

Remarks on the report furnished by Asst. Surgeon E. Hare of his treatment of fever and dysentery during the time he was attached to the general hospital, Calcutta, in 1849-50.

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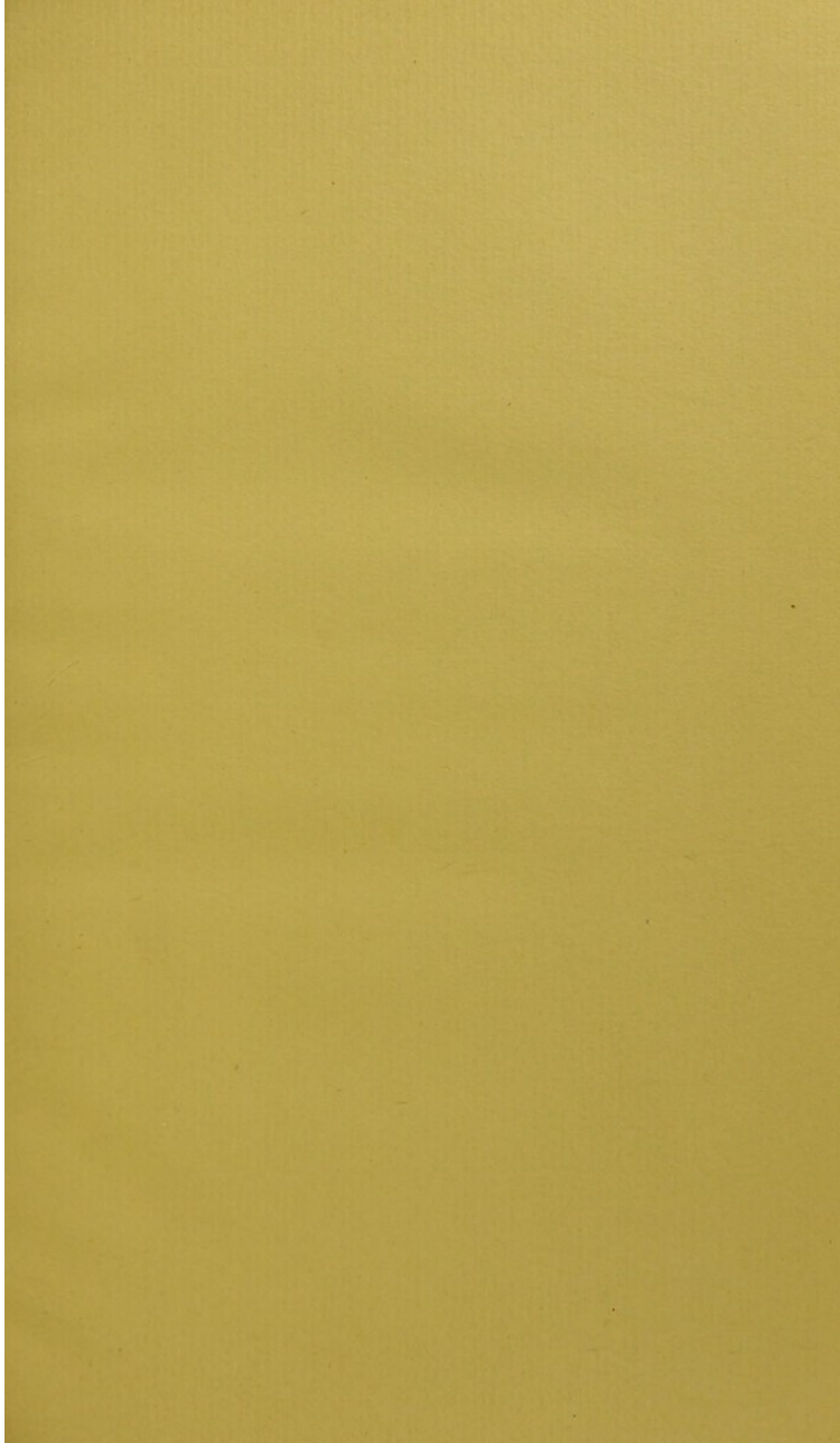
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REMARKS
ON
THE REPORT

FURNISHED BY
ASST. SURGEON E. HARE,
OF HIS TREATMENT OF
FEVER AND DYSENTERY
DURING THE TIME HE WAS ATTACHED TO THE
GENERAL HOSPITAL, CALCUTTA,

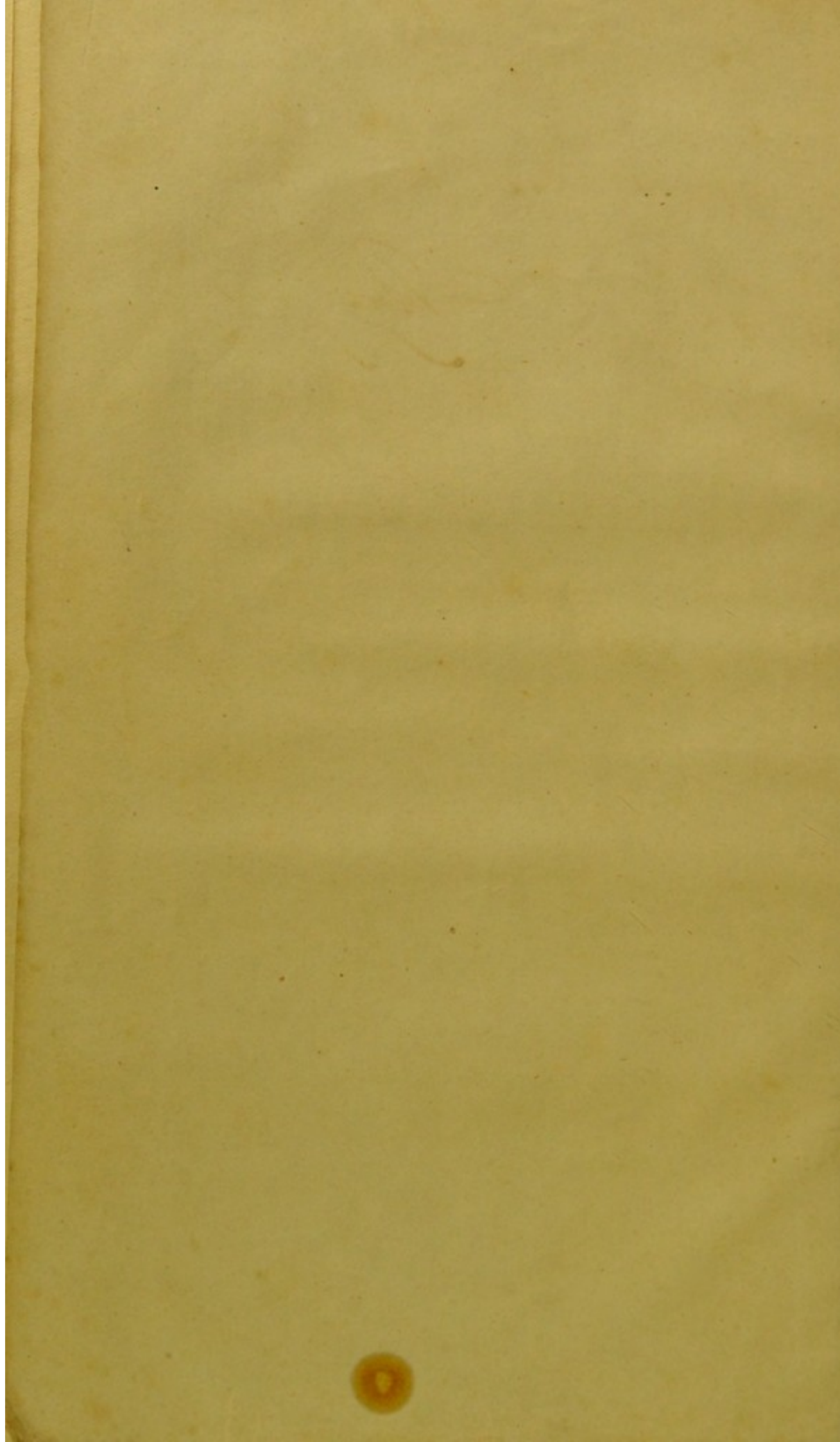
IN
1849-50.

SUBMITTED BY THE MEDICAL BOARD, BY ORDER OF GOVERNMENT.

FORT WILLIAM,
31st MARCH 1851.

CALCUTTA:
F. CARBERY, MILITARY ORPHAN PRESS.
1851.





For the use

of the 1st Cav. Regt. Fusiliers

Recd
8 May 1832 }

C. Remy
S. S.



Chas. J. Smith

R E M A R K S

ON

T H E R E P O R T

FURNISHED BY

ASST. SURGEON E. HARE,

OF HIS TREATMENT OF

F E V E R A N D D Y S E N T E R Y

DURING THE TIME HE WAS ATTACHED TO THE
GENERAL HOSPITAL, CALCUTTA,

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REMARKS

THE REPORT

ASST SURGEON & BARR

IN THE

FEVER AND DYSENTERY

URING THE WINTER OF 1891-92
GENERAL HOSPITAL, CALCUTTA

1893-94

THE REPORT OF THE
ASST SURGEON & BARR
IN THE
FEVER AND DYSENTERY
URING THE WINTER OF 1891-92
GENERAL HOSPITAL, CALCUTTA
1893-94



[No. 1287.]

FROM THE MEDICAL BOARD,

TO THE MOST NOBLE THE

MARQUIS OF DALHOUSIE, K. T.,

Governor General of India.

FORT WILLIAM, MEDICAL BOARD OFFICE,

31st March, 1851.

MY LORD,

WE have now the honor, after considerable delay, which, though unavoidable, we very much lament, to submit our final Report on the subject of the experiment, or rather investigation, on which Assistant Surgeon E. Hare was engaged at the General Hospital here, during eleven months of the past year. Your Lordship's design, in sanctioning the withdrawal of Mr. Hare from his Regimental duties, and his being placed in independent charge of a ward at the General Hospital in Calcutta, was, that the methods of treatment of tropical fever and dysentery, that had for some years been warmly advocated by him, should be subjected to a kind of test, that would leave little room for future doubt or controversy as to their efficacy, and we think it will be admitted that few localities could have been chosen, better calculated for such a purpose, both as regards the variety of types and stages of

these two diseases presented for treatment at that Institution, and the easy means it might be expected to afford of ascertaining the comparative efficacy of methods of treatment, differing so essentially as those recommended and practised by Mr. Hare, and those pursued in other wards of the same Hospital and elsewhere; differing essentially, we repeat, as will, we think, be most satisfactorily demonstrated before this Report is brought to a close, even granting all the weight that is due to all that has been advanced to the contrary by some of the professional Gentlemen and others, who have, during the last three or four years, taken a share in the discussion of these questions.

2. With the claims of Mr. Hare to originality, in the views entertained by him of the origin, causes, exact pathology and method of treatment of the two classes of diseases under investigation, we have nothing to do in this place. It is clearly our duty to confine ourselves to the examination of those portions only of the questions that bear on the methods of treatment actually pursued by Mr. Hare, and the degree of success that has attended his practice; and with these alone it is our intention to deal.

3. In the Report submitted by us in April last, we stated that so far the result of Mr. Hare's proceedings gave earnest of ultimate success in establishing the superiority of the modes of treatment practised by him over those pursued in the Hospitals generally of this Presidency, and in our judgment he has fully redeemed this implied pledge. But the way in which he has done this, we think it will be by far the fairest plan to allow him to describe in his own words; and by appending Tables prepared, with great and laudable industry, by himself from the records of the Presidency General Hospital and Returns of European Troops in Garrison in Fort William, for a series of 20 years. Other Tables have been also prepared in our office from data afforded by its records, to prove, by the least fallible of all methods, that of figures, extracted from a wide field of observation, the absolute

reality of the superiority, positive as well as comparative, which he claims for the methods of treatment pursued by him over those pursued by others. Among the Tables, prepared from Returns in our office, is included one which shows the results of treatment of fever, dysentery and diarrhœa throughout all the European corps, detachments and stations of the Army in this Presidency for the same eleven months embraced by Dr. Hare's proceedings in the General Hospital. We consider this wide extension of the means of comparison advisable, to prevent the risk of suspicion falling on the calculations founded on the practice in Calcutta, and lest any person should feel disposed to cavil at the narrowness and insufficiency of the field of observation, or deem such limited comparison unfair or invidious.

4. In the statement furnished by Mr. Hare in January 1850, of his proceedings in the General Hospital, and in the ward of Her Majesty's 70th Regiment, also placed under his charge, he says,—
 “ It is my wish to direct attention particularly to my cases of fever,
 “ and a method I propose for ascertaining, and settling in a more
 “ certain way, our classification of fevers, from their causes, viz., to
 “ show what fevers have malaria as their origin, and what depend
 “ upon other causes. Hitherto this distinction has been less ne-
 “ cessary, because all Fevers, without distinct remissions, were
 “ alike treated by general antiphlogistic, or mercurial remedies at
 “ first, and tonics, quinine, &c., it mattered little what, when
 “ the inflammatory symptoms were thought to be subdued, but
 “ in my system, where quinine is given at the very first, and where
 “ the success of the treatment depends upon so giving it, and the
 “ more plentifully, the more intense the symptoms, it manifestly is
 “ a point of extreme importance to detect in a very early stage those
 “ fevers which depend on a malarious poison for their cause, from
 “ others of different origin. For large doses of quinine might be
 “ fatal if given in the early and inflammatory stages of common
 “ continued fever, the continued fever preceding small-pox, &c., &c.,
 “ all very much alike in their first symptoms. Now it is acknow-

" ledged that fevers from the poison of malaria may be continued,
 " that is, without any perceptible remission in their symptoms, and
 " of the three forms of malarious fever, continued, remittent and
 " intermittent, it is usually considered that these three correspond
 " to the degrees of intensity of the poison imbibed, the continued
 " being the effects of the poison in its most concentrated form,
 " and this seems proved by numerous facts on record, particularly
 " in the West Indies, where soldiers of the same Regiment on
 " duty in the plains, in the slope, midway, and at the top of the
 " hill, have been all attacked accordingly with the continued,
 " remittent and intermittent form of fever. The cases, therefore,
 " in which the fever is continued in its symptoms, are the most
 " intensely affected with the poison, and require the largest and
 " most frequent doses of quinine, and in the earliest stages.
 " A mistake, therefore, in our diagnosis of the cause of the fever
 " might prove very injurious, if these full doses of quinine were
 " given, and the fever proved afterwards to be simple inflamma-
 " tory, or any other kind, not malarious. It is certain too, that
 " the most severe and fatal cases of remittent fever have a conti-
 " nued form for the first few days. This fact was well known to
 " the writers on fever, Lind and others, before Dr. James Johnson,
 " and accordingly it is to those cases that bark was most early and
 " assiduously given. I have diligently sought in their writings,
 " and in others of later date, for the diagnosis required, but can
 " find none. I have now, however, the Book of Nature open before
 " me, and it will be a great object of my experiment to ascertain,
 " if I can, what symptoms can be depended on in the early stages
 " of fever, to determine whether it be malarious and to be treated
 " by quinine, or some other form of continued fever requiring
 " antiphlogistic or mercurial remedies."

5. In his statement for the month of February, Dr. Hare, after
 enumerating the cases treated, remarks,—“ I have the satisfaction
 “ also of reporting that my apparatus for giving injections is now
 “ very much improved, and appears almost perfect. It is simple,

“powerful and durable, no air can possibly be pumped in into
 “the bowels, and the force of the stream of water can be made
 “most gentle, or as powerful as a horse syringe. Its cost, more-
 “over, is not more than that in common use * * * *”

“My Reports on dysentery, however, must, I fear, during this
 “year, not be so interesting as they may hereafter be made,
 “knowing, as I do, that the continuance of the experiment
 “depends on my being able to show, during this first year, more
 “or less an earnest of my treatment. I am unwilling to try any
 “thing more than what I have already ascertained to be safe and
 “beneficial. I have previously learned, for instance, the great
 “effect of injections containing nitrate of silver, and a few other
 “medicines in certain doses and times of using them, and until I
 “have shown the general success of this very common routine of
 “practice, I feel afraid to try other remedies, some of which
 “would probably fail and increase for the time, my mortality
 “* * * *”

“It is manifestly impossible, but that some remedies may be
 “found which will act in a most beneficial manner upon it (the
 “mucous membrane of the colon) either soothing as in the acute
 “stage, viz., opium, hyosciamus, conium, lead, &c., largely
 “diluted; or in the chronic stage of ulceration, nitrate of silver,
 “sulphates of copper and zinc, catechu, or if there be any great
 “fœtor of stools, which must irritate as they pass over the diseased
 “surface of the intestine, lime water, chloride of lime and more
 “especially, I have reason to think, creosote. No one can for a
 “moment deny that these must have great effect on the mortality
 “of this disease. Who has not witnessed patients in an agony of
 “pain and distress from dysentery, suddenly made comfortable
 “and sent to sleep by injecting a few ounces of starch and
 “laudanum into the rectum? and must it not have a still more
 “soothing effect, if applied, by my method, properly diluted, to
 “the whole diseased surface of the colon, from the cæcum

“downwards? No one would think of treating an ulcer or inflam-
 “mation of the mouth, or external parts, without these local
 “applications, and would think it very absurd if any one doubted
 “their efficacy, especially if the secretions lying in contact with
 “these ulcers, were known to be highly acrid or putrid. I have
 “already proved my system is possible in 65 cases treated by me,
 “some of them with disease, if not ulceration of the rectum, and
 “yet in all these, large injections to reach the cæcum were used
 “twice a day and with no bad result; but on the contrary, quick
 “recovery. It is, therefore, possible to apply to the whole surface,
 “which is the locality of dysentery, any medicinal substance we
 “please. It must, likewise, be considered certain that some of these
 “substances must have great effect in curing the disease, if the
 “right medicine be applied, and in proper doses. Having proved
 “this general possibility, therefore, my next attempt would be to
 “find out, by trial, what medicines had most effect and in what
 “doses.”

“The action of medicines on the body in general when thus
 “applied, to so large and vital an organ as the colon, must be very
 “different to their effects when applied to any other part of the
 “body. I am, therefore, like a man who knows the general
 “analogy and properties of medicines externally (applied?) but
 “quite ignorant of the doses and actions when taken into the
 “stomach. If this man were called to treat patients, he must
 “lose many of them before he had gained the necessary expe-
 “rience. I am now in precisely the same position. I know the
 “effects of nitrate of silver in inflammations and ulcers of the eye,
 “mouth, &c., but can I say, before I try, whether the same
 “strength can be applied, without danger, in quantity sufficient to
 “act on the large and vital surface of the colon, which so rapidly
 “absorbs into the blood every thing contained in it (that is
 “absorbable?) I happen to know from previous experiment some-
 “thing of the effects of nitrate of silver, and find it most valuable,
 “but I am sure I do not know its full value, because I am afraid,

“ under present circumstances, to try any experiments in its dose *
 “ * * * * I will, during this year, show the possibility of
 “ the system in general. I will also show what effect on the
 “ mortality soothing measures have, namely, keeping the bowels
 “ in perfect rest, and abstinence from purgatives and calomel.
 “ I can likewise show in chronic dysentery some of the effects of
 “ nitrate of silver, and perhaps alum, chalk, and such like harmless
 “ palliatives.

* * * * “ It is unfortunate that the chronic cases, where
 “ my treatment is from ignorance most imperfect, are the most
 “ frequent here; but having shown the general practicability of
 “ my system, and perhaps some decrease in mortality, a time may
 “ be granted me perhaps to try these important experiments,
 “ during which my results may not be judged of by my mortality
 “ immediately resulting, but by a final experiment, the result of
 “ this experience.

