	52
1849, ...	26522	1	1	1	4	15	44	94	36	10	2	4	2	214
1850, ...	27636	1	0	2	11	5	15	18	17	12	7	2	2	92
1851, ...	26908	3	1	12	2	6	13	9	78	4	3	3	1	135
1852, ...	27095	0	0	2	2	2	3	23	19	2	1	1	0	55
1853, ...	27102	2	1	0	2	5	13	37	60	15	2	6	1	144
Total, ...	253941	9	8	26	33	58	121	236	285	121	83	74	11	1065

Average strength, ... 25394
Annual Average number of deaths, ... 106
Annual Ratio per Cent. of deaths to strength, ... 0·41

No. III.—Punjaub.

Table shewing the number of deaths by Cholera in the Punjaub Jails in eight years, and the number of deaths in each month.

Years.	Strength.	No. of deaths in January.	No. of deaths in February.	No. of deaths in March.	No. of deaths in April.	No. of deaths in May.	No. of deaths in June.	No. of deaths in July.	No. of deaths in August.	No. of deaths in September.	No. of deaths in October.	No. of deaths in November.	No. of deaths in December.	Total.
1846, ...	896	0	0	0	0	0	0	0	0	0	0	0	0	0
1847, ...	1069	0	0	0	0	0	0	0	0	0	0	0	0	0
1848, ...	758	0	0	0	0	0	0	0	0	0	0	0	0	0
1849, ...	2527	0	0	0	0	0	1	0	0	1	0	0	0	2
1850, ...	7028	0	0	0	0	0	1	0	0	3	0	0	1	5
1851, ...	6888	0	0	0	0	0	0	0	0	0	0	0	0	0
1852, ...	7279	0	1	0	0	0	0	1	0	1	0	0	0	3
1853, ...	8417	0	0	0	0	0	0	0	0	1	0	0	0	1
Total, ...	34802	0	1	0	0	0	2	1	0	6	0	0	1	11

Average strength, ... 4350
Annual Average number of deaths, ... $1\frac{3}{8}$
Annual Ratio per Cent. of deaths to strength, ... 0·2

Dr. J. W. Burt
88 George Street
Edinburgh