* * * * “ Having now laid down my plan for both fever
 “ and dysentery and the course I propose pursuing, I will follow
 “ it, but quietly, in future, merely reporting my cases and my
 “ details of observations in time.”

6. At the time these fair and candid expositions were submitted by Dr. Hare, of the existing amount of his knowledge and experience, and the ends he proposed to gain by a free and unfettered exercise of his own judgment and ingenuity, and by a judicious employment of the time and opportunities afforded him, he had not yet witnessed any of the more formidable and desperate examples of sloughing and hæmorrhagic dysentery, which are admitted to the General Hospital in Calcutta, merely, it would very often seem, to swell the ratio of mortality; for a large proportion of the pauper patients and townsmen are found to have reached a stage at which all remedial measures must be powerless, or applied with but feeble hope of ultimate success. When such

cases did present themselves to him, they unavoidably produced a modification of the opinions which were advanced, on a contemplation of the simple form of the disease (dysentery) only, and led to an alteration in his plans of treatment. The progress of the events and the various reasonings founded on them, which brought about this modification, or we should perhaps more properly say, enlargement of his opinions and more extended application of his mode of treating this, to him in fact, new type of the disease, will be found fully and clearly described in the final Report submitted by him in November last, a lengthened Extract from which we beg leave to subjoin.

“ Having, in my ‘ Statements,’ already laid before the Board
 “ the numerical results of my experiments in both Hospitals,
 “ I proceed to report on what I consider that I have already
 “ accomplished, and how much still remains to be done, with
 “ a practical abstract of the details of my treatment, both for fever
 “ and dysentery. But to explain more clearly what I think I
 “ have done for the present, and hope for the future it will be
 “ best to review, what has been the theory and practice of the
 “ past, by which we shall ascertain more distinctly what our
 “ present position is.”

“ It is perhaps one of the most remarkable facts in the History
 “ of Medicine, that quinine has for so long a time been considered
 “ a dangerous medicine in malarious fever, whilst any symptoms of,
 “ so called, inflammation of internal organs continued, and more
 “ particularly if the head was affected. Through the many years
 “ of Dr. Corbyn’s Journal, by almost every writer on fever, this
 “ caution is most strongly inculcated, and in more than one
 “ paper, a comparison is made between modern practice, and the
 “ old system by bark, and wonder expressed that such doctrines,
 “ so clearly opposed to all rational practice, could have ever been
 “ received. The bark-treatment appeared to them in the same
 “ light of absurdity, as friction for an inflamed leg, or brandy in

“acute phrenitis. That the same opinion is still the prevailing one is proved, by the observations, to me, of a practitioner now in Calcutta, who asked me if I would *venture* to give quinine where fever affected the head, and he evidently doubted me when I told him, that it quickly stopped all head symptoms.”

“The same doctrine is also advocated in Dr. K. Mackinnon’s late book, 1848. In reviewing my pamphlet, he cautions his readers against my practice, being directly, he says, contradicted by his own, and asserts that my doctrines are, at first sight, in their essential nature absurd, dangerous, and opposed to all experience, and correct knowledge of the inflammatory nature of fever: and the conclusion he draws is that if I had experience as he had, (mine being, he thinks, only book theories,) I never would have advanced such absurd opinions.”

“Dr. Raleigh also, in his work on Dysentery, equally ridicules the bark treatment and putrid notions of former days. Yet now there is before the world the startling fact, proved this year by my experiment, that these gentlemen have been following their leader, Dr. Johnson, asserting all this as fact, without once having tried it. In Dr. Johnson’s day, when quinine was unknown, the bark treatment had great inconveniences, and it was not a specific for fever. Its clumsy form, like so much saw-dust, rendered it impossible to give enough of it, to make it a specific, and therefore, in the worst cases, where most was required, it failed, and the necessity there thus was of cramming the patient to vomiting, with the bitter wood by spoonfuls, made the substitution of the energetic and tasteless medicine, calomel, highly desirable. This was, therefore, one great and good reason why the substitution was hailed by every one as an improvement; for they had not, as we have, the most elegant of all medicines, quinine, and which is a perfect specific. And it is most certain, that had they possessed it, Dr. Johnson never

“ would have attempted to change it for calomel, and if he had
 “ tried, no one would have followed him.

“ Another very sufficient reason too, there was for the change.
 “ The Physicians of former days, never dirtied their fingers with
 “ dissection, and contented themselves with accounting for all
 “ diseases by the Humoral Pathology; and to make the poor
 “ theory (true enough in its way), account thus for everything,
 “ was rather difficult, and they were obliged to push it to great
 “ extremes, and rather manifest absurdities. However, this con-
 “ tinued till Dr. Baillie's time, and his work on Morbid
 “ Anatomy commenced a new era; which, while it brought most
 “ invaluable truth, added at the same time many errors. His
 “ book set every Physician to post mortem examinations, and
 “ when the errors of the Humoral Pathology were discovered,
 “ away they started in the opposite extreme, of attempting
 “ to account for all diseases by local lesions, and wherever
 “ blood was found accumulated in an organ, it was attributed
 “ to inflammation of that viscus, and all the general symptoms
 “ were said to be caused by the reaction on the system of
 “ the local disease and irritation, and forthwith came forth
 “ new theories of fever, attributing all its symptoms entirely to
 “ local inflammation; one (Dr. Clutterbuck) of the nervous
 “ systems, spinal column and brain, and another (Broussais) to
 “ subacute inflammation of the stomach and intestines. Now, if
 “ the date of Dr. Johnson's first arrival in Calcutta, and that of the
 “ first Edition of Baillie's Morbid Anatomy be referred to, it will
 “ be seen that Baillie's book had been published just two years
 “ previously, that is, while Dr. Johnson was a Student, and he
 “ came out no doubt thoroughly imbued with its doctrines, and in
 “ his first case on arrival in Calcutta (the case which induced
 “ him to change his practice, for he tells us that he never tried
 “ bark again,) he found, page 37—the liver so gorged with
 “ blood, that it actually fell to pieces on handling it. Indeed it
 “ appears as if the greater number of the vessels had been broken

“ down, and almost the whole of the interior structure converted
 “ into a mass of extravasation. In the brain, marks of turgescence,
 “ particularly in the venous system, were found. Now, Dr. Baillie
 “ and every one else had taught him to believe, that the above
 “ appearances were the effects of inflammation, and this was the
 “ other great and really overwhelming reason, for adopting the
 “ calomel and bleeding system. For bark in those days was
 “ thought to be a corroborant, a strengthener of the system
 “ against a putrid tendency, and it was given with wine, wine
 “ and bark being almost synonymous. Now all this can be
 “ proved as matter of History, and can we wonder at the change
 “ which took place. Would not I and every one of us, seeing what
 “ we had every reason to believe the effects of strong inflammation
 “ before us, infinitely prefer calomel and bleeding, which we knew
 “ to be specifics for inflammation, to bark and port wine, which
 “ every one believed directly increased it? Such was the general
 “ reasoning, and very sufficient reasoning it was, for every one’s own
 “ post mortem examinations fully confirmed Dr. Johnson’s, and
 “ therefore the whole world, knowing from previous experience the
 “ defects of the bark system, at once decided on adopting
 “ Dr. Johnson’s, so confirmed as it was, by both dissection and
 “ practice; for there is no denying that salivation does stop fever.
 “ Thus went on the grand experiment, universally adopted, till
 “ in the course of years, men began to observe the effects of
 “ it, and bad effects they were. Old Indians returning to Eng-
 “ land, suffering from the effects of repeated salivation, like
 “ old syphilitic cases, a burden to themselves and society. These
 “ effects were soon noticed, and the young hands, with much
 “ opposition from their seniors, began to try, if they could not
 “ do without* all this salivating, and sure enough they have
 “ done it, fortunately in fever without harm, for chemistry had
 “ given them in the meanwhile quinine, and they used that
 “ as a substitute, so far as their inflammatory theories would

* Twining tried to substitute a bleeding system, but this was soon dropped.

"allow them. But in dysentery where they used no substi-
 "tute, the result has been, that the mortality is double, and
 "thus have we gone on dreaming and dozing, and priding
 "ourselves on our superiority to the old race of calomel Doctors,
 "as we call them, without ever taking the trouble to see what
 "the statistical results really are, of the do-nothing-system we
 "have adopted. But of late years great progress has been made.
 "Doctor Marshall Hall, besides his magnificent discoveries in the
 "nervous system, the light, as they are of modern days, has done
 "greater things for actual practice, than even these. He has
 "proved that the congestion of blood in internal organs, which
 "Dr. Johnson witnessed, is not inflammation, and so little allied
 "to it that a patient almost bled to death, is the very subject in
 "which it is sure to occur, in its most marked form, and he has
 "proved by infallible* experiment, that the cure for this, is not
 "further depletion, which always before him was adopted, but
 "stimulants and opium, with the best nourishment the stomach
 "will bear. Now it is somewhat disgraceful, I must confess,
 "that this, perhaps the most important discovery of the last
 "century, because it is so practicable, nevertheless seems quite
 "neglected in this country, and to my great astonishment I
 "found, on joining my present Regiment, the treatment recorded
 "there of my predecessor, who to relieve spasms of the stomach
 "to which he was liable, had opened both veins with his own
 "hand. He fainted and fell down in the night on the floor, and
 "was found nearly dead in the morning. He rallied, however,
 "and when re-action took place, and congestions of various organs,
 "he was treated with repeated doses of calomel and colocynth and
 "leeches, and after the patient's death, this gentleman recorded
 "the case, as if he were well satisfied with the treatment, with his
 "own hand, in the Hospital Case Book."

* "All this has since been fully confirmed by every writer, Orfila, Christison, &c.,
 "on Toxicology and Medical Jurisprudence. And now, except the effusion of coagu-
 "lable lymph be evident, no extent of venous vascularity or congestion, is allowed in
 "our Courts of Law, as proof of inflammation. Blushing is a familiar illustration of
 "temporary congestion. The same flushing of the face and irregular action of the
 "circulation, heats and chills, occur after severe depletion, only in greater degree."

“ Dr. Twining’s cases, both those published and in the diaries
 “ here, clearly show that he paid no attention to this truth, for
 “ in many of his patients, his system of repeated bleedings, pro-
 “ duced these congestions of anæmia, or, in other words, unequal
 “ circulation, and when after severe bleeding, re-action took place,
 “ with flushed face and congestions of the head or liver, instead of
 “ correcting his mistake of too copious depletion and giving opium
 “ and stimulants, he invariably mistakes the symptoms for
 “ returning inflammation, and orders more leeches and more
 “ colocynth, and kills his patient. The same has occurred in my
 “ own small practice, and, therefore, cannot be very unfrequent.
 “ I recollect bleeding a young woman in Tirhoot for acute
 “ rheumatism, more freely perhaps than I ought, for she fainted
 “ longer than I liked, and on my return from a distance I heard,
 “ that she had, during my absence, symptoms of sudden excite-
 “ ment and pain in the head, and another practitioner had been
 “ sent for; and to my horror I found her head covered with
 “ leeches; and, remonstrating, was answered, that he had found
 “ her with symptoms of congestion of the brain, and that surely
 “ leeches were good practice. However, the poor woman never
 “ rallied and died in a few hours.”

“ But whether or no such ignorance is common (I hope it is
 “ not) this at least is certain, that the same bugbear of inflamma-
 “ tion, and mistaking congestion for it, has been the cause why
 “ quinine has not been given earlier in fever. This evidently
 “ prevented Dr. Martin from perceiving the full truth, for he
 “ says, page 123”—‘ Early and copious bleeding is very generally
 “ ‘necessary in the severer forms of Bengal remittent fever, and
 “ ‘then full doses of calomel with sudorifics, short of producing
 “ ‘salivation. If the disease does not now yield, leaving, but im-
 “ ‘perfect remissions, then there is imminent danger, and inflam-
 “ ‘mation or acute congestion in some important organ, may be
 “ ‘more than suspected, for in this, in addition to topical bleeding,
 “ ‘mercury, in small repeated doses, with antimonials, must be

“ ‘given, so as mildly to affect the system. It is the *only known**
 “ ‘means of saving the patient, by anticipating the destruction of
 “ ‘some organ essential to life, it (mercury) here becomes, in the
 “ ‘apt words of Dr. Jackson, a remedy of necessity.’

“ How astonished Dr. Martin will be to learn, that all these
 “ are not inflammations, and that salivation is not a remedy of
 “ necessity or copious bleeding, and that calomel and antimony
 “ are not the only known means, but that if quinine be given
 “ freely, these imaginary inflammations vanish, like the vapour
 “ from a marsh, before the rising sun. But now all this false
 “ theory is removed, and we may use the greatest blessing which
 “ Providence has given us freely, without fear of harm, but unmix-
 “ ed benefit. It might be supposed as probable, and I myself
 “ thought so, that the severe distension of the liver, &c., by con-
 “ gestion, might produce injury to its texture, and like all other
 “ injuries, inflammatory action after it. But yet on careful
 “ examination of more than 700 *post mortem* examinations of
 “ death by fever, in the diaries of this Hospital since 1820,
 “ described by so many different observers, not one case of
 “ inflammation have I detected. They are all the same as Dr.
 “ Johnson’s case above quoted, viz., extreme congestion, without a
 “ trace of the only mark which can be relied on of inflammatory
 “ action, viz., the effusion of plastic lymph. Their cases of real
 “ inflammation, viz., peritonitis, effects of the effusion of pus into
 “ the cavity of the abdomen, pleurisy, pneumonia, &c., &c., are des-
 “ cribed plainly enough. In these, lymph is always mentioned as
 “ having been found in quantity, thus proving that they knew
 “ what lymph was well enough when they saw it. But all
 “ their descriptions of the effects of fever, are mere venous vascu-
 “ larity, turgescence of the brain, and engorgement of the liver,
 “ making it look, and break down under the fingers, like a con-

* “ And, gentlemen, read this, in the work of the most eminent of our living prac-
 “ titioners, and which is universally received as our Text Book in India, and quietly
 “ say that my system is nothing new, but known to every one long ago.”

“gested spleen (see Dr. Twining’s case above.) These cases
 “ought to be published, for this important fact which they
 “demonstrate is final.”

“I have thus followed briefly the History of these changes of
 “treatment to the present time, and the result of the whole is
 “this, that we are wiser than our fathers, only because their
 “labors have given us more experience, and chemistry new
 “medicines. That they had full and sufficient reason for every
 “change which they made, and had every authority for so doing
 “from the state of knowledge and opinion of the age in which
 “they lived and in medicine especially, as I have found true in
 “my own case. The difficulty is, not to check new theories and
 “fanciful changes, but to introduce what is really true and good.
 “The greatest drawback to the improvement of our profession no
 “doubt is this, that our best and most intellectual men are those
 “selected for large private practice, and the moment a man
 “becomes involved in the vortex of professional duty, he is fortu-
 “nate when he has time even for food and rest, and it becomes
 “quite impossible that he can find leisure for study and reflec-
 “tion. He is compelled, therefore, to follow the course marked
 “out for him by common practice, except some very evident
 “improvement forces itself upon his attention. There is no
 “deficiency in our profession of zeal, talent or correct judgment,
 “but there is of leisure; for our best men are most occupied in
 “this uninstrusive and irksome practice.”

“I have now proved historically that every change in practice
 “which they have adopted, has been done advisedly and with good
 “reason. It is remarkable, however, (and it shows how much
 “truth has always been mixed up with all our medical theories,
 “the errors being in fact truth exaggerated,) it is remarkable
 “how we are oscillating back to the old Humoral Pathology and
 “theories of putridity in fever. Those who have studied Dr.
 “Prout’s writings will know how closely we have returned to the

" old theories of disease being caused by foreign and noxious matters
 " circulating in the blood. The putrid nature of typhus fever also,
 " from a diseased state of the blood, and its right treatment by
 " wine and early support, are now universally admitted; and from
 " my own experience of the low malarious fever and hemorrhagic
 " dysentery found in Bengal, I certainly myself would think
 " them rightly called putrid, that is, as they also understood the
 " term caused by a morbid and diseased state of the blood shown
 " by its dark color and want of power to coagulate; and I am
 " most decidedly of opinion that wine and early support, such as
 " soup, jelly, &c., are far from being prejudicial, but on the
 " contrary often quite necessary for their recovery. The roast
 " fowls and mutton chops, which one practitioner ridicules, are
 " evidently an exaggeration; for no patient in such a state could
 " swallow such a diet if he tried to do it by his Doctor's prescrip-
 " tion. Nature renders it impossible; he would vomit it; and,
 " moreover, though I have searched the same records, I can find
 " no trace of such a practice."

" My treatment, therefore, is almost a complete return to that
 " of former days, both in theory and practice, except that chemis-
 " try has given me the all-important gift of quinine, and I have a
 " clearer insight derived from experience since their time of the
 " true cause* of this putridity, and that it is not to be got rid of
 " by strengthening and antiseptic medicines, for they looked upon
 " the Cinchona bark to be analogous to oak bark in preventing
 " putridity by introducing a sort of tanning effect on the system.
 " My doctrine is certainly not that, but still my practice is much
 " the same as theirs."

" The result of the investigation which I have this year so
 " closely made (I refer to my mortality tables) is most consoling.

* " Malaria was not recognised as a source of fever in those days. It was sup-
 " posed to arise from some fermentation from heat or otherwise of the vital fluid.
 " Our knowledge of malaria has since been obtained from the Italian writers and
 " their Pontine marshes, but most especially in England from McCulloch."

" For it proves that the men who have hitherto guided our
 " practice in tropical diseases, and those who have been their
 " disciples, and asserted with them the same leading facts, have
 " been neither fools nor dishonest, and it proves most distinctly
 " that their treatment *had* a great effect on the mortality, and
 " the present generation is the most unfortunate of any for seeing
 " the after-injurious effects of salivation, they have given up that,
 " and unfortunately adopted no other to replace it, and a general
 " scepticism has crept in among us that *all* treatment is much
 " alike, and that we had better not be too severe (*i. e.* decided) in
 " our practice, but watch and leave things more to Nature; in
 " fact, treat symptoms only, not adopt a fixed line of practice, and
 " the result I have proved (see my Tables) is a double mortality."

" We have thus run through the History of the treatment of
 " tropical diseases, and the result is that I have thus far no claim
 " in my system for originality. I am simply a zealous labourer,
 " pulling down and building up with the materials of others, and
 " the plan, the basis, is the old bark treatment, so changed, however,
 " by our modern discoveries as to be scarcely recognised as the
 " same. I do, however, lay claim, if not to discovery, certainly to
 " advance in knowledge; but even for this I am indebted to an
 " observation by that deep thinker Dr. Holland, who, doubtless,
 " however, did not see at the time he wrote it, the full practical
 " extent to which his principle (axiom I might call it) may be
 " carried. I will quote it, for it is the key to all my reasoning and
 " practice, (page 150 Medical Notes and Reflections) ' and as bark,
 " ' by curing, associates together many intermittent affections,
 " ' under the presumption of a common cause, so may colchicum
 " ' furnish similar inference regarding affections seemingly remote
 " ' in situation and symptoms. This mode of inference, hitherto
 " ' very limited, may hereafter be extended much further. It is one
 " ' of singular value as closely connecting the treatment with the
 " ' theory of disease, and rendering them mutually corrective of
 " ' each other. It at once enlarges and simplifies our views in

“ ‘ every part of Pathology ; each fact well ascertained is fruitful
 “ ‘ of results far beyond those which directly appear to the view.’ ”

“ Again, page 348, he enumerates these as proofs of a disease
 “ having a common origin with ague, viz., malaria.”

“ 1st.—By their intermissions.”

“ 2nd.—By their succession to, or alternation with, intermitting
 “ fever.”

“ 3rd.—By the influence of the same specific remedy in remov-
 “ ing, or relieving such disorders.”

“ He says, the application of these proofs to particular cases
 “ might be largely made.”

“ Again, page 354. The influence of certain medicines, and par-
 “ ticularly of bark, in curing even the most anomalous varieties of
 “ these intermittent disorders, is a fact of great interest. Like the
 “ use of mercury in obscure syphilitic affections, or colchicum in the
 “ most irregular forms of gout, it enables us to denote and class
 “ together symptoms apparently the most remote in kind, but
 “ which presumably could not thus be relieved unless depending
 “ on some common cause. We have, therefore, in the specific
 “ nature of the remedy, a sort of practical test of the character of
 “ the disease, often of great importance to the consistency and
 “ success of our treatment, and related, through the principle of the
 “ enquiry, to some of the most interesting questions in all Patho-
 “ logy. This it is, as was noticed when speaking elsewhere of
 “ colchicum, which gives peculiar value to all that illustrates the
 “ action of these remedies. They are the interpreters of facts far
 “ beyond their momentary effect, and of connections between
 “ morbid states, which are in noway so definitely made known to
 “ us. Now mark his words ; ‘ may hereafter be extended much

“ ‘further.’ Interpreters of the connections of diseases which
 “ are in noway so definitely made known to us. The whole of
 “ this passage is undoubtedly true, and one of those inspira-
 “ tions of thought, which mean more than the writer probably
 “ anticipated.”

“ Dr. Holland writes this in his treatise on colchicum. He
 “ introduces bark merely as an illustration of the use which he
 “ had made of colchicum, as a test for ascertaining by its effects
 “ the cause and connection of gouty diseases. Dr. Holland prac-
 “ tises among the rich and luxurious. He suspected a gouty taint
 “ of the blood to be a frequent cause of many headaches, dyspep-
 “ sia, &c. which he was called upon to cure. He starts with the
 “ principle that colchicum is a specific for gout, by destroying, in
 “ some unknown way, the poison of gout circulating in the blood,
 “ and he reasons; if I cure these headaches, &c., invariably with
 “ colchicum, I may safely conclude that they are all gouty, and he
 “ uses the colchicum as a test, a chemical test, of what are gouty
 “ diseases and what are not. The success of this method, in his
 “ hands, is well known, for numerous diseases of the joints, eye,
 “ heart, rheumatic, &c., before not suspected even to be from a
 “ gouty origin, are now proved and universally admitted to be so
 “ (see Brodie, Lawrence, Mackenzie, &c.) There has, likewise,
 “ been, as Dr. Holland says above, by curing by bark an associat-
 “ ing together many intermittent affections, under the presump-
 “ tion of a common cause (*i. e.* malaria.)”

“ Now, what Dr. Holland has done for gout and others for some
 “ intermittent affections, I propose and have done this year for all
 “ the principal malarious diseases, not only for the chronic, (the
 “ intermittent) but also for the acute, (the continued and remit-
 “ tent.) In this I am original, and hold my own ground, and a
 “ magnificent field of discovery is laid open by it. If it is
 “ supposed that I came from such a distance to this Hospital

“merely to try a new drug for fever and warm water for dysentery, I scorn the idea. I came here to new classify tropical diseases, to ascertain and fix their causes, and show, as Dr. Holland says, how the treatment and theory of disease may be rendered by his principle mutually corrective of each other, viz., the cause of the disease ascertained from the effects of the remedy, and the right remedy ascertained from the cause. I came to open new and wide paths of discovery.”

“Dr. Holland lives in a society universally tainted with gout. I live in one universally affected with malaria, and what he has done for gout, God willing, I will do for malaria in all its forms, both acute and chronic. And in this purpose, I have during this year made great advances. There is this difference, however, between us, and it is much to my advantage. Colchicum purges and is a diuretic, and *may* cure many diseases—therefore, repletion, inflammatory, &c. by its general antiphlogistic action. But with quinine it is not so, and therefore it is by far the more certain test. Again Dr. Holland had but one test, I have three!! viz., salivation, quinine and arsenic, and the latter I know from experience to be in many cases a still more powerful antidote even than quinine, and *it also* produces no evacuation, for it ought never to be given in such doses. There can be no doubt, therefore, of the truth of my principle, and the ample means I have for accomplishing my full design, and let it be well considered how great that design is. It is a classification of all malarious diseases under their true causes, and these diseases are the scourges of mankind. It gives us the certainty of being able to treat them by direct antidotes, instead of our common antiphlogistic system. It will give us all the varieties of symptoms caused by the malarious poison, with such minute exactness as to excite a strong hope of ascertaining what that poison is, certainly of at once detecting the error, if a wrong one be proposed to us.”

“ This then is the path which I have walked in. Let me now describe how far, during this year, I think myself to have advanced in it.”

“ When I arrived in Calcutta it is known to the Board, through my first Report, how anxious I was to ascertain what varieties of fever were met with in Calcutta. I felt that the success of my experiment in fever depended on this knowledge, for I could not but anticipate great mortality from my large doses of quinine, if there were met with here many varieties of fever not malarious. This accident did occur in some cases of small-pox and measles ; which were sent to my ward as fever in their early stages. It may be conceived, therefore, how carefully I examined every author I could meet with. These authors told me *nothing*. None of them even attempt to classify their varieties of fever by their causes. All that they give is, a few fevers named after some prominent variety in their symptoms only ; such as intermittent, remittent, continued, congestive, ardent, &c. No author attempts to prove their causes, or if he do, it is in a very obscure way. Twining, for instance, in a note, attributes all fevers to the irritation from inflammation and congestion of internal organs. Johnson ascribes it to sudden changes of temperature and congestions of the liver. Martin is still more cautious when he ventures to speak of causes, but from the passage above quoted from him, when he says imperfect remissions immediately make him suspect abdominal inflammation, it is evident that in remittent fever, at least, he thinks that the severity of the symptoms is to be attributed to the local affection, and not to a larger dose of the poison (which last, however, he does not hint at.) Though intermittent and some kinds of remittent, called jungle fever, are perhaps pretty generally believed to be malarious, yet there seems to be no distinct acknowledgment that *all* remittent fevers are so, and for the other forms, ardent, continued, and congestive, which any practical man knows to be real and true varieties. Yet for these, frequent as they are, no

“ cause whatever is given, except stray notices that they are occasioned by changes of temperature or congestion of the liver.”

“ Now I claim as my own that I have clearly ascertained this point. Out of 324 cases of fever, which I have treated here during all the seasons of the year, many of these varieties must certainly have occurred, and yet they have been all cured by quinine. Not cured by quinine mixed with calomel and purgatives, but by quinine alone; and given in such enormous doses that it must have caused death, had the liver, checked perspiration, or any other such cause been the origin of the fever. I say all, because the man who died in Her Majesty's 70th, was known to have caused his sickness by drinking, and had strong symptoms of *delirium tremens*; and the second, in the General Hospital, was not taking quinine at all, for I carelessly thought him convalescent, and had stopped it.”

“ All these fevers have, moreover, been cured at once, for, I think, I may safely challenge a comparison of my average time* of treatment with any Returns for Calcutta fever. My average period, I think, will be found very much smaller, and the improvement has followed instantly after giving the remedy. I have had no cases such as are found so plentifully in the diaries here, to-day improved, to-morrow worse; and all the phases of a fierce struggle between a strong constitution and the disease. I have really had no cases I could describe, except that they came in bad, were next day better, and the 3rd or 4th day convalescent. I have had no black tongues, no nights of delirium, &c. &c., the substance of our recorded cases of fever. The disease is checked

* This is borne out by comparison of the results as shown by other Returns received at the same time. But it is a kind of test liable to be vitiated by so many influences, arising out of difference of disposition, feeling, &c., on the part of the individual practitioner, that we have avoided to bring it prominently into view in the text. It may be remarked, however, that if the fact of diminished mortality, from a certain method of treatment, is established, the diminution of the average duration of treatment almost follows as an unavoidable consequence. At any rate, the difference in this case is too inconsiderable to have much weight in the consideration of the general question.—*Note by Medical Board.*

" by quinine in 24 hours, and finally removed in 48, weakness
 " only remaining to detain the patient in Hospital. No leeching,
 " no blisters, no foul evacuations by stool. It is one rapid progress,
 " from often the last extremity to permanent health. I reason,
 " therefore, as Dr. Holland reasons, that if the same antidote cures
 " all, the same poison must have caused all, and therefore that *all*
 " the 324 fevers, which I have treated this year, continued, conges-
 " tive, ardent, remittent and intermittent, have had one and the
 " same cause, viz., malaria."

" It remains, for this I have not yet done, to make assurance
 " doubly sure, by applying the 3rd test, arsenic; and if this also
 " produce the same effect, as most surely it will, the demonstra-
 " tion is established more certainly, than any fact in medicine; for
 " it is well known, what geometrical quadrupling of proof is,—two
 " tests produce more than one, if they both concur in proving the
 " same fact in the same disease."

" Thus much then I have advanced on fever. For dysentery
 my results are equally remarkable."

" For dysentery, treatment has varied far less then in fever, and
 " the chief difference has been in the quantity of mercury used,
 " and bloodletting, for these have been the standard remedies at
 " all times, and in all places, the quantity of each applied has
 " alone varied; for ipecacuanha and such like, must at least be
 " considered only as adjuvants. It must, therefore, be acknowledg-
 " ed a great point gained, to settle finally the dispute of genera-
 " tions, viz., in what quantity, and with what object, each of
 " these powerful remedies are to be applied."

" I again assert that I have done this. I have done it, however,
 " not by any superior sagacity and genius, but by using the ex-
 " perience of others, and as appears to me almost by a special
 " providence. I have been sent here at a time, and to a place,

“ perhaps the only one in the world, where there are records
 “ sufficient to settle such a point. Moreover, I am most fortunate
 “ also, in my time of arrival, viz., after the system of never saliva-
 “ ting for dysentery had been in use a sufficient number of years,
 “ to compare it with those in which salivation was always attempt-
 “ ed. It is, likewise, the Hospital (and his cases are still here)
 “ where Twining pushed to its limit the antiphlogistic system and
 “ bloodletting. It has been, by good fortune, therefore, and dili-
 “ gence, and not by any talents, that I have proved, by statistical
 “ Tables for 20 years at this Hospital, the most important fact,
 “ that if mercury be used at all in dysentery, we must *salivate*,
 “ that it is the salivation which cures the disease, the action of
 “ the drug otherwise is injurious, and, therefore, the more quickly
 “ we obtain the salivation the better, and fortunately the full
 “ scruple doses of calomel, salivate, of course, quickly, and produce
 “ far less irritation on the bowels. The system of small doses of
 “ mercury has been proved by these Tables to be most injurious,
 “ and to produce double the mortality of the large doses. The
 “ change of mortality also is shown by these Tables to have been
 “ gradual, not sometimes small, at others larger, looking as if it
 “ might arise from accidental causes, but when taken at regular
 “ successive periods of 5 years each, the mortality is proved to
 “ have increased by regular progression, and the case books also
 “ show, that the number of cases salivated has decreased also in
 “ the same proportion, till now, when salivation is never resorted
 “ to for dysentery, the mortality is actually double. These Tables
 “ also show, that the excessive bloodletting, during Dr. Twining’s
 “ time, did not produce any change in the mortality, but that it
 “ continued to increase all the same, because he salivated less
 “ than his predecessors, though still far more than his successor.”

“ Thus then these two important truths have been fixed by me,
 “ that if we use mercury at all, we must use it to produce quick
 “ salivation, and in scruple doses, and that we must not think to
 “ cure dysentery, as Twining did, by bleeding, but only use it to

“ assist the quick production of salivation, and to put the system
 “ generally, in a cool and quiet state.

“ All this may be considered as very safely established, when
 “ based upon such tables as these, containing 4,127 (*a*) cases, and
 “ extending over 20 or 25* years. But how greatly is the de-
 “ monstration strengthened, when having compiled another set of
 “ Tables for the Regimental Hospital, extending over the same
 “ 20 years, and containing 4,134 (*a*) cases, the same facts result,
 “ in a class of patients too, entirely distinct in every circum-
 “ stance and habit, from the former. Can we wish for more
 “ proof than this? †

“ But again I have done more than this for dysentery. Let us
 “ consider how can salivation cure dysentery, and especially
 “ hæmorrhagic dysentery, which the case-books here prove that
 “ it did? Large depletion has been shown not to cure dysentery,
 “ and therefore mercury cannot cure it, simply by its antiphlogis-
 “ tic properties; and besides no one would maintain that
 “ hæmorrhagic dysentery is an inflammatory disease. How then
 “ does mercury cure dysentery? It must be remarked also, that
 “ the same truths have been proved for malarious fever, ‡ viz., that
 “ large bleeding will not cure it, neither will any quantity of
 “ mercury, but that salivation will. The fever subsides from that
 “ moment.”

* The Tables embrace only 20 years.

† “ The averages are taken from, in all, 8,261 cases, through 20 years, an army of
 “ dysentery.”

‡ “ See the facts I quoted in a former Report, (July) with respect to the effects of
 “ salivation on the fatal fevers in Lander’s Niger Expedition.”—See Appendix No. 1.

(*a*) *Note by Medical Board.*—A reference to Dr. Hare’s Tables will show that this
 number is an oversight. The total admissions of diarrhoea and dysentery were
 3,514, and casualties from both diseases 613. These two added together make up the
 sum (4,127) in the text. A similar error occurs in calculating the totals of the Regi-
 mental Table, where the admissions (3,850) and the deaths (284) make together 4,134.

The Regimental Table of Dr. Hare being incomplete when received from him, it
 has been completed in the Medical Board’s Office, which will account for the varia-
 tion of the figures inserted in Doctor Hare’s final Report, and the Regimental
 Table.

“ These facts alone might lead to the suspicion that, according
 “ to Dr. Holland’s axiom, the same remedy, always producing the
 “ same results in two diseases, and those results too unconnected
 “ with any depleting action of the remedy, that the causes of these
 “ two diseases must be identical. Thus much might have been
 “ reasoned before with probability, but I have now gone actually
 “ to the proof. I have applied a double test, for I have proved in
 “ my Tables the effect of salivation; and again I have proved,
 “ during this year, the marvellous effect of quinine also in
 “ hæmorrhagic* dysentery.”

“ This is my own, all my own, for who has ever ventured it
 “ before? Yet see the result; half the usual mortality in the General Hospital, and one-fifth in Her Majesty’s 70th, and this being
 “ the 2nd test, salivation having been before tried on it, I maintain
 “ that the point is proved, and that it is a great discovery.”

“ However, for those whose minds are not accustomed to follow
 “ such reasoning as Dr. Holland’s, all certain as it is, there is
 “ ample evidence of more visible and corporeal nature, to establish
 “ the same truth. I refer principally to the enormous mass of
 “ evidence to be found here in the Hospital diaries of hæmorrhagic
 “ dysentery being constantly preceded, or accompanied† with
 “ fever. I refer, for I have no space to give more than a few
 “ hints of my meaning, to a book which is in every one’s hands,
 “ Dr. Johnson’s own case, page 186, and which deserves attentive
 “ perusal.”

* “ When I speak of hæmorrhagic or malarious dysentery, I mean all forms of it, where much blood is voided by stools. There is a form of common dysentery, which I know is not malarious, and believe to be inflammatory, and though there is blood in the stools in these cases, yet there is far more mucus. I also, by hæmorrhagic dysentery, do not mean *only* the worst forms of it, viz., colliquative pouring out of blood from the bowel, which Raleigh describes, but I include *all* those cases where blood in any quantity appears in stools. Dr. Johnson’s case for instance (see next page)—‘ He was passing blood fast.’ I believe from the result of my experiment here, that all these cases will be found to be malarious.”

† See the quotation from Dr. Holland, viz., his three proofs of the identity of two diseases.

“ *The cause*,—snipe-shooting in the half inundated paddy fields, a few miles below Kedgree. *The symptoms*,—rigors and chills, alternated with flushes of heat. This was the first night. During the next I was better, and concluded that all was well. In the evening, however, griping commenced, and fever came on, my skin now became hot, dry and parched, and by 11 o'clock at night, I could scarcely leave the commode. I was often delirious, &c. I was now passing blood fast, and the fever ran high. The next day passed rather easier than the preceding night, but I had considerable fever. As my night closed in, the exacerbation was great, and I was again delirious; on the 3rd day my fever ran higher, with hot dry skin, and as night approached my apprehension of the usual exacerbation brought on extreme mental agitation.

“ The surgeon endeavoured to cheer me with the hope of ptyalism, which he assured me would alleviate my sufferings; (all this time he was taking calomel,) as the night advanced my symptoms became aggravated, and I was convinced a fatal termination must ensue.”

“ *The end*.—Suddenly at 4 o'clock of this same night, salivation appeared, succeeded with such agreeable sensations, that I ejaculated aloud my heartfelt gratitude to Heaven for my deliverance. He had a natural stool, and his dysentery ceased from that hour. All this is, as much as possible to abridge it, in his own words.* The dysenteric symptoms are principally omitted. How distinctly fever was mixed up with this case.”

“ Again, read Dr. Macpherson's case,—‘ E. Rogerson, September 1st 1849, dysentery—died 15th of the putrid and passive form of the same disease’. Dr. Macpherson remarks on the post mortem. ‘ There was a general oozing of blood from the mucous

* The book itself, Johnson and Martin, is a very common one, and the case well worth referring to. It proves that dysentery is sometimes, at least, malarious.

“ ‘ surface, throughout, to anus. A large quantity of blood was
 “ ‘ found in the rectum. There was no general ulceration of the
 “ ‘ intestine.’ ” “ He again writes,”—‘ This was a case of simple
 “ ‘ dysentery which was yielding to treatment, when fever superven-
 “ ‘ ed, after which the hæmorrhagic tendency developed itself.’
 “ These are merely two, of hundreds of similar cases, equally
 “ ‘ distinct, to be found in the records here.’ ”

“ Again, in their *post mortem*, it is remarkable the large num-
 “ ber of these cases, which are mentioned, as having the colon of
 “ a deep *purple* color, and extreme congestion of, and serous effu-
 “ sion between the coats of the mesentery. Now, this purple
 “ color *must* be owing to venous congestion of the capillaries.”

“ The frequency of this *post mortem* appearance is most remark-
 “ able, and this, connected with the notorious fact of malarious
 “ fever producing extreme abdominal congestion, fulness and heat
 “ of the abdomen, with cold extremities, which Twining so often
 “ calls attention to, plainly leads us to the reason, why fever
 “ should produce this hæmorrhage from the bowels. Dr. Martin
 “ says, page 124—‘ All our complications in the fevers of Bengal
 “ ‘ are abdominal congestion, &c., and this would seem to be the cause
 “ ‘ of the prostration, with tendency to collapse, so common, espe-
 “ ‘ cially during the rainy season, with us, for even within a few
 “ ‘ hours, there exists an oppression of the vital functions, alarming
 “ ‘ to the stranger physician ; or in other words, the action of the
 “ ‘ heart almost ceases from the oppression of extreme congestion.’ ”

“ Now, it is well known, that congestion of the mucous mem-
 “ brane of the lungs, from obstruction of the circulation through
 “ them, arising from thickened and cartilaginous valves of the
 “ heart, will constantly produce spitting of blood and hæmorrhage.
 “ We see, therefore, that it is practically certain, that if the mucous
 “ membrane of the bowel be congested, there will, and *must* be
 “ hæmorrhage.”

" It may appear incredible that such a mass of evidence as this
 " should have passed unnoticed by so many observers, and that
 " I should be the first to discover it. But the fact is, that it has
 " been noticed over and over again, and, moreover, recorded in one
 " or two published papers. But of what use was the knowledge
 " of this truth to the practical man? Fevers and dysentery were
 " treated by colomel and antiphlogistic remedies, both precisely on
 " the same principles. The full proving even of their connection
 " could have made no change in the treatment of either, for it
 " always was the same. But when the far superior efficacy of the
 " quinine treatment for fever had been proved, then it became
 " a point of extreme importance to decide whether dysentery had
 " also a malarious origin, for if it had, it followed as an axiom
 " that in dysentery also, quinine would be very successful; in
 " fact be the only rational method of treating it.

" It has been my glory to have been the first to do this, and
 " from the marked success resulting, even in the long-neglected
 " cases of the General Hospital, I have not only established the
 " truth of the previous argument from the connection of their
 " symptoms, but I have added another and a final one. For I
 " have not only proved by my statistical researches the decisive
 " results in dysentery of one of the tests of malarious fever, viz.,
 " salivation, but this year I have also added, the marked success of
 " the second also, viz., quinine. Their connection, therefore, is now
 " no longer matter for argument; it is proved—it is decided."

" But take this important subject in one more distinct view,
 " and I cannot see myself how the following practical argument
 " from facts, and apart from all theory, can be escaped from. In
 " acute diseases it is well known, that a slight change of treatment
 " for the worse, is sure to aggravate greatly the disease, and pro-
 " duce a large increase in its mortality: and now, just consider my
 " practice. If bleeding, calomel, blue pill, ipecacuanha, leeches,
 " purgatives, &c. &c., be really essential to the treatment of dysen-
 " tery, and the calomel has been proved to be so, how is it that I,

“neglecting *all* these, and giving six scruples a day of quinine
 “as my usual routine, how is it that I, not only have had no
 “increase of mortality, but have actually had only half and one-
 “fifth of my neighbours? It is not, as if I had followed a mixed
 “or undecided practice; or in a place where this disease was
 “not very severe. I have had the worst, the most neglected cases
 “to be found in any part of India without exception, and so
 “decided has been my practice, that I have used no other
 “remedies, but washing out the bowels with water, and giving 6
 “and 7 scruples quinine daily, till the patients often complained of
 “dimness of sight, and were so deaf that they could not hear me
 “speak in the loudest tone. And yet these patients, from the very
 “hour of the quinine taking this effect on their system, instantly
 “recovered, had never any purging again, rapidly obtained their
 “sight and hearing, and were well, just as Dr. Johnson was when
 “salivated.”

“I wish to place great stress on this argument, for it is directly
 “practical matter of fact, and cannot be answered. *I must* have
 “had increased mortality if my theory is wrong, whereas I have
 “had far less.”

“The above, therefore, is what I claim to have accomplished
 “during this experiment. What I think remains to be done, is
 “also known to the Board, viz., the re-testing all these malarious
 “diseases with the liquor arsenicalis, which I know to be, in many
 “instances, a far more powerful antidote even than quinine; and
 “which I have tried very frequently myself, and others in Europe
 “(see my Report for September,*) have given it for skin diseases,
 “in such an extensive and long continued experience, as to leave
 “no doubt, not only of its efficacy, but also of its perfect harmlessness. I have likewise suggested to the Board, what a large
 “saving would result to Government, by the substitution of this
 “cheap and tasteless medicine, for the scarce and expensive drug

* Appendix No. 2.

“ quinine, and which will now, from the result of my experiment,
 “ be still more in demand, and I now again confidently assert,
 “ that the substitution may be almost complete, so as to render
 “ the use of quinine necessary, only in a very few urgent cases ;
 “ and I have good reason to think, that the substitution will be
 “ otherwise advantageous, from the superior power which arsenic
 “ possesses over quinine, to prevent future relapses in the same
 “ patient.”

“ Another most extensive subject still remains to be investigat-
 “ ed, by an extended series of experiments, viz., the local action of
 “ numerous medicinal agents, astringents, &c., when injected to
 “ act on the whole colon. It is well known that on this subject
 “ we are quite ignorant. A few small injections of opium, sugar
 “ of lead, &c., have indeed been used in a few ounces of starch or
 “ water, but these could affect only the rectum, or a little beyond
 “ it. But for the treatment, according to my system, of the ulcers
 “ of chronic dysentery, and to check the bleeding in hæmorrhagic,
 “ it is necessary with the long tube to fill and act upon the whole
 “ colon, from the cæcal valve downwards. The action of medi-
 “ cines thus copiously applied, to such an extended surface, must
 “ evidently be something very different, from their effect on a few
 “ inches of the rectum. They, therefore, will require great caution
 “ in their first use, and very gradual increase of strength, from
 “ large dilution, to the more concentrated and active solutions.
 “ To do this with safety to the patients, requires time, and rather
 “ long time, to go through all the various probable articles of the
 “ *Materia Medica*. This caution is more evidently necessary for
 “ nitrate of silver, sulphate of copper, creosote, opium, &c., but
 “ though less so, it is also much required, in the first application
 “ of our other astringents, viz., alum, catechu, &c. We must be
 “ careful how we treat the extensively absorbing surface of the
 “ colon, which is connected also with the system, by so many
 “ sympathies. However, it must and ought to be done, and if
 “ time be taken, and common caution used, it may be all accom-

"plished in perfect safety, and then, and not till then, will my
 "system of treating dysentery by injections be perfect, or in any
 "way approaching the perfection of which it is capable. For he
 "that will doubt the effect of alum, sulphate of copper, borax, nitrate
 "of silver, &c. &c., on ulcers of mucous surface, or of astringents,
 "alum, sugar of lead, tinct. ferri muriatis, &c. &c., to restrain
 "hæmorrhage in them,—I say that he that will doubt this, is no
 "Surgeon, and has never attended properly his surgical wards
 "when a student."

"To the former, viz., the trial of arsenic, after leaving this
 "Hospital, I can, and will devote my best energies, but this latter,
 "viz., the trial of medicinal injections, being without a regular
 "Hospital, I must leave to others. But now, that the general
 "principles of my system are thus firmly established, I doubt not
 "that others who have these advantages, will adopt my system,
 "and then to supply their own wants, they must, here and there,
 "each for himself, try different medicines in injections, till the
 "whole experiment will be gradually completed, in a better way
 "than I could do it myself."

"One point now only remains, viz., my practice. On this I
 "have but little further to say, for, in explaining the true theory,
 "that is, the true nature of a disease, the principles of its cure are
 "also necessarily included. It is only where we know nothing of
 "the nature of a disease, and perhaps though called by one name,
 "the disease of which we describe the treatment, is really half a
 "dozen diseases of entirely distinct character. It is only in such
 "a medley, named as though it were one disease, that we are
 "obliged to be so cautious in observing and treating symptoms,
 "and find that what cures in one form of the disease, is fatal in
 "another, though the symptoms are very similar. All this diffi-
 "culty arises from our ignorance of the real causes of the disease
 "in these two cases, no doubt essentially different, but which we
 "are obliged to name, and treat as one, because we have no way

“ of distinguishing them. In such cases there is plenty of room
 “ for the large experience of the learned and sagacious physician,
 “ his patient’s life depending on his decision whether he will apply
 “ hot or cold. But, in my treatment of fever and dysentery,
 “ I cannot find opportunity for any display of such learning and
 “ sagacity. My one and only rule is, in fever, give plenty of
 “ quinine, as much, and early as you can. In bad cases you
 “ cannot give too much, for the patient will very soon begin to
 “ complain, that he cannot either see or hear, and then you may
 “ stop, for the fever is gone, and the patient will very soon recover
 “ his health and senses. I have never seen any harm result,
 “ though in many cases I have produced this state. As for other
 “ remedies to assist, as it is said, I have used none of them.
 “ Blisters, leeches, purgatives, &c. Purgatives are bad, for they
 “ prevent the action of the quinine, and for the rest, shaving the
 “ head, and wet cloths to it, and sometimes the liver, are all the
 “ extras I have found necessary. The bowels ought also to be
 “ washed out by enemata every day or so, so that cold water
 “ and quinine, are all the remedies that I have used in bad fevers.
 “ In the mild cases, you may do nothing if you like, but keep the
 “ patient quiet, but he will be well in half the time, if you give
 “ him a little quinine, of course in small doses only. I think,
 “ however, but one full dose in the morning, has a better effect
 “ than the often-repeated small doses; it saves also quinine, and
 “ is certainly less troublesome to the patient. Day-break is the
 “ best hour. I always had it given before me.*”

“ To malarious dysentery, the same rule applies. Quinine again,
 “ only still more of it than in fever; for the purging has a sad
 “ tendency to delay its action; but in this disease you will give

* It would be interesting to register the result upon the numbers invalided, arising from the entire disuse of salivation, and even mercury, in any shape, and the very rare employment of blood-letting. The omission of both these must have great effect, in saving the constitutions of those who are often in Hospital. From the rapidity also, with which quinine puts a stop to the paroxysms of fever, organic disease of the liver and spleen, those constant causes for invaliding, must become far less frequent than hitherto.

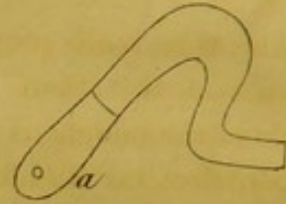
"quinine in vain, except you also take care of the colon, and
 "prevent its sloughing, before the quinine can possibly take effect.
 "You must wash out most carefully and at once, all the highly
 "irritating secretions which it contains, and which are the cause
 "of its sloughing. You must also, if there be profuse hæmorrhage,
 "try and check that for the time, till the quinine can act, by
 "injecting very cold water, or sugar of lead, alum, decoction of bark,
 "with diluted sulphuric acid. The latter perhaps will act doubly,
 "by its local astringent action, and by the absorption of the bark.
 "My rule has been, to repeat these injections, without limit in fre-
 "quency, till the patient's bowels were well cleansed out, and all
 "gripping and hæmorrhage ceased; and then a full dose of
 "laudanum, given with the continued doses of quinine, will be
 "found of the greatest benefit. Laudanum, or crude opium,
 "is, in my experience, far better for dysentery, than morphia,
 "but the dose must be a full one, ʒiiss, is my common prescrip-
 "tion, or four to five grains of opium. My orders always then
 "are to continue to give scruple doses of quinine, six, some-
 "times seven, of them daily, and inject two or three times a
 "day, whenever pain, gripping or hæmorrhage return; and
 "I continue this system, till the patient is well affected with the
 "quinine. He is then safe, the quinine is continued in half doses,
 "and injections once a day, till he is decidedly convalescent.

"For the acute form of dysentery (*i. e.*, not malarious,) bleed-
 "ing is decidedly required to produce one good impression on
 "the system. The bowels are then thoroughly washed out, by
 "one or two large warm injections, and a full dose of laudanum
 "is given. He generally falls into a profuse perspiration, and is
 "soon fast asleep. When he wakes, the gripping often returns, his
 "bowels must then be again washed out, and it is safest to conti-
 "nue to do so twice a day for a day or two, with opiates at night,
 "till all symptoms of dysentery are gone. Leeches, blisters, ipeca-
 "cuanha, &c., I know nothing about. I have never required them.
 "For chronic dysentery, my system is imperfect. But on this, as

“ well as the theory and treatment of the above common acute
 “ dysentery, I have already published my opinions, and therefore
 “ need not repeat them. The whole of the theory there explained,
 “ the reason why the colon is only affected, and in it, especially
 “ the cæcum and sigmoid, and the direct deduction from these of
 “ not only the value, but the necessity of injections, has been still
 “ more than before confirmed to me, by my large experience
 “ during this year.”

“ One point, however, has been so often asked me, that I will
 “ here answer it. It is this. Whether the long tube is constant-
 “ ly, or even often necessary. Whether we cannot, with a little
 “ management, slow injection, &c., do without it. Now this ques-
 “ tion arises from the supposition, that the long tube is a formi-
 “ dable and dangerous instrument, only to be used in great emer-
 “ gencies. This opinion will surely be corrected, when it is
 “ considered, that we pass daily into the urethra, fitted by Nature
 “ to pass fluids only, large steel and silver sounds and catheters,
 “ which stretch its channel far beyond its natural limits, and if
 “ common caution be used, we do this without either danger or
 “ much pain. Whereas the colon is a wide channel, which natu-
 “ rally passes solids, sometimes very hard, and three or four inches
 “ in circumference, and the tube we require to pass through it, is
 “ neither unbending steel or silver, but soft and elastic, and scarce
 “ half an inch in circumference ; and moreover, that the channel
 “ through which it must pass, is not at all more twisted than the
 “ urethra, which takes a sudden turn, almost at right angles.
 “ One often hears of injury done to the urethra, by clumsy hands,
 “ with the metallic, but who has ever heard of any from the elas-
 “ tic catheter, although it is three times the proportionate size of
 “ the tube required to pass up the rectum. That temporary pain
 “ may be caused by passing the tube through a diseased rectum,
 “ I readily admit, but does it, therefore, follow, that it is dangerous.
 “ In fatal disease, pain, if necessary for its cure, will be readily
 “ submitted to, but even this may be almost entirely avoided, by
 “ a little management.”

" The chief source of pain in passing the tube is the sphincter.
 " It is far the most sensitive part of the canal, and it grasps firmly
 " by its muscular action the tube as it passes, so that the whole
 " force, and the whole friction is applied to the most sensitive
 " point of all. Now, add to this, that a tube adapted only for the
 " stomach has been hitherto used, and
 " one which has a large round piece of
 " wood (*a*) fixed at the end of it, and
 " constantly, as I have myself seen,
 " the tube is taken stiff from the box,
 " and no pains taken either to soften
 " or grease it. There can be no sur-
 " prise, therefore, if it has often given
 " pain, though I have frequently seen it passed even thus, with
 " scarcely any."



" However, my way of passing it is this. I first have had the
 " wooden end removed, and the
 " tube made elastic and small,
 " up to its very extremity. The
 " rim at (*b*) ought to be a ring of
 " solid Indian rubber, and then
 " the whole up to the extremity,
 " is soft and elastic. I, more-
 " over, before using it, hold it in hot water, make it soft and
 " flexible, otherwise, when not used, it is apt to be hard and stiff;
 " I then grease it carefully. To insert the tube, I grease two
 " fingers, compress them together, and pass them slowly and
 " carefully into the rectum. I then open the two fingers and
 " thus stretch and open the sphincter. This stretching, as there
 " is no *friction* on the mucous surface, causes little or no pain.
 " I then, holding thus the sphincter open, pass the tube between
 " the fingers, up into the bowel, taking care that it shall rub, in
 " passing, the fingers, and *not* the bowel. It is the friction, on
 " the delicate and sensitive mucous membrane of the orifice, which
 " is so painful, especially if it be ulcerated, for the sphincter grasps



“ the tube as it passes, very firmly ; but in this way, the sphincter
 “ is opened, and all such friction is avoided. The bowel beyond
 “ the orifice has very little sensation. The only painful part of
 “ this operation, is passing the two fingers, and that, if it be
 “ done gently, even in an ulcerated sphincter, is very trifling ; in
 “ all the rest of the process, the tube need not touch it.”

“ This then is the proper way of passing the long tube. It has
 “ been said, that there is fear of the tube passing through the
 “ coats of the bowel. I tried this experiment on a piece of thin
 “ paper, stretched and fixed upright. I held the elastic tube after
 “ warming and greasing the point, about $4\frac{1}{2}$ inches from its end,
 “ which is about the distance used for passing it into the rectum.
 “ I then tried (slowly of course) to push the paper with it, but I
 “ found that the point invariably bent upon itself, and passed
 “ along over the surface of the paper, without any pressure on it
 “ at all. I could, with great management, and applying my force
 “ exactly perpendicular, now and then effect some pressure on the
 “ paper, but the moment the slightest movement of my hand
 “ changed the perpendicular direction, the tube bent on itself and
 “ moved off on the surface. Now let any one consider, whether
 “ to any part of the rectum or sigmoid, the power from without
 “ can be applied perpendicularly. The moment you pass the
 “ sphincter, the rectum inclines off backwards, and to the left
 “ side. It therefore is impossible. I carefully interrogated the
 “ gentleman who originated this objection, and he acknowledged
 “ that he had never used the long tube, he thought only it might
 “ be so. The best answer to which is, that I have tried it pretty
 “ largely, and that it has never been so. As to the comparative
 “ frequency of the use of the long tube or short, in up-country
 “ common acute dysentery, the long tube is always indispensable.
 “ Here, in Bengal, there is seldom swelling of the mucous mem-
 “ brane of the sigmoid, and the short tube will do for perhaps
 “ 8 cases in 9, but for the 9th it is absolutely required. But my
 “ own experience, after so long a trial, is this, that the long tube is

“certainly more effectual in *all* cases in its action; and I think
 “that neither patient nor Doctor, if the latter know how to use it,
 “will ever choose the short tube, rather than the long, if properly
 “made; for this simple reason, that the short tube requires to be
 “forcibly pressed with a napkin, all the time against the anus,
 “and do what you will, the water is constantly gushing back
 “again, and wetting the bed-clothes. Whereas the long tube is
 “passed under the bed-clothes without the least exposure, and
 “once past, it is left in the bowel, and the hand removed. The
 “sphincter clasps it firmly, and not a drop of water escapes during
 “the whole process, which continues outside the bed. No woman,
 “even, can object to the operation.”

“I have likewise arranged an injecting apparatus, which
 “is at once simple, durable and easily worked; only one of my
 “syringes throughout the year has got out of order, and that
 “was from an accidental fall. The cost of each too must be
 “very trifling.”

“This then is the description of my simple theory and prac-
 “tice, and the results of it, after actual trial in two Hospitals,
 “are now before the Board. My patients also have been treated
 “during all the seasons of the year, and in Bengal and the Gene-
 “ral Hospital where the worst forms of disease are found.”

“I may, I fear, in the account I have just given of my year’s
 “labour, be thought somewhat boastful and self-satisfied. I am
 “not, however, I think, on ordinary occasions more vain or given
 “to boasting than my neighbours; but I feel that the occasion
 “now requires it. I am not pleading my own cause, but I am that
 “of my system, which, I believe, and know contains truths of
 “infinite importance to the health and safety of nearly the whole
 “human race. I ask for nothing for myself, but I do ask and
 “hope for my system, that it may be urgently recommended to
 “the profession for their trial.”

“ Our profession are naturally slow and unwilling to change
 “ their established practice without strong probability of advan-
 “ tage, and especially to change it so thoroughly as my system
 “ requires them.”

“ I have no personal vanity or pride of discovery. I merely
 “ claim a great love of my profession and a plodding unwearied
 “ diligence, in patient thinking and research, and this, whether
 “ I succeed now or not, shall still be the employment of my life.
 “ Life is worthless without an object, and, moreover, to be satisfy-
 “ ing, it requires a useful one.”

7. It will be seen that the plans Dr. Hare laid down for him-
 self, in January, as regarded fever, and in February, as regarded
 dysentery, have not been literally followed or completed. As
 respects the declaration of his intention to endeavour to discover
 the signs by which fever, springing from malaria, might be easily
 and infallibly detected at an early or incipient stage from that of
 a purely inflammatory type, or the form which is the forerunner
 of specific eruptive disease, we regard the non-fulfilment as a
 circumstance very much to be deplored, as the accident is very likely
 to happen which he anticipates, from the large exhibition of
 quinine in continued fever of this latter character. If the early
 administration of quinine in the way recommended by him is to be
 generally followed, the absence of a guide to the early diagnosis of
 miasmatic and specific fever will be found embarrassing always,
 and sometimes possibly leading to serious consequences.

8. The Tables show incontrovertibly that Mr. Hare's treatment
 of fever was more successful than the practice followed in the Hos-
 pitals generally of the European portion of the Bengal Army, but
 the extent to which it was so, is shown to be comparatively incon-
 siderable. These Tables show that though fevers are more widely
 and largely prevalent than any other class of disease, yet that the
 mortality in the whole number, annually coming under treatment,

is also inconsiderable, and Mr. Hare himself will admit, we presume, that among the total number of cases that are returned annually as "common continued fever," there is a considerable proportion essentially of an ephemeral character, which yields speedily to remedies that act on the alimentary canal and skin, without the aid of antiperiodic medicines, and many such have doubtless occurred in his own practice.

9. In such cases as these, the exhibition of quinine is not called for at all, much less on the heroic scale in which he has demonstrated it to be not only perfectly safe, but wonderfully efficacious in well-marked cases of miasmatic fever, even at an advanced stage.

10. There is still another object unattained besides that of being able to distinguish malarious from "inflammatory fever," namely; that of being able by certain signs to pronounce at an early stage whether a case of fever (malarious or not) is such as to demand at once the exhibition of scruple doses of quinine to be rapidly repeated, or whether it will yield readily to other remedies.

11. This kind of argument may appear to be trivial in a Report like this, and our reasons for introducing it here may appear to be equally so, but we nevertheless hold the question to be one that demands very serious consideration. For if it once become the fashion in practice to exhibit scruple doses of disulphate of quinine without discrimination, or under the belief and impression, that discrimination is needless, in every case of fever or febrile excitement that is admitted into Hospital, it will very quickly be found that the supply will be unequal to the expenditure! This, at first sight, will probably strike your Lordship as a very improbable and idle apprehension, but we would remark that the fact is already well ascertained, that there has been much improvidence in the management of the cinchona bark trade, and that difficulty has actually been experienced in supplying the market since the introduction of quinine into practice.

The average of expenditure in this Presidency alone has been nearly equal to lbs. 440 of disulphate of quinine during the three past years, and as the annual average number of cases of fever of all types for the same period has been

Europeans,	9,817
Native Troops,	49,118
Jail Hospitals,	22,720

Total, 81,655

it cannot be impertinent to ask here, what would have been the expenditure of the drug had Mr. Hare's mode of administering it been generally followed, or what will be its expenditure should Dr. Hare's recommendation be adopted by the profession generally in the three Presidencies of India, and in all situations besides, throughout the world, where malarious fever is known to prevail?

12. We repeat, therefore, that if our sole reliance in the treatment of every case of fever, and of the other forms of disease to which Dr. Hare has shown it to be applicable, is to be placed on

NOTE.—“The present high price of quinine, and the threatened extinction of the supply of cinchona, have led to the publication in the French Journals of various propositions for substitutes. Among these arsenic deservedly enjoys most favor, especially since the publication of M. Boudin's papers upon its employment, his favorable report being confirmed by many practitioners, both Civil and Military, though demurred to by others.” * * * *

“Other practitioners recommend a more economical employment of quinine. Dr. Pfeufer (see *Periscope*, No. X. page 534,) speaks highly of the efficacy of a single ten grain dose; and we find Dr. Trask, of the United States, where quinine is often administered in quantities that seem to us enormous, and with excellent effects too, in the pernicious forms of the southern latitudes, relating of the intermittent fever he meets with in Westchester county, New York, that he almost always treats it by giving ten grains of the powder, four or five hours before the expected paroxysm; ‘one dose has almost uniformly effected a cure.’ He has often employed *ferroprussiate of iron* with advantage, though it is not comparable to quinine.”

“Although quinine still holds its vantage ground, the importance of these investigations as to the discovery of possible substitutes, and the greater economising of present supplies, are impressed upon us by the unfavorable report of M. Weddel, after five years' investigation of the sources of supply, the high price which places the article already beyond the reach of the poorer classes, and the rapidly increasing adulteration it is subjected to.”

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this drug, the apprehension of failure in the source of supply, cannot be regarded as without foundation, and must, therefore, be admitted as a real and cogent reason for regulating, by all justifiable means, the expenditure of this valuable drug, and this irrespective of the high price of the article, though that cannot altogether be disregarded, seeing that every ounce that is issued from the Public Stores already costs the Government from sixteen to eighteen shillings.

13. But it will be gathered from all that Dr. Hare has written on the subject, that he does not now consider this lavish and indiscriminate exhibition of quinine to be indispensable to the successful treatment of these diseases; and it will also be gathered that he does not ascribe exclusively to quinine the possession of those specific antidotal virtues which he still claims for it in the treatment of miasmatic disease generally.

14. But as in respect to the substitutes he would propose, such as the preparations of arsenic, &c., he has unavoidably left their efficacy to be more fully tested by further experiment, we cannot be expected to offer any opinion on them here. It is different with quinine itself, and it appears to us that although there is still much left to be definitively fixed by future experiment, as regards the early discrimination of the kind of cases in which it will invariably be safe to exhibit it, and the kind of cases also in which its early and large use may be absolutely necessary or advisable, as ensuring a more certain and speedy cure than any other method of treating fevers of all types, the product of the poison of "malaria," (whatever that may be.) We think it cannot be disputed that he has demonstrated incontestably, not only the perfect safety, but the greater efficacy of exhibiting quinine in large or scruple doses repeated three, four, six times, or oftener, in the 24 hours, if the disease do not yield; and that it is not only not necessary, but that it must, in a large proportion of instances of severe or protracted fever, augment the danger to wait for remissions and

changes in the condition of the secretions, and head symptoms, which most practitioners have heretofore considered it prudent to secure, before they believed themselves to be justified in prescribing it even in quantities much more moderate than those insisted on by Mr. Hare as alone effective.

15. With the encouragement, which the details of Mr. Hare's practice and its results, as exhibited in the above extracts of his Reports, and in the annexed Tables, is calculated to give to the profession, a great increase of the expenditure of the drug, is, we think, unavoidable, and we see no reason to doubt that, with his notes before them as a guide, the success of practice will be in a ratio commensurate with that increase.

16. In regard also to the other subject of investigation, (dysentery) Mr. Hare has been under the unavoidable necessity of leaving much to be decided by further experiment and practice; more especially as relates to the testing of medicinal agents, in the efficacy of which, when applied in solution to the diseased surface of the large intestine, although he declares his belief, we do not consider it necessary to submit our opinion here, on that or any other point that avowedly remains unproved. We can only venture to state that Mr. Hare has, in our judgment, indisputably proved, after a severe trial, that the method of treating the disease, which he has so earnestly advocated, is absolutely superior to that commonly practiced in the Hospitals of this Presidency, and that ~~no~~ ~~judgment~~, no medical officer, in charge of an European Hospital, can hold himself exonerated from blame, who, being acquainted with Mr. Hare's mode of administering enemata in dysentery, does not, with his own hands, or by those of well-trained subordinates, carry out in future, to the amplest extent, the remedial measures, the efficacy of which, to alleviate suffering and to aid in curing the disease, has been so triumphantly demonstrated by his practice in Calcutta, as exhibited in his Reports, and in the Tables annexed to this communication.

17. With reference to the opinions of Indian practitioners of the past generation, and to those of practitioners in India and elsewhere more ancient still, respecting the malarious origin of hæmorrhagic dysentery, and its consequent identity with malarious fever, we think it will be interesting to your Lordship to know that at the very time Mr. Hare's experiment was in process, sentiments, very similar to his own, both as to the exact nature of the disease and some of the principles of cure, were expressed by Surgeon Taylor, of Her Majesty's 80th Regiment, one of the ablest and most efficient Medical Officers now serving in this Presidency. We take the liberty of transcribing here the remarks embodied in his Annual Report for 1849-50, on this most formidable species of dysentery, which is so fully described in Mr. Hare's final report, and which is unhappily known as a very destructive endemic disease at Dinapore, where Dr. Taylor is now serving.

"The cases (fever) of the period under report, have in the worst instances been undistinguishable from the cases of dysentery which will be treated of presently. In both, the liquid black hæmorrhagic purging existing, with hot head, and more or less lividity or dusky sallowness of the face. Sometimes the febrile, sometimes the dysenteric symptoms are found to be predominant, or these two orders of symptoms may completely alternate in the same case. These cases of fever and the generality of cases of this class, during the year, have been of the common continued type on admission, changing, however, in most instances to the remittent type, whenever the progress of the case was unfavourable or protracted."

"Diseases of the Stomach and Bowels."

"Of the total 207 admissions and 27 deaths in this class, 169 admissions and all the deaths are by dysentery. Taking the average of seven years, the proportion of deaths by dysentery to

“ all deaths by diseases in Hospital, is 1 in 3. In the twelve
 “ months under report, the proportion is 1 in $1\frac{2}{3}$, not, however,
 “ because the number of deaths by dysentery is increased, but
 “ because the proportion of deaths by other diseases is less than
 “ usual.

The following Statement, showing a decrease of late years in the
 Sickness and Mortality by Dysentery, is taken from a Table
 entered in Section I.—

Period of Averages.	By Dysentery.		
	Per Cent. of Strength.		Died per Cent. of Admitted.
	Admitted.	Died.	
Average of 5 years, 1821 to 1825 inclusive,.....	47·18	6·2	13·07
Average of the last 7 years, 1843 to 1850,	23·30	3·16	13·64

“ The prevalent and fatal type of dysentery at Dinapore is the
 “ hæmorrhagic, or the dysentery by sloughing phagedenic ulcera-
 “ tion of the large intestines. It is the dysentery characterised by
 “ black evacuations from the bowels, these evacuations being
 “ generally very copious and liquid, sometimes more consistent,
 “ like black mud.

“ In the severer cases, these discharges are seen and smelt to
 “ be nearly all blood in a more or less putrid state. Sometimes
 “ black jelly-like clots are found in these stools, having the odour
 “ of putrid blood. With stools of this kind, the general state of
 “ the patient is that of collapse, skin cold and clammy, pulse
 “ barely perceptible, perhaps hiccup; sometimes there is a dark
 “ areola round the sunken eyes, and general lividity, giving to the

“collapse a resemblance to that of cholera, rendering the case
“equally eligible for either classification.

“Two of the cholera cases of the period under review were of
“this description. To this classification it may be objected that
“the stools were black liquid and not congee-like liquid, and that
“there was no vomiting, but excepting in these respects, the
“cases were cases of cholera, and I may point out the near
“alliance between the type of dysentery here alluded to, and
“cholera by reference to a case recently under treatment. In
“this case there was the general state of collapse of cholera, and
“the patient on admission passed two congee stools, and vomited
“a yellowish watery fluid, yet very shortly after there came away
“a liquid black stool, the head got warm, but the patient remained
“cold and livid, and passed liquid or soft black stools for several
“days. This case not only shows the alliance between this type
“of dysentery and cholera, but also its alliance with fever.”

“The severer cases of this hæmorrhagic and sloughing type of
“dysentery, those with copious liquid black stools, or putrid
“bloody stools, attended with lividity of surface, and general
“collapse, with few exceptions, prove nearly as rapidly fatal as
“cases of cholera.”

“In the less severe cases of this type of dysentery the stools
“are less purely of putrid blood, they are of a deep black appear-
“ance, generally fluid and copious, sometimes of the consistence
“of soft mud; these stools have not much, sometimes little, or no
“fæcal odour. If the patient improves, the stools generally change
“from black to the appearance of coffee-grounds of semi-fluid
“consistence, but very different to the eye in other respects from
“a loose bilious motion. Occasionally a few drops of pure blood
“pass after these motions, and are seen on the surface. In this
“type of dysentery, there is little, if any, pain in the abdomen,
“and the same may be said, as respects the other usual symptoms

“ of dysentery, the tenesmus and tormina. These are rarely
 “ prominently noticed by the patient, but his usual complaint is of
 “ pain in his seat. This pain in some cases occasions great suffer-
 “ ing. In these less severe cases, there is sometimes no fever, in
 “ others there is more or less fever of a remittent type. And in
 “ many cases, the patient seems for some days to suffer from
 “ dysentery, without fever, then again, for a certain period,
 “ the case seems to be one of dysentery with fever, when further
 “ perhaps, another change occurs, the febrile symptoms become
 “ altogether predominant, and the patient may be said to labour
 “ under fever with dysentery. So complete sometimes is this last
 “ change of phase in the disease, that it is impossible to form any
 “ other opinion than that the copious, bloody, and black dejections
 “ do not necessarily indicate solution of continuity of the mucous
 “ membrane of the large intestines.

“ Even in these less severe cases of this type of dysentery, a
 “ dusky injected lividity of the surface, particularly of the face, is
 “ observable, or the complexion has that dirty bruise-like yellow-
 “ ness, generally considered as indicating a broken-down state of
 “ the blood. In the course of the disease an irruption of indolent
 “ pustules or small boils about the face is very common.”

“ In the severe and rapidly fatal cases of this disease, there will
 “ be found numerous sloughs scattered throughout the large
 “ intestine occupying the centres of confluent phagedænic ulcera-
 “ tions. The destruction of the mucous membrane is frequently
 “ equal to its entire extent, not a patch of membrane to be found,
 “ the spaces between the approaching phagedænic edges of the
 “ ulcers being thickened and irreconisable as mucous membrane.
 “ The bases of those ulcers, where sloughs have separated, are
 “ formed frequently of the muscular coat, more developed than in
 “ the natural state, but the destruction is often deeper, perforation
 “ is frequent, or the gut is in such a state, as to be broken through
 “ in the process of removal for examination.”

“ In no instance has the disease been found to extend higher
 “ than the ileo-cæcal valve ; the ileum, it is true, is always highly
 “ injected, but not more so than is found to be the case after
 “ death from other diseases.”

“ I cannot but consider the type of dysentery in question to be
 “ of systematic origin, and essentially of a febrile nature. I
 “ regard this variety of dysentery to be as distinct in its nature
 “ from the ordinary tropical dysentery, as the variety known as
 “ scorbutic dysentery is by all parties admitted to be. Those who
 “ maintain the inflammatory nature of dysentery, with exception
 “ of the scorbutic variety, are evidently aware of some abuse of
 “ the term inflammatory as applied to dysentery of this hæmorrhagic and sloughing phagedænic type. Mr. Twining (page 101)
 “ evidently felt, though he does not expressly admit, that this
 “ type of dysentery did not accord with his systematic description
 “ of dysentery generally, and his systematic mode of treating that
 “ disease. Cases of this type probably formed exceptions only
 “ in Mr. Twining’s practice, whilst here at Dinapore they form
 “ the rule. Mr. Twining, notwithstanding his objections to opium
 “ in dysentery, admits that in cases of this kind he could resort to
 “ no other medicine. He also in other respects waived his rules
 “ of treatment, and admitted the necessity of stimulants and
 “ quinine.”

“ Mr. Raleigh describes this hæmorrhagic and phagedænic type
 “ of dysentery as the hæmorrhagic ‘ stage ’ of idiopathic dysentery, and he argues the local seat of dysentery from the morbid
 “ appearances after death, being observed to be strictly confined
 “ to the large intestines. Why Mr. Raleigh should consider this
 “ type as only a stage of ordinary idiopathic dysentery, I cannot
 “ conceive, when he admits, these cases either die or rapidly recover
 “ without presenting any signs of his other stages of idiopathic
 “ dysentery. Mr. Raleigh’s stages are, in fact, distinct types of
 “ dysentery. So far from agreeing to the local seat of this

“ hæmorrhagic variety of dysentery, I have already declared my
 “ belief that the disease is essentially of a febrile nature, the
 “ febrile action determining upon the mucous membrane of the
 “ large intestines producing hæmorrhagy and sloughing, in the
 “ same way as hæmorrhagy and sloughing of the intestinal mucous
 “ membrane attends yellow fever, and the severer or epidemic
 “ varieties of remittent fever.”

“ Whatever the nature of this variety of dysentery, the treatment
 “ required is the reverse of antiphlogistic. Stimulants, such as
 “ wine and brandy, together with opium, alone offer themselves as
 “ resources in the severer cases. It is astonishing the quantity
 “ of opium required to produce any effect in these cases. Fifteen to
 “ thirty grains a day may be given without producing stupor or
 “ even headache. When the case is less severe, allowing more time,
 “ quinine is resorted to by all who have treated this disease, showing
 “ the general suspicion of its febrile nature.”

“ Quinine and opium is an useful combination in these cases.
 “ Three, four, or six leeches to the anus frequently give relief to
 “ the pain complained of in that part. The warm bath and
 “ fomentations also give relief. Believing the mucous membrane
 “ of the large intestine to be actively congested in this type of
 “ dysentery, I was induced to make trial of Ward's Paste, or the
 “ confect. piperis nigri, which seems to have so beneficial
 “ a control over the congestion of the lower bowel in hæmorrhoi-
 “ dal cases. This remedy, after a tolerably extensive trial, I
 “ believe to be very serviceable, and particularly so in combination
 “ with opium. Fifteen grains, or a scruple of the confection, with
 “ one or two grains of opium, is the dose I give, twice a day, in the
 “ dysentery cases with these black and bloody stools, where the
 “ febrile symptoms are not marked, and when there is no collapse.”

18. We beg leave also to insert here an extract of the Annual
 Report of another able Medical Officer, Dr. Inglis, in charge of

Her Majesty's 10th Foot, in whose Hospital the treatment of dysentery by enemata appears to have been partially followed, and its advantages freely admitted.

“ Another powerful remedy, and a more than mere adjunct, is
 “ the large tepid water enema, which of late years has been most
 “ successfully adopted in the treatment of dysentery, and seems
 “ singularly efficacious in removing the diseased secretions and
 “ excrementitious matter from the surface of the swollen and
 “ inflamed mucous membrane, and which obviates the necessity of
 “ giving doses of irritating purgatives, requisite if the more
 “ rational mode of removing acrid and diseased secretions from
 “ the large intestines be not performed by unirritating and fre-
 “ quently-repeated copious enemata. This treatment in all stages
 “ of dysentery is pretty regularly carried out, and as a proof of its
 “ anodyne effect and value, I need only mention that the sensa-
 “ tions of the patient after the enema are always agreeable, and its
 “ repetition is frequently requested. From what I have stated as an
 “ explanation of our general treatment of acute dysentery, it may
 “ be inferred, that we regard the disease as colitis, or acute inflam-
 “ mation of the great intestine, and that the treatment is directed
 “ towards the subduing of that action, and to prevent the serious
 “ consequence which would cause ulceration and sloughing of the
 “ coats of the intestines were it not checked, and the primary
 “ treatment to achieve this result is antiphlogistic (as I have
 “ mentioned) the offending and acrid secretions being chiefly
 “ removed by large enemata, instead of by irritating purgatives.
 “ When tenesmus is severe, opium as a suppository, and added to
 “ the enema, affords relief, and in annoying tormina, Dover's
 “ powder with hydrarg-c-creta is found useful.”

19. It seems only needful to state further, that it was, early in the course of the experiment, made a condition that no selection should be made of cases of dysentery and fever for admission to Mr. Hare's Wards at the General Hospital, but that his share of

the admissions under those heads should be assigned to him in his turn with the other medical officers, as the cases presented, so that in all those that he has treated, he has had a fair proportion of such as were of a severe and unpromising type.

We have the honor to be, &c.,

(Signed) G. LAMB, *Physician General*.
 „ W. S. STIVEN, *Surgeon General*.
 „ J. THOMSON, *Inspector General*.

FORT WILLIAM,
 MEDICAL BOARD OFFICE,
The 31st March, 1851. }

The following report was made to the
Board of Directors of the
Company on the 1st day of
January 1881.

The Board of Directors of the

Company, G. L. M. Board of Directors

W. S. Smith, Secretary

J. Thompson, Treasurer

Report of the Board of Directors
for the year ending 31st Decr 1880.

The Board of Directors of the
Company, G. L. M. Board of Directors

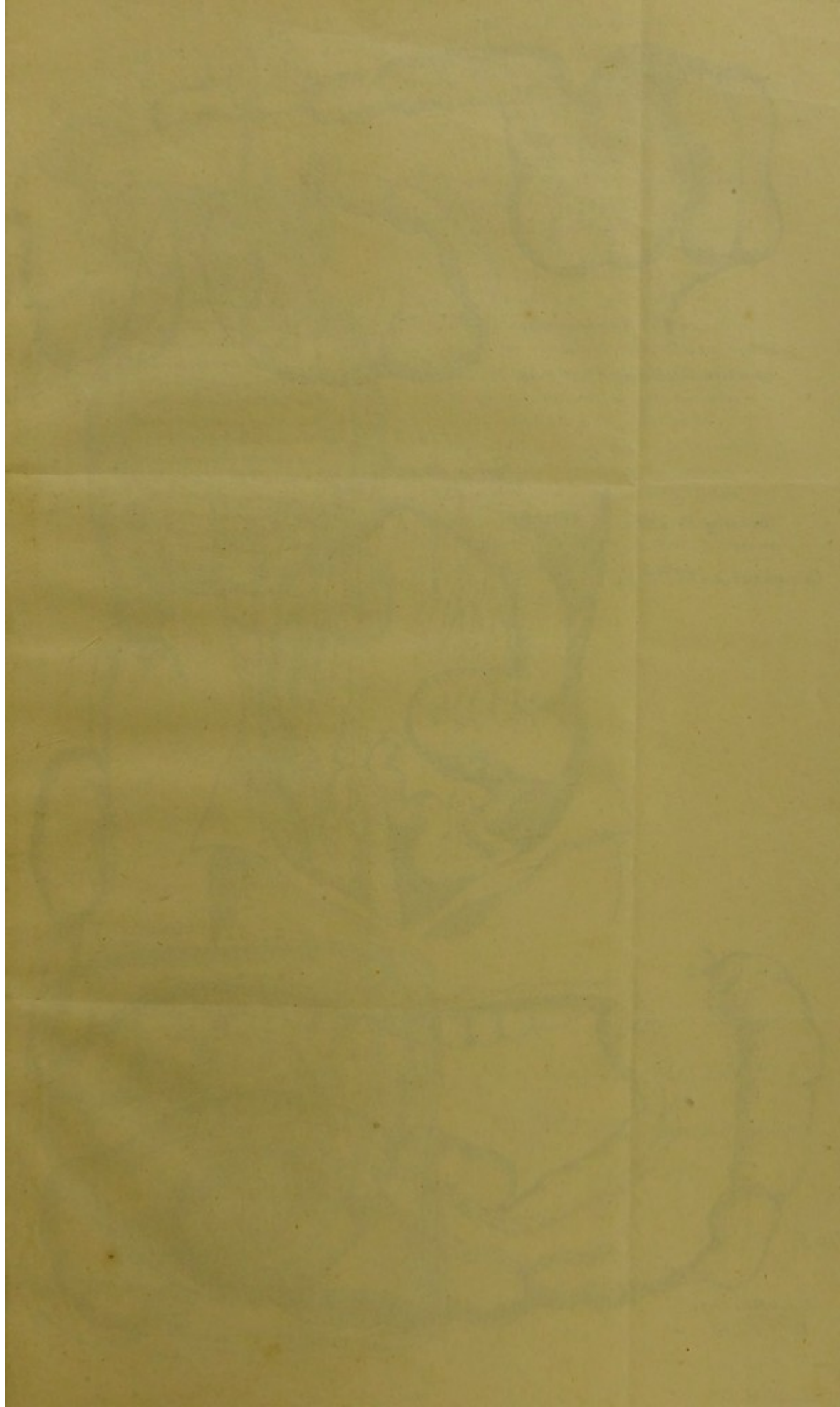
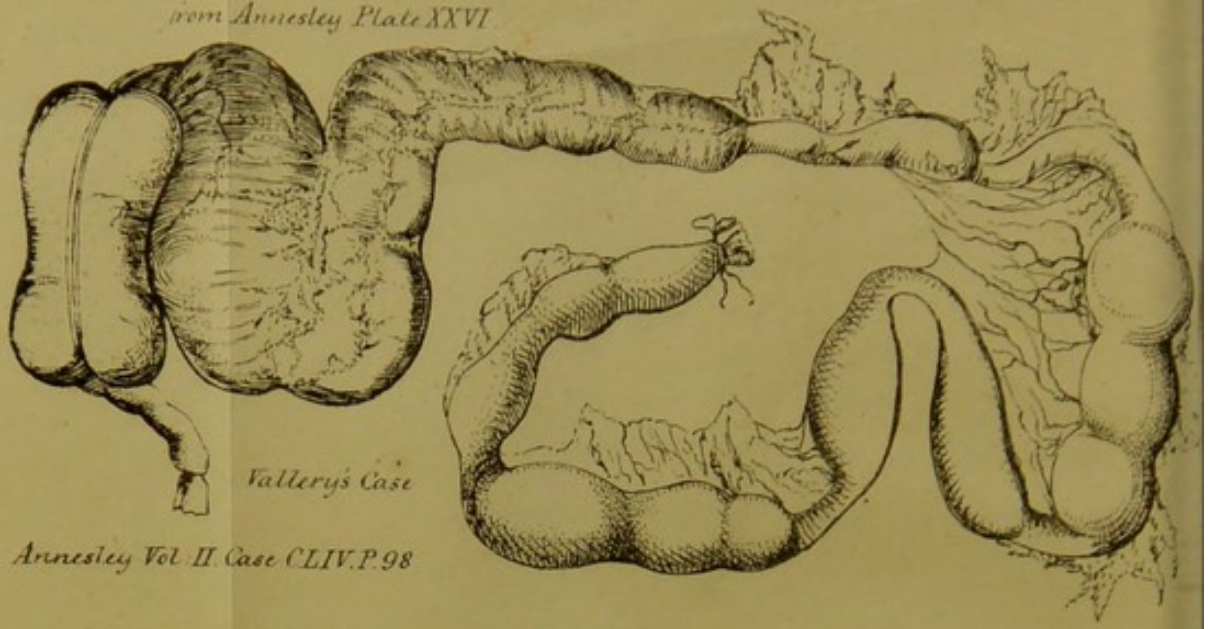


FIG:3.

from Annesley Plate XXVI



Vallery's Case

Annesley Vol. II. Case CLIV. P. 98

FIG:4.

Annesley Pl. XXX.

Cavanagh's Case Vol. I. P. 492

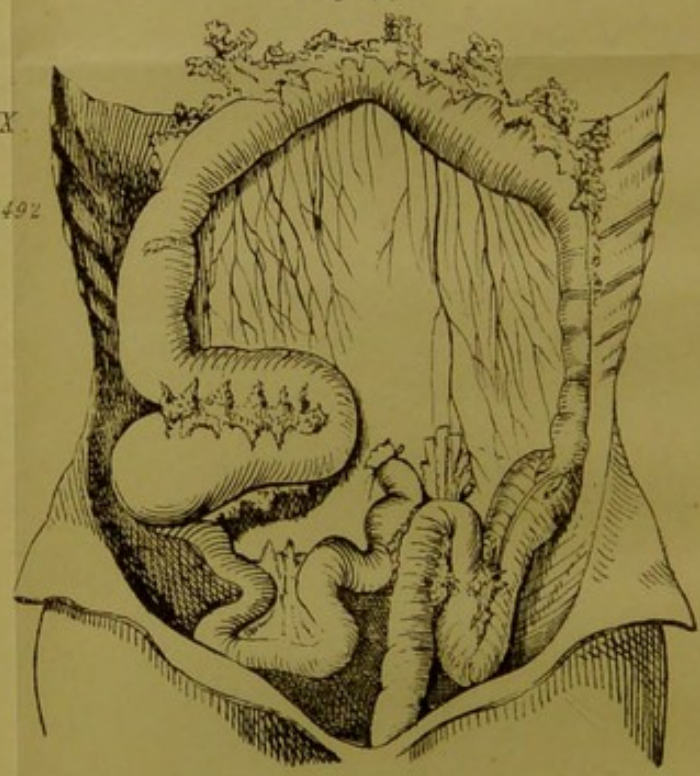
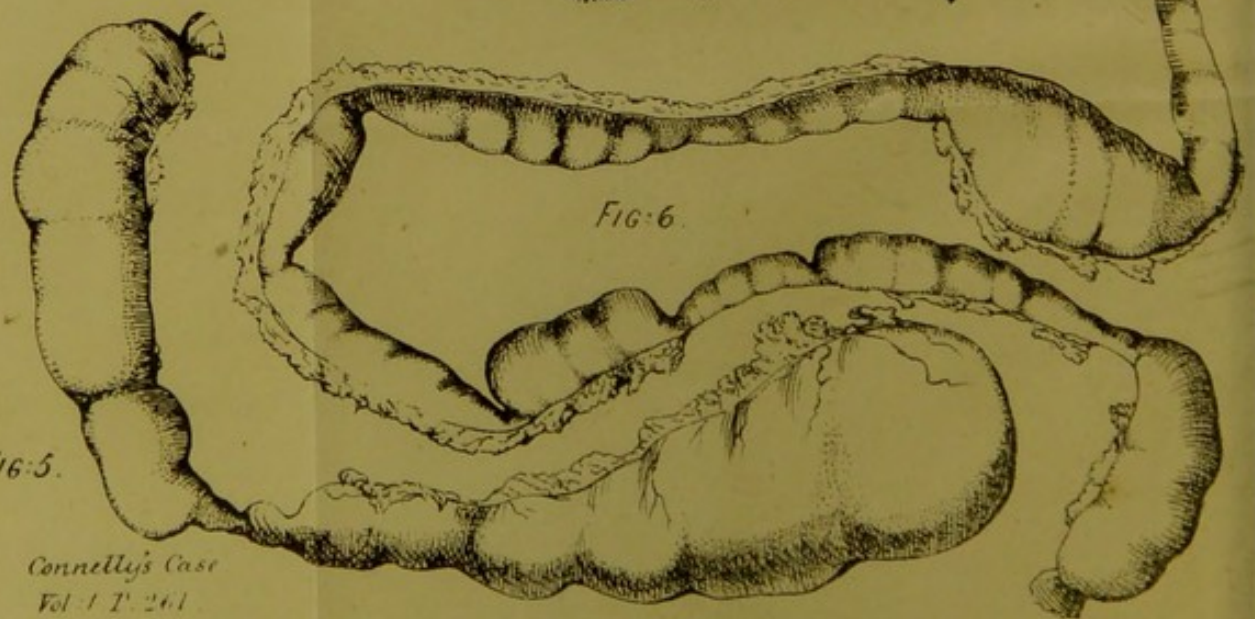
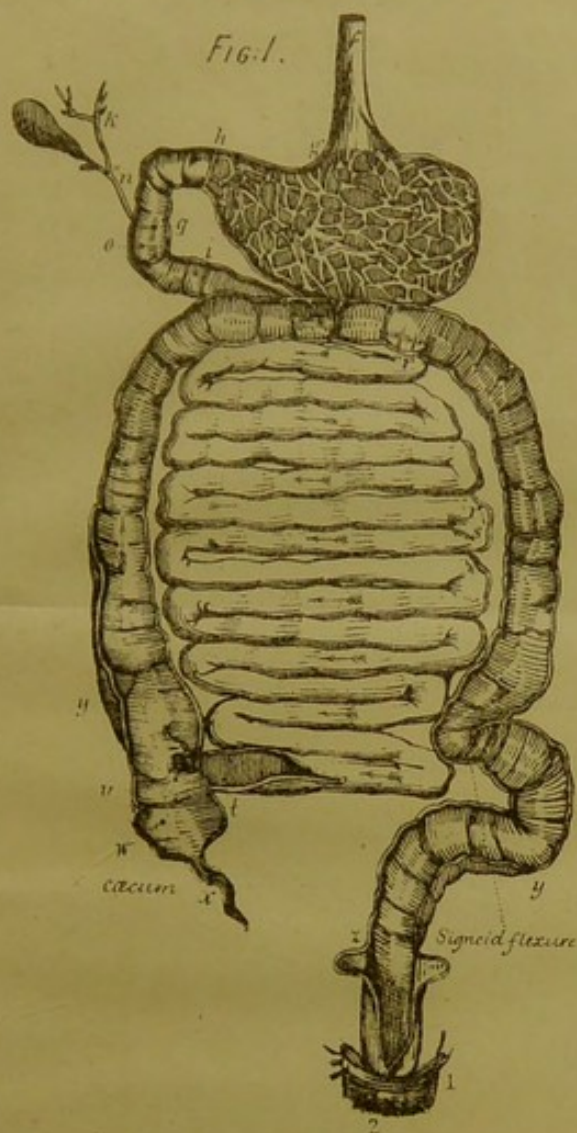


FIG:6.

FIG:5.

Connelly's Case
Vol. I. P. 261





REFERENCES.

f. oesophagus, the upper part having been cut away to shorten the drawing.
g. cardia. *h.* pylorus, the space between *g* and *h*, being the cavity of the Stomach.
i. duodenum. *k.* hepatic duct.
l. gall bladder: *m.* cystic duct.
n. ductus communis choledochus, formed of the two: *o.* the opening of the choledochus into the duodenum. *p.* pancreatic duct; *q.* its opening into the duodenum, which here is distinct from that of the choledochus: *r.* jejunum: *s.* ileum: *t.* termination of ileum in caecum: *u.* superior fold of the valve of colon; *v.* inferior of ditto: *w.* caecum: *x.* vermiform process: *y, y.* colon: *z.* rectum.
1. part of levatores ani: *2.* anus.

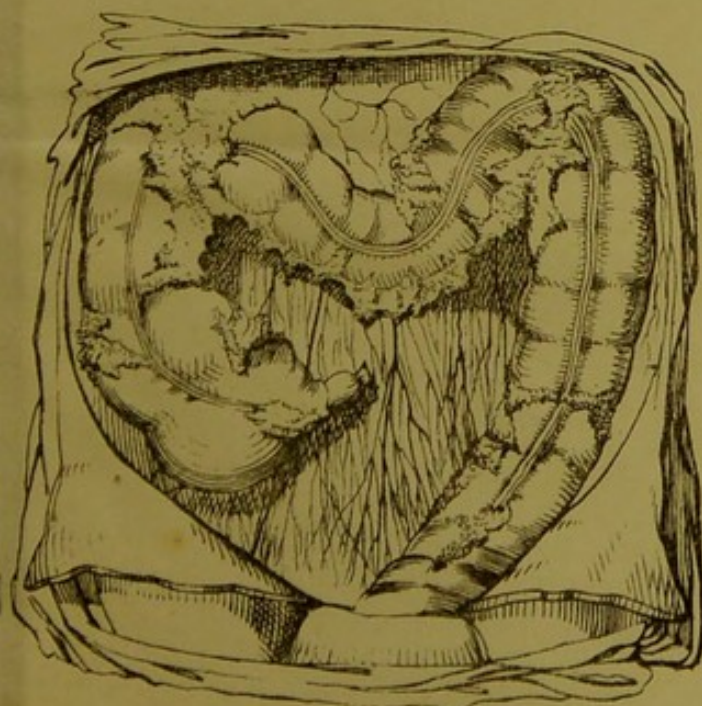


Fig. 2.

From Annesley. Plate XXIV.

Vol. 2nd P. 101. DISEASED STRUCTURE.

WYLLEY'S CASE.



Table showing the Admissions and Deaths from Diarrhoea, Dysentery, and Fever, in the European General Hospital at Calcutta, from the year 1830 to the year 1849, inclusive.

	1830.						1831.						1832.						1833.						1834.					
	Diarrhoea.	Dysentery.	Fever.	Diarrhoea.	Dysentery.	Fever.	Diarrhoea.	Dysentery.	Fever.	Diarrhoea.	Dysentery.	Fever.	Diarrhoea.	Dysentery.	Fever.	Diarrhoea.	Dysentery.	Fever.	Diarrhoea.	Dysentery.	Fever.	Diarrhoea.	Dysentery.	Fever.	Diarrhoea.	Dysentery.	Fever.			
Admissions.			
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	Diarrhoea.	Dysentery.	Fever.	Deaths.	Admissions.	Deaths.	Diarrhoea.	Dysentery.	Fever.	Deaths.	Admissions.	Deaths.	Diarrhoea.	Dysentery.	Fever.	Deaths.	Admissions.	Deaths.	Diarrhoea.	Dysentery.	Fever.																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																		
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	1845.						1846.						1847.						1848.						1849.					
	Diarrhoea.			Dysentery.			Fever.			Diarrhoea.			Dysentery.			Fever.			Diarrhoea.			Dysentery.			Fever.			Diarrhoea.		
	Admissions.	Deaths.	Admissions.	Deaths.	Admissions.	Deaths.	Admissions.	Deaths.	Admissions.	Deaths.	Admissions.	Deaths.	Admissions.	Deaths.	Admissions.	Deaths.	Admissions.	Deaths.	Admissions.	Deaths.	Admissions.	Deaths.	Admissions.	Deaths.	Admissions.	Deaths.	Admissions.	Deaths.	Admissions.	Deaths.
January,	5	1	18	3	9	2	6	1	10	2	7	...	5	...	6	1	6	...	34	3	21	4	23	1	7	1	7	1	9	1
February,	3	...	5	...	9	1	3	2	3	2	4	...	1	...	6	2	7	...	3	1	5	1	6	...	6	...	4	1	4	...
March,	3	1	1	2	13	...	3	...	1	2	4	...	4	...	4	2	2	...	6	1	3	...	6	...	2	2	4	1	4	...
April,	2	1	2	1	4	7	1	4	...	8	2	7	...	6	3	4	...	7	2	8	...	26	1	7	1	11	1
May,	4	1	4	...	15	2	2	...	9	3	11	1	6	...	9	3	12	2	4	...	8	2	18	2	7	1	8	5	14	...
June.	3	...	9	3	26	1	3	1	7	...	1	...	3	1	10	3	6	1	16	2	5	...	23	...
July,	1	...	10	5	39	2	8	...	11	3	27	6	6	1	8	3	18	...	2	...	5	2	37	2	4	1	12	3	42	3
August,	11	5	26	3	7	1	7	2	29	2	1	...	5	...	12	2	4	2	9	2	35	...	35	2	11	1	111	5
September,	3	...	4	2	23	1	2	...	7	3	27	4	3	...	5	1	15	2	3	...	9	3	36	4	17	2	7	8	70	4
October,	2	...	5	1	11	...	20	...	3	1	15	3	6	...	7	3	21	...	2	...	8	4	30	5	7	...	11	3	79	...
November,	14	2	8	2	17	1	70	5	16	2	32	4	10	1	10	1	40	5	5	...	4	3	16	2	14	...	9	2	100	3
December,	5	...	14	7	16	2	13	1	10	5	16	1	4	1	9	...	54	...	2	...	3	1	12	...	5	1	9	2	21	...
Total, ...	45	6	91	31	208	15	134	10	87	27	183	21	55	5	79	17	204	17	69	7	88	25	243	18	130	11	94	28	488	17

GENERAL ABSTRACT.

Years.	Diarrhœa.		Dysentery.		Fever.	
	Admissions.	Deaths.	Admissions.	Deaths.	Admissions.	Deaths.
1830, ...	41	...	144	22	639	45
1831, ...	14	2	128	20	406	38
1832, ...	35	2	124	20	409	38
1833, ...	24	2	128	19	856	107
1834, ...	42	9	147	39	605	77
1835, ...	119	3	71	20	233	16
1836, ...	31	8	55	10	113	12
1837, ...	54	6	64	15	99	4
1838, ...	45	3	52	13	112	18
1839, ...	130	7	78	15	194	13
1840, ...	47	9	68	11	221	21
1841, ...	134	35	172	42	289	33
1842, ...	111	9	147	41	414	29
1843, ...	92	12	88	19	209	10
1844, ...	116	6	141	27	235	16
1845, ...	45	6	91	31	208	15
1846, ...	134	10	87	27	183	21
1847, ...	55	5	79	17	204	17
1848, ...	69	7	88	25	243	18
1849, ...	130	11	94	28	488	17
Total, ...	1468	152	2046	461	6360	565

Dysentery, 1 death in 6.54 admissions.
 Dysentery, 1 death in 6.40 "
 Dysentery, 1 death in 6.20 "
 Dysentery, 1 death in 6.73 "
 { (This was also the ratio from 1826
 to 1830, see Dr. M.'s Tables, p. 61.)

Dysentery, 1 death in 3.22 admissions.
 Dysentery, 1 death in 4.64 "
 Dysentery, 1 death in 3.52 "
 Dysentery, 1 death in 3.35 "

General Average. { Dysentery, 1 death in 4.438 admissions.
 Bowel Complaints, 1 death in 5.732 admissions, i. e. Diarrhœa and Dysentery.
 Fevers, 1 death in 11.256, admissions.

Percentage of Mortality for 20 years, taken during periods of 10 and 5 years.

Periods of 10 years,	Diarrhœa and Dysentery.	Fever.
From 1830 to 1839,	15.399,	10.038 deaths per Cent.
" 1840 to 1849,	19.014,	7.312 " "

Periods of 5 years.	Diarrhœa and Dysentery.	Fever.
From 1830 to 1834,	16.324,	10.463 deaths per Cent.
" 1835 to 1839,	14.306,	8.388 " "
" 1840 to 1844,	18.906,	7.967 " "
" 1845 to 1849,	19.151,	6.636 " "

TABLE No. 1.

Table shewing the Admissions and Deaths from Diarrhoea, Dysentery and Fever in the European Troops stationed in Fort William, from 1830 to the year 1849, inclusive.

	1830.				1831.				1832.				1833.			
	Diarrhoea.		Dysentery.		Diarrhoea.		Dysentery.		Diarrhoea.		Dysentery.		Diarrhoea.		Dysentery.	
	Admitted.	Died.	Admitted.	Died.	Admitted.	Died.	Admitted.	Died.	Admitted.	Died.	Admitted.	Died.	Admitted.	Died.	Admitted.	Died.
January,	10	...	17	3	9	...	11	1	12	2	3	1	17	3	9	...
February,	33	...	16	1	13	...	17	...	7	1	9	...	2	...
March,	46	...	11	2	12	2	1	...	41	1	12	1	4	...
April,	34	...	4	...	18	...	1	...	31	1	...	1	11	...	6	...
May,	57	...	15	...	26	...	1	...	34	31	...	16	...
June,	19	1	14	1	33	15	2	22	1
July,	5	1	16	1	21	25	26	...	39	1
August,	27	...	25	2	49	2	15	4	12	...	32	3
September,	36	...	9	1	81	9	31	7	16	6
October,	22	...	16	1	67	11	1	...	64	3	31	1
November,	14	1	45	5	122	11	48	1	...	1	46	1	7	2
December,	17	...	50	5	71	3	1	...	30	18	1	5	...
Total,	320	3	238	22	522	38	33	1	353	15	27	2	255	9	158	13
	538	15

TABLE No. 2.

	1834.				1835.				1836.				1837.			
	Diarrhoea.		Dysentery.		Fever.		Diarrhoea.		Dysentery.		Fever.		Diarrhoea.		Dysentery.	
	Admitted.	Died.	Admitted.	Died.	Admitted.	Died.	Admitted.	Died.	Admitted.	Died.	Admitted.	Died.	Admitted.	Died.	Admitted.	Died.
January,	7	1	10	...	45	1	15	1	18	5	...
February,	1	...	13	20	...	3	7	...
March,	2	...	1	...	23	23	...	44	11	...
April,	2	...	4	...	46	1	21	...	29	2	4	...
May,	4	...	7	...	74	1	18	...	36	1	1	...
June,	12	...	39	2	17	1	23	1	...
July,	14	4	55	4	26	3	27	59	...
August,	3	1	14	2	83	3	18	4	32	2	44	...
September,	3	...	11	1	53	1	12	1	22	30	...
October,	11	...	32	2	9	...	21	8	...
November,	2	...	7	1	21	2	9	1	23	15	2
December,	7	1	8	1	14	1	34	1	39	1	9	1
Total,	30	3	100	9	503	18	7	...	222	12	317	8	29	1	71	4
															246	7

N. B.—Tables from the year 1833 to 1849 inclusive, were not furnished by Dr. Hare.

TABLE No. 3.
European Troops, Fort William.

	Fever.		Dysentery.		Diarrhœa.	
	Admitted.	Died.	Admitted.	Died.	Admitted.	Died.
1830,	522	38	238	22	320	3
1831,	353	15	232	29	33	1
1832,	255	9	187	27	27	2
1833,	538	15	158	13	23	2
1834,	503	18	100	9	30	3
1835,	317	8	222	12	7
1836,	288	12	121	5	29	1
1837,	246	7	71	4	31
1838,	107	4	38	2	10
1839,	364	1	60	4	49
1840,	537	16	184	15	146	2
1841,	542	13	147	6	126
1842,	840	19	144	6	302	4
1843,	783	39	81	21	139	1
1844,	182	1	60	4	67
1845,	158	2	90	8	30	2
1846,	179	6	114	11	108
1847 to April 1st,	36	1	27	5	16
1847-48,	268	6	96	6	68	1
1848-49,	512	18	198	18	71	2
1849 to December 1st,...	550	22	131	40	414	1
Total,.....	8080	270	2699	267	2046	25

FEVER.—Ratio of Mortality.

From 1830 to 1839, 1 death in 27·503 admissions.

„ 1840 to 1849, 1 death in 32·076 „

DYSENTERY.—Ratio of Mortality.

From 1830 to 1839, 1 death in 11·236 admissions.

„ 1840 to 1849, 1 death in 9·085 „

DIARRHŒA.—Ratio of Mortality.

From 1830 to 1839, 1 death in 46·583 admissions.

„ 1840 to 1849, 1 death in 114·384 „

Remarks on the last Table No. 3.

It will be observed from the periodic Ratios of Mortality resulting from these Tables, that the Mortality for

I. *Fever* has decreased since 1830, from 1 death in 27 cases to only 1 death in 32 cases.

Whereas for

II. *Dysentery* it has increased, and that the Mortality is 1 death in every 9 cases instead of 1 death in 11 cases.

The Ratio for

III. *Diarrhœa* gives a decreased Mortality, from 1 death in 46 cases to only 1 death in 114 cases.

These results agree with the Tables for the General Hospital for the same 20 years, (viz.)—the Mortality by fever has somewhat decreased since quinine has lately been more used ; but the Mortality by dysentery has increased to what it was when scruple doses of calomel were in use instead of the blue pill, ipecacuanha and sugar of lead treatment at present in fashion.

(Signed) E. HARE,
Assist. Surgeon.

(True Copies)
(Signed) J. FORSYTH, *Senior Surgeon,*
Secretary, Medical Board.

STATEMENT showing the number of cases of Diarrhoea, Dysentery and Fever, treated and died in the General Hospital and European Regimental Hospitals in the Bengal and North-Western Provinces, during eleven months, from 1st December 1849 to 31st October 1850.

FORT WILLIAM, MEDICAL BOARD OFFICE, 31st March, 1851.

	DIARRHOEA.										DYSENTERY.										FEVER.									
	Remained.	Admitted.	Total.	Discharged cured.	Transferred.	Invalid.	Died.	Remain- ing.	Total.	Remained.	Admitted.	Total.	Discharged cured.	Transferred.	Invalid.	Died.	Remain- ing.	Total.	Remained.	Admitted.	Total.	Discharged cured.	Transferred.	Invalid.	Died.	Remain- ing.	Total.			
Calcutta,	0	2	2	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0			
General Hospital, (Non Military),	0	2	2	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0			
1st Troop, 1st Brigade, Horse Artillery,	0	16	16	7	10	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0			
2nd Troop, 1st Brigade, Horse Artillery,	0	10	10	10	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0			
Head Quarters and 2nd Troop, 1st Brigade, Horse Artillery,	0	7	7	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0			
Head Quarters and 1st Troop, 1st Brigade, Horse Artillery,	0	7	7	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0			
2nd Troop, 2nd Brigade, Horse Artillery,	0	3	3	3	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0			
3rd Troop, 2nd Brigade, Horse Artillery,	0	3	3	3	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0			
1st and 2nd Troops, 1st Brigade, Horse Artillery,	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0			
Head Quarters and 1st Troop, 1st Brigade, Horse Artillery,	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0			
1st Company, 1st Battalion, Foot Artillery,	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0			
2nd Company, 1st Battalion, Foot Artillery,	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0			
3rd Company, 1st Battalion, Foot Artillery,	0	14	14	12	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0			
Head Quarters and 4th Company, 1st Battalion, Foot Artillery,	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0			
Head Quarters 1st, 2nd and 4th Companies, 2nd Battalion, Foot Artillery,	0	18	20	15	1	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0			
2nd Company, 2nd Battalion, Foot Artillery,	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0			
Head Quarters and 1st Company, 2nd Battalion, Foot Artillery,	0	11	11	11	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0			
2nd Company, 2nd Battalion, Foot Artillery,	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0			
2nd and 4th Companies, 2nd Battalion, Foot Artillery,	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0			
Head Quarters and 1st Company, 4th Battalion, Foot Artillery,	0	12	14	10	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0			
2nd and 4th Companies, 4th Battalion, Foot Artillery,	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0			
1st Company, 4th Battalion, Foot Artillery,	0	3	3	3	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0			
Head Quarters and 2nd and 3rd Companies, 4th Battalion, Foot Artillery,	0	17	17	17	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0			
4th Company, 4th Battalion, Foot Artillery,	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0			
1st Company, 4th Battalion, Foot Artillery,	0	7	7	7	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0			
Head Quarters and 2nd Company, 4th Battalion, Foot Artillery,	0	7	7	7	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0			
3rd Company, 4th Battalion, Foot Artillery,	0	7	7	7	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0			
4th Company, 4th Battalion, Foot Artillery,	0	7	7	7	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0			
Her Majesty's 3rd Light Dragoons,	0	20	20	25	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0			
Her Majesty's 3rd Regiment of Foot,	0	10	10	10	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0			
Her Majesty's 14th Light Dragoons,	0	80	65	60	1	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0			
Her Majesty's 3rd Regiment of Foot,	0	10	10	10	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0			
Her Majesty's 3rd Regiment of Foot,	0	18	18	18	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0			
Her Majesty's 2nd Regiment of Foot,	0	108	104	105	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0			
Her Majesty's 2nd Regiment of Foot,	0	10	10	10	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0			
Her Majesty's 2nd Regiment of Foot,	0	20	20	25	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0			
Her Majesty's 2nd Regiment of Foot,	0	10	10	10	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0			
Detachment—Her Majesty's 3rd Regiment,	0	5	5	5	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0			
Her Majesty's 3rd Regiment of Foot,	0	10	10	10	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0			
Her Majesty's 3rd Regiment of Foot,	0	2	2	2	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0			
Her Majesty's 3rd Regiment of Foot,	0	2	2	2	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0			
Her Majesty's 3rd Regiment of Foot,	0	2	2	2	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0			
Her Majesty's 3rd Regiment of Foot,	0	2	2	2	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0			
Her Majesty's 3rd Regiment of Foot,	0	2	2	2	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0			
Her Majesty's 3rd Regiment of Foot,	0	2	2	2	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0			
Her Majesty's 3rd Regiment of Foot,	0	2	2	2	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0			
Her Majesty's 3rd Regiment of Foot,	0	2	2	2	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0			
Her Majesty's 3rd Regiment of Foot,	0	2	2	2	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0			
Her Majesty's 3rd Regiment of Foot,	0	2	2	2	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0			
Her Majesty's 3rd Regiment of Foot,	0	2	2	2	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0			
Her Majesty's 3rd Regiment of Foot,	0	2	2	2	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0			
Her Majesty's 3rd Regiment of Foot,	0	2	2	2	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0			
Her Majesty's 3rd Regiment of Foot,	0	2	2	2	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0			
Her Majesty's 3rd Regiment of Foot,	0	2	2	2	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0			
Her Majesty's 3rd Regiment of Foot,	0	2	2	2	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0			
Her Majesty's 3rd Regiment of Foot,	0	2	2	2	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0			
Her Majesty's 3rd Regiment of Foot,	0	2	2	2	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0			
Her Majesty's 3rd Regiment of Foot,	0	2	2	2	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0			
Her Majesty's 3rd Regiment of Foot,	0	2	2	2	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0			
Her Majesty's 3rd Regiment of Foot,	0	2	2	2	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0			
Her Majesty's 3rd Regiment of Foot,	0	2	2	2	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0			
Her Majesty's 3rd Regiment of Foot,	0	2	2	2	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0			
Her Majesty's 3rd Regiment of Foot,	0	2	2	2	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0			
Her Majesty's 3rd Regiment of Foot,	0	2	2	2	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0			
Her Majesty's 3rd Regiment of Foot,	0	2	2	2	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0			
Her Majesty's 3rd Regiment of Foot,	0	2	2	2	0	0	0	0	0	0	0	0	0	0	0															

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APPENDIX No. 1.

“See Dr. Oldfield’s remark on his experience in the unfortunate African Expedition up the Niger with Lander, (page 343).”

“‘I treated Captain Mitchell’s fever in the usual way, viz., 10 grains of calomel every 4 to 6 hours, but the calomel had no effect. He lingered till the 10th, when he died. When I found that the calomel took no effect on the system, I felt assured that he would not recover, for in several cases where it had failed, death had most surely taken place in a few days.’”

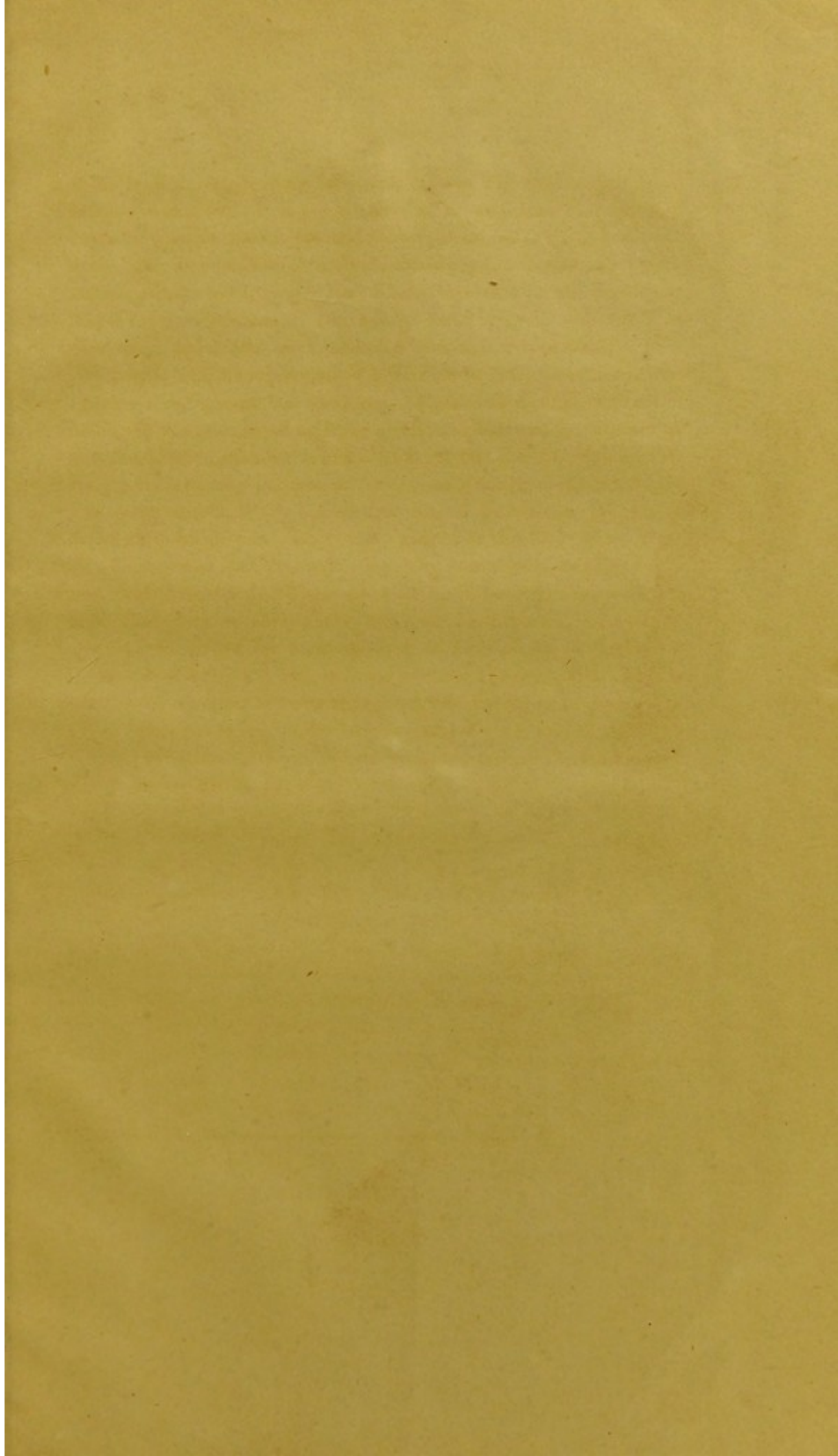
APPENDIX No. 2.

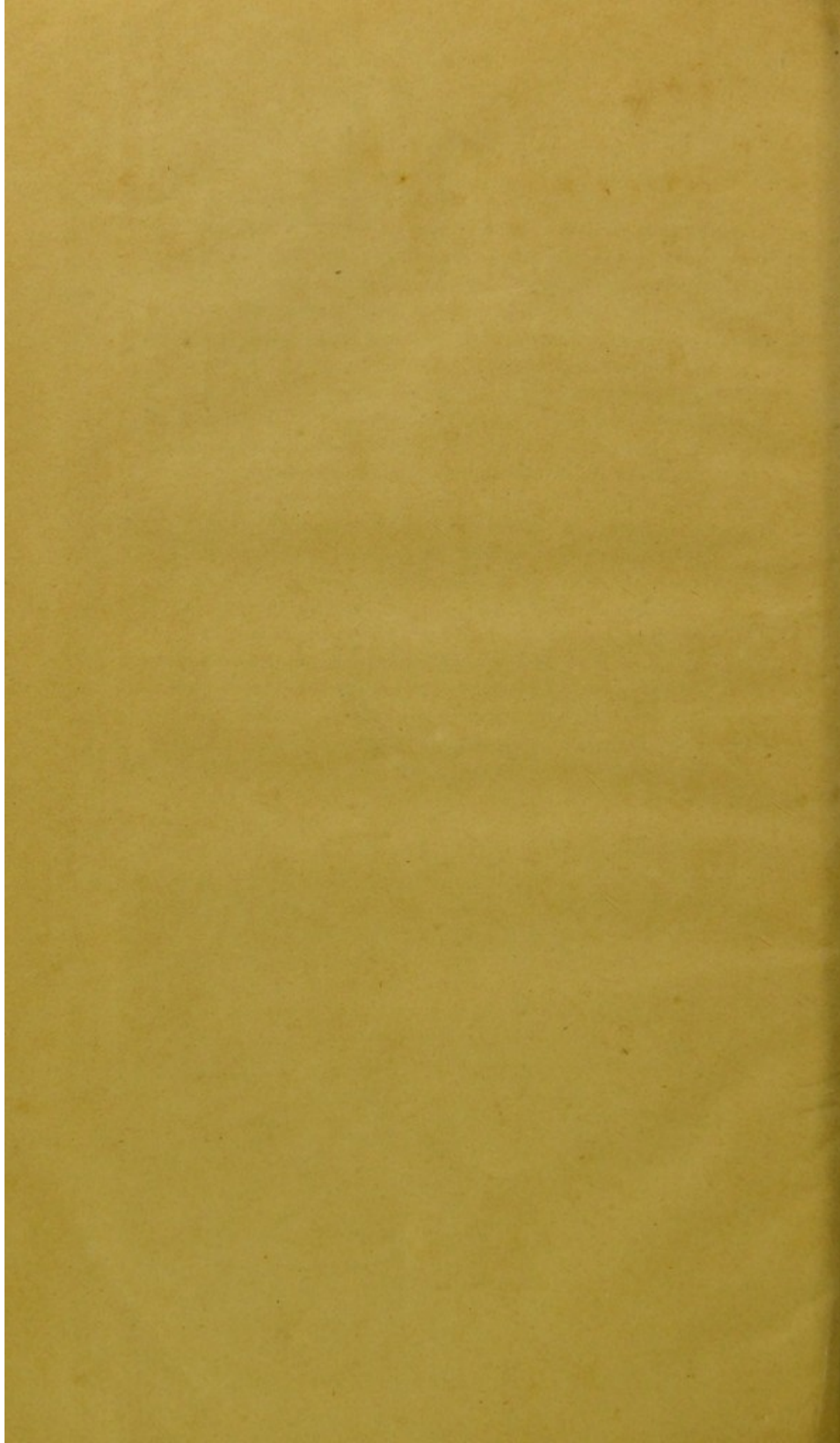
“The liquor arsenicalis is never used in this country, as, I think, may be shown by the indents for it on the Government Dispensary, which can scarcely amount to more than replacing the waste by evaporation in our numerous Hospitals, where it is kept in small quantities but never used. We can know nothing, therefore, by experience against it, for though sulphuric acid and iodine are quite as violent as arsenic in their effects in poisonous doses, yet when rightly used they may be given any length of time, not only with no injury, but with a most powerful tonic and restorative effect on the constitution. All the experiments which have been made in Europe, and latterly in England, for diseases of the skin, show that arsenic may be given for many months’ continuance without any harm but great benefit, not only to the disease but also to the general health. M. Biett used it for years in the large Hospital for skin diseases in Paris. M. Cazenave, and others also in England, and one and all agree that they never saw any harm from it, and to show the length of time it may be continued, a gentleman in England (Dr. Hunter), who seems to have had great experience, lays it down as a

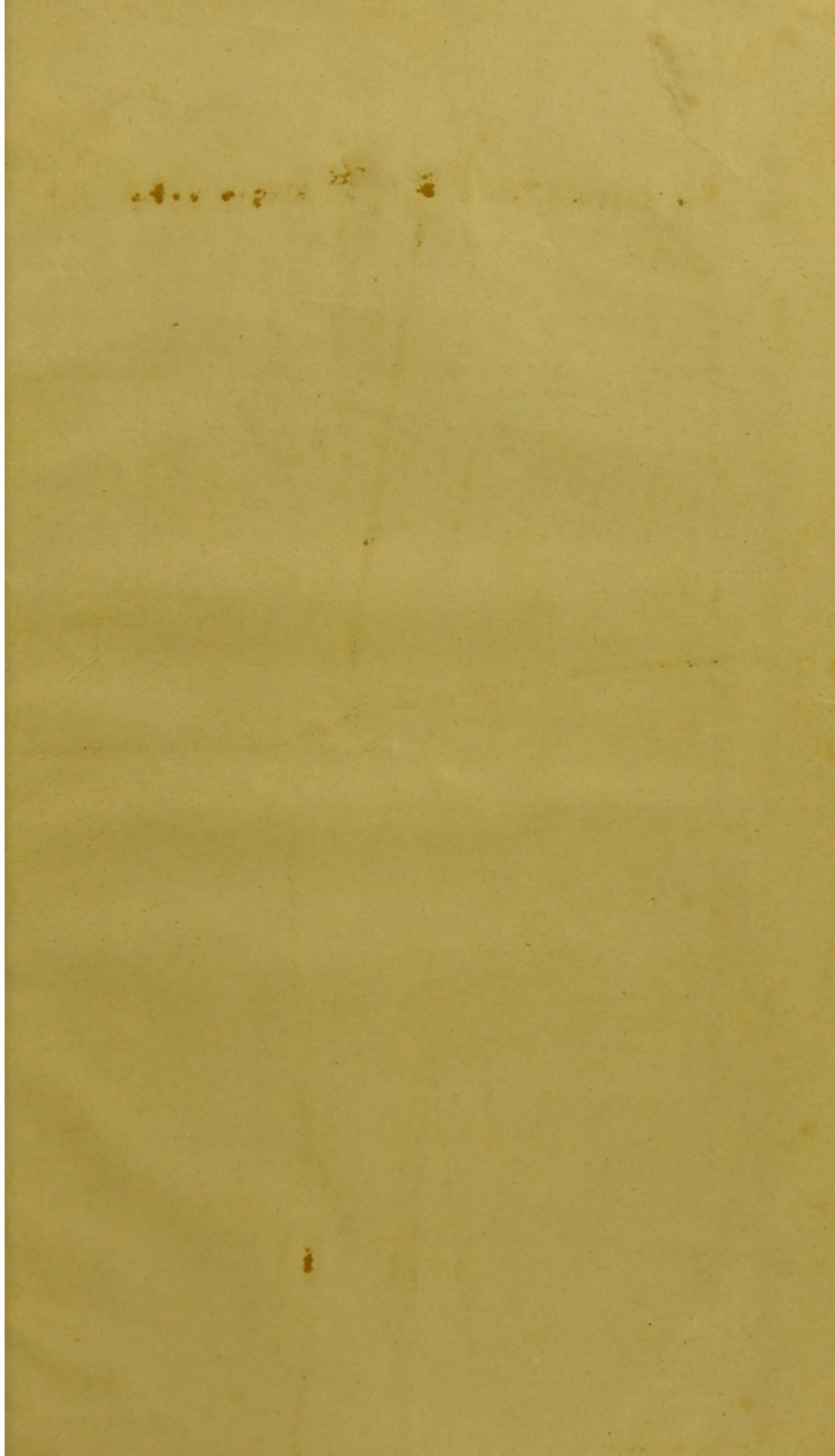
“ rule, that for the cure of lepra, arsenic must be continued as many months
 “ as the disease had existed years, which, in most cases, are not few. Now it
 “ is known that calomel cannot be continued in the same way without injury ;
 “ it follows, therefore, that arsenic is the safer remedy of the two ; and more-
 “ over it is a far more powerful antidote than calomel, and even than quinine
 “ in some cases, and will cure always in moderate doses, whereas calomel will
 “ not till it salivates : to produce which, in many cases, requires the doses used
 “ to be poisonous. Therefore, whatever popular prejudice there may be to
 “ arsenic, from its common use as a poison, yet let each answer for himself,
 “ if he ever saw or read of a case where it produced injury, when given in
 “ proper doses even for months in continuance. I have studied the subject
 “ carefully and have not yet been able to find one. I have, moreover, myself
 “ had large experience of its use. I was for 5 years with the 7th Irregulars,
 “ and did not lose one case of fever, though the men were constantly employed
 “ in unhealthy posts on the borders of the Nepal Serai. My expenditure of
 “ quinine, during those years, was not greater than my neighbours, because
 “ I used arsenic freely, and the more I use it, the more confidence I have in
 “ it. There are many cases of old fevers where relapses have been frequent,
 “ and where quinine becomes at last quite inert. In very many of these cases
 “ I have given arsenic, and I have yet to witness a case of its failure. Now,
 “ ardent admirer as I am of quinine, and believing that the Cinchona is, indeed,
 “ a tree given for the healing of the nations, I cannot deny that it is like so
 “ many other blessings for the rich man more than the poor. A liberal
 “ Government, however, can scarcely deny to their servants a medicine, which
 “ my experiment proves to save hundreds of lives. Human life must not be
 “ sacrificed to economy, and yet there can be no doubt that the result of my
 “ present experiment will enormously increase the demand for this useful but
 “ scarce and expensive medicine, and that the increased demand must also
 “ greatly increase its price, for the supply is limited. It behoves the Govern-
 “ ment, therefore, most seriously to consider if no other efficient substitute can
 “ be found for it. Decoction and powder of bark never can be, and never will
 “ be, a remedy to be relied on in severe fever. They cannot be given in
 “ sufficient quantity ; and from the way in which I have seen the decoction
 “ prepared, and given in Regimental Hospitals in India, my own suspicion is,
 “ that it might be just as well omitted, and that the patients would recover
 “ from their slight fevers with a little tartar emetic and purgatives, just as soon
 “ without it.

“ We have the remedy in the liquor arsenicalis.—‘The tasteless ague drops’
 “ as it has been called. It is the cheapest of all medicines, it requires only
 “ very small doses, is tasteless, produces neither purging nor vomiting, headache,
 “ or any bodily inconvenience. A quart of it would physic a Regiment for a year,
 “ and cost perhaps two Rupees. Now I here affirm that all this, novelty as
 “ it is, is not mere conjecture. I have been found to speak with reason in
 “ other cases, and I now speak advisedly here, that it is positively certain
 “ that not only may the expenditure of quinine not be increased by the general
 “ adoption of my system, but that it may be positively diminished almost to
 “ nothing by the substitution of liquor arsenicalis, and with no increase of
 “ mortality, perhaps a diminution ; for I have good reason for thinking, that
 “ arsenic, besides curing the present fever, gives immunity to the constitution
 “ from future attacks, which quinine does not. I have strong evidence to
 “ induce me to believe this. I have many patients in the 70th, whom, I am
 “ sure, I have saved from dangerous relapses by its use, besides similar experi-
 “ ence before I came here. There are some cases, however, fortunately rare
 “ in our Army practice, where quinine can never be dispensed with. They
 “ are, where the patient has been neglected till he is almost gone, or where
 “ the fever is of a very bad form, and runs its course in a few hours. In such
 “ cases, which can never be frequent, we must, I think, continue to give quinine,
 “ for we cannot give so large and quickly repeated doses of arsenic, as we can
 “ of quinine, to saturate the system in a few hours, and stop the fever soon
 “ enough to save life.”

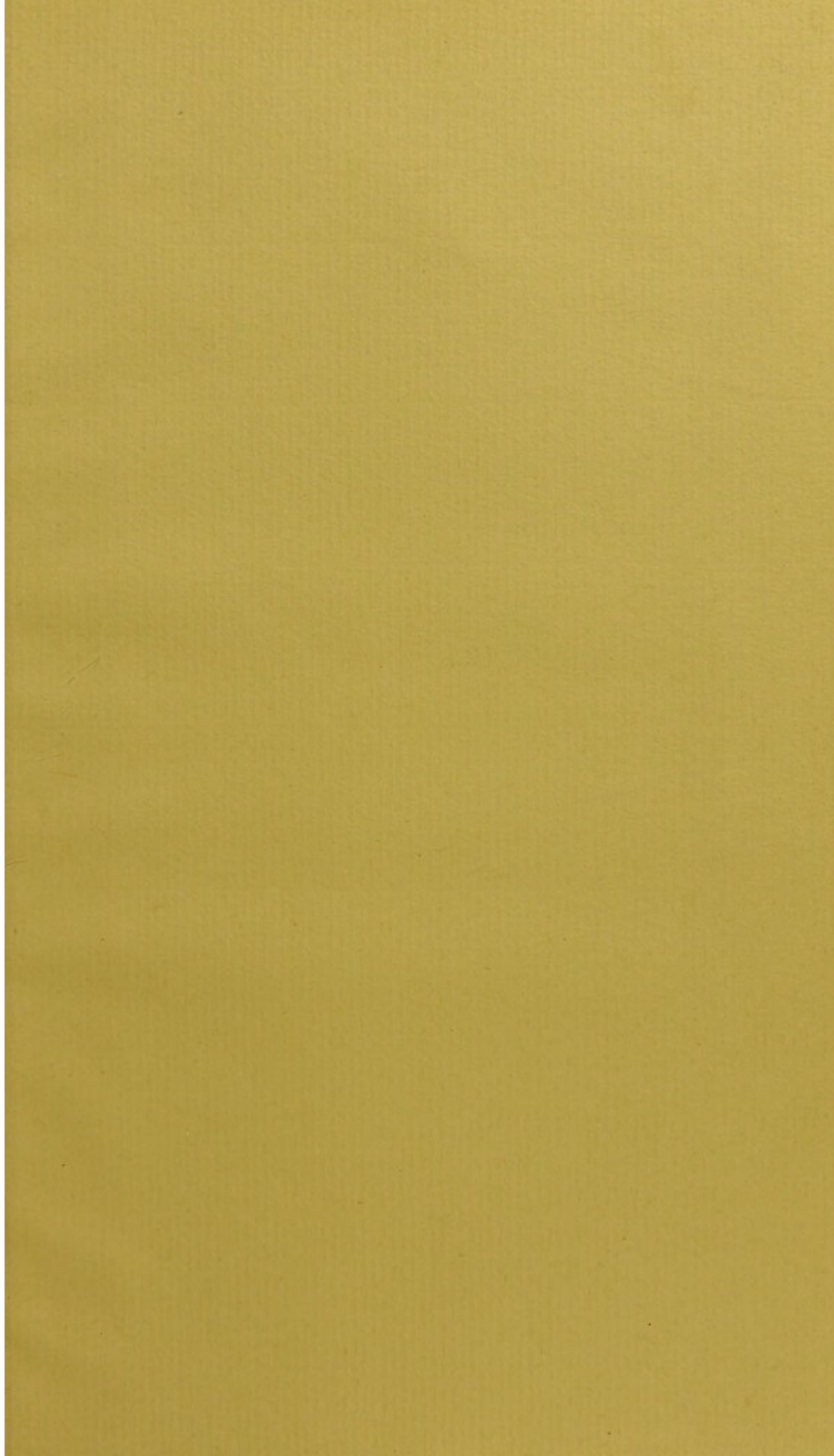




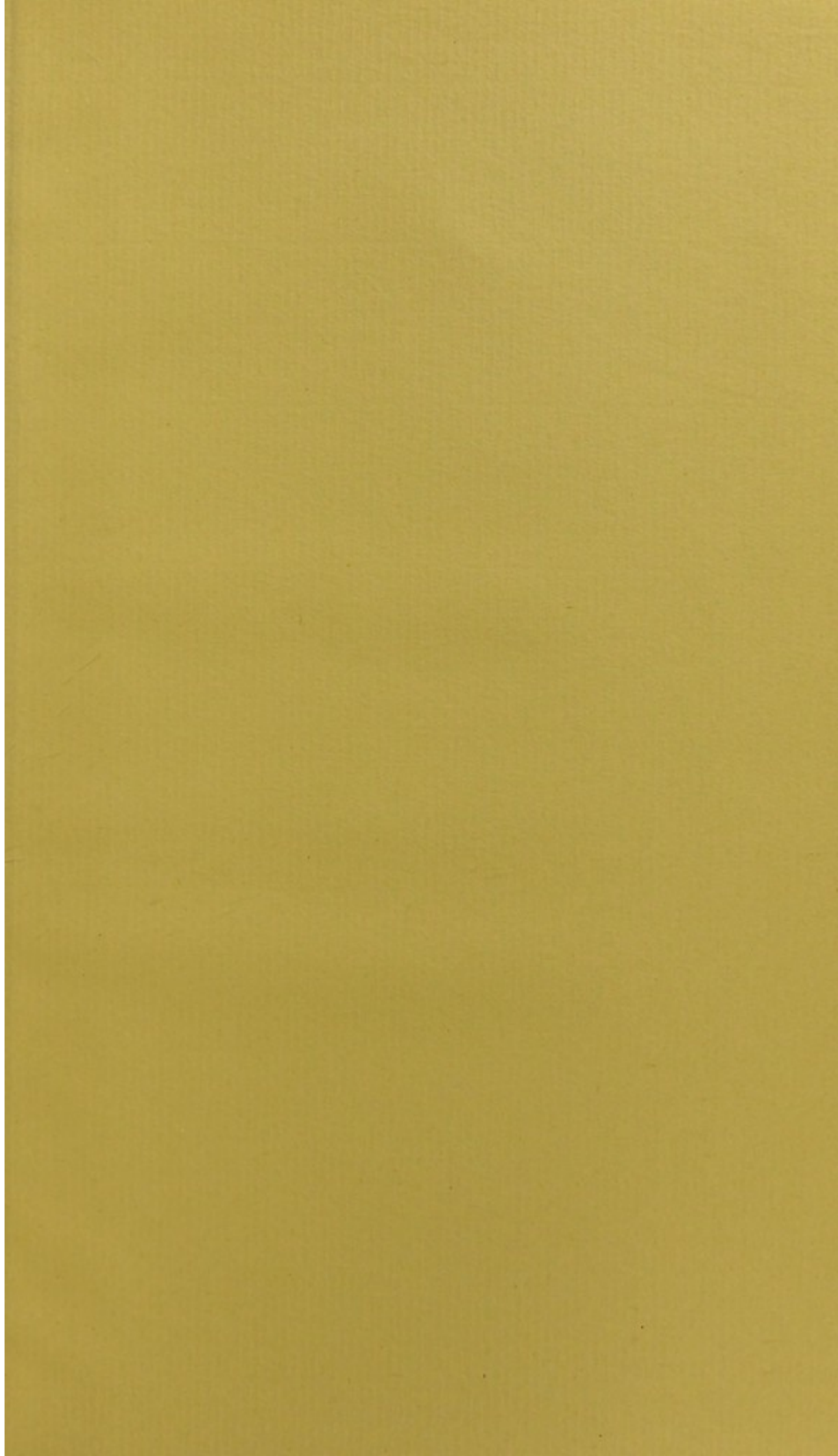


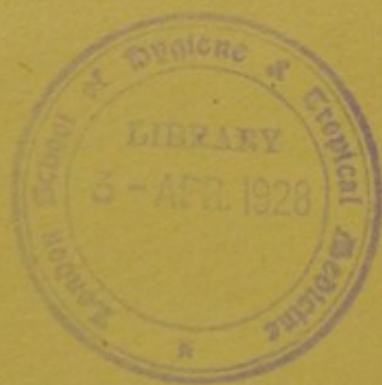












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